2017

Essay on Incentives, Economic Conditions, and Human Capital Formation

Masayuki Onda
Louisiana State University and Agricultural and Mechanical College

ESSAYS ON INCENTIVES, ECONOMIC CONDITIONS, AND HUMAN CAPITAL FORMATION

A Dissertation

Submitted to the Graduate Faculty of the
Louisiana State University and
Agricultural and Mechanical College
in partial fulfillment of the
requirements for the degree of
Doctor of Philosophy

in

The Department of Economics

by
Masayuki Onda
B.S., Hosei University, 2004
M.S., Yokohama National University, 2012
M.S., Louisiana State University, 2014
August 2017

Dedicated to my wife, Shizuko, and son, Kai

Acknowledgments

I would like to thank my adviser, Professor Ozkan Eren under who I have been studying for four years. I hope that this dissertation can prove that I am a qualified scholar.

I am grateful to the rest of my committee members, Professor Areendam Chanda and Professor Carter Hill, for their invaluable advice and the Dean's representative, Professor Margaret-Mary Sulentic Dowell. This dissertation would not be possible without them. I also would like to thank Professor Douglas McMillin, Professor Robert Newman, and Ms. Judy Collins. They are my living encyclopedia at LSU. I would also like to thank my coauthor, Professor Bulent Unel, for sharing his splendid insight into economics.

I finally would like to thank my friends, Guangli Lu, Bahadir Dursun, Sujana Kabiraj, Sara Oloomi, Yuangbo Zheng, Dachao Ruan, and Ting Wang.

Table of Contents

Abstract

In this dissertation, I offer three independent studies. The first examines the impact of foreign direct investment (FDI) on entrepreneurial activities over the period 1996-2008. We find that FDI has no discernible effect on entrepreneurial activity in probusiness states identified by the existence of Right-to-Work (RTW) states. In non-RTW states, however, we find that an increase in FDI decreases the average monthly rate of business creation and destruction.

The second study assesses the impact of breastfeeding on early childhood outcomes. Using Birth Cohort of Early Childhood Longitudinal Survey (ECLS-b) data and employing a recently developed econometric technique, I estimate the upper and lower bounds of the effect of breastfeeding on early childhood health and cognitive ability. I find that even a small fraction of selection on unobservables explains the full effect of breastfeeding on early childhood outcomes.

The third study evaluates the effect of Ban-the-Box (BTB) policy on recidivism. BTB policies restrict employers from conducting background checks on employment applications and delay them until interviews are completed. I examine if BTB policies prevent ex-offenders from returning to prison. Using the National Correctional Reporting Program 2000-2014 dataset in a differences-in-differences framework, I find that BTB policies reduce one-year rates of recidivism. I also observe a large reduction in recidivism for black males in BTB counties but do not detect any evidence for females. Finally, I find that employment opportunities in industries which employ more ex-offenders are complements with BTB policies.

Chapter 1. Introduction

This dissertation presents three independent manuscripts focusing on U.S. society. Chapter 2 explores the impact of foreign direct investment (FDI) on entrepreneurial activities at the individual-owners level during the period 1996-2008. I investigate the differential effect of FDI in pro- and non pro-business states distinguished by Right-to-Work (RTW) and non RTW status. Chapter 3 examines the causal impact of breastfeeding on early childhood outcomes. I use Altonji, Elder, and Taber's (2005) econometric approach to test sensitivity of the effect of breastfeeding. Chapter 4 estimates the effect of Ban-the-Box (BTB) policy on recidivism. Under the parallel trend assumption, my difference-in-differences (DID) estimate identifies the causal effect of BTB. Chapter 5 discusses my findings and concludes.

1.1 Effects of FDI on Entrepreneurial Activity: Evidence from US States.

Foreign Direct Investment (FDI) has been flowing into the United States and a number of foreign companies in operation employ millions of Americans whose parents were formerly small business owners. Americans identify as independent, self-sustaining, and entrepreneurial, so it is interesting to ask: how would Americans respond to a massive inflow of FDI over the last half a century? Would they change their responses to alternative job opportunities created by foreign companies under the pro- and non pro-business environments? Existing theoretical studies yield mixed predictions (Grossman, 1984: Rodriguez-Clare, 1996: Markusen and Venables, 1999). Hence, an answer to this question depends on empirical evidence. We address these questions by using a variety of available public data.

We have two empirical challenges in identifying the causal effect of FDI. The first challenge is lack of universal metrics for entrepreneurial activity, particularly at the individual-owner level. To address this issue, we employ the Kauffman Index of Entrepreneurship micro

dataset, which measures the flow of business creation (Fairlie, 2014). In addition, referring to Fairlie's (2014) definition of business creation, we create rate of business destruction using Current Population Survey dataset. The second challenge is a potential association between unobserved state characteristics simultaneously influencing entrepreneurial activities and FDI. To partial out state characteristics and macroeconomic factors, we control for state and year fixed effects. Furthermore, we control for state-specific linear time trends to adjust for trends affecting entrepreneurial activities. As long as the unobserved characteristics associated with entrepreneurial activity would remain constant from a state's trend when FDI varied from trend, we can identify the reliable estimate of FDI on entrepreneurial activities.

We do not detect discernible evidence that FDI affects business creation and destruction rates for contagious U.S. during 1996-2008. However, we find negative effects of FDI on business creation and destruction in non pro-business states, separating RTW and non-RTW states - proxies for pro- and non pro-business environments. With certain tests, our results are robust.

The most plausible explanation is a difference in the expected opportunity cost of starting up business. Non RTW states are characterized by an inflexible labor market and high wage rates. Higher FDI creates more wage jobs with better job security. Workers in non RTW states are induced to select these jobs over starting up businesses. Consequently, the pool of entrepreneurs in non-RTW states shrinks, reducing competition among existing entrepreneurs. This lowers the rate of business destruction in non RTW states.

1.2 Breastfeeding and Early Childhood Outcomes: Is There a Causal Relationship?

This research topic originates from my personal experience. A few months prior to our son's delivery, my wife stated that breastfeeding would be generally beneficial in our child's development but also would require substantial amount of time. I knew that breastfeeding

was beneficial in developing countries due to the inferior hygiene and underdeveloped social infrastructure, but had no idea what would be the causal effects of breastfeeding on early childhood outcomes in the United States.

Supported by federal agencies and non-profit organizations promoting breastfeeding, the incidence of breastfeeding has been steadily increasing over the last three decades in the United States. However, the basis for a causal mechanism is arguably weak because the empirical conclusion is mostly inferred from observed variation in breastfeeding incidence collected by a large-scale survey. The lack of evidence supported by experimental and quasi-experimental approaches questions if the positive effects of breastfeeding are causal.

I have two objectives in my second chapter. Using Early Child Longitudinal Survey of Birth Cohort (ECLS-B), I examine if we observe positive associations between breastfeeding and early childhood outcomes. Next, I conduct a sensitivity check developed by Altonji et al. (2005) under the assumption of equality of observables and unobservables.

I find the results consistent with those of the observational studies. The estimates of breastfeeding from regressions are mostly significant and positive on early childhood outcomes. My sensitivity analysis, however, show that the lower bound of the effect of breastfeeding is not different from zero at the conventional level and even negative for some outcomes. I further confirm that only 10 percent of unobserved characteristics relative to observed characteristics is sufficient to account for the positive effect of breastfeeding on early childhood outcomes.

1.3 Ban the Box and Recidivism.

Many Americans have past criminal histories and 650,000 ex-offenders are returning to society from federal and state prisons every year. Most job application forms in the United States ask if an applicant has been convicted of a crime and, if the box for yes has been ticked, the application tends to be discarded at the initial screening stage. Ban-the-Box

(BTB) policies restrict employers from asking about applicants' criminal histories on job applications and delay background checks until interviews are completed. Recent studies on BTB policy have focused on its effect on labor market outcomes, particularly statistical discrimination. However, there are few studies investigating the effect of BTB policy on rates of recidivism.

I estimate the effect of BTB policy for offenders recently released using the National Correctional Reporting Program (NCRP) 2000-2014 dataset (Bureau of Justice Statistics, 2016). NCRP data is a rich panel dataset of prison terms for the same offender, and also contains information on the county where the sentence was imposed. Under the assumption that the county of an offender's sentence is the county where he/she resides upon release from prison, I aggregate a dataset of millions of first-sentence ex-offenders to create recidivism variable, rates of recidivism at the year-county level. I then assign adoption of BTB policy to counties across time. I align our research samples with the two age groups on which existing studies focused, age 18-65 (working-age) and age 25-34 (young) offenders.

I employ a difference-in-differences (DID) approach to identify the effect of BTB on one-year rates of recidivism. As long as that the average rate of recidivism in BTB counties would evolve similarly in non-BTB counties over the sample period with no BTB policy, my empirical model captures the causal effects of BTB. I test the validity of my identification assumption by estimating a main specification augmented with leads and lags of BTB adoption.

I find a large and significant effect of BTB on recidivism. The estimated effects relative to the pre-adoption means are a 29% reduction for working-age ex-offenders and a 24% reduction for young ex-offenders. My results also indicate that the effect of BTB policy grows substantially over time upon its adoption: The reduction grows from 3.4 percentage points in the first BTB post-adoption year to 7.4-8.1 percentage points in the following years. Further examination on heterogeneous effects reveals that BTB policies disproportionately prevent Black male ex-offenders from returning to prison but benefit little female ex-offenders

or young ex-offenders in highly educated counties. In a labor market analysis, we find that, as Doleac and Hansen (2016) report, BTB laws induce employers to statistically discriminate against Blacks in the working age and young populations. We also find that employment opportunities in industries which employ more ex-offenders are complement with BTB policies to prevent ex-offenders from returning to prison: The estimated effect in the post-BTB counties is a 3.1% reduction in recidivism for young ex-offenders.

Chapter 2. Effects of FDI on Entrepreneurial Activity: Evidence from US States

2.1 Introduction

Technological progress coupled with reductions in trade and investment barriers observed over the last decades have dramatically intensified Foreign Direct Investment (FDI) around the globe. The share of FDI in the world GDP steadily rose from about 0.5 percent in 1980 to about 2.6 percent in 2013 (World Development Indicators, 2015). The growing importance of FDI has also kindled a debate over its implications for economic growth, labor markets, and domestic entrepreneurial activity. The existing literature has extensively investigated the impact of FDI on growth, labor-market outcomes, and technology transfer (Alfaro, 2014). The relationship between FDI and entrepreneurial activity, however, has attracted little attention. Given that many academics and policy makers view entrepreneurial dynamism as a spur to innovation and growth, a route out of poverty and labor market discrimination, absence of a detailed and robust empirical analysis seems surprising.[1]

Existing literature on the interactions between FDI and entrepreneurial activity has been largely theoretical, and the conclusion from these studies is mixed. The model developed in Grossman 1984 predicts that the inflow of foreign firms crowds out local entrepreneurship because higher earnings prospects in foreign firms induce individuals to choose waged employment. Rodriguez-Clare (1996) proposes a model relating foreign firms with domestic intermediate-good producers through linkages. His model predicts a positive impact of FDI on entrepreneurship when linkages are strong. Markusen and Venables (1999) also reach a similar conclusion.

[1] For example, Haltiwanger et al. (2013) find that small-business start-ups created about 3.5 million net new jobs in the U.S. private sector in 2005. See also Aghion et al. 2014, Hout and Rosen 2000, Fairlie and Robb 2008 and Hunt 2011 for a detailed discussion of entrepreneurial dynamism and its role in the economy.

On empirical side, there are a few notable studies. Barrios et al. (2005), for example, find that FDI has a positive net impact on the creation of local firms in Ireland. Danakol et al. (2014), using business creation rates data across 70 countries over the 2000–2009 period, find a negative effect of FDI on entrepreneurship. Results from the aforementioned theoretical models coupled with limited empirical evidence call for a detailed investigation of the relationship between FDI and entrepreneurial activity.

In this paper, we examine the impact of FDI on entrepreneurship at the individual-owner level in US states over 1996-2008. In doing so, we investigate how the impact differs across states with respect to their right-to-work (RTW) status, a convenient proxy pertaining to states' business climate (Holmes, 1998). In its simplest form, a RTW law states that employees have the right to work without being forced to join to a union. These laws are also associated with several other business friendly policies including but not limited to lower hiring and firing regulations, lax environmental protection, and lower taxes (Holmes, 1998). As discussed further below, differences in the local labor-market conditions (e.g., average wages, labor market flexibility) across these two types of states may affect the opportunity cost of doing entrepreneurial activity.

There are two main empirical challenges to estimating the causal effect of FDI on business creation and destruction. First, the identification of entrepreneurial activity at any level is a difficult task. The main problem stems from the distinction between employer and non-employer firms. For example, Business Employment Dynamics (complied by the U.S. Bureau of Labor Statistics) focuses only on the former and measures entrepreneurial activity by employer establishment birth and death rates. However, employer-based counts represent only a small share of all entrepreneurial activity (Fairlie, 2014). In this paper, we use Fairlie's (2014) Kauffman Entrepreneurship micro-data files, which cover both employer and non-employer firms. More precisely, we measure entrepreneurial activity at the individual-owner level as the average monthly rate of non-business owners becoming entrepreneurs (business creation rate), and as the average monthly rate of entrepreneurs exiting from business ownership (business destruction rate).

Second, unobserved variables that may affect the level of FDI may also be correlated with entrepreneurial activity. As such, in order to identify the causal effect, it is imperative to consider arguably exogenous variation in the flow of FDI. Our empirical analysis uses state-level data and our estimates are based on regression specifications that control for state level fixed effects, state-specific time trends, and year fixed effects. Under the assumption that unobservable variables related to entrepreneurial activity do not deviate from a state's trend when FDI deviates from trend, we can uncover the causal effect of FDI on business creation and destruction. Falsification tests presented throughout the text provide supportive evidence for the validity of our identifying assumption.

Using data from several sources over the 1996-2008 period, we reach the following empirical conclusions. The results from the full sample indicate a negative but barely significant effect of FDI on average monthly business creation and destruction rates. When we restrict the sample to RTW states, we do not observe any effects of FDI on entrepreneurial activity. In non-RTW states, however, an increase in FDI significantly decreases both the business creation and destruction rates. Specifically, we find that a 10 percent increase in FDI decreases average monthly rate of business creation and destruction by roughly 4 and 2.5 percent (relative to sample mean), respectively. Our estimates further imply that the net impact of FDI on entrepreneurial activity in non-RTW states is positive. For example, using population numbers from the 2010 CPS-ORG, we show that a 10 percent increase in FDI let about 17,500 entrepreneurs stay in business. We carefully examine several potential explanations that may drive the results, and find that an explanation related to differences in the opportunity costs of occupational choices in RTW and non-RTW states is more convincing. Several robustness checks support the baseline results of the paper.

This paper is related to a large literature on entrepreneurship. Several studies have analyzed the determinants of business formation.[2] We contribute to this literature by

[2]Among many others, see Holtz-Eakin et al. (1994), Blanchflower and Oswald (1998), Fairlie (1999), Hout and Rosen (2000), Cullen and Gordon (2007), Hall and Woodward (2010), and Dinopoulos and Unel (2015).

examining the existence of a causal link between FDI and entrepreneurship. This paper also relates to a large empirical literature on FDI and its effects on economic outcomes.[3] We show that the effect of FDI on entrepreneurial activity depends on local labor market conditions. Finally, this paper complements the literature on RTW laws and their role in state economies.[4] Our findings may shed light on legislative decisions in states where the debate over RTW laws is currently intense (e.g., West Virginia).

The rest of the paper is organized as follows. The next section reviews RTW laws and compares main economic characteristics across states. Section 3 discusses the data and Section 4 describes the details of the econometric methodology used in the paper. Section 5 presents the results, provides a discussion of potential channels leading to our findings, and presents several robustness checks. Section 6 concludes.

2.2 Right-to-Work Laws

In 1935, with the passage of the Wagner Act, the U.S. Congress granted organized labor statutory sanction to fire employees for refusal to join a union. The law gave rise to a movement to curb the additional power bestowed upon unions at the state level. The 1947 Taft-Hartley amendments to the 1935 Wagner Act granted states the power to ban the union shop (the so-called Right-to-Work laws), a contract provision that requires all employees to join and pay dues to the union. By the time of the Taft-Hartley Act, five states had already passed such laws (Arkansas and Florida in 1944, and Arizona, Nebraska and South Dakota in 1946). Since then and until the most recent cases of Indiana and Michigan in 2012 and Wisconsin in 2015, twenty-two states have passed RTW laws.[5]

[3]This literature is vast, see Yeaple (2013) and Alfaro (2014) for comprehensive reviews.

[4]See, for example, Hirsch (1980), Ellwood and Fine (1987), Holmes (1998), Farber (2005), and Eren and Ozbeklik (2016).

[5]These states are Alabama, Arizona, Arkansas, Florida, Georgia, Idaho, Iowa, Kansas, Louisiana, Mississippi, Nebraska, Nevada, North Carolina, North Dakota, Oklahoma, South Carolina, South Dakota,

RTW states are perceived as pro-business because of their attitudes towards labor unions as well as their other business friendly policies such as lax environmental regulations and lower taxes (Holmes, 1998). There are striking differences between RTW and non-RTW states in terms of economic progress. For example, relative to non-RTW states, RTW states annually grew by 0.6 percentage points faster over the last three decades. RTW states have also generated higher growth in employment, and have generally experienced lower unemployment rate.[6] Holmes (1998) shows that, on average, manufacturing activity (measured by its share of total employment) increases abruptly by about one-third when one crosses the border from a non-RTW state to a RTW state.

Particularly important for our study are the labor market flexibility and wage structures (which together determine the opportunity cost of becoming an entrepreneur) that prevailed in RTW and non-RTW states. Figure 1 displays the average-market flexibility index over our sample analysis period (1996-2008) and it measures the intensity of hiring and firing regulations, centralized collective bargaining, minimum wages, working hours regulations, etc. (Stansel et al., 2015). Higher values of the index indicate more flexible labor markets; as such, labor markets have always been more flexible in RTW states.

Figure 2 plots the real hourly wages (in 2009 dollars) over the same period obtained from micro-level data (Current Population Survey files) after having purged out the effects of individual characteristics (i.e., age, race, gender and educational attainment) and time.[7] As shown in the figure, average wages have always been higher in non-RTW states. Among several other factors, stronger labor unions and more rigid local labor market conditions arguably have played an important role in these wage differentials (Holmes, 1998; Eren and Ozbeklik, 2016).[8]

Tennessee, Texas, Utah, Virginia, and Wyoming. Figure A.1 in the appendix shows all RTW states and the years these laws were enacted.

[6]These statistics are obtained using data from the U.S. Bureau of Economic Analysis and Current Population Survey monthly files.

[7]Raw data trends are very similar to those presented in the text and are available upon request.

[8]See also Hirsch (1980), Ellwood and Fine (1987) and Farber (2005) for detailed investigation of RTW

2.3 Data and Descriptive Statistics

The data used in this study are taken from several publicly available sources covering a time span from 1996 to 2008. Entrepreneurial activity is measured by (i) the rate of business creation, and (ii) the rate of business destruction, both of which are at the individual-owner level. The data for business creation are drawn from Fairlie's (2014) Kauffman Entrepreneurship Micro-Data Files (1996 and onwards), which are constructed using the Current Population Survey Outgoing Rotation Group (CPS-ORG) monthly files.[9] The data for each year covers more than 650,000 observations from the U.S. adult population ages 20 to 64 years old and includes basic demographic information (i.e., gender, race and age), educational background, and labor market information (i.e., employment status, worker class, hours worked per week, etc.).

Self-employed individuals who own a business as their main job and work at least 15 hours per week are considered entrepreneurs in the CPS-ORG data.[10] To identify business creation (or destruction) at the individual-owner level, individuals must be tracked over time. The CPS is a household survey and unfortunately it does not include individual identifiers. However, respondents in the CPS-ORG data are surveyed on a monthly basis for four consecutive months.[11] Exploiting this feature of the data, Fairlie (2014) matches individuals over two consecutive months using information on household ID, household number, record lines, survey month, sex, race, and age.[12] He then identifies new entrepreneurs as those individuals who do not own a business as their main job in one survey month but become a business owner in the subsequent month. In a similar fashion, we also identify exiting

laws and state economies.

[9]Due to revisions in the household identifiers implemented to protect the confidentiality of survey respondents, a matching algorithm cannot be performed to several earlier years (e.g., 1985, 1994 and 1995) of the CPS-ORG data (Madrian and Lefgren, 1999; Fairlie, 2014).

[10]Individuals with imputed information on worker class (i.e., self-employed or not) and hours of work are excluded. The results presented in the paper remain unchanged if we do not exclude imputed observations (which constitutes less than 3.5 percent of the effective sample).

[11]Each household is resurveyed for another four consecutive months a year later and then leaves the sample permanently.

[12]The success rate of this matching procedure from 1996 and onwards is roughly 95 percent (Fairlie,2014).

entrepreneurs as those individuals who are entrepreneurs in one survey month but become a non-business owner in the next month.

For each state and year, we then calculate the average business creation rate as the weighted monthly fraction of non-business owners who created new businesses, and the average business destruction rate as the weighted monthly fraction of entrepreneurs who became non-business owners. In all calculations CPS weights are used. It is important to emphasize that we are able to replicate the business creation rate, the so-called Kaufmann Entrepreneurial Activity Index reported in Fairlie (2014). Our ability to precisely replicate Kauffman Entrepreneurial Activity Index for each year and state provides validity to our construction of the business destruction index. Finally, we do not distinguish between the creation of high-growth potential businesses (i.e., opportunity entrepreneurship) and individuals starting businesses because of limited job opportunities (i.e., necessity entrepreneurship) in our construction of a business creation index. Dropping new entrepreneurs coming out of unemployment and limiting our attention to only opportunity entrepreneurship do not affect the estimated effects presented throughout the text. All these results are available upon request.

Table 1 reports the basic summary statistics of entrepreneurial activity for all states (Column 1) and by RTW status (Columns 2 and 3, respectively) over the sample period.[13] The first row presents the average number of entrepreneurs per 1,000 adults, and note that roughly more than 8 percent of the adult population is entrepreneurs. Looking at the second row of Table 1, we see that the average monthly rate of business creation per 1,000 non-business owners is 3 adults (0.3 percent). This fraction is slightly higher in the RTW states. Specifically, 3.1 and 2.8 adults out of 1,000 non-business owners created new businesses each month in the RTW and non-RTW states, respectively. We also see that about 80 out of 1,000 entrepreneurs (8 percent) shut down their businesses each month over the sample period (Row 3, Table 1).[14]

[13] States enacting RTW laws after 2008 are considered as non-RTW states in the data.

[14] One might wonder why we do not differentiate between high-growth and individual business. The rate of

Figure 3.a displays the data trends in the average monthly rate of business creation from 1996 to 2008. We do not observe any discernible patterns in trends across states. That being said, the average business creation rate is almost always higher in RTW states than that in non-RTW states. Turning to time series for business destruction, we observe an upward trend in these rates (Figure 3.b). As such, the average business destruction rate increased more than 30 percent from 1996 to 2008 for both RTW and non-RTW states.[15]

Annual data on Foreign Direct Investment (FDI) inflows come from the U.S. Bureau of Economic Analysis (BEA), and cover the financial structure and operations of non-bank U.S. affiliates of foreign direct investors.[16] FDI data are measured by the total value of foreign investment in property, plant, and equipment, and are available at state level over the 1977–2007 period. We use state-level GDP deflators (from the BEA) to convert nominal values into millions of chained 2009 dollars.[17] The last row of Table 1 presents the basic summary measures and Figure 4 displays the log average real FDI separately for RTW and non-RTW states. FDI inflows exhibit similar trends across the states and it is interesting to note that non-RTW states attracted more FDI than RTW states.[18] Unfortunately, as we discuss further below, FDI data on several sub-sectors are not available for a large number of states and there is also not a uniform industry level classification of FDI over our sample period. We can only consistently disaggregate total FDI into two parts as manufacturing and non-manufacturing FDI. Based on this crude classification, perhaps suprisingly, the

business creation is measured at the individual-owner level using Current Population Survey, so separating high-growth business including computer software industries and individual business is infeasible. See Fairlie (2014) for more details.

[15]We calculated entrepreneurial activity up to 2013. The average business creation rate shows a sharp decline beginning in mid-2009 for both RTW and non-RTW states. The average business destruction rate has been somewhat stable for RTW states from 2009 and onwards, while it showed a reversal trend in non-RTW states following the years after the Great Recession.

[16]A U.S. affiliate is a business enterprise in which foreign investors have 10 percent or more of the voting securities or an equivalent interest.

[17]The results of the paper remain the same if we instead use the aggregate price index for fixed non-residential investment to convert nominal values into real terms.

[18]The sharp decline in FDI observed between 2000 and 2002 mainly reflects the 2000 stock market crash and the recession following it. During our sample period, California and New York are the top two non-RTW states that attracted most of the FDI, while Texas was ranked the top across RTW states.

fraction of manufacturing FDI is somewhat similar (48 and 43 percent of total FDI are in manufacturing sector) in RTW and non-RTW states, respectively.

In some of the estimations reported below, we also control for a variety of time-variant state characteristics obtained from various sources. Specifically, our regression specifications control for log real gross domestic product, log population, unemployment rate, real minimum wage, and lag value of percent of adult population that were self-employed. State economic variables are obtained from the BEA, while the fraction of entrepreneurs comes from Fairlie (2014). We also include the percent of the state's adult population that are white, male, and college graduates, as well as the private sector unionization rates, all of which are extracted from CPS-ORG files. Finally, a composite-tax index from Stansel et al. (2015) is added to our specifications. The index measures the intensity of takings and discriminatory taxes at the state level constructed from data on tax revenue as a percentage of state GDP, top marginal income tax rate, indirect tax revenue as a percentage of state GDP, and sales taxes collected as a percentage of state GDP.[19]

2.4 Empirical Methodology

To obtain the effects of FDI on entrepreneurial activity, we estimate a regression of the following form

$$E_{st} = \sum_{j=1}^{2} \beta_j FDI_{st-j} + \alpha_t + \alpha_s + \alpha_s t + X'_{st}\lambda + \varepsilon_{st} \tag{2.1}$$

where E_{st} is the the rate of business creation or destruction (i.e., entrepreneurial activity) expressed per 1,000 adults in state s and year t, FDI_{st-j} denotes lag values (up to two lags) of log FDI, α_t are year fixed effects to capture changes occurring across all states, α_s are state

[19]Using the Panel Study of Income Dynamics for the 1978-1993 period, Gentry and Hubbard (2000) estimate the effects of tax system on the likelihood of becoming self-employed. They find that progressive marginal tax rates discourage entry into self-employment and business ownership. In another study, Desai et al. (2011) investigate the extent to which tax deferral policies affect U.S. direct investment abroad, and find that, over the 1982–2010 period, these policies made foreign investment dynamically inefficient.

fixed effects to control for time-invariant state characteristics, $\alpha_s t$ are state-specific time trends to control for linearly trending state characteristics, X_{st} is the set of time-varying control variables and ε_{st} is the error term. We opt out of using contemporaneous FDI to circumvent concerns stemming from reverse causality. Besides, FDI may take time to manifest itself. Employing different functional forms such as using FDI per capita rather than total FDI has no significant impact on the point estimates reported throughout the paper.

The key identifying assumption underlying this framework is that unobservable variables that are related to the outcome do not deviate from a state's trend when its lagged FDI deviates from trend. Under this assumption, the coefficient estimates of $\beta_j s$ can be interpreted as the causal impact of FDI on entrepreneurial activity. We provide several sensitivity checks (e.g., falsification test) throughout the text regarding the validity of this assumption.

To minimize any potential contamination in inference procedure that may arise due to a small number of clusters (22 RTW and 28 non-RTW states), we obtain $p-$values associated with test of significance for each coefficient estimate using the wild bootstrap t-procedure clustered at the state level (Cameron et al., cgm2008).

2.5 Empirical Results

2.5.1 Baseline Results

We report the effects of FDI on the average monthly business creation rate in Table 2. We present the results for all states and by RTW status of the states. Columns 1-3 provide the results by only including state and year fixed effects and state-specific time trends, while Columns 4-6 report the results by adding time-variant state controls. In Columns 1 and 4, we report the effects of FDI using only lag one, while we use lag two in Columns 2 and 5, and finally, we include lag one and two at the same time in Columns 3 and 6. The $p-$values

obtained from 999 wild bootstrap repetitions clustered at the state level are reported beneath each coefficient estimate.

Focusing first on all states, we do not find any strong effect of FDI on business creation rate (Panel A, Table 2). The reported coefficients are all negative but they are not different from zero at the 5 percent level. Similar to full sample results, we continue to observe no effect of FDI in RTW states with generally positive point estimates (Panel B). As for non-RTW states, however, the coefficients (in absolute value) are considerably larger in magnitude and the estimate on lag two is highly significant (Column 2, Panel C). The effect size is -1.1, which implies that a 10 percent increase in FDI decreases the average monthly rate of business creation by 4 percent, relative to its sample mean. Including both lags has virtually no impact on the estimated effects (Column 3).

Columns 4-6 provide the same set of estimates but this time additionally including all the time-variant state characteristics described in Section 3. Note that the point estimates are very similar in magnitude to those reported in Columns 1-3. The insensitivity of our coefficient estimates to the inclusion of a relatively rich set of covariates provides some assurance to our identifying assumption.

Our next set of results pertain to average monthly rate of business destruction. Table 3 presents the estimated effects of FDI. We do not observe any impact of FDI on business destruction when we consider all states (Panel A) or only RTW states (Panel B). Turning to non-RTW states, however, we once again observe statistically significant and non-negligible effects of FDI. It appears that both the first and second lags are precisely estimated when entered one at a time, while it is only the first lag of FDI that yields a statistically significant coefficient estimate when we add them at the same time to the model specification. The effect size from lag one is -19.968 (Column 3, Panel C, Table 3). This implies that a 10 percent increase in FDI decreases the average monthly rate of business destruction by roughly 2.5

percent, relative to its sample mean. Controlling for state characteristics has little effect on the estimated effects of FDI on business destruction (Columns 4-6).

Combining the estimates of the effects of FDI from average monthly business creation and destruction specifications in non-RTW states, our calculations imply that the net impact of FDI on the level of entrepreneurial activity is positive. To see this, let P_t denote the adult population (in 1,000) of non-RTW states in period t, and e_t the fraction of entrepreneurs in the population. Using our estimates, a 10 percent increase in FDI decreases the number of businesses created and the number of business destroyed by $1.1(1 - e_t)P_t$ and $20e_tP_t$, respectively. The net effect of FDI on entrepreneurial activity then is $20e_tP_t - 1.1(1-e_t)P_t = (21.1e_t - 1.1)P_t$, and note that this effect is always positive as long as $e_t \geqslant 0.052$. That is, an increase in FDI has a net positive impact on the level of entrepreneurial activity if entrepreneurs constitute at least 5.2 percent of the adult population in non-RTW states. The average share of entrepreneurs in non-RTW states is always above 8 percent (see the first row in Table 1).

For example, using data on business and non-business owners population from the 2010 CPS-ORG, we find that a 10 percent increase in FDI translates into roughly 10,500 fewer businesses created and 13,000 fewer business destroyed each month during the year.[20] Consequently, a 10 percent increase in FDI saved about 17,500 entrepreneurs in non-RTW states during 2010.[21]

It is important to emphasize that FDI reduced both business creation and destruction rates in non-RTW states, which might have undesirable economic consequences in these states. Lower business creation may slow down innovation, growth, and job creation driven

[20] The business and non-business owner populations in non-RTW states in 2010 were roughly 93.5 and 6.5 million, respectively. Multiplying these population numbers with the corresponding estimated effects of FDI (Column 3 of Panel C in Tables 2 and 3), we obtained the monthly estimates.

[21] As noted in Fairlie (2014), annual estimates are not twelve times monthly estimates because individuals can potentially become entrepreneurs multiple times within the same year. He suggests that annual rates are about 6 to 8 times monthly ones. In our calculation of the number of entrepreneurs saved during 2010, we multiply the average number of entrepreneurs saved each month (2,500) by 7.

by new entrepreneurs, and lower business destruction may induce inefficient entrepreneurs to stay in the market.

2.5.2 Potential Channels

Stepping back and viewing the complete set of results, we have two main findings. First, there is no effect of FDI on entrepreneurial activity at the individual-owner level in RTW states. Second, our results indicate that an increase in FDI in non-RTW states decreases the average monthly business creation and destruction rates. In this section, partly borrowing from various theoretical models, we shall try to explain the mechanism underlying our findings.

One potential explanation pertains to market stealing effects, which may stem from an increase in product market competition.[22] Foreign firms, on average, are larger and more productive than domestic firms, and thus can strategically charge lower prices which in turn potentially deter new startups.[23] This explanation is not entirely convincing on two accounts. First, more intense product market competition should also lead to displacement of existing entrepreneurs. However, we do not see such a pattern. Second, given the pro-business nature of RTW states, one would expect at least similar effects in RTW states as well.

Another potential explanation is related to the differences in composition of FDI across states. It is conceivable to argue that RTW states predominantly attract FDI where the nature of inflows are not highly correlated with business activity at the individual level. In this case, it may not be surprising to observe a null effect of aggregate FDI on business creation and destruction in RTW states. To further explore this hypothesis, we disaggregate total FDI into two parts: manufacturing FDI (FDI_m) and non-manufacturing FDI (FDI_o). Our choice of disaggregation is driven by data restrictions, and given that manufacturing

[22] Tirole (1988) reviews various strategic actions (such as capacity expansion, advertising, learning-by-doing, etc.) through which firms affect competitive conditions and deter entry into markets.

[23] In an influential paper, Helpman et al. (2004), using U.S. exports and affiliate sales data that cover 52 manufacturing sectors and 38 countries, show that only the most productive firms engage in FDI activity.

FDI plays a more important role in RTW states (48 and 43 percent of total FDI in RTW and non-RTW states, respectively), our crude disaggregation may still provide insights.[24] As such, if our results are driven by differences in composition, one would expect similar effects of manufacturing FDI on entrepreneurial activity across these states. Table 4 reports the results from a slightly modified version of equation (1) where we include manufacturing and non-manufacturing FDI separately. The points estimates on manufacturing FDI for RTW states continue to be imprecisely estimated (Panel A), whereas the estimated effects for non-RTW states are consonant with those presented from Tables 2 and 3 (Panel B). Taking this analysis one step further and excluding states where the nature of manufacturing FDI is potentially different than the rest of the nation (California, New York, and Massachusetts) does not alter the findings (Columns 3 and 4).

A more plausible and convincing explanation for our results comes from a mechanism operating through differences in local labor market conditions in RTW and non-RTW states, and thereby in the opportunity costs associated with different occupational choices (Lucas, 1978; Kihlstrom and Laffont, 1979). Recall that labor markets are less flexible and average wages are always higher in non-RTW states (Figures 1 and 2). The extent of job security coupled with better average pay implies higher expected opportunity cost for an agent to choose entrepreneurship over wage work. FDI inflows create new job opportunities for individuals, and these jobs pay higher wages and provide more job security in non-RTW states, which in turn may induce individuals (risk-averse ones in particular) to become employees. Lower rates of business destruction following FDI would then be a natural extension of this explanation. Decreased supply of small-business owners alleviates the competitive pressure on existing ones and lowers their exit from the market.

To further explore this potential channel, we replace our entrepreneurial activity measures from all sectors with the rate of business creation and destruction occurring in only the manufacturing sector. Under our proposed explanation, one would expect an FDI driven

[24]FDI data on several sub-sectors are not available for a large number of states and there is also not a uniform industry level classification of FDI over our sample period.

decrease in the business creation rate also lowers the business destruction rate. This is indeed what we observe in the data. The point estimates for the first and second lags of FDI on business creation rate in manufacturing sector are negative but less precisely estimated, -1.61 ($p-$value=0.14) and -1.54 ($p-$value=0.60) in non-RTW states, respectively. The corresponding estimates on the rate of average monthly business destruction are -54.64 ($p-$value=0.05) and 20.16 ($p-$value=0.23), respectively.[25] We continue to observe null effects in RTW states.

Our proposed explanation is consonant with recent empirical studies that analyze relation among risk, wage, and entrepreneurship. For example, using data on US companies receiving venture funding between 1987 and 2008, Hall and Woodward (2010) compare the difference between cash rewards that entrepreneurs actually received with the cash that they would have received from a risk free waged job. They find that a venture-backed entrepreneur has a below-market salary and about 75 percent of them receive nothing at exit. Given this nature of entrepreneurship, individuals associated with relatively lower risk aversion and higher initial assets generally become entrepreneurs. A higher risk-free payoff (wages) in non-RTW states may induce more individuals to become wage workers rather than becoming entrepreneurs and facing an uncertain payoff.[26]

2.5.3 Robustness Checks

We implement several sensitivity checks to examine the validity of our results. Tables 5 and 6 present these additional estimates of the effects of FDI for average monthly rate of business creation and destruction, respectively. First, we additionally control for quadratic state-specific time trends in the specifications. The results from this exercise are reported

[25] Our estimates based on non-manufacturing sector are qualitatively similar to the above results.

[26] Several other empirical studies examine the relationship between risk and the supply of entrepreneurs. For example, Olds (2014) finds that the State Child Health Insurance Program (SCHIP) increased incorporated self-employment by 31%. He attributes the surge in self-employment to reduced risk within the household following SCHIP availability.

in the first column of Tables 5 and 6. The point estimates are very similar to our baseline results.

Second, following Duflo 2001, we interact baseline covariates with a linear trend and estimate the following slightly modified version of equation (1)

$$E_{st} = \sum_{j=1}^{2} \beta_j FDI_{st-j} + \alpha_t + \alpha_s + X'_{st}\lambda + t\sum_{k} \delta_k X_{(s,1996),k} + \varepsilon_{st}. \tag{2.2}$$

In doing so, we allow FDI inflows to be related to different underlying time trends in entrepreneurial activity across states, depending on baseline state characteristics. Column 2 of Tables 5 and 6 report the estimates based on equation (2.2) and our conclusions remain intact.

Third, it is conceivable to argue that FDI inflows to the neighboring states are correlated with our variable of interest and entrepreneurial activity. Although we condition on a detailed composite tax index measure in our specifications, a similar omitted variable bias argument can be made if our composite measure does not fully capture the impact of all relevant tax variables. To minimize concerns over these confounding effects, we extend our specifications by including the sum of FDI inflows to the neighboring states, and state-level corporate and average tax rates. The results from this exercise are reported in Column 3 of Tables 5 and 6. The coefficients on FDI measures are unchanged.

Fourth, we include the lagged dependent variable to our baseline model. Doing so has almost no effect on our point estimates (Column 4 of Tables 5 and 6). It is well known that fixed effects models with lagged dependent variables are biased for small T. Our goal in including lagged entrepreneurial activity is only to show that the coefficients on FDI measures remain the same, thereby providing additional evidence that our results do not suffer from omitted variable bias.

Fifth, given the count-based nature of our dependent variable, we re-run our baseline specifications using a Poisson regression model. The results from this alternative specification

are presented in Column 5 of the tables. Specifically, a 10 percent increase in FDI decreases the average monthly rate of business creation by roughly 3.1 percent (Panel B, Table 5), while the same increase in FDI decreases the average monthly rate of business destruction by 2.6 percent (Panel B, Table 6). These results are consistent with those obtained from OLS regressions. We also experiment our analysis with a negative binomial regression and the results remain the same.

Sixth, we perform a falsification test. We replace the lagged values from equation (1) with the lead value of FDI (FDI_{st+1}). Under the assumption that our results are not driven by spurious correlations, one would expect FDI_{st+1} to have no effect on business creation and destruction at time t. The presence of a significant association compromises our identification strategy. Looking at the last column of Panel B of Table 5, the point estimate for non-RTW states flips sign. Therefore, if anything, this implies a positive selection bias, i.e., the estimated effect of FDI on business creation in non-RTW states from Table 2 potentially serves as a lower bound for the unknown true population parameter. Turning to Table 6, the point estimates on the effect of lead FDI value are all highly insignificant and are very small in magnitude. Recall that BLS collected FDI data over the 1977-2007 period. When we run the falsification test, the effective sample is restricted to observations between 1994 and 2006. To have a more fair comparison, we also re-estimate our baseline specifications with this subsample of observations. The corresponding results are similar to those presented in Tables 2 and 3.[27]

Finally, to examine whether our results are driven by one particular state, we estimate equation (1) repeatedly, each time removing one state. Out of a total of 50 regressions, the estimated effects of FDI on business creation rate are always precisely estimated in non-RTW states, whereas the point estimates for the effect in RTW states are all small in magnitude and are not different from zero. We find exactly the same pattern when we consider FDI

[27]More precisely, the estimated effects from lag one and two on business creation in non-RTW states are -0.145 (p-value=0.72) and -0.871 (p-value=0.09), respectively. The estimated effects from lag one and two on business destruction in non-RTW states are -16.054 (p-value=0.01) and 1.178 (p-value=0.82), respectively. No other point estimates are different from zero.

and business destruction rate. We also tried longer lags (e.g., lag three) in our baseline specifications. None of the coefficient estimates on these additional lags are different from zero. Moreover, the estimated coefficient effects are not sensitive to the inclusion of these longer lags. All these additional results are available upon request.

2.6 Conclusion

The growing importance of FDI has also kindled a debate over its implications on economic outcomes. The existing literature has extensively analyzed the impact of FDI on growth, labor market outcomes and technology transfer. Surprisingly, little attention has been devoted to the relationship between FDI and entrepreneurship. In this paper, we examine the effect of FDI on entrepreneurial activity (measured by average monthly business creation and destruction) at the individual-owner level. We utilize RTW status to make a crude but convenient distinction pertaining to states' business climate as well as local labor market conditions. To the extent that unobservable variables related to entrepreneurial activity do not deviate from a state's trend when FDI deviates from the trend, our estimation approach uncovers the causal effect of FDI on business creation and destruction.

Considering all states over the 1996–2008 period, we find that FDI has a negative but barely significant impact on both the average business creation and destruction rates. FDI has no effects on the entrepreneurial activity in RTW states. In non-RTW states, however, we find a negative impact of FDI on business creation and destruction rates. Our results indicate that a 10 percent increase in FDI decreases average monthly rate of business creation and destruction by roughly 4 and 2.5 percent (relative to sample mean), respectively. Surprisingly, our estimates further imply that the net impact of FDI on entrepreneurial activity in non-RTW states is positive. For example, population numbers from the 2010 CPS-ORG imply that a 10 percent increase in FDI kept about 17,500 entrepreneurs in business. Several robustness checks and falsification tests support our findings. We evaluate

several potential channels underlying our findings and propose an explanation related to differences in the opportunity costs of occupational choices in RTW and non-RTW states.

From a policy point of view, it may be misleading to draw a firm conclusion regarding welfare implications of local labor-market policies (operating through entrepreneurial activity) in non-RTW states. Lower business creation rate may eschew several benefits (such as innovation, growth, and job-creation) driven by new entrepreneurs. In addition, lower business destruction rate may reflect a less competitive environment, which adversely affects consumer welfare and induces inefficient entrepreneurs to stay in the market.

Table 1. Summary Statistics on Entrepreneurial Activity and FDI

	All States	RTW States	Non-RTW States
	I	II	III
Entrepreneurs	82.213	84.647	80.301
	(19.604)	(19.596)	(19.423)
Business-Creation	2.916	3.100	2.800
	(0.816)	(0.765)	(0.827)
Business-Destruction	79.445	80.316	78.894
	(13.855)	(14.199)	(13.621)
Log FDI	9.508	9.301	9.670
	(1.089)	(1.134)	(1.021)

Notes: The data on business creation and destruction rates are from Fairlie's (2014) Kauffman Entrepreneurship Micro-Data Files (1996–2008) and the authors' calculations, and FDI data are from the Bureau of Economic Analysis (1995–2007). Numbers in parentheses are standard deviations.

Table 2. Impact of Log FDI on the Average Business-Creation Rate

Variable	Without Controls			With Controls		
	1	2	3	4	5	6
Panel A. All States						
FDI_{t-1}	-0.239		-0.083	-0.244		-0.106
p-value	[0.290]		[0.690]	[0.316]		[0.646]
FDI_{t-2}		-0.452*	-0.416		-0.458*	-0.417
p-value		[0.098]	[0.140]		[0.092]	[0.138]
Panel B. RTW States						
FDI_{t-1}	0.025		-0.088	-0.072		-0.247
p-value	[0.916]		[0.868]	[0.882]		[0.622]
FDI_{t-2}		0.268	0.306		0.511	0.595
p-value		[0.410]	[0.382]		[0.262]	[0.136]
Panel C. Non-RTW States						
FDI_{t-1}	-0.477		-0.104	-0.514		-0.179
p-value	[0.186]		[0.702]	[0.172]		[0.578]
FDI_{t-2}		-1.083***	-1.038***		-1.104***	-1.033***
p-value		[0.004]	[0.004]		[0.000]	[0.000]

Notes: The sample sizes in panel A (All States), panel B (RTW States), and panel C (Non-RTW States) are 600, 264, and 336 observations, respectively. All regressions include state and year fixed effects, and state-specific linear trends. Control variables in Columns 4–6 include state's log real GSP, log population, unemployment rate, real minimum wage, percentage of the state's adult population that are white, male, and college graduates, private sector unionization rate, composite measures of tax, and fraction of adult population that are entrepreneurs at t-1. Numbers in square brackets are p-values obtained from wild bootstrapping (999 repetitions) clustered at the state level; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3. Impact of Log FDI on the Average Business-Destruction Rate

Variable	Without Controls 1	2	3	With Controls 4	5	6
Panel A. All States						
FDI_{t-1}	-7.427		-5.932	-8.654*		-6.739
p-value	[0.142]		[0.268]	[0.064]		[0.172]
FDI_{t-2}		-6.529	-3.989		-8.419*	-5.827
p-value		[0.160]	[0.400]		[0.092]	[0.230]
Panel B. RTW States						
FDI_{t-1}	7.141		7.334	5.054		5.978
p-value	[0.224]		[0.330]	[0.384]		[0.420]
FDI_{t-2}		2.657	-0.518		-1.098	-3.150
p-value		[0.656]	[0.944]		[0.902]	[0.756]
Panel C. Non-RTW States						
FDI_{t-1}	-22.642***		-19.968***	-22.014***		-19.666***
p-value	[0.004]		[0.002]	[0.000]		[0.002]
FDI_{t-2}		-16.020**	-7.438		-14.986**	-7.226
p-value		[0.030]	[0.208]		[0.024]	[0.246]

Notes: The sample sizes in panel A (All States), panel B (RTW States), and panel C (Non-RTW States) are 600, 264, and 336 observations, respectively. All regressions include state and year fixed effects, and state-specific linear trends. Control variables in Columns 4–6 include state's log real GSP, log population, unemployment rate, real minimum wage, percentage of the state's adult population that are white, male, and college graduates, private sector unionization rate, composite measures of tax, and fraction of adult population that are entrepreneurs at t-1. Numbers in square brackets are p-values obtained from wild bootstrapping (999 repetitions) clustered at the state level; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table 4. Impact of Log FDI Composition on Entrepreneurial Activity

			Excluded States: CA, MA, NY	
	Entry	Exit	Entry	Exit
Variable	1	2	3	4
Panel A. RTW States				
$FDI_{m,t-1}$	-0.162	-2.604		
p-value	[0.426]	[0.612]		
$FDI_{m,t-2}$	0.359	-0.376		
p-value	[0.246]	[0.918]		
$FDI_{o,t-1}$	-0.062	4.940		
p-value	[0.824]	[0.210]		
$FDI_{o,t-2}$	0.508	0.675		
p-value	[0.110]	[0.896]		
Panel B. Non- RTW States				
$FDI_{m,t-1}$	0.002	-7.574	0.035	-7.754
p-value	[0.988]	[0.246]	[0.856]	[0.236]
$FDI_{m,t-2}$	-0.615**	-3.013	-0.596**	-3.696
p-value	[0.016]	[0.400]	[0.018]	[0.326]
$FDI_{o,t-1}$	-0.228	-7.656**	-0.215	-8.177**
p-value	[0.362]	[0.046]	[0.406]	[0.046]
$FDI_{o,t-2}$	-0.514*	-2.937	-0.530	-2.951
p-value	[0.078]	[0.530]	[0.132]	[0.534]

Notes: All regressions include state and year fixed effects, state-specific linear trends, and control variables specified in equation (1). Numbers in square brackets are p-values obtained from wild bootstrapping (999 repetitions) clustered at the state level; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table 5. Robustness Checks: Impact of Log FDI on the Average Business-Creation Rate

Variable	Add Quadratic Trend 1	Base Covars Inter. with Time Trend 2	Additional Controls Variables 3	Add Lagged Dependent Variable 4	Poisson Regression 5	Falsification Test 6
Panel A. RTW States						
FDI_{t-1}	-0.245	-0.197	-0.241	-0.269	-0.072	
p-value	[0.622]	[0.692]	[0.584]	[0.580]	[0.539]	
FDI_{t-2}	0.595	0.462	0.517	0.592	0.174	
p-value	[0.140]	[0.198]	[0.234]	[0.270]	[0.168]	
FDI_{t+1}						-0.423
p-value						[0.248]
E_{st-1}				-0.159***		
p-value				[0.000]		
Panel B. Non-RTW States						
FDI_{t-1}	-0.178	0.092	-0.149	-0.254	-0.044	
p-value	[0.578]	[0.816]	[0.624]	[0.442]	[0.673]	
FDI_{t-2}	-1.033***	-0.687**	-1.013***	-1.096***	-0.313***	
p-value	[0.000]	[0.020]	[0.000]	[0.004]	[0.000]	
FDI_{t+1}						0.504
p-value						[0.118]
E_{st-1}				-0.198***		
p-value				[0.000]		

Notes: All regressions include state and year fixed effects, state-specific linear trends. Columns 1-5 also include state's log real GSP, log population, unemployment rate, real minimum wage, percentage of the state's adult population that are white, male, and college graduates, private sector unionization rate, composite measures of tax, and the fraction of adult population that are entrepreneurs at t-1. Numbers in square brackets are p-values obtained from wild bootstrapping (999 repetitions) clustered at the state level for columns 1–4 and 6, and from analytical standard errors clustered at the state level for column 5; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table 6. Robustness Checks: Impact of Log FDI on the Average Business-Destruction Rate

Variable	Add Quadratic Trend 1	Base Covars Inter. with Time Trend 2	Additional Controls Variables 3	Add Lagged Dependent Variable 4	Poisson Regression 5	Falsification Test 6
Panel A. RTW States						
FDI_{t-1}	5.983	7.378	4.206	5.818	0.078	
p-value	[0.420]	[0.322]	[0.526]	[0.428]	[0.403]	
FDI_{t-2}	-3.148	0.426	-0.781	-3.080	-0.053	
p-value	[0.756]	[0.960]	[0.944]	[0.810]	[0.665]	
FDI_{t+1}						-2.351
p-value						[0.758]
E_{st-1}				-0.067***		
p-value				[0.000]		
Panel B. Non-RTW States						
FDI_{t-1}	-19.648***	-16.953***	-18.809***	-19.880***	-0.257***	
p-value	[0.002]	[0.000]	[0.000]	[0.002]	[0.000]	
FDI_{t-2}	-7.219	-0.902	-6.239	-9.867	-0.084	
p-value	[0.246]	[0.858]	[0.268]	[0.254]	[0.229]	
FDI_{t+1}						-2.651
p-value						[0.712]
E_{st-1}				-0.155***		
p-value				0.000]		

Notes: All regressions include state and year fixed effects, state-specific linear trends. Columns 1-5 also include state's log real GSP, log population, unemployment rate, real minimum wage, percentage of the state's adult population that are white, male, and college graduates, private sector unionization rate, composite measures of tax, and the fraction of adult population that are entrepreneurs at t-1. Numbers in square brackets are p-values obtained from wild bootstrapping (999 repetitions) clustered at the state level for columns 1–4 and 6, and from analytical standard errors clustered at the state level for column 5; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

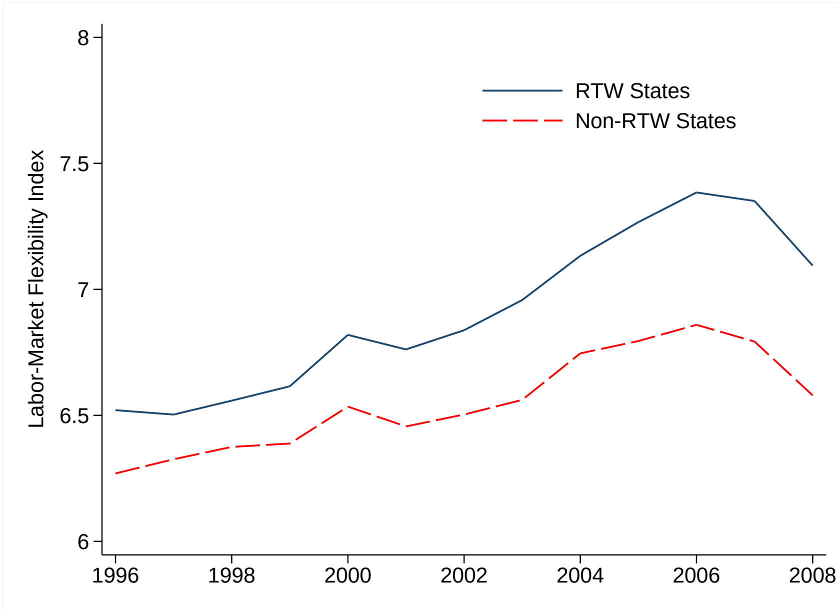


Figure 1: Average Labor-Market Flexibility Index, 1996–2008

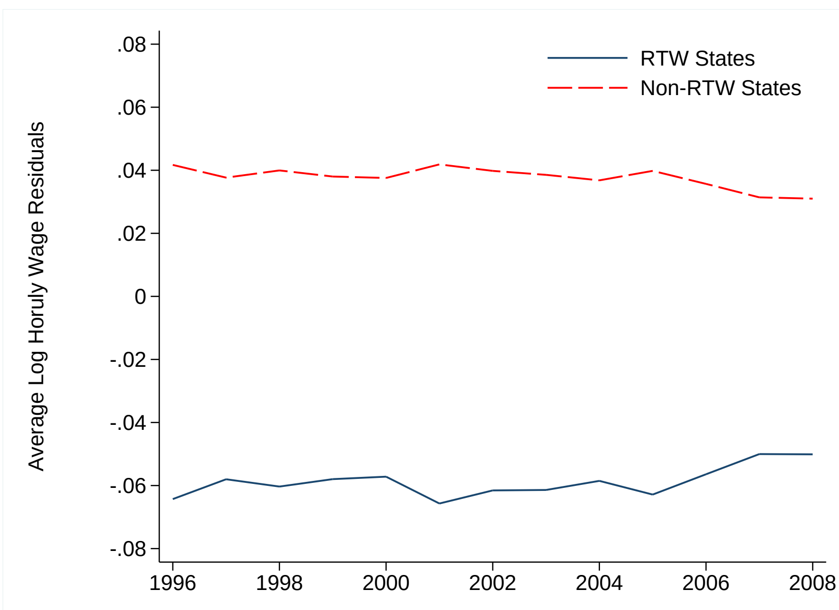
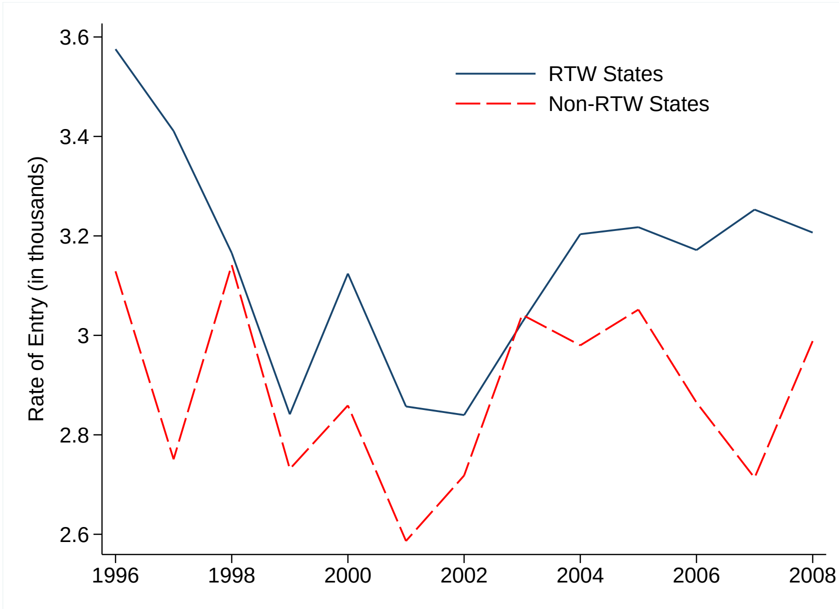
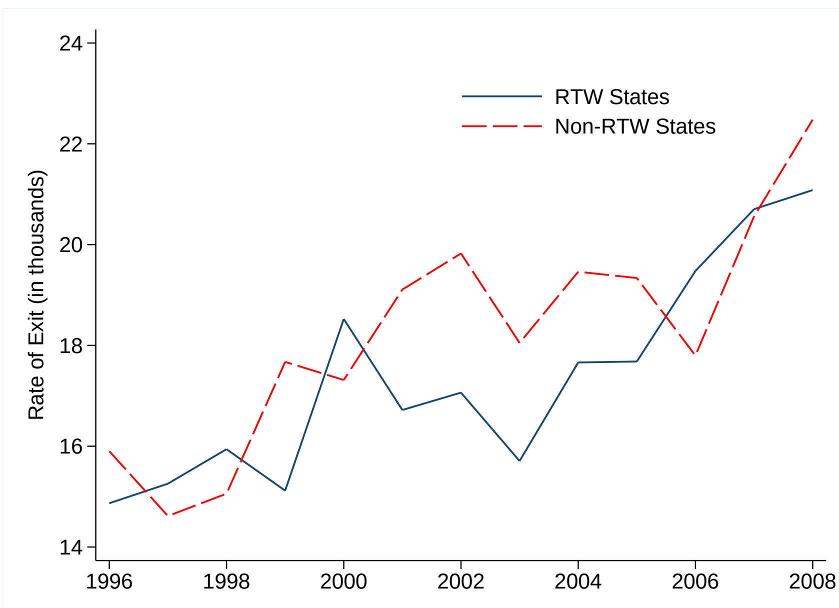


Figure 2: Average Log Hourly Wage Residuals, 1996–2008



a. Rate of Business Creation in 1,000 Non-Business Owners



b. Rate of Business Destruction in 1,000 Entrepreneurs

Figure 3: The Average Rate of Business Creation and Destruction, 1996–2008

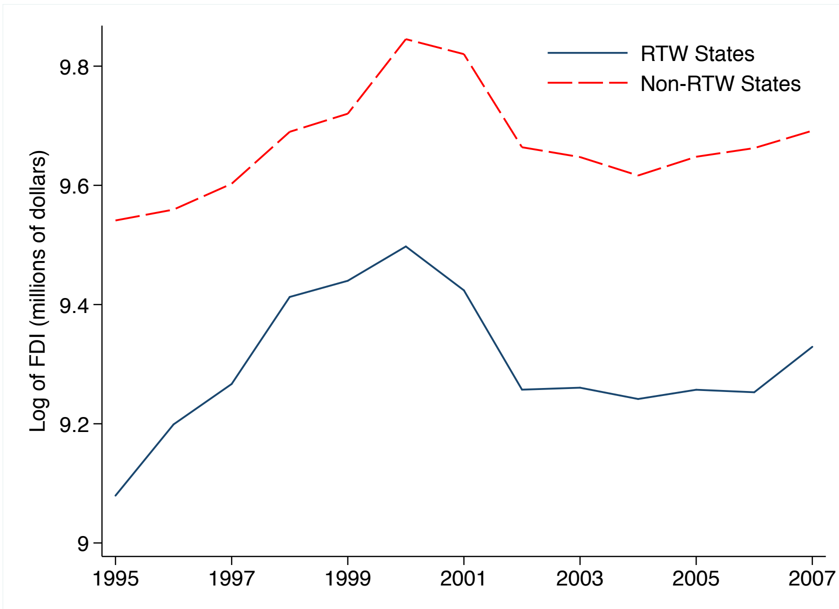


Figure 4: Log FDI (in Millions of Chained 2009 Dollars), 1995–2007

Chapter 3. Breastfeeding and Early Childhood Outcomes: Is There a Causal Relationship?

3.1 Introduction

Breastfeeding is a prime source of nutrients. The observational research suggests that breastfeeding reduces the risk of infection and obesity and advances cognitive development. Referring to studies reporting benefits of breastfeeding on a wide range of child outcomes, the World Health Organization (WHO) and United Nations Children's Emergency Fund (UNICEF) recommend breastfeeding babies for the first six months of life exclusively. Breastfeeding has also been promoted in the United States.[1] Figure 3.1 presents time-series of the incidence of breastfeeding for the period of 2002-2012: the number of U.S. children who were *ever breastfed* and *breastfed at least 6 months* has been increasing to 80.0 and 51.4 percent from 71.4 and 37.9 percent over a decade (Department of Health and Human Service 2015). Healthy People 2020 (2010) aims to achieve an even higher incidence of breastfeeding, 81.9 percent and 60.6 percent for *ever breastfed* and *at least 6 month breastfed* respectively.

The common perception that breastfeeding benefits child outcomes has also been greatly reinforced by the supporting research (Morrow-Tlucak, Haude, and Ernhart 1988; Lucas et al. 1992; Dewey, Heinig, and Nommsen-Rivers 1995; Raisler, Alexander, and O'Campo 1999; Miralles et al. 2006; Belfield and Kelly 2012). However, caution must be warranted. Most studies depend on observed variations in breastfeeding which are arguably endogeneous to the specifications. The lack of evidence supported by valid instrumental variables and randomized control trials raises concerns on the causal effect of breastfeeding on various outcomes. Potential violation of the zero conditional mean assumption - independence of

[1] Government agencies (e.g. the U.S. department of Health and Human Services), professional associations (e.g. the American Academy of Pediatrics), and non profit organizations (e.g. La Leche League International and United States Breastfeeding Committee) promote breastfeeding.

the decision to breastfeed from child outcomes - may lead to misleading inference about the benefits of breastfeeding.

Of all the existing studies in breastfeeding literature, a countable number of research papers investigating the causal impact of breastfeeding on early childhood outcomes report mixed findings at best. Baker and Milligan (2008) find that an increase in mandatory maternity leave in Canada increases a mother's time away from work and breastfeeding duration but has no effect on a child's health outcomes. Using the Birth Cohort of Early Childhood Longitudinal Survey (ECLS-B) data, Jenkins and Foster (2013) report that breastfeeding has little benefit on early childhood test scores. Utilizing random variation in breastfeeding support service on weekdays and weekends, Fitzsimons and Vera-Hernández (2015) identify a positive effect of breastfeeding on a child's cognitive development but no statistically significant effect on health outcomes in U.K.[2]

This paper has two objectives. First, we revisit the associations between breastfeeding and outcomes pertaining to early childhood health and cognitive ability using the restricted ECLS-B data. We leverage the rich collection of the information on observables including prenatal, child, and parents characteristics and breastfeeding practice (e.g., the incidence and duration of breastfeeding) to explore these correlations. Second, we evaluate if one can claim these correlations are causal under the various assumptions (e.g., equality of selection on unobservables and observables and random selection). As such, we use the sensitivity check conducted by Altonji, Elder, and Taber (2005). With their technique, we can estimate both the lower and upper bounds of the effect of breastfeeding, allowing us to judge whether these correlations are causal. Furthermore we calculate the ratio of selection on unobservables to selection on observables that accounts for the entire positive association between breastfeeding and early child outcomes.

[2]Implementing a randomized Promotion of Breastfeeding Intervention Trial (PROBIT) in Belarus, Kramer et al. (2001, 2007, 2008, 2009) find that prolonged and exclusive breastfeeding has a limited positive effect on a child's health, no discernible effect on nonverbal-IQ, and a positive effect on verbal-IQ. Note that Kramer et al.'s PROBIT includes only breastfed babies and captures the intention-to-treatment of breastfeeding promotion program, Baby Friendly Hospital Initiative (BFHI). See Kramer et al. (2001) for detail of their research design.

Our findings using the ECLS-B data is mostly consistent with those of the observational studies: the estimated effects of breastfeeding are positive and significant on early childhood health and cognitive development. Controlling for child and family characteristics rarely alters the result, although the effects of breastfeeding on 48-month outcomes are imprecisely estimated. Including prenatal attributes continues to produce the relatively large effect of breastfeeding. Our sensitivity checks, however, raise concerns regarding the causal effect of breastfeeding on these outcomes. The lower bound of the effect of breastfeeding is not different from zero at the five percent level of significance on a child's health outcomes and are even negative on the cognitive ability. Our further examination indicates that even 10 percent of selection on unobservables is sufficient to account for the entire positive associations between breastfeeding and early childhood outcomes.

The remainder of this paper is organized as follows. Section 2 describes empirical strategy and section 3 explains data. Section 4 reports the estimation results and section 5 concludes.

3.2 Empirical Methodology

3.2.1 Baseline Model

We estimate the outcome equation:

$$Y_i = X_i\gamma + \alpha BF_i + \epsilon_i. \tag{3.1}$$

where Y_i is the various outcomes for child i, BF_i is an indicator variable taking the value of one if the child was breastfed and zero otherwise, X_i' is the set of observable characteristics, and ϵ_i is the error term.[3] Ordinary least squares (OLS) estimator of α is unbiased to the

[3]We mostly follow the empirical methodology employed by Eren and Ozbeklik (2015).

extent the mean of the error term conditional on observed characteristics is independent of parents' decision to breastfeed, $E[\epsilon_i|BF_i, X_i] = 0$.

3.2.2 Nonrandom Selection and Assessment of Selection Bias

The causal interpretation of the effect of breastfeeding hinges on the zero conditional mean assumption. However, parents' decision to breastfeed is possibly endogenous to the OLS specification. For example, a mother who invests more household resources in her child's education may be inclined to breastfeed as she knew benefits of breastfeeding reported by observational studies. Her enthusiasm for her child's success, which is unobserved to researchers, may be positively associated with the decision to breastfeed and early childhood outcomes. Selection on unobservables of this kind consequently bias the estimated coefficient on the decision to breastfeed upward. Ethical issues to implement a randomized control trial for the decision to breastfeed and difficulties in coming across with a valid instrument prevent drawing credible inferences about causality. To circumvent these challenges, we employ the econometric technique proposed by Altonji et al. (2005).[4] Our approach is based on the idea of using the portion of selection on observables that determine the decision to breastfeed to measure how much selection there is on unobservables that determine a child's outcomes.

Consider the bivariate probit model:

$$\begin{aligned} BF_i &= 1(X_i'\beta + \upsilon_i > 0), \\ Y_i &= 1(X_i'\gamma + \alpha BF_i + \epsilon_i > 0), \\ \begin{bmatrix} \upsilon_i \\ \epsilon_i \end{bmatrix} &\sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix} \right). \end{aligned} \tag{3.2}$$

where BF_i, Y_i, X_i', and ϵ_i are the same as equation 4.1, and υ_i is the usual error term

[4] Oster (2014) develops the econometric technique to identify the omitted variable bias under the proportional selection assumption.

for breastfeeding equation. Altonji et al. (2005) prove that one can identify an upper bound on α that happens when we assume $Cov(BF_i, \epsilon_i)/Var(\epsilon_i) = 0$ and a lower bound that happens when we assume that $Cov(BF_i, \epsilon_i)/Var(\epsilon_i) = Cov(BF_i, X_i'\gamma)/Var(X_i'\gamma)$. An intuitive interpretation of these conditions is as follows: The former condition states that the portion of Y_i associated with the unobservable characteristics has no association with the decision to breastfeed BF_i; in contrast, the latter condition states that the portion of Y_i that is associated with the observable characteristics and the portion associated with the unobservable characteristics are equally associated with the decision to breastfeed. As such, denoting ρ to be the correlation between the error terms of breastfeeding and outcome equations, these conditions simplify an interval of ρ with an upper and a lower bounds as

$$0 \leq \rho \leq \frac{Cov(BF_i, X_i'\gamma)}{Var(X_i'\gamma)}. \tag{3.3}$$

In our subsequent empirical work, we estimate the bivariate probit model using a set of equations (3.2) and maximize the likelihood by imposing $\rho = Cov(BF_i, X_i'\gamma)/Var(X_i'\gamma)$. If the lower-bound estimates yield substantial positive effects of breastfeeding, we interpret them as evidence of causal impacts of breastfeeding.

The equality of observables and unobservables is derived from the key assumption that the set of covariates observable to researchers is randomly drawn from the full set of elements that determines the decision to breastfeed and the early childhood outcomes (Altonji et al., 2005). While this assumption might not hold completely, it is more likely to be applicable to the nature of a large-scale survey than an OLS assumption. Large-scale datasets including ECLS-B are designed to provide a wide variety of information rather than to address the unique question that a randomized controlled experiment is. Additionally, because large-scale surveys such as these have limited budgets and are intended to collect information for several fields of research, the available elements in these datasets are likely to be a good approximation of a random subset of all the elements that address a unique question (e.g.

the effect of breastfeeding). Other necessary assumptions are a relatively large number of observables are available and none of the observables and unobservables play a dominant role on determining outcome variables.

3.2.3 The Relative Extent of Selection on Unobservables

In addition to bounding the treatment effect under various assumptions, we can also calculate the normalized extent of the relationship between the decision to breastfeed and unobservables that determine early child outcomes by the ratio $\{E[\epsilon_i|BF_i = 1] - E[\epsilon_i|BF_i = 0]\}/Var(\epsilon_i)$.[5] Similarly, the normalized extent of the relationship between breastfeeding and a set of observables can be expressed as the ratio $\{E[X_i'\gamma|BF_i = 1] - E[X_i'\gamma|BF_i = 0]\}/Var(X_i'\gamma)$.[6] The equality of these ratios - equivalent to equality of selection on observables and unobservables - enables us to ask how large the portion of selection on unobservables relative to the portion of selection on observables is required to account for the entire positive association between breastfeeding α and a child's outcomes.

We now return to the breastfeeding equation of model (3.2). Regression of outcomes on observables and breastfeeding provides us with the fitted values of $X_i'\hat{\beta}$ and the residuals $\hat{\upsilon}_i$ ($BF_i = X_i'\hat{\beta} + \hat{\upsilon}_i$). Replacing BF_i in the outcome equation with the fitted values and the residuals, we can express equation (4.1) as $BF_i = X_i'(\gamma + \alpha\hat{\beta}) + \alpha\hat{\upsilon}_i + \epsilon_i$. The OLS estimator of α converges in probability to plim $\hat{\alpha} = \alpha + Var(BF_i)/Var(\hat{\upsilon}_i)\{E[\epsilon_i|BF_i = 1] - E[\epsilon_i|BF_i = 0]\}$ where the second term is the bias. Equating the portion of selection on observables to that on unobservables, the bias term is

$$\frac{Var(BF_i)}{Var(\hat{\upsilon}_i)}\left\{\frac{E[X_i'\gamma|BF_i = 1] - E[X_i'\gamma|BF_i = 0]}{Var(X_i'\gamma)}Var(\epsilon_i)\right\}. \tag{3.4}$$

Under the null hypothesis that there is no effect of breastfeeding, estimating the equation

[5] Note that ϵ_i denotes the error terms from equation (4.1) and the outcome equation of model (3.2).

[6] X_i' and γ are a vector of observables and and parameters from equation (4.1).

4.1 imposing α=0 yields consistent estimates of γ and hence $E[X_i'\gamma|BF_i]$. We can compute an estimate of the bias by plugging the value of $Var(BF_i)$, the variance of the residuals, and the estimate of γ in bias 3.4.

Altonji et al (2005) defines the ratio of the OLS estimate of α from equation 4.1 to the bias 3.4 as the implied ratio. It gauges how much the portion of selection on unobservables to the portion of selection on observables would be required to account for the entire positive association between breastfeeding and early childhood outcomes. Although we have no clear cutoff of large and small values in an implied ratio, we may reasonably conclude that a value in a range of zero to one means that the positive associations between breastfeeding and a child's outcomes are sensitive to selection on unobservables, and greater than one as a sign of a causal relationship (Altonji et al. 2005).

3.3 Data

The ECLS-B is a longitudinal study of children during the first six years. The ECLS-B follows a sample of approximately 14,000 children and oversamples for racial minority, twins, and low-birth weight children. Information about these children were collected in 2001-02 (roughly nine months old), 2003-04 (roughly 24 months old), and 2005-06 (roughly 48 months old).

The ECLS-B collects information on a number of outcomes, including cognitive ability, physical development, and a record of health problems. Parent and child surveys in the nine-month wave provide a wide range of information on child characteristics, family background, geological location, a mother's prenatal attributes, and most importantly the decision to breastfeed and duration of breastfeeding. Each child was also administered a series of shorter and full versions of the Bayley Short Form (BSF) to measure mental development at nine and 24 months old and tests used on Kindergarten Cohort of Early Childhood Longitudinal Survey (ECLS-K) at 48 months old. The parent responded whether her baby had suffered

from asthma, respiratory illness, gastrointestinal illness, and ear infection. Moreover, the body mass index (BMI) measures for the children were collected at the 24- and 48-month wave.

Our outcomes of interest are as follows: (i) an indicator variable for *no health problem* taking the value of one if a child did not suffer from respiratory illness, asthma, gastrointestinal illness, or ear infection by the date of the nine-, 24-, and 48-month survey, and zero otherwise, (ii) an indicator for *normal weight* that takes the value of one if a child was categorized as neither overweight nor obese at the time of the 24- and 48-month survey, and (iii) *cognitive score* which is standardized to have a mean zero and a standard deviation one.

We define two indicator variables for variables of interest. The *ever breastfed* indicator takes the value of one if a child was ever breastfed, and zero if *never*. To capture intensity, we also define an indicator variable that takes the value of one if a child was breastfed for more than six months, and zero if *never*. Table 3.1 shows the summary statistics for breastfeeding variables. About 67 percent of children were ever breastfed and 27 percent were at-least-6-month breastfed relative to those who were never breastfed.[7] Although these frequencies of breastfeeding on the ECLS-B data are slightly lower than the corresponding numbers reported in the National Immunization Survey (70.3 and 34.5 percent respectively) owing to an oversample for racial minority and low-birth weight children, it is reassuring that our measures are not far off from those of the national representative survey.

Table 3.2 reports the means of a set of nine-, 24-, and 48-month outcomes for children who were *ever breastfed* and were *breastfed for more than 6 months* relative to those who were *never breastfed*. Contrasting these unconditional means shows a stark difference. Breastfed children are far less likely to have health problems than the never-breastfed children and are less likely to be overweight. The difference in cognitive scores is large - for example,

[7] Our incidence of *ever breast-fed* is similar to that of Belfield and Kelly (2012).

about 0.27 standard deviation units higher on the 24-month *cognitive score* for the breastfed children.

Table 3.2 also presents the means of various child characteristics, family background, geographical information, and mother's prenatal attributes. White, married parents with higher education are more likely to breastfeed their babies. A wealthier household with few children is also more likely to select into breastfeeding. It is noteworthy that mothers who have higher BMI measures during pregnancy are less likely to breastfeed their babies, although lighter babies at birth are less likely to be breastfed. There are few noticeable differences between the sample of children who were *ever breastfed* and were *breastfed for more than 6 month.* We thus only report the estimates using the sample of the children who were breasted for more than six months and were never breastfed in the text and relegate findings on those who were *ever breastfed and never* to Appendix A.

3.4 Results

3.4.1 Breastfeeding and Early Childhood Outcomes

Panel A of Table 3.3 presents the coefficient estimates on breastfeeding from probit and OLS models for *no health problem*, and *cognitive score* at nine months. Huber-White robust standard errors are reported in parentheses. The raw difference in means for *no health problem* is 0.063, as reported by the average marginal effect in the square brackets (column 1). When we add the child characteristics, the marginal effect slightly increases in magnitude, which is indicative that observable child characteristics explain a small fraction of the variation in the *no health problem.* The size of this estimated effect is somewhat large - 8.4 percent reduction in probability that a child suffers from illness. The estimate contracts slightly to 0.059 when we include family background and region controls in column (3) and

contracts further to 0.052 when we include a set of prenatal attributes including mother's BMI and indicators for whether a mother smoked, drank, was employed, and received WIC. The estimated effect of breastfeeding is stable and marginally significant.

Cognitive scores, however, provide a different pattern. The difference in mean for *cognitive score* is 0.108 and significant at the five percent level. The estimated effect of breastfeeding on *cognitive score* is moderate - 0.108 standard deviation units increase in cognitive score. When we add a set of a child's age in month, birth weight, gender, and race, statistical significance has however disappeared. The estimated effects of breastfeeding remain insignificant once additional sets of controls are included.

Panel B of Table 3.3 presents estimates of the effect of breastfeeding on 24-month outcome variables. The first row of the panel presents the estimated effect of breastfeeding on the probability that a child is neither overweight nor obese. Inclusion of control variables leaves the estimated coefficient on breastfeeding almost intact: the unconditional difference of 0.107 contracts to 0.102 when child characteristics, family background, and region are added to the control variables and to 0.088 when prenatal attributes are further added. The estimated effect is a nine percent increase in the probability that a child is a normal weight. The third row presents the estimates of the effect of breastfeeding on cognitive score. The raw difference of 0.385 contracts to 0.183 once all the detailed controls have been included. The estimated effect of breastfeeding is positive and is significant - 0.183 standard deviation units increase in cognitive score. We further examine the effect of breastfeeding on 48-month outcomes in panel C of Table 3.3. None of the estimates are precisely estimated when we have added a full set of control variables.[8]

Of all eight outcome variables, the estimated effects on *no health problem* at nine months old as well as on *normal weight* and *cognitive score* at 24 months old are positive and

[8]Gelbach (2016) suggests that one abandon sequentially including control variables to a base model in order to check the stability of the estimates of interest. Aligned with the existing studies, we report the results from conventional sequential inclusions, but we also employed Gelbach's conditional decomposition. This result is available upon request.

precisely estimated in our most extensive specification. These estimates can be assumed to be unbiased under the zero conditional mean assumption. Although we control for a rich set of observables, it is still possible that our point estimates are confounded by selection on unobservables. We now further explore this possibility.

3.4.2 Sensitivity Analysis

Table 3.4 reports point estimates of the impact of breastfeeding that correspond to various values of ρ, the correlation in the error terms between breastfeeding and outcome equations (3.2). We report results for nine-month *no health problem* in panel A and 24-month *normal weight* and *cognitive score* in panel B, and present probit estimates and average marginal effects in brackets. We set ρ equal to zero (univariate probit case) and increase the value of ρ up to 0.2 by estimating bivariate probit models simultaneously setting ρ to the specific value. We use a 24-month *cognitive score* indicator as a substitute for the continuous *cognitive score*. [9] The raw difference in the probability that a child had no health problem is 0.063. When $\rho = 0$, the estimated effect is 0.052, and the figure contracts to 0.019 when $\rho = 0.05$ and even further to -0.013 when $\rho = 0.1$. The former estimate is insignificant and the latter even has a negative sign. Given that a small value of selection to breastfeeding can wipe out the entire positive association, we have the considerably weaker evidence for a strong effect of breastfeeding than indicated by the results from a regression treating the decision to breastfeed as exogenous.

Panel B of Table 3.4 presents the results for the 24-month *normal weight* and *cognitive score*. The results are quite similar to the nine-month *no health problem*. The effect of breastfeeding on the probability that a child weighs normal is 0.088 when $\rho = 0$, and contracts to 0.030 and is insignificant when $\rho = 0.1$. The sign of the estimate even becomes negative when we impose stronger correlation $\rho = 0.2$. The effect of breastfeeding on the probability

[9] Cognitive score takes one if a child earns Bayley Short Form - Research Edition (BSF-R) mental score greater than the median of the distribution of the scores, and zero otherwise.

that a child earns a cognitive score greater than the median of its score distribution shows a similar pattern. The effect is 0.072 when $\rho = 0$, which points to a positive, significant effect similar to the OLS estimate on *cognitive score*. It contracts to 0.011 when $\rho = 0.1$, and becomes negative when $\rho = 0.2$.

Having shown some preliminary evidence on the sensitivity of the results, we now proceed to formally testing the sensitivity of the positive associations reported in Table 3.4 using the strategy developed in Altonji et al. (2005).

3.4.3 Results under Equality of Observables and Unobservables

The last column of Table 3.4 presents the estimates of the effect of breastfeeding in the bivariate probit model by imposing $\rho = Cov(BF_i, X_i'\gamma)/Var(X_i'\gamma)$ as discussed on equation (3.3). For nine-month *no health problem*, the estimate of ρ is 0.11. The estimate of the effect of breastfeeding is -0.058 (0.071), which is insignificant with the negative sign. Under the assumption of equality of selection on observables and unobservables, the estimate suggests that there be no evidence of the causal effect of breastfeeding on health at nine months old.

The result for 24-month *normal weight* follows a similar pattern, although the assumption of equality of selection on observables and unobservables leads to a positive estimated effect of breastfeeding 0.105 (0.086) along with $\rho = 0.09$. The average marginal effect is moderate 0.036 and imprecisely estimated. Moreover, the result for 24-month *cognitive score* shows how sensitive the relationship is between parents' decision to breastfeed and a child's cognitive development. The entire positive effect of breastfeeding has disappeared when $\rho = 0.47$, indicating strong nonrandom selection to breastfeeding. Given the result of our sensitivity analysis and evaluation on selection on unobservables relative to selection on observables, there is little evidence that breastfeeding advances a child's health and cognitive ability at the age of nine, 24, and 48 months.

3.4.4 Nonrandom Selection

As a final check, we report the implied ratios in Table 3.5. Each column corresponds to nine-month *no health problem* and 24-month *normal weight* and *cognitive score*. The first row of Table 3.5 presents the unconstrained estimate of the effect of breastfeeding α on these three outcome variables. The second row presents the estimate of the bias 3.4. The third row displays the implied ratio by dividing the unconstrained estimate by the bias. Recall that one can use the value of one and above as indicator of a potential causal relationship. For nine-month *no health problem*, the implied ratio is only 0.12; that means, if the normalized extent of selection on unobservables relative to that on observables is 12 percent, the positive association between breastfeeding and a child's health is fully explained. Similarly, the implied ratios for 24-month *normal weight* and *cognitive score* are well below one: the implied ratio is 0.07 for *normal weight* and 0.11 for *cognitive score*. It indicates that 7 and 11 percent of selection on unobservables relative to that on observables are sufficient to account for the positive associations between breastfeeding and a child's weight and cognitive scores. Overall, the positive associations between breastfeeding and a child's outcomes appear to be quite sensitive to selection into breastfeeding.

3.5 Conclusion

In the last decade or so, factors advancing early childhood health and cognitive development have received considerable attention among researchers and policy makers. This paper examines one of the potential factors, breastfeeding. Using the ECLS-B data and employing the econometric methodology proposed by Altonji et al. (2005), we evaluate the associations between breastfeeding - the prime source of nutrients at the earliest stage of human lives - and health as well as cognitive development. We find breastfeeding is positively associated with the probabilities that a child has no health problem, is at a normal weight, and acquires

better cognitive ability. Our further findings, however, suggest that the positive breastfeeding effect is very sensitive to nonrandom selection to breastfeeding and 10 percent of selection on unobservables is sufficient to account for the positive associations between breastfeeding and early childhood outcomes.

Before we conclude, there are two caveats to keep in mind. First we explore the impact of breastfeeding on the population of children born in 2001 in the United States. Given the high degree of economic inequality and constrained maternity leaves, our findings might not be generalized to other countries. Second, it is crucial to note that breastfeeding may affect outcomes other than the early childhood outcomes examined in our study. There may be effects on child health and academic performance in later stages of life (Rees and Sabia 2009; Fletcher 2011). The increase in mother-child interaction owing to breastfeeding may foster development of a bond between mothers and children in early infancy (Fergusson and Woodward 1999; Britton, Britton, and Gronwaldt 2006). The aggregate effect of breastfeeding would have to be examined by accounting for all beneficial effects in these dimensions. We may explore them in future research.

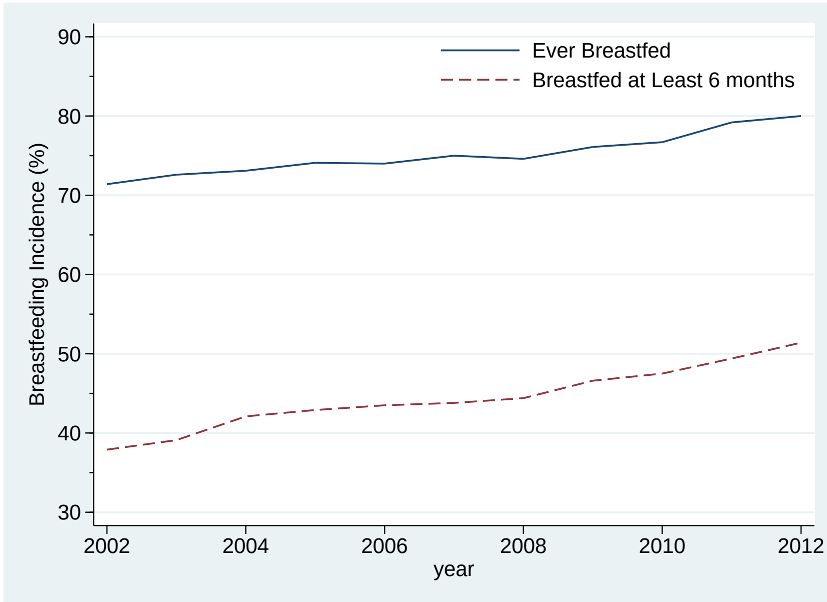


Figure 3.1: Breastfeeding among U.S. Children Born 2002-2012

Notes: National Immunization Survey, Department of Health and Human Service (2015).

Table 3.1: Summary Statistics for Breastfeeding Variables ECLS-B

	Frequency %	Observations
Ever breast-fed	0.67	10,550
Breast-fed for 6+ months	0.27	4,700

Source: ECLS-B nine month wave.
Note: *Not ascertained*, *don't know*, *refused*, and *Not applicable* are assigned to missing values. All variables are measured at nine months from the response by the mother.

Table 3.2: Summary Statistics For Outcome and Explanatory Variables

	Ever BF vs Never			6m+ bf vs never		
	No bf	Bf		No bf	6+ bf	
	Mean	Mean	Diff	Mean	Mean	Diff
9 month outcome variables						
No health problem	0.47	0.54	-0.06***	0.47	0.55	-0.08***
Cognitive score	-0.09	0.05	-0.14***	-0.09	0.11	-0.20***
24 month outcome variables						
Normal weight	0.71	0.75	-0.04***	0.71	0.76	-0.05**
No health problem	0.51	0.51	-0.00	0.51	0.51	-0.00
Cognitive score	-0.18	0.09	-0.27***	-0.18	0.21	-0.40***
48 month outcome variables						
Normal weight	0.66	0.70	-0.03**	0.66	0.72	-0.06***
No health problem	0.59	0.58	0.01	0.59	0.56	0.03
Cognitive score	-0.10	0.05	-0.15***	-0.10	0.06	-0.16***
Child information						
Age (month)	10.51	10.46	0.05	10.51	10.86	-0.35***
Birth weight (kg)	2.79	2.98	-0.19***	2.79	3.04	-0.25***
Male	0.51	0.51	0.00	0.51	0.50	0.02
White	0.39	0.42	-0.03**	0.39	0.45	-0.06***
Family information						
Married	0.51	0.73	-0.22***	0.51	0.81	-0.30***
Mother's years of schooling	12.34	13.76	-1.42***	12.34	14.32	-1.98***
Father's years of schooling	12.97	13.97	-1.00***	12.97	14.44	-1.47***
Number of siblings	1.20	1.03	0.16***	1.20	1.08	0.11**
Log income	10.05	10.55	-0.49***	10.05	10.73	-0.68***
Socioeconomic Scale	-0.41	0.12	-0.54***	-0.41	0.32	-0.73***
Geographical information						
Urban	0.80	0.88	-0.08***	0.80	0.89	-0.09***
Prenatal information						
BMI	31.29	30.56	0.73***	31.29	29.87	1.42***
Smoked	0.20	0.09	0.12***	0.20	0.04	0.16***
Drinking	0.01	0.01	0.00*	0.01	0.00	0.01
Working	0.70	0.71	-0.01	0.70	0.70	-0.00
WIC	0.56	0.35	0.21***	0.56	0.28	0.27***
N	3450	7100	10550	3450	1300	4700

Source: ECLS-B nine-, 24-, and 48-month wave.

Note: *Not ascertained*, *don't know*, *refused*, and *not applicable* are assigned to missing values. Birth weight, parents' years of schooling, and log household income are converted to contenuous variables from categorical variables by assigning the mean of the upper/lower bounds of each interval. Missing values on age, parents' education, log household income, and variables used for mother's BMI are imputed with mean of variables. *, **, and *** difference are statistically significant at the .1, .05, and .01 level.

Table 3.3: Probit and OLS Estimates of Breastfeeding Effects

6m+ BF vs Never	None	Child characteristics	Col.2 plus family background and region	Col.3 plus prenatal attributes
	(1)	(2)	(3)	(4)
Panel A: 9 months	No health problem (N=3,850)			
:Breastfeeding	0.158**	0.217***	0.154**	0.135*
	(0.063)	(0.065)	(0.071)	(0.072)
	[0.063]	[0.084]	[0.059]	[0.052]
Pseudo R^2	0.00	0.02	0.03	0.03
	Cognitive score (N=3,800)			
Breastfeeding	0.108**	0.048	-0.006	-0.000
	(0.049)	(0.048)	(0.050)	(0.051)
R^2	0.00	0.04	0.07	0.07
Panel B: 24 months	Normal weight (N=3,300)			
:Breastfeeding	0.302***	0.338***	0.296***	0.257***
	(0.072)	(0.073)	(0.080)	(0.081)
	[0.107]	[0.117]	[0.102]	[0.088]
Pseudo R^2	0.01	0.03	0.03	0.04
	No health problem (N=3,300)			
:Breastfeeding	-0.049	-0.048	-0.063	-0.095
	(0.068)	(0.069)	(0.076)	(0.076)
	[-0.019]	[-0.018]	[-0.024]	[-0.036]
Pseudo R^2	0.00	0.02	0.02	0.03
	Cognitive score (N=3,300)			
Breastfeeding	0.385***	0.320***	0.180***	0.183***
	(0.051)	(0.049)	(0.050)	(0.051)
R^2	0.03	0.11	0.15	0.15
Panel C: 48 months	Normal weight (N=2,950)			
:Breastfeeding	0.186**	0.239***	0.123	0.045
	(0.074)	(0.075)	(0.083)	(0.084)
	[0.069]	[0.086]	[0.044]	[0.016]
Pseudo R^2	0.00	0.03	0.03	0.06
	No health problem (N=2,950)			
:Breastfeeding	-0.007	0.018	0.037	0.024
	(0.071)	(0.073)	(0.080)	(0.081)
	[-0.003]	[0.007]	[0.015]	[0.009]
Pseudo R^2	0.00	0.01	0.02	0.02
	Cognitive score (N=2,850)			
Breastfeeding	0.101*	0.084	0.008	0.008
	(0.061)	(0.062)	(0.069)	(0.070)
R^2	0.00	0.01	0.02	0.02

Note. ECLS-B panel weights are used. Huber-White standard errors are in parentheses. Average marginal effects are in brackets. * 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 3.4: Sensitivity of Estimates of Breastfeeding Effects on Health Outcomes and Cognitive Score to Assumptions about Selection Bias in ECLS-B

6m+ BF vs Never	CORRELATION OF DISTURBANCES[a]				
	$\rho = 0$	$\rho = 0.05$	$\rho = 0.1$	$\rho = 0.2$	$\rho =$ Cov(X$'\beta$,X'γ) / Var(X'γ)[b]
Panel A: 9 months	No health problem				
:Breastfeeding	0.135	0.050	-0.034	-0.201	-0.058
	(0.072)	(0.072)	(0.072)	(0.071)	(0.071)
	[0.052]	[0.019]	[-0.013]	[-0.077]	[-0.022]
Constraint on ρ					0.11
Panel B: 24 months	Normal weight				
:Breastfeeding	0.256	0.171	0.085	-0.084	0.105
	(0.086)	(0.086)	(0.086)	(0.085)	(0.086)
	[0.088]	[0.059]	[0.030]	[-0.029]	[0.036]
Constraint on ρ					0.09
	Cognitive score				
:Breastfeeding	0.202	0.117	0.032	-0.135	-0.596
	(0.082)	(0.081)	(0.081)	(0.080)	(0.075)
	[0.072]	[0.042]	[0.011]	[-0.048]	[-0.210]
Constraint on ρ					0.47

Note.- The ECLS-B sampling weights are used in the computations. The control variables are identical to those described in note a, b, and c of Table 3.3. Average marginal effects are in brackets. Huber-White standard errors are presented in parentheses. Cognitive score takes one if a child earns BSF-R mental score greater than the medians of its distribution, and zero otherwise.

[a] Models estimated as bivariate probits with the correlation ρ between u and ϵ set to the values in column headings.

[b] The model is $BF = 1(X'\beta + u > 0)$ and $Y = 1(X'\gamma + \alpha BF + \epsilon > 0)$; β, γ, and α are estimated simultaneously as a constrained bivariate probit model.

Table 3.5: Amount of Selection on Unobservables Relative to Selection on Observables Required to Attribute the Entire Breastfeeding Effect to Selection Bias

6m+ BF vs Never	9 months	24 months	
	No health problem	Normal weight	Cognitive score[c]
$\hat{\alpha}$	0.05	0.09	0.18
$Cov(BF, \epsilon)/Var(\hat{\upsilon})^a$	0.45	1.28	1.65
Implied ratio[b]	0.12	0.07	0.11

Note. The model Y = $1(\mathrm{X'}\gamma + \alpha BF + \epsilon > 0)$ for *no health problem* and *normal weight* and Y = $\mathrm{X'}\gamma + \alpha BF + \epsilon$ for cognitive scores were estimated by OLS. The $\hat{\gamma}$ used to evaluate $[\hat{E}(X'\hat{\gamma}|BF = 1) - \hat{E}(X'\hat{\gamma}|BF = 0)]/\hat{Var}(X'\hat{\gamma})$ is estimated under the restriction $\alpha = 0$, using the sample of the more than 6 month breastfed and never breastfedd infants. The ECLS-B panel weights are used. See notes a, b, and c of Table 3.3 for a description of the controls.

[a] To the extent equality of selection on observables and unobservables holds: that is, $[E(\epsilon|BF = 1) - E(\epsilon|BF = 0)]/Var(\epsilon) = [E(X'\gamma|BF = 1) - E(X'\gamma|BF = 0)]/Var(X'\gamma)$.

[b] The implied ratio in row 3 on each panel is the ratio of standardized selection on unobservables to observables under the hypothesis that there is no breastfeeding effect.

[c] Cognitive score is a standardized continous variable.

Chapter 4. Ban the Box and Recidivism

4.1 Introduction

70 million Americans have past criminal histories (Friedman, 2015; Groggins and DeBacco, 2015) and 650,000 ex-offenders are released from federal and state prisons every year (United States Department of Justice, 2017). Most job application forms in the United States ask if an applicant has been convicted of a crime and, if the box for yes has been ticked, the application tends to be discarded at the initial screening stage. Ban-the-Box (BTB) policies restrict employers from asking about applicants' criminal histories on job applications and delay background checks until interviews are completed. BTB policies are intended to give people with criminal records an opportunity to display their qualifications in the hiring process before being assessed and potentially rejected based on this record (Stacy and Cohen, 2017). Other application settings such as university application (State University of New York, 2016), housing application (Kurzius, 2016), and job application for medical centers (Thill, Abare, and Fox, 2014) also employ BTB policies (Stacy and Cohen, 2017).

Recent studies on BTB policy have focused on its effect on labor market outcomes, particularly statistical discrimination. Agan and Starr (2016) utilize the recent adoption of BTB policies in New Jersey and New York City. Agan and Starr randomly vary the race and felony conviction status of the applicants and send out thousands of fake online job applications to employers before and after BTB policies were adopted. Agan and Starr find that White applicants receive slightly more call backs than similar Black applicants before the adoption of BTB, and BTB increases this White-Black disparity dramatically. Doleac and Hansen (2016) exploit variation in the timing of BTB adoption across jurisdictions over a period of 2004-2014 and estimate the effect of BTB on employment for White, Black, and Hispanic age 25-34 males without college degree. Doleac and Hansen find that BTB reduces

the probability of being employed by 5.1% for young, low skilled Black males. Doleac and Hansen conclude that lack of information on applicants' criminal histories induces employers to select less applicants who are more likely to have criminal records (Black males). Shoag and Veuger (2016) compare employment for the residents of high-crime census tracts, a proxy for those with criminal records, to employment for the residents of low-crime census tracts, a proxy for those without criminal records, before and after the introduction of BTB policies. Shoag and Veuger find that BTB improves the employment level of high-crime neighborhoods and induces employers to substitute employment of Black males with employment of female workers. However, there are few studies, as far as we are concerned, devoted to investigating the effect of BTB on rates of recidivism.[1]

We contribute to this strand of the literature by analyzing the effect of BTB policy for offenders recently released using the National Correctional Reporting Program (NCRP) 2000-2014 dataset (Bureau of Justice Statistics, 2016). Until recently, there were limited panel datasets that allowed researchers to link prison terms for the same offenders over time (Yang 2016).[2] Recently, Bureau of Justice Statistics (BJS) linked prison terms using a offenders' characteristics, making it possible to study a representative analysis of recidivism at a national scale. NCRP data also contain information on the county where sentence of each offender was imposed. Following the commonly-applied assumption that the county of an offender's sentence is the county in which he/she resides and to which he/she returns upon release (Schnepel, 2016; Yang, 2016), we aggregate a dataset of more than four million first-sentence ex-offenders to define rates of recidivism at the year-county level. We then assign adoption of BTB policy to counties across time. We align our research samples with

[1] D'Alessio, Stolzenburg, and Flexon (2015) examine the effect of BTB law in Hawaii on repeat offending. D'Alessio, Stolzenburg, and Flexon find that a criminal defendant prosecuted in Honolulu for a felony crime was 57% less likely to have a prior criminal conviction after the adoption of BTB law.

[2] An exception is Schnepel (2016). Even before NCRP panel data was made publicly available, Schnepel uses prison release and parole outcome data of NCRP for prison release for a period of 1993-2008 and match them using date of birth, date of prison release, and county of sentencing. Note his analysis sample consists of offenders released in California.

the two age groups on which existing studies focused, age 18-65 (working-age) and age 25-34 (young) offenders.

We employ a difference-in-differences (DID) approach to identify the effect of BTB on one-year rates of recidivism. We control for year and county fixed effects, county-specific linear time trends, and county characteristics. As long as that the average rate of recidivism in BTB counties with no BTB policy would evolve similarly in non-BTB counties over the sample period, our empirical model identify causal effects of BTB policies. We test the validity of our identification assumption by estimating a main specification augmented with leads and lags of BTB adoption.

We find a large and significant effect of BTB on recidivism. The estimated effects relative to the pre-adoption means are a 29% reduction for working-age ex-offenders and a 24% reduction for young ex-offenders. Our results also indicate that the effect of BTB policy grows substantially over time upon its adoption: The reduction grows from 3.4 percentage points in the first BTB post-adoption year to 7.4-8.1 percentage points in the following years. Further examination on heterogeneous effects reveals that BTB policies disproportionately prevent Black male ex-offenders from returning to prison but may benefit little female ex-offenders or young ex-offenders in highly educated counties. In a labor market analysis, we find that, as Doleac and Hansen (2016) report, BTB laws induce employers to statistically discriminate against Blacks in the working age and young populations. We also find that employment opportunities in industries which employ more ex-offenders are complement with BTB policies to prevent ex-offenders from returning to prison: The estimated effect in the post-BTB counties is a 3.1% reduction in recidivism for young ex-offenders. With certain tests, these findings are robust.

The remainder of this paper is organized as follows: Section 2 describes background, section 3 explains data, section 4 discusses empirical strategy, section 5 reports results, section 6 discusses policy implication, and section 7 concludes.

4.2 Background

Employment applications typically ask applicants to "check a box" if they have a criminal record. Typically, employment applications with positive responses to this question (and a checked box) are thrown away. As the name suggests, BTB law prohibits employers from including the box and gives ex-offenders more opportunities to display their qualifications before their background checks have been conducted at the employment decision stage. First implemented in Hawaii in 1998, BTB laws have been adopted by states, counties, and cities in the contiguous United States since 2005. Figure 4.1 and 4.2 show counties with active BTB laws in 2000 and 2014.

BTB laws are comprised of three broad categories; those that affect public employers; those that affect private employers with government contracts; and those that affect all private employers. Public BTB laws are the most common, and private BTB laws are normally the final step a jurisdiction takes. Every jurisdiction in our sample with a contract BTB and a private BTB has a public BTB law. Following the approach of Doleac and Hansen (2016), our analysis focuses on the effects of having adopted a public BTB law.

Public BTB law is expected to promote ex-offenders' employment in both public and private sectors. These BTB policies were intended to induce employers to provide ex-offenders a fair chance. In response to these public BTB laws and social pressure, major national corporations such as the Home Depot, American Airline, and Koch Group voluntarily removed the box on their employment applications. Because BTB laws likely provide ex-offenders fairer employment opportunities, we focus on the net effect of BTB on one-year rates of recidivism in a county.

4.3 Data

4.3.1 Rates of Recidivism

We use prison term records of NCRP for offenders released during the period 2000-2014 (Bureau of Justice Statistics, 2016). The NCRP compiles offender-level data on admissions and releases voluntarily provided by states.[3] Until recent years, NCRP was comprised of a year-by-year accounting of prison admission, releases, and prison stocks. BJS recently linked individual offenders with multiple prison terms by using inmate ID, date of birth, admission, release, and offense and sentencing information in the NCRP data (Laullen et al, 2014; Yang, 2016). Because Yang (2016) documents the extensive steps of data cleaning for the NCRP 2000-2013 dataset, we follow her methods to construct our analysis sample.

The data contain information on the prison admission and release dates for each prison term. Offender characteristics include race, ethnicity, gender, age, education, and type of offense. We set age at the date of the first observed prison release so that all offender characteristics are time-invariant through the sample period.

We make six sample selection restrictions. First, we include only the first observed prison term for each offender to investigate the impact of BTB law on the first return to prison. Second, we exclude offenders whose county of incarceration is missing. Third, we exclude offenders whose prison release dates are missing.[4] Fourth, we exclude observations whose prison release date is prior to 2000. Fifth, we exclude observations whose reason of prison release is death. Finally, we exclude offenders who were older (younger) than sixty-five (eighteen) years old. After imposing these sample selections, we have a total of 4,139,415 offenders released from prison in the sample period 2000-2014. We call this group working age (age 18-65) ex-offenders.

[3]States participating in the NCRP has increased from 38 states in 2000 to 48 in 2013 (Yang, 2016).

[4]Appendix Table ?? shows a list of states that provided the information on dates of offender's release in NCRP data.

We also investigate the effect on BTB policy on recidivism for age 25-34 ex-offenders. We focus on this group for two reasons: First, existing research uses age 25-34 population as a primary age group of interest (Doleac and Hansen, 2016). Second, 60 percent of criminal offenders were age 30 or younger in 2012 (Kearney et al. 2014) and employers are most reluctant to hire those who were recently incarcerated (Holzer, Raphael, and Stoll, 2007), so the effect of BTB policy is expected to be least pronounced for this group even if BTB policy works. We label this group as young (age 25-34) ex-offenders.

NCRP also contains information on the county in which each sentence was imposed. The criminology literature on criminal mobility suggests that the county of sentencing is likely the offender's county of residence prior to incarceration (Wiles and Costello, 2000; Bernasco et al, 2013). Offenders on parole are required to return to and reside in the original county or last county of residence (Raphael and Weiman, 2007; Sabol, 2007; Schnepel, 2016; Yang, 2016). Following existing studies and assuming that the county in which the offender was released from prison is that in which he/she remained, we use a sentencing county as a unit of analysis. However, one limitation of the NCRP data is, because BJS only links offenders who return to custody within a state, we cannot distinguish offenses committed by ex-offenders moving from other states from those committed by first-time offenders (Yang, 2016).[5]

The outcome variable in our analysis is one-year rate of recidivism, defined as a return to prison in a year in the same state due to a 'court commitment' or technical parole violation (Yang, 2016). BJS defines recidivism as "criminal acts that result in the rearrest, reconviction, or return to prison with or without a new sentence during a period of three years following the prisoner's release" (Schnepel, 2016). Because our data does not include neither arrest nor conviction information, we use returning to prison as definition of recidivism.[6]

[5] Approximately ten percent of the ex-offenders released in 30 states during 2005 were arrested in another state (Durose, Snyder, and Cooper, 2015; Yang, 2016). Doleac and Hansen (2017) find little evidence that BTB policy is associated with a change in demographic composition and an increase in migration into and out of labor market affected by BTB policy.

[6] This definition is commonly used in existing studies. Note that the three-year rate of return-to-prison recidivism in Table 4.1 is lower than the three-year rate of rearrest recidivism. For example, United States Department of Justice (2017) reports that approximately two-thirds of ex-offenders are rearrested within three years of release.

Table 4.1 presents summary statistics for offenders released from their first sentence.[7] The first (last) two columns report means and standard deviations for working age (young) ex-offenders. Panel A and B present summary statistics at the ex-offenders' level and year-county level. The first three rows of Panel A report the unconditional probabilities of returning to prison within one, two, and three years for the offenders after their first prison release. Because we restrict the sample to the first release per offender, these rates of recidivism are not affected by the ex-offenders who recidividate more than one time. 14 and 26 percent of working age ex-offenders return to prison within one and three year(s) of release.[8] Among offenders released in 42 states, 48 percent are White, 36 percent are Black, and 17 percent are Hispanic. The average age at the time of prison release is 34 years old. The proportion of residents whose education level is less than high school graduation is 33 percent. The average sentence is a little more than two years.[9] When classified by the type of offense associated with an offender's imprisonment, all of three offense represent about 27 percent for working-age ex-offenders whereas drug offense represents slightly more than violent and property offense for young ex-offenders.

To control for labor market condition, demographic characteristics, and labor demand, we aggregate the offender-level data by county-year. Specifically, for a given county and year, we divide the number of prisoners who were released and recidivated within one, two and three years, by the total number of prisoners released. We collect county-year characteristics from various sources: unemployment rates from Local Area Unemployment Statistics; proportions of male, residents whose education level is less than or equal to high school graduates, White, Black, and Hispanic from American Community Survey and Decennial Census; and new hires in manufacturing & construction industries and all other industries from Quarterly Workforce Indicators. We then match all the county characteristics with each county-year cohort using

[7] Appendix Table ?? tabulates the states and years available in our analysis dataset.

[8] These rates of recidivism are similar to those using the older version of NCRP data (Yang, 2016). Rhodes et al. (2014) document that about two-thirds of prisoners released will never return to prison. The figure is comparable to our rates of recidivism.

[9] The average sentence is slightly shorter than three years reported by Schnepel (2016). Schnepel uses the California data of NCRP 1993-2009.

the MSA county crosswalk of National Bureau of Economic Research (NBER).[10] Panel B of Table 4.1 reports descriptive statistics at the county-year level. The numbers of counties and unique counties are 31,192 (29,378) and 2,709 (2,683) for working age (young) offenders released. Rates of recidivism and most county characteristics are comparable across age groups except for new hires.

4.3.2 Ban the Box Laws

We analyze BTB policies effective for a period of January 2000 - December 2014, the available data period of NCRP. We collect information on the timing and details of BTB policy from Doleac and Hansen (2016) and Rodriguez and Avery (2017). Following Doleac and Hansen's BTB adoption date assignment rule, we use the date as the start date of BTB policy if information about a policy's effective date is available. If it is not, we then use the date when the policy was announced or passed by legislature. If only the year (month) of implementation is available, we use January 1st of that year (the first day of that month) as the start date.

Adoption of BTB policy may be non-random and cause sorting. Doleac and Hansen (2017) document that the larger fraction of the residents in communities that eventually adopt BTB policies is Black, is young and male, and has a college degree on average. Doleac and Hansen (2017) also detect little discernible evidence that BTB adoption is correlated with intra-state, inter-state, and within-county migration for four demographic groups, ruling out the possibility that BTB adoption induces sorting in the local labor market and counties. Furthermore, the timing of BTB policy adoption is likely to be associated with local interest in hiring those with criminal records (Doleac and Hansen, 2016), so it may overestimate the effect of BTB law on recidivism. To mitigate a systematic increase in local labor demand for returning citizens, we include the number of new hires in the right-hand-side variables

[10] The MSA county crosswalk is available from the NBER webpage `http://www.nber.org/data/cbsa-msa-fips-ssa-county-crosswalk.html`.

(Schnepel, 2016; Yang, 2016), and, additionally, conduct a falsification test to check if such unobserved elements threaten an identification of the parameter of interest.

Our goal is to measure the effect of BTB on rates of recidivism in the economy, so we assign treatment at the county level. Following Doleac and Hansen (2016), we consider counties treated by BTB if the state in which the county is located has a BTB policy, or if any jurisdiction in the county has a BTB policy. Figure 4.3 shows counties that we considered being in effect of BTB law in 2014. We leverage the variations in timing of implementing BTB laws across counties for identification.[11]

Table 4.2 reports the number of states and counties by year of BTB policy adoption. The first and last four columns correspond to the samples of working-age and young offenders released. No state adopted BTB policy prior 2008 and, since then, one or two state(s) has(have) implemented BTB policy year by year (column 1 and 5). Similarly, the number of counties that implemented BTB policy has rapidly increased since 2005 (column 2 and 6) and the number of counties that have not adopted BTB has declined (column 3 and 7). Column 4 and 8 report the number of counties that never implemented BTB law over our sample period.[12] In total, we have 491 (485) unique counties that adopted BTB in any year and 2,218 (2,198) unique counties that never adopted BTB for working age (young) offenders. For brevity, we call counties that have already adopted BTB law “BTB adopters" (column 2 and 6), counties that have not adopted it yet or never adopted it “non-BTB counties" (column 3-4 and 7-8), and counties that adopted BTB policy in any year “BTB counties" (column 2-3 and 6-7).

[11] Figure 4.2 and 4.3 show that the counties used for our analysis are slightly less than those actually in effect of BTB law mainly owing to voluntarily participation of NCRP.

[12] The number of counties has increased through our sample period because more states voluntarily participated in NCRP.

4.4 Empirical Strategy

We employ a DID approach to identify the effect of BTB policy on rate of recidivism. Temporal variation in rate of recidivism and county characteristics may confound the effect of BTB policy. To partial out these confounding factors from effect of BTB policy, we estimate the equation

$$Recidivism_{ct} = \alpha_0 + \alpha_1 BTB_{ct} + X'_{ct}\alpha_2 + \alpha_t + \alpha_c + \alpha_c t + \epsilon_{ct}, \tag{4.1}$$

where $recidivism_{ct}$ is the one-year rate of recidivism in county c at year t and X'_{ct} is a set of county characteristics that account for variation in recidivism (i.e., unemployment rate, fraction of male, White, Black, and Hispanic population, fraction of residents whose education level is less than or equal to high school graduation, and number of new hires in manufacturing & construction and other industries per a thousand working-age population). We also add year fixed effects α_t, county fixed effects α_c, and county-specific linear time trends $\alpha_c t$, and ϵ_{ct} is the error term. Robust standard errors clustered at county level are presented.[13]

BTB_{ct} is an indicator variable that equals one in the BTB adoption year t if the jurisdiction adopted BTB policy between January 2 and January 31. BTB_{ct} first takes a value of one in the year t+1 if the BTB policy was first adopted in January 1 or between February 1 and December 31 of the year. Recall that we assign January 1 to any counties whose BTB adoption dates are unknown. To mitigate the potential bias caused by this assignment rule, we give treatment to only counties which adopted BTB in the beginning of the adoption year (January 2 - 31) in the adoption year and to other counties in the following

[13] Researchers should use bootstrap standard errors when the number of cluster is less than 50 (Bertrand et al. 2004; Cameron et al. 2008).

year.[14] We will also present the results when we assign all BTB adopters treatment in their BTB adoption years in the robustness check section.

Our counterfactual setting that we have in mind is the average rate of recidivism in BTB counties would evolve similarly in non-BTB counties over the sample period if no jurisdiction implemented BTB policy. This setting gives us an equivalent parallel trend assumption, necessary for identification of parameter of interest. For example, if the county implemented BTB law due to the time-varying social mood that integrates ex-prisoners into the local community and that simultaneously reduces the rate of recidivism, it would confound the effect of BTB policy. In addition, as Doleac and Hansen (2016, 2017) document, states which adopted BTB policies have more Black and college-educated population with higher earnings. Because counties that adopted BTB may be different from those that did not, we test if the trends in the rate of recidivism prior to BTB adoption is significantly different across BTB and non-BTB counties. Furthermore, recall that BTB counties adopt the private and contract BTB laws after the implementation of the public BTB law. To capture dynamics of BTB policy, we replace the BTB indicator variable with leads and lags and estimate an augmented specification of equation 4.1

$$Recidivism_{ct} = \alpha_0 + \sum_{q=0}^{m} \beta_{-q} BTB_{c,t-q} + \sum_{q=1}^{n} \beta_{+q} BTB_{c,t+q} + X'_{ct}\alpha_2 + \alpha_t + \alpha_c + \alpha_c t + \epsilon_{ct}. \quad (4.2)$$

where $BTB_{c,t-q}$ and $BTB_{c,t+q}$ are indicator variables for four years or more than four years prior to adoption, 3-1 years before adoption, 0-1 years after adoption, and year 2 and forward. Any significant coefficient estimates on leads indicates potential violation of the parallel trend assumption.

[14]For example, Muskegon county of Michigan adopted BTB in January 12, 2012, so we set Muskegon to be treated by BTB in 2012. There are only two effective counties, Muskegon county of Michigan and Hillsborough county of Florida, whose jurisdiction adopted BTB between January 2 and January 31.

4.5 Results

4.5.1 BTB and Recidivism

We start by addressing our main research question: Does BTB policy affect recidivism? Table 4.3 presents the point estimates of BTB policy from equation 1. Column 1 - 3 (column 4 - 6) correspond to the results for a sample of working-age (young) ex-offenders. We control for only year and county fixed effects (column 1 and 4), add a set of county characteristics (column 2 and 5), and include county-specific linear time trend (column 3 and 6). We present the average rates of recidivism prior to BTB adoption at the top of each column. Estimates of the effects of BTB policies on rates of recidivism are significant and negative across all specifications. Our main specification in column 3 and 6 demonstrates an adoption of BTB law reduces recidivism by 5.7 percentage points (29%) and 4.7 percentage points (24%). These effects are substantial relative to the mean of pre-BTB adoption. The estimated impact of BTB policy is smaller in magnitude for the group of younger ex-offenders, partially reflecting the fact that age-profile of criminal offenders is concentrated at their early adulthood.

We have evidence suggesting that BTB policy reduces recidivism, but our discrete model does not allow us to capture the dynamics of BTB policy. To this end, we replace the BTB indicator variable in equation 4.1 with lags of BTB implementation - one and two years after adoption and year 3 onward. We report estimates from this model in Table 4.4. We observe a similar pattern in statistical significance, but it appears that the effect of BTB law for the working-age ex-offenders increases over time upon implementation. The results from column 3 report that in the first year of adoption BTB policy reduces recidivism by only 3.4 percentage points and intensifies in magnitude to 7.4-8.1 percentage points in the following years. The pattern of the estimates for the sample of young ex-offenders is similar whereas the point estimates of the first year adoption is imprecisely estimated.

The growing effects may be related to the role-model effect of public BTB policy or lack of information shared on the labor supply sides. Recall that public BTB law was adopted so that private firms could follow the best practice in hiring (Doleac and Hansen, 2016). The effect might have grown over time as more private firms voluntarily removed the box from their application forms. National chains may have been persuaded to exclude the box entirely due to the need to comply with a list of state and local BTB law (Agan and Starr, 2017). Alternatively, ex-offenders might not know their rights under BTB law. Indeed, few complaints against employers have been submitted to the District's Office of Human Resource one year after adoption (Berracasa et al, 2016), suggesting that people with criminal records may not be aware of their rights (Stacy and Cohen, 2017). Exploring how these channels are contributed to the accelerating effect of BTB policy is crucial for policy planning, but it is beyond the scope of our research to measure the effect of such diffusing information.

We next examine the internal validity of our research design. Our DID approach hinges on the parallel trend assumption, where BTB and non-BTB jurisdictions would have similar trends in rates of recidivism over the same period if they did not adopt BTB law. To verify if this assumption holds, we estimate equation 4.2. Figure 4.4 and 4.5 depict the trends of rates of recidivism for working-age and young ex-offenders by excluding an indicator variable for one year before adoption. Vertical bands represent 95 percent confidence intervals. The coefficient estimates on lead terms are close to zero and insignificant. As reported in Table 4.4, the pattern of our estimates show an accelerating effect of BTB law on recidivism. Table 4.5 presents the results for working-age and young ex-offenders along with those from a sample of age 18-40 (18-50) ex-offenders. The estimated effects on lead terms are not different from zero at the conventional level. On the whole, we do not find any evidence that BTB counties are evolving differently in rates of recidivism than non-BTB counties over the sample period.

4.5.2 Heterogeneous Effects

We have shown the results of pooling all the data of ex-offenders into county-level rates of recidivism. Employing our preferred specification and aggregating the data of particular groups of ex-offenders into the rates of recidivism, we next investigate whether particular types of ex-offenders differently responded to BTB policy. Panel A and B of Table 4.6 present the coefficient estimates for samples of working-age and young ex-offenders respectively. We report the average one-year rates of recidivism for each group at the top of each column.

The first two columns of Table 4.6 show the coefficient estimates of BTB policy on recidivism by gender. We find a similar pattern of point estimates for males but do not find any discernible evidence that females benefit from BTB law. Specifically, BTB reduces recidivism by 6.2 percentage points (30%) for working-age male ex-offenders, although the estimated effect of BTB policy is merely 2.3 percentage points and highly insignificant for female ex-offenders. We find the similar results by race and ethnicity. White and Black offenders released benefit from BTB but the estimated effect for Hispanic is marginally significant. The estimated effect for Black is particularly pronounced. As such, BTB policy reduces 10.9 percentage points (45%) in recidivism for working-age Black offenders released and 10.4 percentage points (48%) for young offenders. These results are consistent with the fact that states with BTB policy have more Black residents (Doleac and Hansen, 2016) and the finding that Black men benefit but women do not on net employment rate from BTB policy (Shoag and Veuger, 2016).

We also report the estimated effects of BTB policy by crime type (column 6-8). There are no noticeable differences in the effect of BTB on recidivism by different crime types. It implies that BTB laws uniformly deter recidivism regardless of crime type committed. Finally, we take the median of the proportion of residents whose education level is equal to or less than high school graduation, and divide the counties into two groups - counties with the less than or equal to (more than) the median. Namely, counties in a group of 'less

than or equal to the median' are high education counties. The last two columns of Table 4.6 present the point estimates on recidivism by education (column 9 and 10). We find that BTB has a larger reduction in rates of recidivism for working age ex-offenders in the high education counties (column 9 of Panel A) whereas it has no discernible effect for young ex-offenders (column 9 of Panel B). States with BTB policies have more college-educated and high income residents (Doleac and Hansen, 2016) and young offenders tend to take part in risky activities and be associated with criminals (Heller et al., 2017). Crime may increase if an improvement in labor market conditions is associated with an increase in opportunities to steal (Cantor and Land, 1985; Freedman and Owens, 2016; Schnepel 2016). The pattern of these estimates may be explained by a combination of behavioral factors and increased returns to crime associated with increases in community income.

4.5.3 Trade-off and Mechanism

Our analysis so far reveals rich differential responses to BTB policy by groups. Motivated by these findings, we first extend our analysis to explore how BTB policies affected labor markets for working age and young population. BTB policy is intended to improve employment opportunities for those with criminal records. However, employers statistically discriminate against demographic groups that are more likely to have a criminal record when an applicant's criminal history is unavailable (Agan and Starr, 2016; Doleac and Hansen, 2016). Following the Doleac and Hansen's (2016) research and using Current Population Survey (CPS) data available at National Bureau of Economic Research, we estimate a variant of their statistical discrimination model over a period of 2000-2014.[15][16] Robust standard errors clustered at states are reported.

[15]A few differences in our specification to Doleac and Hansen's one are our unit of BTB adoption is a county (theirs is Metropolitan Statistical Area), our sample consists of age 18-65 and age 25-34 workers (theirs consist of 25-34 males), and our sample period is 2000-2014 (theirs is 2004-2014). Consequently, our sample size differs from Doleac and Hansen's.

[16]As noted in the footnote of Doleac and Hansen (2016), we confirmed that about half of CPS respondents are matched to counties.

We report estimates for working age population and young population in Panels A and B of Table 4.7. Although there are some differences in specification and sample composition, we obtain point estimates of BTB policy on employment probability comparable to those of Doleac and Hansen (2016). For example, BTB is associated with a reduction in average probability that Black workers without college degrees are employed by 3.5-4.0 percentage points (5.8%-6%) (column 4 of Panel A and B). These results combined with existing evidence indicate that BTB has unintentionally done harm to a particular group of job seekers (Agan and Starr, 2016; Doleac and Hansen, 2016).

This labor market analysis poses a question: what is the effect of employment opportunities - particularly in the industry which employs more ex-offenders - following the BTB policy adoption on recidivism? Existing studies find that an employment opportunity in low-skilled jobs in a county of release decreases rate of recidivism (Schnepel 2016; Yang 2016). Borrowing Schnepel's finding that an increase in new hires in manufacturing & construction industries reduces recidivism in California, we estimate a variant of equation 4.1

$$Recidivism_{ct} = \alpha_0+\alpha_1 BTB_{ct}+\gamma_0 NewHires^i_{ct}+\gamma_1 BTB_{ct}*NewHires^i_{ct}+X'_{ct}\alpha_2+\alpha_t+\alpha_c+\alpha_c t+\epsilon_{ct}, \tag{4.3}$$

where $NewHires^i_{ct}$ is the number of new hires in industry i per one thousand working-age persons. Industries are all industries, manufacturing & construction, or all other industries.

The first (last) two columns of Table 4.8 present the point estimates of new hires and its interaction with the BTB indicator variable on recidivism for working age (young) ex-offenders. Panel A shows the results obtained from number of new hires in all industries. The estimated effect of new hires in all industries is very small in magnitude and not statistically different from zero (column 1, panel A). Similarly the estimated coefficient on the interaction term is highly insignificant (column 2, Panel A). The pattern of the estimated coefficients

for young ex-offenders is practically identical. As Schnepel (2016) documents, changes in aggregate labor demand appear to have little effect on recidivism even in BTB counties.

We next sort all industries into manufacturing & construction industries and all other industries. Panel B of Table 4.8 presents the results from this exercise. Analysis by disaggregation reveals that an increase in new hires in manufacturing & construction industries is significantly associated with a decrease in recidivism (column 1, panel B). We further include interaction terms with a BTB indicator variable and new hires in each industries to allow the effects to differ depending on whether jurisdictions adopted BTB policy. We find a modest and statistically significant decrease (increase) in recidivism is associated with an increase in new hires in manufacturing & construction (all other) industries following the adoption of BTB. Specifically, 0.17 (0.02) percentage points decrease (increase) in recidivism is associated with one extra manufacturing & construction (all other industries) hire per one thousand working-age persons in a county after BTB policy is in effect (column 2, Panel B). Turning to young ex-offenders, we find that the effects of new hires in each industry are amplified following BTB adoption. That is, a similar increase in manufacturing & construction (all other industries) hires is associated with a 0.6 (0.06) percentage point decrease (increase) in recidivism (column 4, Panel B). Relative to the BTB preadoption mean of recidivism, a similar change in manufacturing & construction (all other industries) hire is associated with a reduction (an increase) in one-year recidivism by 3.1% (0.3%). To put this into perspective, our estimated effect of new hires in manufacturing & construction after the BTB adoption is twice the observed effect in new hires in California's manufacturing industry (Schnepel 2016) and one-third the effect of eligibility for welfare (Yang, 2017).

4.5.4 Robustness Check

In Table 4.9 we test if our main results are robust. First we check if the timing of BTB adoption matters. Recall the BTB indicator variable in our main specification takes a value

of one, for example, in 2010 (in 2011) and onward if a jurisdiction adopts BTB policy from Jan 2 to Jan 31, 2010 (on January 1, 2010 or any dates between February 1 and December 31, 2010). We replace it with the BTB indicator variable taking a value of one in 2010 and onward if a jurisdiction adopted BTB policy anytime in 2010. The results using this alternative indicator variable are presented in column 1 of Table 4.9. Note that the estimated effects on recidivism are smaller than those reported in Table 4.3 (column 3 and 6). To investigate difference in magnitude, we estimate the dynamic effect model in Table 4.4 with these alternative BTB indicator variables. Unsurprisingly, consistent with the results from Table 4.4, we only find large and significant second and third year effects in our main specification. As such, the second and third post-adoption effects of BTB are 3.6 and 8.0 percentage points decreases in rate of recidivism and significant at conventional levels (column 3, Table **??**). Table **??** reports the results from the falsification test with this BTB treatment indicator variable.[17] Taken together, if anything, we find these point estimates in column 1 reflect a difference in the effects of BTB policy in the first and following year(s) of adoption.

Second, recall that we consider a county treated by BTB if a city encompassed by the county has BTB policy. Figure 4.6 depicts a map of Hennepin county and Minneapolis showing this relationship. Hennepin county which covers other areas is considered to be treated because Minneapolis adopted BTB policy in 2006, and such BTB adoption rule may underestimate the effect of BTB policy. Excluding these counties barely alter the estimates (column 2). Third, our measure of one-year rate of recidivism in 2014 may be contaminated owing to right-censoring. For example, an ex-offender released on December 1, 2014 may return to prison any date between Jan 1 and November 30, 2015, but our one-year rate of recidivism for the year 2014 does not count this ex-offender. To examine if this is a threat for robustness, we exclude all counties in 2014 to see whether the last year of the sample period

[17] In appendix Table Table **??**, we provide the results from falsification test using this BTB indicator. We do not observe any sign for discernible trends in recidivism for young ex-offenders, but we do observe statistically significant pre-treatment trends for working-age ex-offenders.

drives the negative effect. The results remain intact (column 3). Fourth, recall January 1 of a year is set as the start date if only the year of BTB implementation is available (Doleac and Hansen, 2016). To rule out the possibility that these counties are influential observations, we drop counties which are set to implement BTB policy in January 1. The effect of BTB policy is slightly larger in magnitude, but still consistent with those for the entire sample (column 4).

Fifth, to test if regional macroeconomic factors affect the effect of BTB policy, we include region by year fixed effects in our model (column 5). It is reassuring that the estimates remain unchanged. Finally, we report the association of BTB policy to two- and three-year rates of recidivism (column 6 and 7). Note that the pre-treatment mean of the rate of recidivism presented in the top of each column increases as a year elapses from two to three. Irrespective of analysis population, the estimated effects contract as the duration for which ex-offenders are allowed to recidivate widens. For example, BTB is associated with a 21.7% (14.7%) decrease in two-year (three-year) rates of recidivism (column 6 and 7, Panel A). Although caution must be warranted to interpret these results due to right-censoring on two- and three-year rates of recidivism, these estimates reinforce our main finding that BTB policies reduce recidivism.

4.6 Policy Discussion

Throughout this paper we present evidence that BTB policies reduced one-year rates of recidivism. Although we cannot disentangle the effects of the role-model effect of public BTB policy and lack of information shared on the labor supply sides, the effect of BTB policies appears to grow following its implementation. Our finding resembles that the effect of BTB policies on Black male employment is large and grow over time (Doleac and Hansen, 2016). Recall also that BTB policies reduce recidivism for Black male offenders released but have no discernible effect on recidivism for female ex-offenders. Consistent with Shoag and

Veuger (2016), these results indicate that BTB policies disproportionately benefit Black male ex-offenders over females. Our subgroup analysis by county education level also reveals that BTB policy benefits working-age ex-offenders but does not show any evidence of benefiting young ex-offenders. Potential explanations may be related to a combination of behavioral factors (e.g., young ex-offenders have a tendency to associate with criminals) and increased returns to crime associated with increases in community wealth.

These results motivate us to extend Doleac and Hansen's (2016) research to investigate the effect of BTB policy on employment probability of minority workers for our age groups of interest. As documented by Doleac and Hansen, BTB policy reduces Black male employment significantly. This result coupled with our findings leads us to a conclusion that BTB policy is a "double-edged sword". We further investigate the interactive effect of BTB and local employment opportunities on recidivism. We find that employment opportunities in manufacturing & construction industries reduce recidivism, and the effect is particularly amplified for young ex-offenders.

To translate these estimates into the effect of BTB policy in terms of returning citizens, we perform a straightforward back-of-envelope calculation. There are approximately 62,000 prisoners released from BTB state prisons in 2014 (Carson, 2015).[18] Our estimated effect of BTB policy is a 29% reduction in one-year rate of recidivism. There would be a total of 17,980 (62,000×(-0.29)) working-age ex-offenders who would not return to prison within one year. Thus, our results indicate that BTB policy prevents ex-offenders from recidivating.

We also perform a similar calculation for the impact of employment opportunities in BTB states. New hires in manufacturing & construction industries upon BTB adoption are associated with a reduction in recidivism by 0.9%. There would be a total of 5,580 (0.009×62,000) working-age ex-offenders who would not recidivate within one year. The average annual cost per inmate is 41,000 dollars in 2010 value (Henrichson and Delaney,

[18] The finest level of granularity in the number of ex-offenders released in a year is state. BTB states in 2014 are CA, CO, NE, MN, MA, and RI in our analysis sample. Table 7 of Carson (2015) allows us to calculate a total of 61,875 prisoners released in 2014 in these six BTB states.

2012).[19] Hence, the estimated reduction in annual correctional spending is 228 million dollars in 2010 value. Given the operation cost for federal and state prison is 80 billion dollars (Picchi, 2014), a fiscal policy to expand employment opportunities in manufacturing & construction industries accompanied with BTB policies reduces a 0.3 % reduction in the operational cost, indicating that it is an effective way to reduce recidivism.

4.7 Conclusion

BTB policies have been adopted by different levels of jurisdictions to support fair employment opportunities for citizens with criminal histories. The number of BTB jurisdictions exceeds 100 and 12 states have active BTB law in 2014. Despite the widespread adoption of BTB laws, the empirical evidence is concentrated in statistical discrimination literature and few studies have been devoted to studying the effect of BTB on recidivism - returning citizens that BTB policies are intended to work for. Under the assumption that the average rate of recidivism in BTB counties would evolve similarly in non-BTB counties over the sample period if no jurisdiction implemented BTB policy, our DID approach teases out the causal effects of BTB policy. Using a large-scale NCRP dataset and extracting variation in the timing of BTB adoption for a period of 2000-2014, we obtain a set of empirical results applicable to public policy.

We find a large and significant effect of BTB on recidivism and the effect of BTB policy grows over time. We also observe that male Black ex-offenders benefit from BTB policy but female ex-offenders may not. Additionally, we do not detect a significant reduction in recidivism for young ex-offenders in highly educated counties, suggesting that this result may be related to a combination of behavioral factors and an increase in return to crime

[19] BTB states in our 2014 sample are CA, CO, NE, MN, MA, and RI in 2014. Figure 2 of Henrichson and Delaney (2012) allows us to calculate 40,848 dollars as the average annual cost per inmate. Note that the annual cost per inmate is not available in MA.

in high income neighborhoods. Furthermore, we confirm that BTB laws induce employers to statistically discriminate against Black males. Finally, an increase in new employment opportunities in industries which hire more young ex-offenders upon BTB adoption is associated with a 3.1% reduction in recidivism.

From a public policy standpoint, BTB policy is a double-edged sword. It hurts Black male workers with and without any criminal records but benefits those recently released from prison. An important takeaway from our analysis is BTB policy coupled with fiscal policy targeting job creation in particular industries (e.g., manufacturing and construction industries) is an effective way to help ex-offenders return to society.

Table 4.1: Summary Statistics of Released Offenders and Counties 2000-2014

	Age 18-65		Age 25-34	
Panel A: Offender Characteristics	Mean	SD	Mean	SD
One Year Recidivism	0.137	0.343	0.132	0.338
Two Year Recidivism	0.219	0.414	0.215	0.411
Three Year Recidivism	0.264	0.441	0.262	0.440
White	0.476	0.499	0.466	0.499
Black	0.363	0.481	0.353	0.478
Hispanic	0.172	0.378	0.195	0.396
Male	0.867	0.339	0.858	0.349
Age at Prison Release	34.020	10.304	29.642	2.881
$<$ High School Graduation	0.333	0.471	0.333	0.471
Sentence Length (years)	2.141	3.224	2.006	2.418
Violent	0.266	0.442	0.270	0.444
Property	0.274	0.446	0.262	0.440
Drug	0.286	0.452	0.305	0.460
Sample Size	4139415		1444741	
Panel B: County Characteristics (County-Year)				
Outcome variables				
One Year Recidivism	0.127	0.126	0.127	0.161
Two Year Recidivism	0.205	0.157	0.211	0.201
Three Year Recidivism	0.247	0.171	0.257	0.217
Control Variables				
Unemployment Rate	0.068	0.029	0.068	0.029
Proportion of male	0.501	0.021	0.501	0.021
Proportion of $\leq$ High School Graduation	0.535	0.112	0.535	0.113
Proportion of White	0.872	0.146	0.869	0.145
Proportion of Black	0.081	0.132	0.084	0.133
Proportion of Hispanic	0.077	0.122	0.078	0.123
New hires in manuf&const (per 1000)	3.606	11.718	1.106	3.493
New hires in other industries (per 1000)	23.062	84.109	7.117	26.447
Sample Size	31192		29378	
Number of Counties	2709		2683	

Source: National Corrections Reporting Program, 2000-2014 (Bureau of Justice Statistics, 2016), Local Area Unemployment Statistics, American Community Survey 2009-2014, and Decennial Census 2000 (Bureau of Labor Statistics, 2017), and Quarterly Workforce Indicators 2000-2014 (United States Bureau of Census, 2017)

Table 4.2: State and County BTB Adoption

	Age 18-65				Age 25-34			
		BTB Counties	non-BTB Counties			BTB Counties	non-BTB Counties	
Year of Adoption	# of BTB States (1)	# of Counties adopted (2)	# of Counties not adopted yet (3)	# of Counties never adopted (4)	# of BTB States (5)	# of Counties adopted (6)	# of Counties not adopted yet (7)	# of Counties never adopted (8)
2000	0	0	414	740	0	0	368	714
2001	0	0	434	1047	0	0	384	995
2002	0	0	432	1291	0	0	392	1228
2003	0	0	441	1308	0	0	400	1255
2004	0	0	443	1305	0	0	405	1251
2005	0	2	438	1586	0	2	405	1510
2006	0	4	440	1756	0	4	407	1681
2007	0	8	436	1813	0	8	395	1736
2008	0	10	434	1825	0	10	396	1732
2009	1	94	353	1908	1	79	327	1813
2010	2	150	292	1957	2	143	266	1856
2011	0	176	284	2046	0	170	267	1940
2012	1	242	227	2148	1	223	202	1999
2013	1	253	186	2086	1	236	177	1971
2014	1	326	0	1857	1	288	0	1743
Total States / Counties	6	6519		24673	6	5954		23424
Total Unique Counties		491		2218		485		2198

Notes: The sample includes all years starting over a period of 2000-2014. Columns (1) and (5) refer to states adopting BTB law at any year over the sample period. Columns (2)-(3) and (6)-(7) refer to counties of which jurisdictions were adopting BTB law at any year. See text for further details.

Table 4.3: The Estimated Impact of BTB Law on Recidivism within One Year

	Age 18-65			Age 25-34		
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Treatment Mean	0.194	0.194	0.194	0.190	0.190	0.190
BTB	-0.074***	-0.068***	-0.057***	-0.076***	-0.067***	-0.047***
	(0.010)	(0.009)	(0.011)	(0.012)	(0.011)	(0.015)
Year and county FE	Yes	Yes	Yes	Yes	Yes	Yes
Control	No	Yes	Yes	No	Yes	Yes
County Linear Trend	No	No	Yes	No	No	Yes
R^2	0.404	0.406	0.499	0.297	0.299	0.400
Sample Size	31192	31192	31192	29378	29378	29378

Notes: BTB stands for Ban the Box. The dependent variable is annual rate of recidivism within a county. Ordinary least squares estimates are presented. Huber-White robust standard errors clustered at counties allow for arbitrary correlation of residuals within each county. * 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 4.4: Dynamic Effects of BTB Law on Recidivism within One Year

	Age 18-65			Age 25-34		
	(1)	(2)	(3)	(4)	(5)	(6)
1st Year Postadoption	-0.043***	-0.037***	-0.034***	-0.044***	-0.035***	-0.025
	(0.011)	(0.011)	(0.011)	(0.014)	(0.013)	(0.016)
2nd Year Postadoption	-0.085***	-0.079***	-0.074***	-0.087***	-0.079***	-0.065***
	(0.012)	(0.011)	(0.013)	(0.016)	(0.015)	(0.018)
3rd Year and Forward	-0.088***	-0.081***	-0.081***	-0.091***	-0.081***	-0.070***
	(0.012)	(0.011)	(0.013)	(0.014)	(0.014)	(0.019)
Year and county FE	Yes	Yes	Yes	Yes	Yes	Yes
Control	No	Yes	Yes	No	Yes	Yes
County Linear Trend	No	No	Yes	No	No	Yes
R^2	0.405	0.407	0.500	0.297	0.299	0.400
Sample Size	31192	31192	31192	29378	29378	29378

Notes: Huber-White robust standard errors clustered at counties allow for arbitrary correlation of residuals within each county. See notes of table 4.3 and the text for further detials. * 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 4.5: Effects of BTB Law on Recidivism within One Year by Years Elapsed from Adoption

	Age			
	18-65	18-40	18-50	25-34
BTB law leads and lags:	(1)	(2)	(3)	(4)
4th Year and More Prior	0.009	0.005	0.005	0.014
	(0.011)	(0.015)	(0.012)	(0.016)
3rd Year Prior	0.014	0.013	0.007	0.022
	(0.012)	(0.015)	(0.013)	(0.015)
2nd Year Prior	-0.005	-0.013	-0.014	0.001
	(0.012)	(0.014)	(0.013)	(0.015)
1st Year Prior - Omitted	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
1st Year Postadoption	-0.032**	-0.045***	-0.043***	-0.018
	(0.013)	(0.015)	(0.013)	(0.017)
2nd Year Postadoption	-0.073***	-0.094***	-0.085***	-0.060***
	(0.014)	(0.017)	(0.015)	(0.019)
3rd Year and Forward	-0.083***	-0.093***	-0.088***	-0.068***
	(0.014)	(0.017)	(0.015)	(0.020)
Year and county FE	Yes	Yes	Yes	Yes
Control	Yes	Yes	Yes	Yes
Region by Year FE	Yes	Yes	Yes	Yes
H_0: $\beta_{t1}^{post} = \beta_{t3}^{post}$	0.00	0.00	0.00	
R^2	0.500	0.476	0.493	0.400
Sample Size	31192	30776	31055	29378

Notes: Year prior to BTB law adoption is the omitted category. BTB Law change indicators (3rd Year Prior - 2nd Year Postadoption) are equal to one in only one year each per adopting county. 4th Year and More Prior (3rd Year and Forward) indicator variables are equal to one in every year beginning with the fourth year after (the third year before) BTB law adoption. * 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 4.6: Heterogeneous Effects of BTB Law on Recidivism within One Year

	Gender		Race/Ethnicity			Crime Type			$\leq$ High School Grad	
	Male	Female	White	Black	Hispanic	Violent	Property	Drug	$\leq$ Median	$>$ Median
Panel A: Age 18-65	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pre-Treatment Mean	0.203	0.143	0.193	0.244	0.162	0.210	0.240	0.150	0.194	0.195
BTB	-0.062***	-0.023	-0.060***	-0.109***	-0.034*	-0.088***	-0.047***	-0.048***	-0.087***	-0.051***
	(0.010)	(0.017)	(0.012)	(0.022)	(0.018)	(0.015)	(0.017)	(0.013)	(0.031)	(0.011)
R^2	0.486	0.349	0.454	0.434	0.410	0.390	0.403	0.378	0.442	0.546
Sample Size	31097	26273	30893	21957	16821	29091	29304	28312	16188	15004
Panel B: Age 25-34										
Pre-Treatment Mean	0.194	0.166	0.197	0.216	0.150	0.208	0.232	0.148	0.194	0.195
BTB	-0.057***	-0.005	-0.066***	-0.104***	-0.042*	-0.063***	-0.053**	-0.059***	-0.030	-0.047***
	(0.014)	(0.021)	(0.017)	(0.024)	(0.023)	(0.019)	(0.022)	(0.018)	(0.041)	(0.016)
R^2	0.382	0.368	0.379	0.434	0.440	0.350	0.355	0.354	0.343	0.455
Sample Size	29115	21402	28451	18180	12963	24507	25378	24660	15221	14157
Year and county FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The model specifications are identical to those in the column (3) and (6) of table 4.3. Huber-White robust standard errors clustered at counties allow for arbitrary correlation of residuals within each county. * 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 4.7: The Estimated Impact of the BTB Law on Employment 2000-14.

Panel A: Age 18-65 with No College Degree				
Pre-Treatment Mean	(1)	(2)	(3)	(4)
White	0.716	0.716	0.716	0.716
Black	0.605	0.605	0.605	0.605
Hispanic	0.661	0.661	0.661	0.661
BTB	-0.052***	-0.039***	0.004	0.008
	(0.010)	(0.010)	(0.004)	(0.005)
BTB × Black	-0.098***	-0.035**	-0.041***	-0.035***
	(0.016)	(0.014)	(0.013)	(0.011)
BTB × Hispanic	-0.010	-0.001	0.001	0.002
	(0.015)	(0.011)	(0.010)	(0.011)
R^2	0.017	0.077	0.080	0.081
Sample Size	2986246	2986246	2986246	2986246
Panel B: Age 25-35 with No College Degree				
Pre-Treatment Mean				
White	0.752	0.752	0.752	0.752
Black	0.664	0.664	0.664	0.664
Hispanic	0.719	0.719	0.719	0.719
BTB	-0.053***	-0.040**	0.004	0.008
	(0.014)	(0.016)	(0.009)	(0.010)
BTB × Black	-0.110***	-0.039**	-0.047***	-0.040***
	(0.020)	(0.018)	(0.016)	(0.013)
BTB × Hispanic	-0.001	-0.005	-0.002	0.001
	(0.022)	(0.019)	(0.018)	(0.018)
County FEs	Yes	Yes	Yes	Yes
Demographics	No	Yes	Yes	Yes
Time * Region FEs	No	No	Yes	Yes
County-Specific trends	No	No	No	Yes
R^2	0.020	0.039	0.045	0.048
Sample Size	555325	555325	555325	555325

Notes: Current Population Survey 2000-2014 (National Bureau of Economic Research, 2017). Ordinary least squares estimates are presented. Huber-White robust standard errors clustered at states allow for arbitrary correlation of residuals within each state. * 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 4.8: The Estimated Impact of New Hires on Recidivism within One Year.

	Age 18-65		Age 25-34	
Panel A: All Industries	(1)	(2)	(3)	(4)
Recidivism Mean	0.194	0.194	0.190	0.190
BTB	-0.0571***	-0.0547***	-0.0470***	-0.0474***
	(0.0106)	(0.0115)	(0.0150)	(0.0167)
New Hires	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0001)	(0.0001)
BTB × New Hires		-0.0000		0.0000
		(0.0000)		(0.0001)
R^2	0.499	0.499	0.400	0.400
Sample Size	31192	31192	29378	29378
Panel B: Construction & Manufacturing				
BTB	-0.0572***	-0.0560***	-0.0471***	-0.0449***
	(0.0105)	(0.0117)	(0.0150)	(0.0171)
Const & Manuf New Hires	-0.0006**	-0.0004	-0.0018*	-0.0009
	(0.0002)	(0.0003)	(0.0010)	(0.0010)
All Other New Hires	0.0001*	0.0000	0.0003**	0.0001
	(0.0000)	(0.0000)	(0.0002)	(0.0002)
BTB × Const & Manuf New Hires		-0.0017***		-0.0059**
		(0.0006)		(0.0026)
BTB × All Other New Hires		0.0002**		0.0006**
		(0.0001)		(0.0002)
R^2	0.499	0.499	0.400	0.400
Sample Size	31192	31192	29378	29378
Year and county FE	Yes	Yes	Yes	Yes
County Linear Trend	Yes	Yes	Yes	Yes
Control	Yes	Yes	Yes	Yes

Notes: See the text for the detail. Control variables include unemployment rate and proportions of male, less than high school graduates, White, Black, and Hispanic. Huber-White robust standard errors clustered at counties allow for arbitrary correlation of residuals within each county. * 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 4.9: Robustness Check

	Set BTB Adoption Timing to January 1	Exclude Counties Where Cities Adopted BTB First	Exclude Counties Adopted BTB in 2014	Exclude Counties Adopted BTB in January 1	Add Region by Year Fixed Effects	Two Year Recidivism	Three Year Recidivism
Panel A: Age 18-65	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pre-Treatment Mean	0.194	0.194	0.194	0.194	0.194	0.276	0.311
BTB	-0.026***	-0.064***	-0.056***	-0.072***	-0.054***	-0.060***	-0.046***
	(0.009)	(0.012)	(0.011)	(0.013)	(0.011)	(0.011)	(0.011)
R^2	0.498	0.496	0.507	0.509	0.502	0.536	0.566
Sample Size	31192	30623	28529	28468	31192	31192	31192
Panel B: Age 25-34							
Pre-Treatment Mean	0.190	0.190	0.190	0.190	0.190	0.275	0.314
BTB	-0.027**	-0.053***	-0.046***	-0.080***	-0.040***	-0.064***	-0.043**
	(0.013)	(0.017)	(0.015)	(0.019)	(0.015)	(0.016)	(0.017)
R^2	0.399	0.397	0.408	0.413	0.402	0.433	0.453
Sample Size	29378	28809	27020	26871	29378	29378	29378
Year and county FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region by Year FE	No	No	No	No	Yes	No	No

Notes: The model specifications are identical to those in the column (3) and (6) of table 4.3. * 0.10, ** 0.05 and ***0.01 denote significance levels.

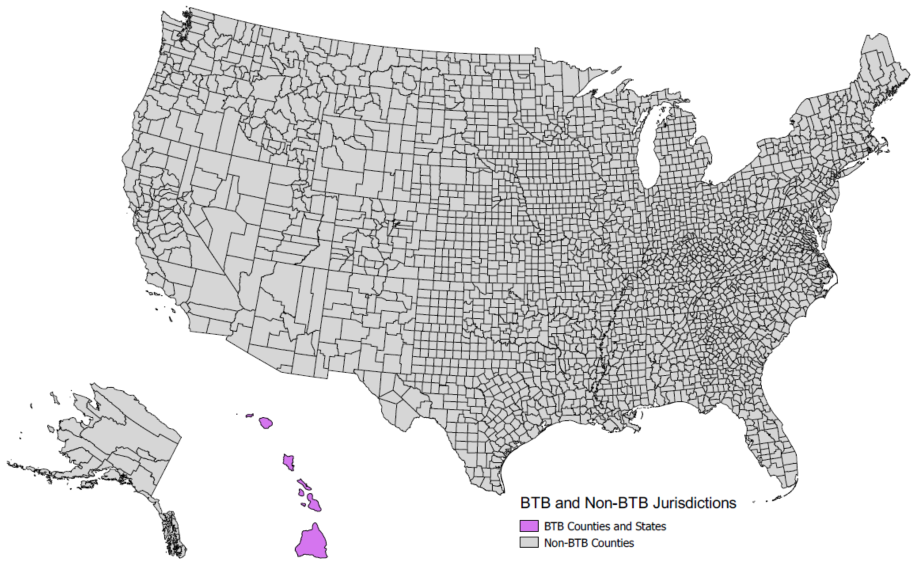


Figure 4.1: BTB and non-BTB Counties in 2000.

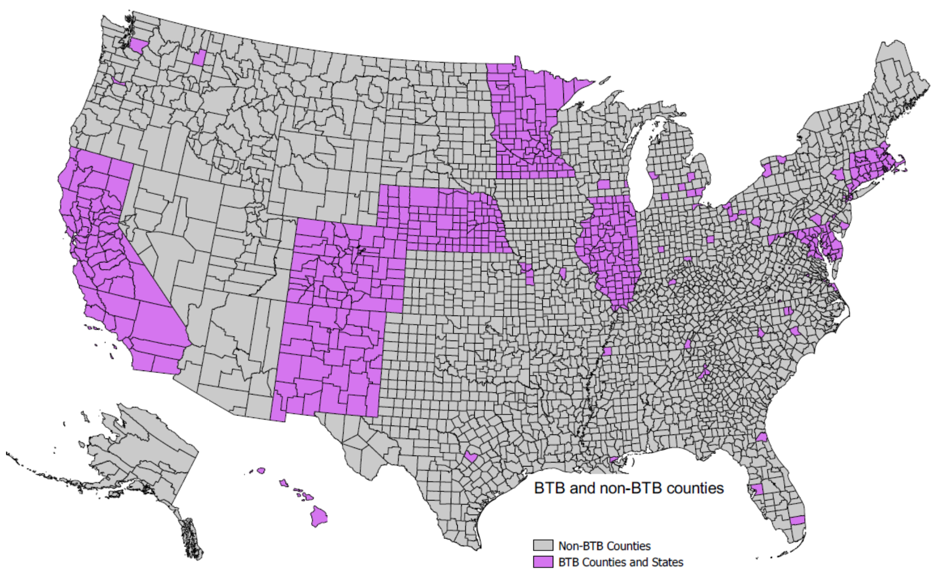


Figure 4.2: BTB and non-BTB Counties in 2014

Notes: Figure 4.1 and 4.2 show BTB and non-BTB counties in 2000 and 2014. The counties marked purple are in effect of BTB laws, whereas those marked gray are not. See text for the details.

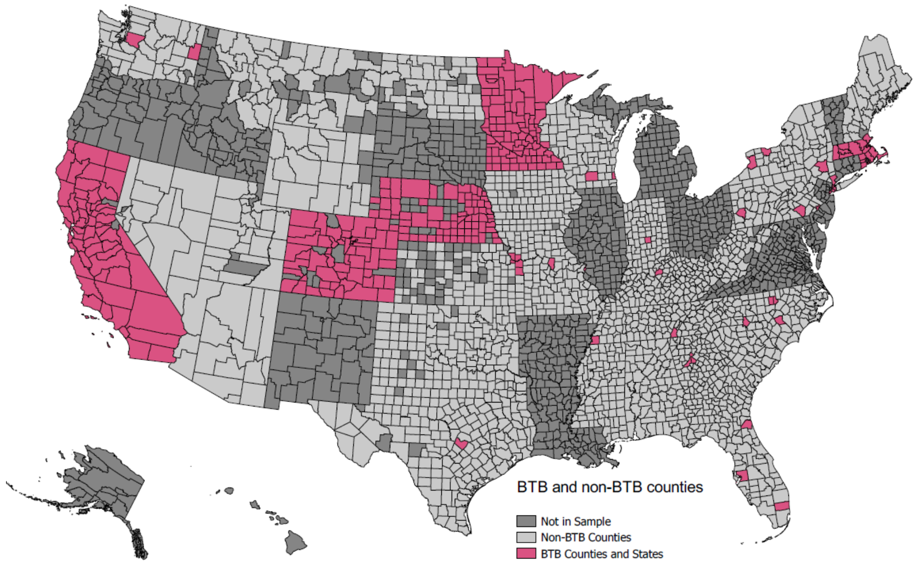


Figure 4.3: BTB and non-BTB counties Used for Our Main Analysis in 2014.

Notes: Figure 4.3 shows BTB and non-BTB counties used for our main analysis. See text for the details.

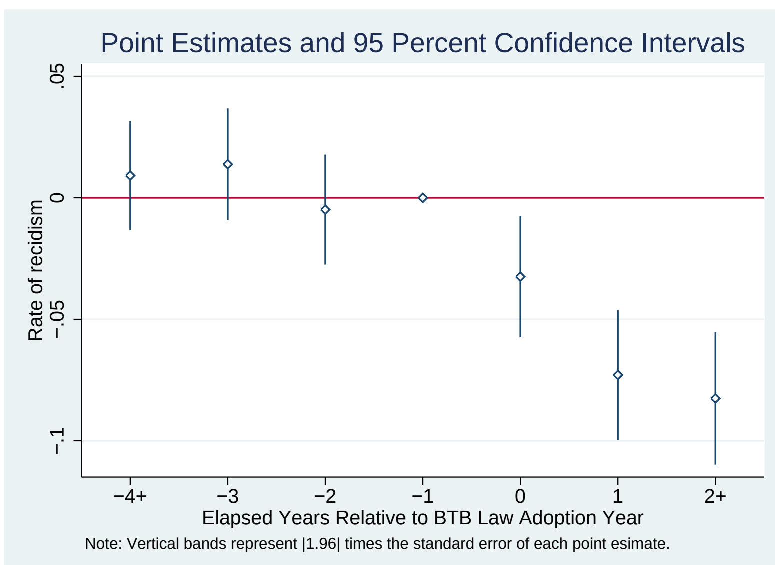


Figure 4.4: Falsification Test: Age 18-65

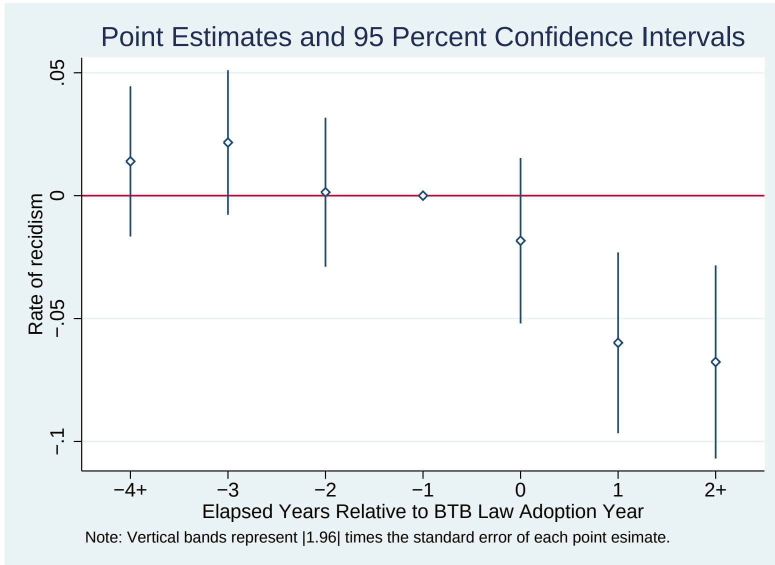


Figure 4.5: Falsification Test: Age 25-34

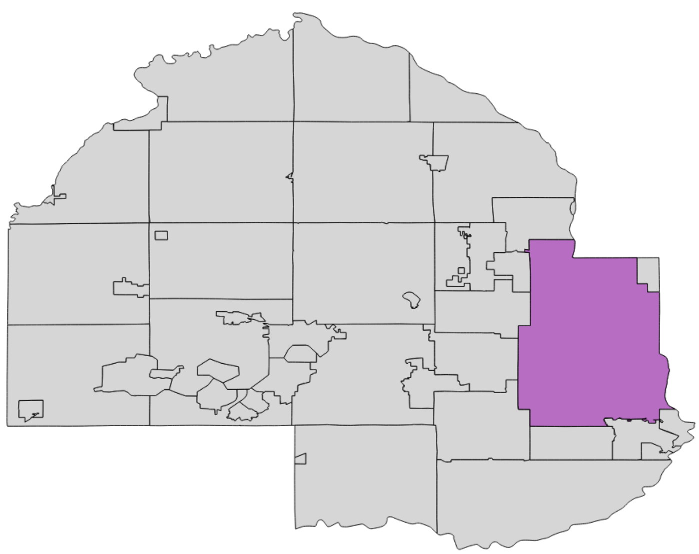


Figure 4.6: Hennepin County and Minneapolis

Notes: This figure shows an example of a county encompassing a city jurisdiction which first adopted BTB. The gray area represents Hennepin County while the purple area represents Minneapolis

Chapter 5. Conclusion

I have presented studies related to the recent U.S. society in my dissertation. Unlike my home country, the U.S. has a dynamic economy, a higher degree of inequality, and racial division. The U.S. also provides empirical researchers with fairer access to the data. The three essays presented in my dissertation are solely focused on the U.S. because I am curious about changes that the U.S. has been going through.

The first chapter analyzes how the inflow of foreign direct investment (FDI) affects entrepreneurial activities. Separating a pool of contiguous U.S. states into pro- and non pro-business states identified by Right-to-Work status, we find that FDI slows down the business creation and destruction rates in the non-pro business states. The second chapter examines if the positive benefits of breastfeeding reported by observational research is causal. Employing a newly developed sensitivity check methodology, we find that decision to breastfeeding is sensitive to non-random selection into breastfeeding. The third chapter investigates if Ban-the-box (BTB) policy - the law prohibits employers to ask job applicants' criminal history on the job application forms - reduces recidivism. We find that BTB policy reduces recidivism. We also find that the employment opportunities in the industries that hire more ex-offenders upon BTB adoption prevent recidivism.

My research interest lies in the empirical studies related to the U.S. economy and society. I continue my research activities and provide policy relevant findings to the society. This is how I contribute to the society as a scholar.

References

Agan, Amanda and Sonja Starr. 2016. "Ban the box, criminal records, and statistical discrimination: A field experiment." *Law and Economics Research Paper Series* 16-012.

———. 2017. "The Effect of Criminal Records on Access to Employment." *American Economic Review* 107 (5):560–64.

Aghion, Philippe, Ufuk Akcigit, and Peter Howitt. 2014. "Chapter 1 - What Do We Learn From Schumpeterian Growth Theory?" In *Handbook of Economic Growth*, vol. Volume 2, edited by Aghion Philippe and N. Durlauf Steven. Elsevier, 515–563.

Alfaro, Laura. 2014. "Foreign Direct Investment: Effects, Complementarities, and Promotion." *Harvard Business School, Working Paper: 15-006.* .

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):151–184.

Baker, Michael and Kevin Milligan. 2008. "Maternal employment, breastfeeding, and health: Evidence from maternity leave mandates." *Journal of Health Economics* 27 (4):871–887.

Bank, World. 2015. *World development indicators 2015.* Washington D.C.: World Bank Group, 2015.

Barrios, Salvador, Holger Görg, and Eric Strobl. 2005. "Foreign direct investment, competition and industrial development in the host country." *European Economic Review* 49 (7):1761–1784.

Belfield, Clive R. and Inas Rashad Kelly. 2012. "The Benefits of Breast Feeding across the Early Years of Childhood." *Journal of Human Capital* 6 (3):251–277.

Bernasco, Wim, Richard Block, and Stijn Ruiter. 2013. "Go where the money is: modeling street robbers' location choices." *Journal of Economic Geography* 13 (1):119–143.

Berracasa, Colenn;, Alexis Estevez;, Charlotte Nugent;, Kelly Roesing;, and Jerry Wei;. 2016. "The Impact of 'Ban the Box' in the District of Columbia." Tech. rep., Office of the Distric of Columbia Auditor.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How much should we trust differences-in-differences estimates?" *The Quarterly journal of economics* 119 (1):249–275.

Blanchflower, David G and Andrew J Oswald. 1998. "What makes an entrepreneur?" *Journal of labor Economics* 16 (1):26–60.

Britton, John R., Helen L. Britton, and Virginia Gronwaldt. 2006. "Breastfeeding, Sensitivity, and Attachment." *Pediatrics* 118 (5):e1436–e1443.

Cameron, A Colin, Jonah B Gelbach, and Douglas L Miller. 2008. "Bootstrap-based improvements for inference with clustered errors." *The Review of Economics and Statistics* 90 (3):414–427.

Cantor, David and Kenneth C. Land. 1985. "Unemployment and Crime Rates in the Post-World War II United States: A Theoretical and Empirical Analysis." *American Sociological Review* 50 (3):317–332.

Carson, E. Ann. 2015. "Prisoners in 2014." Tech. Rep. NCJ 248955, Bureau of Justice Statistics.

Centers for Disease Control and Prevention. 2015. "Nutrition, physical activity and obesity data, trends and maps web site."

Cullen, Julie Berry and Roger H Gordon. 2007. "Taxes and entrepreneurial risk-taking: Theory and evidence for the US." *Journal of Public Economics* 91 (7):1479–1505.

D'Alessio, Stewart J, Lisa Stolzenberg, and Jamie L Flexon. 2015. "The effect of HawaiiâĂŹs Ban the Box Law on repeat offending." *American Journal of Criminal Justice* 40 (2):336–352.

Danakol, Seçil Hülya, Saul Estrin, Paul D Reynolds, and Utz Weitzel. 2014. "Foreign Direct Investment and Domestic Entrepreneurship: Blessing or Curse?" *IZA Working Paper, No. 7796.* .

Desai, Mihir A., C. Fritz Foley, and Jr. Hines, James R. 2011. "Tax Policy and the Efficiency of U.S. Direct Investment Abroad." *National Tax Journal* 64 (4):1055–1082.

Dewey, Kathryn G, M Jane Heinig, and Laurie A Nommsen-Rivers. 1995. "Differences in morbidity between breast-fed and formula-fed infants." *The Journal of Pediatrics* 126 (5):696–702.

Dinopoulos, Elias and Bulent Unel. 2015. "Entrepreneurs, jobs, and trade." *European Economic Review* 79:93–112.

Doleac, Jennifer L. and Benjamin Hansen. 2016. "Does "Ban the Box" Help or Hurt Low-Skilled Workers? Statistical Discrimination and Employment Outcomes When Criminal Histories are Hidden." *National Bureau of Economic Research Working Paper Series* No. 22469.

———. 2017. "Moving to Job Opportunities? The Effect of "Ban the Box" on the Composition of Cities." *American Economic Review* 107 (5):556–59.

Durose, Matthew R., Howard N. Snyder, and Alexia D. Cooper. 2015. "Multistate Criminal History Patterns of Prisoners Released in 30 States." Tech. rep., Bureau of Justice Statistics.

Ellwood, David T. and Glenn Fine. 1987. "The Impact of Right-to-Work Laws on Union Organizing." *Journal of Political Economy* 95 (2):250–273.

Eren, Ozkan and Serkan Ozbeklik. 2015. "Leadership Activities and Future Earnings: Is There a Causal Relation?" *Journal of Human Capital* 9 (1):45–63.

———. 2016. "What Do Right-to-Work Laws Do? Evidence from a Synthetic Control Method Analysis." *Journal of Policy Analysis and Management* 35 (1):173–194.

Esther, Duflo. 2001. "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment." *American Economic Review* 91 (4):795–813.

Fairlie, Robert W. 1999. "The absence of the African-American owned business: An analysis of the dynamics of self-employment." *Journal of Labor Economics* 17 (1):80–108.

———. 2014. "Kauffman Index of Entrepreneurial Activity 1996-2013." *Technical Report, Ewing Marion Kauffman Foundation 2014.* .

Fairlie, Robert W and Alicia M Robb. 2008. "Race and entrepreneurial success: Black-, Asian-, and White-owned businesses in the United States." *MIT Press Books* 1.

Farber, Henrys. 2005. "Nonunion Wage Rates and the Threat of Unionization." *ILR Review* 58 (3):335–352.

Fergusson, David M and Lianne J Woodward. 1999. "Breast feeding and later psychosocial adjustment." *Paediatric and Perinatal Epidemiology* 13 (2):144–157.

Fitzsimons, Emla and Marcos Vera-Hernández. 2015. "Breastfeeding and Child Development." *mimeo.* .

Fletcher, Jason M. 2011. "Long-term effects of health investments and parental favoritism: the case of breastfeeding." *Health Economics* 20 (11):1349–1361.

Freedman, Matthew and Emily G Owens. 2016. "Your friends and neighbors: localized economic development and criminal activity." *Review of Economics and Statistics* 98 (2):233–253.

Friedman, Matthew. 2015. "Just Facts: As Many Americans Have Criminal Records As College Diplomas." *Brennan Center for Justice, New York University School of Law* .

Gelbach, Jonah B. 2016. "When Do Covariates Matter? And Which Ones, and How Much?" *Journal of Labor Economics* 34 (2):509–543.

Gentry, William M and R Glenn Hubbard. 2000. "Tax policy and entrepreneurial entry." *The American economic review* 90 (2):283–287.

Groggins, Becki R. and Dennis A. DeBacco. 2015. "Survey of State Criminal History Information Systems, 2014." Tech. rep., SEARCH, The National Consortium for Justice Information and Statistics.

Grossman, Gene M. 1984. “International Trade, Foreign Investment, and the Formation of the Entrepreneurial Class.” *American Economic Review* 74 (4):605–614.

Hall, Robert E. and Susan E. Woodward. 2010. “The Burden of the Nondiversifiable Risk of Entrepreneurship.” *The American Economic Review* 100 (3):1163–1194.

Haltiwanger, John, Ron S Jarmin, and Javier Miranda. 2013. “Who creates jobs? Small versus large versus young.” *Review of Economics and Statistics* 95 (2):347–361.

Heller, Sara B., Anuj K. Shah, Jonathan Guryan, Jens Ludwig, Sendhil Mullainathan, and Harold A. Pollack. 2017. “Thinking, Fast and Slow? Some Field Experiments to Reduce Crime and Dropout in Chicago*.” *The Quarterly Journal of Economics* 132 (1):1–54.

Helpman, Elhanan, Marc J Melitz, and Stephen R Yeaple. 2004. “Export versus FDI with Heterogeneous Firms.” *American Economic Review* 94 (1):300–316.

Henrichson, Christian and Ruth Delaney. 2012. “The price of prisons.” *Vera Institute of Justice* .

Hirsch, Barry T. 1980. “The Determinants of Unionization: An Analysis of Interarea Differences.” *ILR Review* 33 (2):147–161.

Holmes, Thomas J. 1998. “The effect of state policies on the location of manufacturing: Evidence from state borders.” *Journal of political Economy* 106 (4):667–705.

Holtz-Eakin, Douglas, David Joulfaian, and Harvey S Rosen. 1994. “Sticking it out: Entrepreneurial survival and liquidity constraints.” *Journal of Political economy* 102 (1):53–75.

Holzer, Harry J, Steven Raphael, and Michael A Stoll. 2007. “The effect of an applicant’s criminal history on employer hiring decisions and screening practices: Evidence from Los Angeles.” *Barriers to reentry* :117–150.

Hunt, Jennifer. 2011. “Which immigrants are most innovative and entrepreneurial? Distinctions by entry visa.” *Journal of Labor Economics* 29 (3):417–457.

Jenkins, Jade Marcus and E. Michael Foster. 2013. “The Effects of Breastfeeding Exclusivity on Early Childhood Outcomes.” *American Journal of Public Health* 104 (S1):S128–S135.

Kearney, Melissa S, Benjamin H Harris, Elisa Jácome, and Lucie Parker. 2014. “Ten economic facts about crime and incarceration in the United States.” *The Hamilton Project* .

Kihlstrom, Richard E and Jean-Jacques Laffont. 1979. “A general equilibrium entrepreneurial theory of firm formation based on risk aversion.” *Journal of political economy* 87 (4):719–748.

Kramer, Michael S, Frances Aboud, Elena Mironova, Irina Vanilovich, Robert W Platt, Lidia Matush, Sergei Igumnov, Eric Fombonne, Natalia Bogdanovich, and Thierry Ducruet. 2008. “Breastfeeding and child cognitive development: new evidence from a large randomized trial.” *Archives of general psychiatry* 65 (5):578–584.

Kramer, Michael S, Beverley Chalmers, Ellen D Hodnett, Zinaida Sevkovskaya, Irina Dzikovich, Stanley Shapiro, Jean-Paul Collet, Irina Vanilovich, Irina Mezen, and Thierry Ducruet. 2001. "Promotion of Breastfeeding Intervention Trial (PROBIT): a randomized trial in the Republic of Belarus." *Jama* 285 (4):413–420.

Kramer, Michael S, Lidia Matush, Irina Vanilovich, Robert W Platt, Natalia Bogdanovich, Zinaida Sevkovskaya, Irina Dzikovich, Gyorgy Shishko, Jean-Paul Collet, and Richard M Martin. 2009. "A randomized breast-feeding promotion intervention did not reduce child obesity in Belarus." *The Journal of Nutrition* 139 (2):417S–421S.

Kramer, Michael S, Lidia Matush, Irina Vanilovich, Robert W Platt, Natalia Bogdanovich, Zinaida Sevkovskaya, Irina Dzikovich, Gyorgy Shishko, Jean-Paul Collet, Richard M Martin, George Davey Smith, Matthew W Gillman, Beverley Chalmers, Ellen Hodnett, Stanley Shapiro, and for the Promotion of Breastfeeding Intervention Trial Study Group. 2007. "Effects of prolonged and exclusive breastfeeding on child height, weight, adiposity, and blood pressure at age 6.5 y: evidence from a large randomized trial." *The American Journal of Clinical Nutrition* 86 (6):1717–1721.

Kurzius, Rachel. 2016. "Council Passes Bills To 'Ban The Box' For Housing, Bar Employers From Asking About Credit History."

Luallen, Jeremy, Kevin Neary, Brendan Rabideau, William Rhodes, Gerald Gaes, and Tom Rich. 2014. "White Paper # 3 : A Description of Computing Code Used to Identify Correctional Terms and Histories." Tech. rep., Abt Associates Inc.

Lucas, Alan, Ruth Morley, TJ Cole, G Lister, and C Leeson-Payne. 1992. "Breast milk and subsequent intelligence quotient in children born preterm." *The Lancet* 339 (8788):261–264.

Lucas Jr, Robert E. 1978. "On the size distribution of business firms." *The Bell Journal of Economics* :508–523.

Madrian, Brigitte C and Lars Lefgren. 1999. "A note on longitudinally matching Current Population Survey (CPS) respondents."

Markusen, James R and Anthony J Venables. 1999. "Foreign direct investment as a catalyst for industrial development." *European economic review* 43 (2):335–356.

Michael, Hout and Rosen Harvey. 2000. "Self-Employment, Family Background, and Race." *Journal of Human Resources* 35 (4):670–692.

Miralles, Olga, Juana Sánchez, Andreu Palou, and Catalina Picó. 2006. "A physiological role of breast milk leptin in body weight control in developing infants." *Obesity* 14 (8):1371–1377.

Morrow-Tlucak, Mary, Richard H Haude, and Claire B Ernhart. 1988. "Breastfeeding and cognitive development in the first 2 years of life." *Social Science & Medicine* 26 (6):635–639.

Olds, Gareth. 2014. "Entrepreneurship and public health insurance." Tech. rep., Harvard Business School Working paper.

Oster, Emily. 2014. "Unobservable selection and coefficient stability: Theory and evidence." *University of Chicago Booth School of Business Working Paper* .

Picchi, Aimee. 2014. "The high price of incarceration in America $80 billion."

Raisler, Jeanne, Cheiyl Alexander, and Patricia O'Campo. 1999. "Breast-feeding and infant illness: a dose-response relationship?" *American Journal of Public Health* 89 (1):25–30.

Raphael, Steven. 2010. "Improving Employment Prospects for Former Prison Inmates: Challenges and Policy." *National Bureau of Economic Research Working Paper Series* No. 15874.

Raphael, Steven and David F Weiman. 2007. "The impact of local labor market conditions on the likelihood that parolees are returned to custody." *Barriers to reentry* :304–332.

Rees, Daniel I and Joseph J Sabia. 2009. "The effect of breast feeding on educational attainment: Evidence from sibling data." *Journal of Human Capital* 3 (1):43–72.

Rhodes, William, Gerald Gaes, Jeremy Luallen, Ryan Kling, Tom Rich, and Michael Shively. 2014. "Following Incarceration, Most Released Offenders Never Return to Prison." *Crime & Delinquency* 62 (8):1003–1025.

Rodriguez, Michelle N and Beth Avery. 2017. "Ban the box: US cities, counties, and states adopt fair hiring policies." *National Employment Law Project* .

Rodriguez-Clare, Andres. 1996. "Multinationals, linkages, and economic development." *The American Economic Review* :852–873.

Sabol, William J. 2007. "Local labor-market conditions and post-prison employment experiences of offenders released from Ohio state prisons." *Barriers to reentry* :257–303.

Schnepel, Kevin T. 2016. "Good Jobs and Recidivism." *The Economic Journal* :n/a–n/a.

Stacy, Christina and Mychal Cohen. 2017. "Ban the Box and Racial Discrimination: A Review of the Evidence and Policy Recommendations." *Research Report* .

Stansel, Dean, José Torra, and Fred McMahon. 2015. *Economic Freedom of North America 2015*. Technical Report, Fraser Institute.

The State University of New York. 2016. "SUNY Board Votes to "Ban the Box" Following Student Assembly Recommendation, Campus Visits." Tech. rep.

Thill, Zoey, Marce Abare, and Aaron Fox. 2014. "Thinking Outside the Box: Hospitals Promoting Employment for Formerly Incarcerated PersonsHospitals Promoting Employment for Formerly Incarcerated Persons." *Annals of internal medicine* 161 (7):524–525.

United States Department of Justice. 2017. “Prisoners and Prisoner Re-Entry.”

United States Department of Justice. Office of Justice Programs. Bureau of Justice, Statistics. 2016. “National Corrections Reporting Program, 2000-2014.”

US Department of Health and Human Services, Office of Disease Prevention and Health Promotion, US Department of Health and Human Services, and Office of Disease Prevention and Health Promotion. 2010. “Healthy people 2020.”

Veuger, Stan and Daniel Shoag. 2016. “No woman no crime: Ban the Box, employment, and upskilling.” Tech. rep., American Enterprise Institute.

Wiles, Paul and Andrew Costello. 2000. *The'road to nowhere': the evidence for travelling criminals*, vol. 207. Research, Development and Statistics Directorate, Home Office London.

Yang, Crystal S. 2016. “Local Labor Markets and Criminal Recidivism.” Tech. rep., Harvard Law School, John M. Olin Center.

———. 2017. “Does Public Assistance Reduce Recidivism?” *American Economic Review* 107 (5):551–55.

Yeaple, Stephen Ross. 2013. “The Multinational Firm.” *Annual Review of Economics* 5 (1):193–217.

Appendix A. States with Right-to-Work Laws

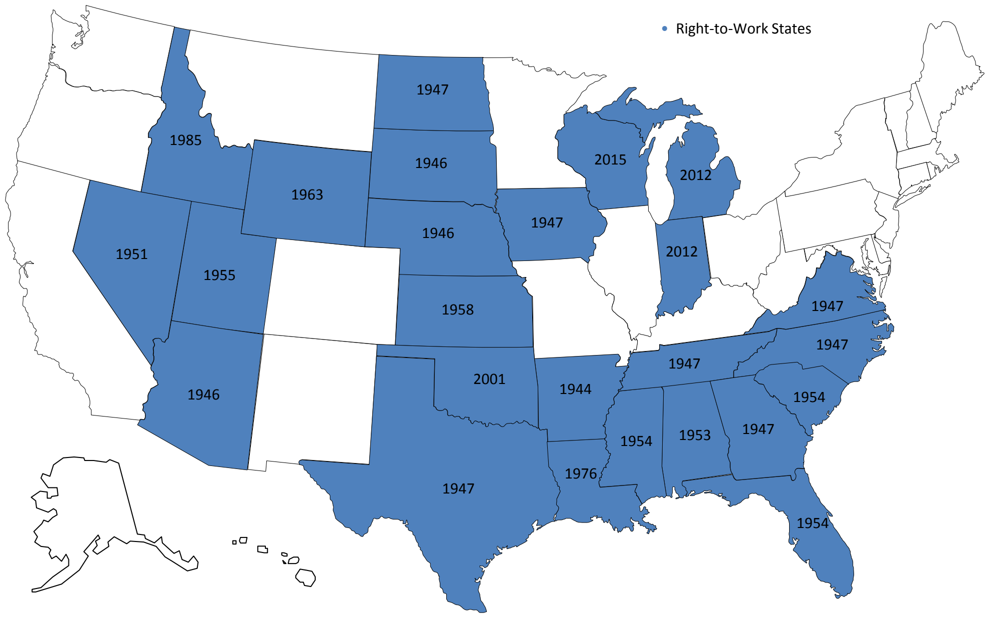


Figure A.1: States with Right-to-Work Laws

Appendix B. Six Months Breastfeeding vs. No Breastfeeding

Table B.1: Probit and OLS Estimates of Breastfeeding Effects

Ever BF vs Never	None	Child characteristics	Col.2 plus family background and region	Col.3 plus prenatal attributes
	(1)	(2)	(3)	(4)
Panel A: 9 months	No health problem (N=8,350)			
:Breastfeeding	0.172***	0.187***	0.132***	0.118***
	(0.039)	(0.040)	(0.042)	(0.043)
	[0.068]	[0.073]	[0.051]	[0.046]
Pseudo R^2	0.00	0.02	0.02	0.03
	Cognitive score (N=8,350)			
Breastfeeding	0.070**	0.036	-0.002	-0.007
	(0.028)	(0.028)	(0.029)	(0.029)
R^2	0.00	0.04	0.05	0.06
Panel B: 24 months	Normal weight (N=7,350)			
:Breastfeeding	0.229***	0.247***	0.210***	0.192***
	(0.045)	(0.045)	(0.048)	(0.048)
	[0.078]	[0.083]	[0.070]	[0.064]
Pseudo R^2	0.01	0.02	0.02	0.03
	No health problem (N=7,350)			
:Breastfeeding	0.008	0.022	0.012	-0.004
	(0.042)	(0.043)	(0.045)	(0.046)
	[0.003]	[0.009]	[0.005]	[-0.001]
Pseudo R^2	0.00	0.01	0.02	0.02
	Cognitive score (N=7,350)			
Breastfeeding	0.262***	0.218***	0.087***	0.087***
	(0.033)	(0.031)	(0.032)	(0.032)
R^2	0.02	0.12	0.17	0.18
Panel C: 48 months	Normal weight (N=6,650)			
:Breastfeeding	0.081*	0.106**	0.037	0.003
	(0.046)	(0.047)	(0.050)	(0.050)
	[0.030]	[0.038]	[0.013]	[0.001]
Pseudo R^2	0.00	0.02	0.03	0.04
	No health problem (N=6,600)			
:Breastfeeding	0.023	0.033	0.054	0.040
	(0.045)	(0.045)	(0.048)	(0.049)
	[0.009]	[0.013]	[0.021]	[0.016]
Pseudo R^2	0.00	0.01	0.01	0.01
	Cognitive score (N=6,400)			
Breastfeeding	0.149***	0.143***	0.097***	0.099***
	(0.034)	(0.034)	(0.036)	(0.036)
R^2	0.01	0.02	0.02	0.02

Note. See Table 3.3 for the details.

Table B.2: Sensitivity of Estimates of Breastfeeding Effects on Health Outcomes and Cognitive Score to Assumptions about Selection Bias in ECLS-B

Ever BF vs Never	CORRELATION OF DISTURBANCES[a]				
	$\rho = 0$	$\rho = 0.05$	$\rho = 0.1$	$\rho = 0.2$	$\rho =$ Cov(X$'\beta$,X'γ) / Var(X'γ)[b]
Panel A: 9 months	No health problem				
:Breastfeeding	0.118	0.035	-0.048	-0.212	-0.246
	(0.043)	(0.043)	(0.042)	(0.042)	(0.042)
	[0.046]	[0.014]	[-0.019]	[-0.081]	[-0.094]
Constraint on ρ					0.22
Panel B: 24 months	Normal weight				
:Breastfeeding	0.188	0.104	0.021	-0.142	-0.008
	(0.051)	(0.051)	(0.051)	(0.051)	(0.051)
	[0.063]	[0.035]	[0.007]	[-0.048]	[-0.003]
Constraint on ρ					0.12
	Cognitive score				
:Breastfeeding	0.087	0.004	-0.079	-0.242	-0.912
	(0.051)	(0.050)	(0.050)	(0.050)	(0.044)
	[0.030]	[0.001]	[-0.028]	[-0.084]	[-0.305]
Constraint on ρ					0.61
Panel C: 48 months	Cognitive score				
:Breastfeeding	0.154	0.070	-0.013	-0.176	-0.379
	(0.050)	(0.050)	(0.050)	(0.049)	(0.048)
	[0.054]	[0.025]	[-0.004]	[-0.062]	[-0.134]
Constraint on ρ					0.32

Note. See Table 3.4 for the details.

Table B.3: Amount of Selection on Unobservables Relative to Selection on Observables Required to Attribute the Entire Breastfeeding Effect to Selection Bias

Ever BF vs Never	9 months	24 months		48 months
	No health problem	Normal weight	Cognitive score[c]	Cognitive score[c]
$\hat{\alpha}$	0.05	0.07	0.09	0.10
$Cov(BF, \epsilon)/Var(\hat{v})^a$	0.76	0.79	1.01	3.32
Implied ratio[b]	0.06	0.08	0.09	0.03

Note. See Table 3.5 for the details.

Appendix C. BTB and Recidivism

Table C.1: Dynamic Effects of BTB Law on Recidivism within One Year

	Age 18-65			Age 25-34		
	(1)	(2)	(3)	(4)	(5)	(6)
1st Year Postadoption	-0.001	0.004	-0.004	-0.012	-0.005	-0.011
	(0.009)	(0.009)	(0.010)	(0.011)	(0.011)	(0.014)
2nd Year Postadoption	-0.043***	-0.037***	-0.036***	-0.045***	-0.036***	-0.028
	(0.011)	(0.011)	(0.012)	(0.014)	(0.014)	(0.018)
3rd Year and Forward	-0.087***	-0.080***	-0.080***	-0.092***	-0.081***	-0.073***
	(0.011)	(0.011)	(0.014)	(0.013)	(0.013)	(0.019)
Year and county FE	Yes	Yes	Yes	Yes	Yes	Yes
Control	No	Yes	Yes	No	Yes	Yes
County Linear Trend	No	No	Yes	No	No	Yes
R^2	0.405	0.407	0.500	0.297	0.299	0.400
Sample Size	31192	31192	31192	29378	29378	29378

Notes: Huber-White robust standard errors clustered at counties allow for arbitrary correlation of residuals within each county. See notes of table 4.3 and the text for further detials. * 0.10, ** 0.05 and ***0.01 denote significance levels.

Table C.2: Dynamic Effects of BTB Law on Recidivism within One Year

	Age			
	18-65	18-40	18-50	25-34
BTB law leads and lags:	(1)	(2)	(3)	(4)
4th Year and More Prior	0.002	0.006	0.008	0.007
	(0.010)	(0.012)	(0.011)	(0.014)
3rd Year Prior	0.027***	0.032***	0.033***	0.021
	(0.010)	(0.011)	(0.011)	(0.014)
2nd Year Prior	0.020*	0.028**	0.023**	0.022
	(0.011)	(0.012)	(0.011)	(0.014)
1st Year Prior - Omitted	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
1st Year Postadoption	0.011	0.019	0.020	0.001
	(0.012)	(0.015)	(0.013)	(0.016)
2nd Year Postadoption	-0.019	-0.023	-0.020	-0.016
	(0.013)	(0.016)	(0.013)	(0.018)
3rd Year and Forward	-0.058***	-0.067***	-0.059***	-0.062***
	(0.014)	(0.018)	(0.015)	(0.020)
Year and county FE	Yes	Yes	Yes	Yes
Control	Yes	Yes	Yes	Yes
County Linear Trend	Yes	Yes	Yes	Yes
H_0: $\beta_{t1}^{post} = \beta_{t3}^{post}$	0.00	0.00	0.00	0.00
R^2	0.491	0.469	0.487	0.392
Sample Size	32231	31738	32069	30531

Notes: All models include leads and lags of adoption of BTB laws. BTB Law change indicators (3rd Year Prior - 2nd Year Postadoption) are equal to one in only one year each per adopting county or state. 4th Year and More Prior (3rd Year and Forward) indicator variables are equal to one in every year beginning with the fourth year after (the third year before) BTB law adoption. * 0.10, ** 0.05 and ***0.01 denote significance levels.

Table C.3: States Provided the Information on Dates of Offender's Release in NCRP Data

State	Years	State	Years	State	Years
AL	2007-2014	MA	2010-2014	OH	2009-2013
AK	2005-2013	MI	2010-2013	OK	2000-2014
AZ	2000-2014	MN	2000-2014	OR	2001-2013
CA	2000-2014	MS	2004-2014	PA	2001-2014
CO	2000-2014	MO	2000-2014	RI	2004-2014
FL	2000-2014	MT	2010-2014	SC	2000-2014
GA	2000-2014	NE	2000-2014	SD	2000-2012
IL	2000-2013	NV	2008-2014	TN	2000-2014
IN	2002-2014	NH	2011-2014	TX	2005-2014
IA	2006-2014	NJ	2003-2013	UT	2000-2014
KS	2011-2014	NM	2004-2014	WA	2000-2014
KY	2000-2014	NY	2000-2014	WV	2000-2014
ME	2012-2014	NC	2000-2014	WI	2000-2014
MD	2000-2014	ND	2002-2014	WY	2006-2014

Notes: This table lists the states and years that are available in the NCRP data after the sample selection conditions are imposed.

Vita

Masayuki Onda was born in Tokyo, Japan in 1980. He attended Hosei University where he earned a Bachelor of Science with a major in Economics in 2004. In 2012, he completed his Master of Science in Economics at Yokohama National University. He entered graduate school in the Department of Economics at Louisiana State University (LSU) and received his Master of Science in Economics in 2014. During graduate school at LSU, he worked as a teaching assistant as well as an instructor, leading classes in Principles of Microeconomics and Macroeconomics. He will join College of Saint Benedict and Saint John's University as a visiting assistant professor in Saint Joseph, Minnesota, following his anticipated graduation in the Summer of 2017.