<u>STATE BUSINESS INCENTIVES, JOB CREATION, AND ENTREPRENEURSHIP</u>

by

Abraham Keunwon Song
A Dissertation
Submitted to the
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Doctor of Philosophy
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DEDICATION

This is dedicated to my loving wife, Bobae Song.

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LIST OF EQUATIONS

LIST OF ABBREVIATIONS

ANOVA .... Analysis of Variance
BDS .... Business Dynamics Statistics
BEA .... Bureau of Economic Analysis
BLS .... Bureau of Labor Statistics
C2ER .... Council for Community and Economic Research
CBP .... County Business Patterns
CES .... Center for Economic Studies
CJTS .... Customized Job Training Subsidy
CPI .... Consumer Price Index
FE .... Fixed Effect
GDP .... Gross Domestic Product
HQ .... Headquarter
IRS .... Internal Revenue Services
ITC .... Investment Tax Credit
JCTC .... Job Creation Tax Credit
LBD .... Longitudinal Business Dynamics
MSA .... Metropolitan Statistical Area
NAICS .... North American Industry Classification System
NASDA .... National Association of State Development Officers
NBER .... National Bureau of Economic Research
NETS .... National Establishment Time Series
PDIT .... Panel Database of Incentives and Taxes
PTA .... Property Tax Abatement
QCEW .... Quarterly Census of Employment and Wages
QWI .... Quarterly Workforce Indicators
R&D .... Research and Development
RDTC .... Research and Development Tax Credit
SBIR .... Small Business Innovation Research
SIC .... Standard Industrial Classification
SUSB .... Statistics of U.S. Businesses

ABSTRACT

STATE BUSINESS INCENTIVES, JOB CREATION, AND ENTREPRENEURSHIP

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Whether state business incentives spur job creation and investments in local economies remains an important but understudied policy issue. Analyzing the most comprehensive database on incentives and taxes covering 45 industries in 46 cities in 33 states between 1990 and 2015, this dissertation conducts a program evaluation using econometrics. The first chapter examines the employment and earnings effects of state hiring credit across business cycles. The second chapter investigates the employment effect of state business incentives by enterprise size and age: small vs. large vs. young. The third chapter explores policy impact on firm dynamics (births, deaths, expansions, and contractions).

CHAPTER 1. INTRODUCTION

State business incentives have become the *modus operandi* of economic development policies. But, over the years, the bargaining power of state and local governments has declined, raising important questions to their merit and cost effectiveness. The recent Amazon HQ2 search has served to highlight the desperation and the power disparity between state governments and businesses. In fact, in the last two decades, incentives have tripled, costing states $50 billion annually.

Incentives are "… a direct reduction, deduction, deferral, or exemption" in tax liability from specific business activities encouraged by the state (Pollard 2015). Subsidized lending or other business cost subsidies, reduced taxes on equity investments, reduced hiring costs, provision of information or other market-making mechanisms, location-specific or industry-specific subsidies to start a business in a given location or industry are among a few examples of this (Acs et al. 2016). Bartik (2017) classifies incentives into five types: property tax abatements (PTA), customized job training subsidies (CJTS), investment tax credits (ITC), job creation tax credits (JCTC), and research and development tax credits (RDTC).[1] Tax credits subsidize capital investments, job creation, and research and development activities; exemptions and deductions typically

[1] Given that my dissertation takes a state-level approach, I choose to omit PTA, which is an incentive determined at the city-level.

reduce, defer, or exclude tax liabilities from specific business activities encouraged by the state, such as the acquisition of property. There are also skills training subsidies, such as customized job training subsidy and manufacturing extension programs.

The *primary objective* of incentives is to promote local investment and hiring (Bartik 2019). Following the Great Recession, states have become hard-pressed to create jobs, protect jobs, and reduce high unemployment rates (Neumark and Grijalva 2017; Criscuolo et al. 2019). To this end, incentives are deployed to reduce local unemployment and reinvigorate underinvested regions.

The *primary target* of incentives is the relocation and expansion of medium to large firms (Buss 2001). The expansion and relocation of large firms, and the subsequent investments to a locality are considered to be a shortcut to higher productivity, more prosperity, and higher tax revenues from other firms and from higher incomes (Garcia-Mila and McGuire 2002; Henderson, 2003; Greenstone and Moretti 2003; Greenstone, Hornbeck and Moretti 2010; Aghion et al. 2015).

Incentives are highly criticized as a wasteful redistribution of taxpayer's money to large firms (Mattera, Tarczynska, and LeRoy 2014; Tarczynska, Cafcas and LeRoy 2016). Critics question the government's ability to pick the right winners, and believe that incentives slow down the allocative efficiency of capital (Acemoglu et al. 2018). They argue that probability of successfully targeting productive firms with incentives is low, while the associated risks of introducing market distortions too high. Also, incentives are becoming highly politicized and a contentious policy issue during governor election cycles, often undermining the economic rationale (Slattery 2020).

The standard economic theory suggests that in the absence of market failure, the market is better off with minimal government intervention. This notion is generally supported by empirical evidence on taxation – lower taxes are associated with greater economic activity. If taxation is broad-base, incentives are a narrow-base fiscal policy. Zidar and Slattery (2019, p. 1) state, "Firm-specific incentives can attract marginal firms at lower cost than a corporate tax cut for all firms." If true, well-targeted incentives (e.g, pick the "right winners") may achieve desired economic objectives of taxes at a fraction of the cost. At the very least, incentives present a policy context ripe for empirical study. Suarez Serrato and Zidar (2018) find that state tax base and credit rules explain more of the variation in state tax revenues than state tax rates since the change in the former is more common than the latter.

At the heart of this debate is the question of whether incentives enhance or deter the allocative efficiency of capital. Well-targeted incentives can enhance the allocative efficiency. Chatterji, Glaeser, and Kerr (2014) identify three theoretical justifications in support of state business incentives: (i) redistribution, (ii) externalities and (iii) credit constraints. The redistributive aim of the policy is best seen in "Empowerment Zones" that incentivize investment in disadvantaged areas (Papke 1994). Positive externalities usually refer to job multipliers and knowledge spillovers (Moretti 2011; Babina and Howell 2018).[2] The credit constraint narrative is tied to the tax literature. Incentives lower the effective tax

[2] Positive externalities manifest themselves through the following channels: (i) export-base or high-tech industry firm generates sufficiently large job multipliers (Moretti 2011). (ii) Attracting to the region complementary establishments who do business with them, and would not have otherwise moved to the region. (iii) Acting as seedbeds of knowledge, and through knowledge spillovers, leading to the creation of businesses that would not have existed otherwise (Babina and Howell 2018).

rate, which lessens the credit constraint, permitting firms to put this excess capital to productive use (Criscuolo et al. 2019; Garrett, Ohrn, and Serrato 2019).

Incentives can be *discretionary* or *non-discretionary*. Discretionary incentives are firm-specific going to large firms in a form of megadeals.[3] Non-discretionary incentives apply to all eligible firms, usually in specific industries. JCTCs and PTAs tend to be more discretionary, while ITC and RDTC written more broadly into the tax code as non-discretionary incentives.

Poorly targeted incentives pose serious risks to deterring allocative efficiency. For example, incentives can complicate the tax system by narrowing the tax base, driving up tax rates for ineligible firms, distorting the market, and failing to generate economic growth. Incentives are a form of capital reallocation that can influence firm exit probabilities. For example, incentives allocated to relatively inefficient firms could potentially increase their survival likelihood or prevent natural death, and thus slow business dynamism (reallocation process of capital from less to most productive firms).[4]

To this day, incentives remain poorly understood – careful program evaluation on incentives are sparse and empirical findings remain mixed. In part there is a methodological issue and in part a data issue. Rodrik (2009) summarizes three major problems with studying industrial policies: absence of an explicit counterfactual, selection bias, and difficulties of generalization to other settings. Previous attempts have estimated incentives but were limited to only a few years or few industries (Fisher and Peters 1998; Peters and

[3] Incentive packages that are valued in the excess of $75 million. According to Bartik (2017), these incentives comprise about a tenth of total annual incentive dollars.
[4] Given the persistence in firms' productivity, exiting firms tend to experience several years of failing productivity levels before the actual exit (Carreira and Teixeira 2011).

Fisher 2002; Cline, Phillips and Neubig 2011). However, differences in empirical findings have less to do with the type of data used (aggregate data or microdata), variables included, or how taxes/credits are measured; they pertain to the time period analyzed or industry studied (Wasylenko 1997).

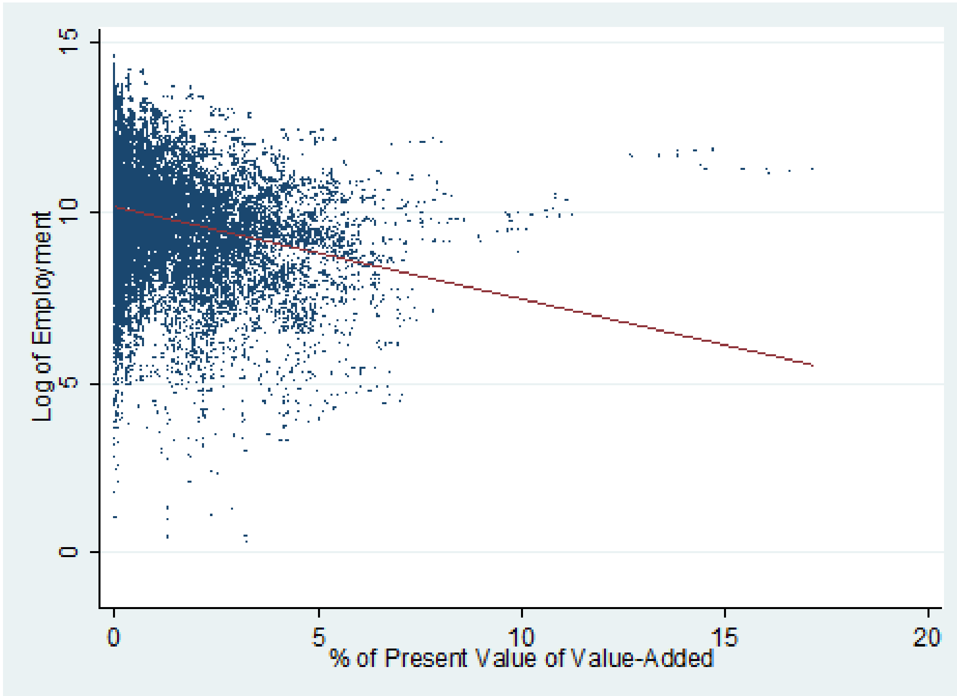
This dissertation takes advantage of a comprehensive database on incentives and taxes constructed by Bartik (2017).[5] PDIT is provides estimated value of incentives by type, city, state, industry, and year, allowing for a comparison. Most studies focus on one type of incentives, but using this database allows me to study all major types of incentives. It is the first of its kind to cover incentives and taxes for 45 industries that comprise more than 90 percent of U.S. Gross Domestic Product (GDP) in 47 cities in 33 states from 1990 to 2015.[6]

I merge PDIT data with County Business Patterns (CBP), the Statistics of U.S. Businesses (SUSB), and the Quarterly Workforce Indicators (QWI). CBP provides employment, annual payroll and establishment information for 1990-2015 with four-digit Standard Industrial Classification (SIC) for 1990-1997 and North American Industry Classification System (NAICS) for 1998-2015. I use CBP to examine both employment and earnings effect of state hiring credits in the first chapter. CBP program and the SUSB program tabulate the same data in different ways: in the former, the size category is always

[5] The Council for Community and Economic Research (C2ER) has a database of state's specific incentives but does not provide a numeric value of incentives or incentive details. Good Jobs First keeps a database of discretionary incentives and the corresponding dollar values. These database, however, are incomplete, as they record what is promised rather than what is actualized.
[6] The database takes a rule-based approach and simulates incentive dollar that a typical firm would receive. Database is a byproduct of meticulous work using balance sheet information, information on state and local taxes, and information on rules for how incentives are determined based on firm characteristics. A unique feature to this database is the availability of value-added percentage of incentives.

based on the size of the individual establishment (physical location), whereas in the latter, it is based on enterprise that owns the establishment(s). Using SUSB program, I explore the differential effect of state business incentives across the size distribution. I augment this analysis with QWI, which tabulates establishment-level data by state and industry by the firm age category, thus I am able to analyze the question of small vs. large vs. young. In the third chapter, I explore the questions related to business dynamics using establishment birth, death, expansion, and contraction information from the SUSB annual changes files. By doing so, I am able to examine both the primary (establishment expansion) as well as the secondary (establishment birth) effects of state business incentives.

A simple scatter diagram of PDIT-CBP merged data in Figure 1 reveals a startling negative relationship between incentives and employment. The rest of the dissertation will explore in greater detail these relationships using rigorous econometric methods.



Source: Panel Database of Incentives and Taxes, County Business Patterns. 1990-2015, 33 states, 45 industries. N=38,240.
Note: Employment data from CBP. Percent of Present Value of Value-added from PDIT.

Figure 1: Scatterplot of Total Incentives (% of Present Value of Value-Added) on Employment

My empirical strategy using aggregate data is simple but demanding. Partridge et al. (2019) and Tuszynski and Stansel (2018) have to make numerous assumptions (which are often *ad hoc* and open up many disagreements) about what are the relevant confounding variables. Instead, I use the most demanding specification with the three-way interacted fixed effects based on state-industry-year level data. I conduct both state (33 states) and city-level (46 cities) analyses.

This dissertation contributes to the burgeoning literature on tax incentives, drawing on literature in public finance, labor economics, industrial organization, and entrepreneurship.

The second chapter of the dissertation examines the employment and earnings effects of state hiring credits by putting Neumark's (2013) theoretical framework to an empirical examination. He argues that broad-based state hiring credits could be an integral fiscal policy tool to boost job creation, particularly in response to recessions when both credit constraints and labor markets are tighter. Of particular interest to this paper is the job creation tax credit, a labor subsidy designed specifically for job creation. JCTCs are best suited for this type of empirical study because the policy, by design, requires net new job creation. Three research questions are explored: (i) Do state hiring credits increase the level of employment? (ii) Do state hiring credits increase earnings per worker? (iii) Are there differential employment and earnings effects observed across business cycles? I merge PDIT with the County Business Patterns data. A policy evaluation of state hiring credits across business cycles suggests that it is an ineffective. Credits induce a significantly slower employment growth rate, though accompanied by slightly positive earnings effect.

The third chapter of the dissertation examines the employment effect of four different state business incentives across the firm size distribution. According to the Kauffman Foundation commissioned survey, 79 percent of entrepreneurs believe government incentives are essentially a redistribution policy that favor big businesses over small ones. On average, only two percent of a state's employers have more than 100 employees but they receive between 80 and 90 percent of all incentive dollars (LeRoy et al. 2015). Yet, incentives to such large firms tip less than 25 percent of relocation or expansion decisions (Bartik 2019). Based on these findings, the credit constraint narrative does not hold for large firms; neither is the externalities argument particularly convincing. What the literature overlooks is a possibility of differential policy effects across firm size distribution, and four types of incentives. I explore the following questions: (i) what is the differential employment effect of four types of state business incentives? (ii) what is the differential employment effect of these incentives across firm size and firm age? Using the SUSB and QWI data, I find evidence that customized job training subsidy boosts it for startups and small businesses. The key policy takeaway of the study is that incentives improving local levels of human capital work best in the interest of both local governments and firms, particularly for startups and small businesses.

The fourth chapter of the dissertation examines the impact of state business incentives on the rate of entrepreneurship. The existing literature suggests that startups are important for the regional economic growth as vehicles of job creation and facilitators of technological innovation that leads to productivity growth (Haltiwanger, Jarmin, and Miranda 2013). Theory and evidence suggest that a substantial fraction of aggregate productivity growth is accounted for by the reallocation of capital from lower-productivity to higher-productivity firms, which is largely driven by firm entry and firm exit (Syverson 2011; Bartelsman, Haltiwanger and Scarpetta 2013). A policy favoring large businesses could potentially slow down reallocation and thereby dampen economic growth (Acemoglu et al. 2018). Specifically, I address the following questions: (i) What is the differential effect of four types of state business incentives on firm expansion and contraction? (ii) What is the differential effect of four types of state business incentives on firm birth and firm death? (iii) Conditional on the effectiveness of state business incentives, is the policy effect accompanied productivity growth in employment and earnings? Using the SUSB data, the most striking finding is the persistently negative effect of Investment Tax Credit on establishment birth and expansion, accompanied by lower earnings per worker, suggesting that the policy introduces market distortions and dampens local productivity growth.

In general, my findings are consistent with the existing literature: the majority of incentives, whether directly or indirectly, are a zero sum game for states, a race to the bottom, generating market distortions that slow the process of capital reallocation from less productive to more productive firms (Acemoglu et al. 2018). Although incentives are designed and implemented to encourage incumbents to undertake greater investments, increase productivity and protect employment (Buss 2001; Aghion et al. 2015), most incentives fail to deliver even on their primary objective: increasing firm expansion (Donegan, Lester, and Lowe 2019; Cahuc et al. 2019; Criscuolo et al. 2019). The majority of incentives go to large firms but desired economic activities turn out to be difficult to spur with incentives; most jobs and investments will have been made in lieu of incentives (Bartik 2018). Some incentives, such as job creation tax credit and investment tax credit appear to go as far as to dampen growth.

Designing and implementing targeted economic development policies like incentives requires careful deliberations on potential disincentives. Since picking the right winners is a challenging task, the state government can best mitigate risks and increase the odds of success by committing to transparency and accountability. Acemoglu et al. (2018) suggest that successfully targeting productive firms with incentives is too challenging, risks of introducing market distortions too large, and argue that taxing the continued operations of the incumbents is a preferred policy prescription, because taxes fall disproportionately on less productive firms, which are more likely to be near the exit margin anyway. The findings of my dissertation are generally in support of their policy prescription. Tax policies are much better understood than tax incentives. Tax policies can effectively target certain economic activities.

When conducting program evaluations, state governments should be careful from drawing overly simplistic conclusions based on relocation, expansion, and startup indicators. Expansions and relocation of large firms in the export-base sector can generate positive externalities in job multipliers and knowledge spillovers. But these benefits are almost always moderated by negative externalities. There is suggestive evidence that foregone tax dollars translate into a decline in public services, mainly primary education (Chava, Malakar, and Singh 2019). Also, incentives to large firms could create barriers to entry and displace many more small businesses that are substitutable (Partridge et al. 2019; Tuszynski and Stansel 2018).

State business incentive are evaluated based on the jobs created or investments made. However, such metrics are incomplete. When evaluating state business incentives, productivity should be one of indicators. Higher productivity is closely associated with long-term economic growth (Syverson 2011). The policy impact could materialize not just in employment but also earnings, a crude proxy for productivity change. This dissertation examines both factors.

Policymakers should make sure to evaluate the micro-level and macro-level indicators simultaneously. This dissertation differentiated incentives by type, demonstrated a way to evaluate incentive effects using aggregate data; such studies should be complemented by firm-specific evaluations using microdata. Supplementing employment effects with earnings effects was an attempt to best capture policy effects. Unless state business incentives can enhance the allocative efficiency, spurring job creation or investment are a short-sighted goal. Hence, it is important to evaluate incentives both at the short-term as well as long-term horizon.

Economic development policies should take a holistic approach where incentives are a part of a larger strategic plan. According to 2018 Area Development's Annual Survey, state and local incentives rank seventh among site selection factors, behind quality of life (6), tax exemptions (5), corporate tax rate (4), highway accessibility (3), labor cost (2), and availability of skilled labor (1).[7] Incentives are never the full story to expansion, relocation, and startup decisions. In today's knowledge economy, the most important commodity is human capital. My research suggests that the only positive sum incentives may be those that incentivize investments in knowledge, skills, and talent. Customized job training subsidy and research and development credit are two incentives that directly improve a region's level of human capital and knowledge creation (Wu 2008; Fazio, Guzman, and Stern 2019; Babina and Howell 2018; Bartik 2018).

[7] https://www.areadevelopment.com/Corporate-Consultants-Survey-Results/Q1-2019/33nd-annual-corporate-survey-15th-annual-consultants-survey.shtml

CHAPTER 2. JOB CREATION EFFECTS: EVIDENCE FROM STATE HIRING CREDITS

2.1 Abstract

Whether state hiring credits actually induce job creation in local economies remains an important but understudied policy issue. Standard labor economic theories suggest a negative effect of the policy, but the assessment is largely based on evaluation of credits targeting the disadvantaged areas. Analyzing the most comprehensive database on incentives and taxes covering 45 industries in 33 states between 1990 and 2015 constructed by Bartik (2017), I find (i) statistically significant and strongly negative employment effects, and statistically significant and weakly positive earnings per worker effects. (ii) With adjustment cost controls, employment effects remain negative and statistically significant, but to a lesser degree. (iii) When using state unemployment proxy for business cycles, the negative employment effects is magnified, but state-industry shiftshare proxy suggests that incentives mitigate credit constraints. (iv) Restricting to export-base industries also does not significantly alter results, weakening the positive externalities argument. The key takeaway from the large state variation in incentives is that there are many idiosyncratic reasons for policy adoption, and some states are better at designing and implementing hiring credits than others.

2.2 Introduction

States have used various business incentives to influence business relocation, expansion, and startup decisions (Buss 2001). Following the Great Recession, interest in the topic has increased among scholars (See Neumark 2013; Neumark and Grijalva 2017, Cahuc, Carcillo and Barbanchon 2019). In general, economic theory predicts a negative relationship between business taxes and economic development, implying that incentives (effectively lower taxes) should spur economic development through increased job creation, entry and investment. However, empirical findings are mixed. Considering that state business incentives have remained large and increased over time, extant literature is dated and needs to be revisited (e.g., See Bartik 1992; Wasylenko 1997; Buss 2001).

The focus of this paper is on one type of state business incentive: the Job Creation Tax Credit (JCTC), a policy designed specifically for the purpose of job creation.[8] Between 1990 and 2015, state business incentives have tripled, and two-thirds of this growth was accounted for by JCTC. A body of research argues that hiring credits are ineffective (Katz 1998; Dickert-Conlin and Holtz-Eakin 2000; Bartik 2001). However, these negative assessments are primarily based on evaluation of credits targeting the disadvantaged areas. Neumark (2013) argues that a broad-based state hiring credits could be an integral fiscal policy tool to boost job creation, particularly in response to recessions. Whether state hiring credits actually induce job creation in local economies remains an important but understudied policy issue.

[8] In this chapter, JCTC and hiring credits are used interchangeably.

By analyzing the Panel Database of Incentives and Taxes (PDIT), a new database from the W.E. Upjohn Institute for Employment Research, I examine whether JCTC spurs job creation. Previous attempts have estimated incentives but were limited to only a few years or few industries (Fisher and Peters 1998; Peters and Fisher 2002; Cline, Phillips and Neubig 2011). By comparison, PDIT is the first of its kind to cover incentives and taxes for 45 industries that comprise more than 90 percent of U.S. Gross Domestic Product (GDP) in 33 states from 1990 to 2015.

This paper addresses the following questions: (1) Do hiring credits increase the level of employment? (2) Do hiring credits increase earnings per worker? (3) Are there differential employment and earnings effects observed across business cycles?

The first question is at the heart of what motivates policymakers to offer business incentives and engage in a bidding war against other states (Hanson 1993). For decades, "smokestack chasing" led to bidding wars between state governors offering large incentives to manufacturing plants making location decisions (Greenstone, Hornbeck and Moretti 2010). But is it an empirical fact that incentives generate jobs in the first place? JCTCs are best suited for this type of empirical study because the policy, by design, requires net *new* job creation. Therefore, it merits a causal study of labor rather than other incentives, such as investment tax credit for which the effect is ambiguous (e.g., both or either labor or capital factors of production).

The second question pertains to the secondary effect of hiring credits. Empirical evidence on the effectiveness of JCTCs is mixed but examining the earnings effect may potentially enrich the discussion or even shed new light into the old debate. With all else equal, subsidies can either lead to net new employment or raise the average earnings per workers. Finding evidence in support of higher average earnings per workers would suggest that there may be an unintended income effect resulting from firms exploiting this policy by various means (e.g. frequent job churning, relocation of workers from across establishments, hopping state borders).

The third question is based on an argument that hiring credits should be examined across different business cycles. Government interventions are most effective in tackling market imperfections. In downturns, firms will be more financially constrained, increasing the likelihood of companies taking up hiring credits for net job creation. Meanwhile, during upturns, firms will be more financially stable and at best, hiring credits could be reflected in higher earnings per worker. What Neumark (2013) claims is that in recessions, hiring credits could be much more effective than worker subsidies; the former helps the financially distressed company to hire while the latter will likely just increase consumer expenditures. However, most hiring credits thus far have tended to narrowly target disadvantaged regions.

2.3 Literature Review

Previous literature has examined the effect of business incentives on growth at the regional, industry, and firm levels (Bartik 1991; Wasylenko 1997; Buss 2001). Literature on business incentives falls under the larger umbrella of place-based policies (Neumark and Simpson 2015; Austin, Glaeser and Summers 2018). Researchers have studied enterprise zones, discretionary grant-based policies and clusters (e.g. university research).

Another literature that intersects with place-based policies pertains to taxation (See Bartik 1991, p. 216-234).

The most common measure of economic development are income, employment, investment, plant expansions, relocations, and births (Wasylenko 1997). By far the most studied measure is employment growth driven by the policymakers' interest in job creation and job growth. According to a review conducted by Bartik (1994), average interregional elasticity is -0.3 for the tax responsiveness of location and economic growth, and the range of elasticity estimates is between -0.1 and -0.6. In other words, 10 percent lower taxes will raise employment, investment, or firm births between 1 and 6 percent. The range of elasticities, however, are not estimated with great precision.

Empirical studies of federal JCTCs date back to the 80s (Perloff and Wachter 1979; and Bishop 1981). The criticism of JCTC is the stigmatization of eligible workers and low firm participation rates – all of these factors together reduce or eliminate employment effects. The empirical results remain mixed, and subject to sample selection and decomposition. Chirinko and Wilson (2016) use Bureau of Labor Statistics' Current Employment Statistics data to study the state hiring credits in the US established prior to the Great Recession (1990-2007). They find that the cumulative effect of the JCTCs is positive but takes three years for the full effect to realize.

Neumark and Grijalva (2017) construct state hiring credits database, which provides information on job creation programs in all 50 states for the period 1969 to 2012 for which 147 hiring credits are identified. Merged with Quarterly Census of Employment and Wages (QCEW), they find no evidence of an effect of state hiring credits when no distinctions are made across different features of hiring credits. However, once decomposed across specific types of state hiring credits – including those targeting the unemployed, those that allow states to recapture credits when job creation goals are not met, and refundable hiring credits – appear to have succeeded in boosting job growth, particularly during the Great Recession period.

Merging National Establishment Time Series (NETS) with Good Jobs First Incentive Database and State Economic Development Expenditure Database, Donegan, Lester and Lowe (2019) find that incentivized firms fail to create more jobs than matched control establishments. When decomposed by size, they find a positive employment effect among small establishments and a large negative employment effect in large establishments.

Cahuc, Carcillo and Barbanchon (2019) examine the effectiveness of hiring credits in France (it was restricted to firms employing 10 or less workers), and observe that hires and employment rise (Their measures are in growth rates rather than levels) quickly three months after the credit is introduced. This quick turnaround of the policy is in contrast to the findings of Chirinko and Wilson (2016), who find an effect three years later. Neumark and Grijalva (2017) find an effect within 8 to 12 months, which falls somewhere in-between the two studies.

These differences are not related to the type of data used (aggregate data or micro data), variables included, or how taxes/credits are measured but to the time period analyzed or industry studied (Wasylenko 1997). Thus, the usage of most comprehensive database on taxes and incentives marks a significant improvement over others studies affected by the same problem. Previous studies in the literature have not factored in business dynamics. Neumark and Grijalva (2017) are a notable exception in narrowly focusing on 2007-2011 time period or using predicted state employment, a measure of shiftshare accounting for business cycles. Rather, as Chirinko and Wilson (2016) do, studies focus on pre-recessions to avoid introducing more noise. Furthermore, no other previous study has accounted for industry heterogeneity when it is clear that it very important, perhaps second only to the state heterogeneity. In fact, I find state-industry heterogeneity to be one of most important ones to account for that the previous literature has ignored.

2.4 Economic Theory

In theory, JCTC can have three possible outcomes: (1) positive effect (2) no effect (3) negative effect. The JCTC could have positive effect on job creation by decreasing the effective labor cost to the firm or alleviating credit constraints during a recessionary period, for example. If JCTC is a merely weak policy tool that does not alter firm behavior, then results would show no significant effect on job creation. If this is the case, JCTC is a waste of public tax dollars that solely benefits the incentive recipient firms at the expense of everyone else. Such findings would undermine the positive externalities argument. Finally, JCTC could have a negative effect, which implies that employment in incentivized state-industries grow slower than in non-incentivized state-industries. A negative effect is arguably the most challenging outcome to interpret. One possibility is that the study fails to adequately control for endogenous selection of poorly performing states. Another plausible explanation is that JCTC is as a reactionary policy that mitigates the effects of downward employment trends ("stopping the bleeding"), but is insufficient to overturn them. These two interpretations assume that endogeneity remains intact. However, assuming that the model sufficiently controls for endogenous selection, a simple interpretation could be that the policy creates a disincentive whereby firms engage in job churning that may be reflected in temporary job growth but not in new job growth.

Another major challenge posed to studying state hiring credits is that outcomes (1) and (2) are muddied by the inter-jurisdictional displacement effect, which refers to business relocation from one state to another that results in a positive effect at the state-level, but at the national-level is a net zero effect. A large multi-establishment firm could equally shuffle employment in one state to another, or partake in strategic "employee churn" to take advantage of the credit without actually generating net new employment (Neumark and Kolko 2010). Alternative, a large retailer like Walmart could enter a local economy and create 500 new jobs but displace 600 mom-and-pop jobs. Econometrically, the displacement effect will overstate the positive effect or give the false impression of a positive effect where there is none. While job churning is a problem that is difficult to detect, one way to minimize capturing the displacement effect resulting from the likes of Walmart entry example is to restrict the analysis to export-base industries.

A large number of studies conducted by scholars and think tanks alike heavily criticize state business incentives as ineffective and wasteful as policy tools for job creation. From the perspective of the state, the rise of the state hiring credits over the last two decades, at least, partially reflects the race-to-the-bottom where state policymakers cannot afford to withdraw from this zero-sum game. Some argue that they are a wasteful redistribution of capital from taxpayers to capital owners, subsidizing low-wage jobs and benefiting large, profitable companies whose investment decisions are largely independent of subsidies anyway (Mattera, Tarczynska, and LeRoy 2014; Tarczynska, Cafcas and LeRoy 2016). Others cast doubt to the government's ability to pick winning firms or industries.

Economic theory predicts a negative relationship between business taxes and economic development. Incentives are essentially a subsidy that permit higher than equilibrium levels of employment. The state hiring credit affect the profit maximization of the firm. Presumably, a labor subsidy should induce firms to use more labor over capital. And the credit would make more sense in industries where capital-labor substitution is rather more flexible, feasible, and economically viable. Or simply for more labor-intensive industries. And since, the credit can induce deadweight loss, it should be a labor-intensive, export-base industry.

Consider the following firm's profit maximization problem consisting of just labor and investment decisions (Jorgensen 1963). Let *p* be the price of output, *w* the wage rate, *s* the price of capital goods, Q the quantity of output, E the quantity of variable input, and *I* the rate of investment. Without state hiring credit would look as follows:

Equation 1

$$\mathrm{f(E,I)} = pQ - wE - qI$$

The hiring credit lowers the effective tax rate or the labor cost, since it applies only to labor and not capital. Hence, in the presence of the hiring credit, firms in competitive markets would use more labor than capital. Hence, with state hiring credit, the profit maximization would look as follows:

Equation 2

$$\mathrm{f}(\mathrm{E}, \mathrm{I}) = pQ - (1 - t)wE - qI$$

With t being the job creation tax credit, the first order condition will look as follows:

Equation 3

$$\text{No tax credit:} \quad \frac{\partial \mathrm{E}}{\partial \mathrm{I}} = \frac{w}{q}$$

$$\text{With tax credit:} \quad \frac{\partial \mathrm{E}}{\partial \mathrm{I}} = \frac{(1-t)w}{q}$$

Most of hiring credits are often a package deal in enterprise zones or other types of economic development policies in disadvantaged localities. While the rationale is that areas with high local unemployment rates and lagging economic activities could be invigorated through these incentives, the evidence is sparse. Neumark (2013) suggested that broad-based state hiring credits applicable to the broader population rather than merely the residents in disadvantaged areas. I expect the hiring effect to be greater during recessionary periods when the labor market is looser (unemployment rate high) and credit constraint greater.

This is demonstrated in Figure 3 where the initial equilibrium wage is at E(w) where $S(w) = D(w)$. The effect of hiring credit is depicted in Panel A of Figure 2. The employment rate is in disequilibrium during a recession, and the hiring credit reduces the effective wage paid by employers, which shifts the labor demand curve up and increases employment. As the state hiring credit (c) shifts the demand curve from D to D', it increases employment levels from E to E$'$. Bartik (2018) estimates that a typical state incentive program, of 2 to 3 percent of wages, only induces the creation of about 10 to 15 percent of the projected jobs. This means that the increase in employment levels from $\mathrm{E}(\mathrm{w})$ to $\mathrm{E}'(\mathrm{w}(1-\mathrm{c}))$ is about 10-15 percent.

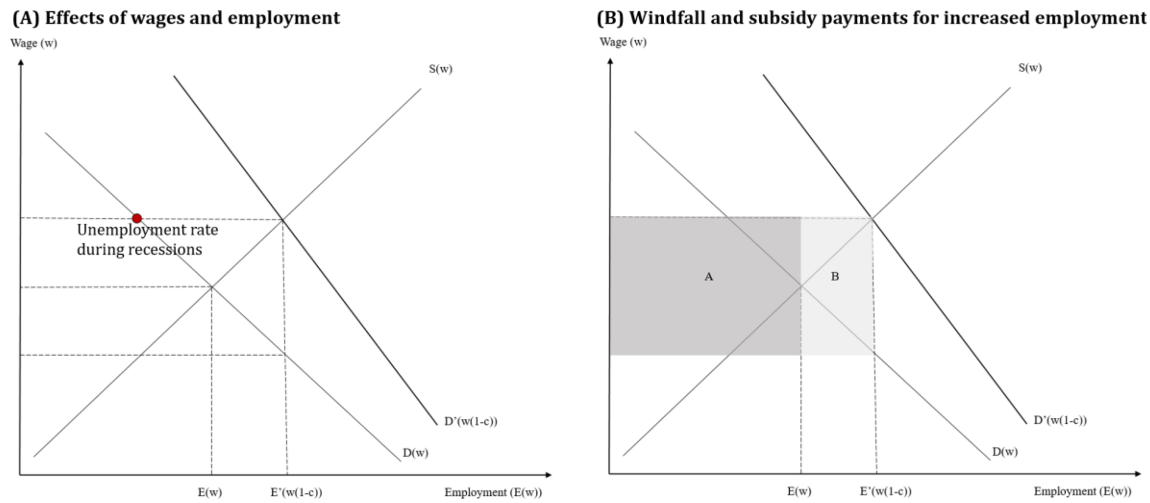


Figure 2: The employment effect of business incentives

The Panel B of Figure 2 illustrates potential problems of hiring credits. If hiring credits pay for net new jobs, the cost is estimated by the rectangle B but if hiring credits pays for all jobs, then the additional cost is estimated by rectangle A. The incentive-adopter state would assume the cost of various displacement effects that is proportional to the rectangle A.

Consider that a large retailer, like Walmart, offers to invest in the local economy and hire workers in return for state business incentives. Walmart is in the retail industry, which a non-export sector with low multiplier effect. Walmart will enter the local market and create x number of jobs but wipe out z number of jobs in local grocery businesses. If

$x < z$, then the equilibrium employment in the local economy will decline. Given that the tax incentives are large, it is likely that the retailer's long-run tax contribution to the state will be less than the total tax contribution of all mom-and-pop shops; then the total tax revenue for the state will decline as a result. One possible outlay is earnings. If the retailer takes the state business incentives, it may not increase the level of employment but raise the earnings per worker by passing some of its capital gains. But if there is not even an increase in earnings per worker, it is fair to assume that attraction of the Walmart will have created a deadweight loss and decreased the total welfare to the local economy.

2.5 Data

2.5.1 Description of Data

I merge PDIT to the County Business Patterns (CBP), the annual series that provides subnational economic data by industry, including employment during the week of March 1 and annual payroll. The PDIT, constructed by Bartik (2017), simulates average taxes and incentives by considering the following scenario. A business in a city c, state s, industry i, starts an operation in some year t. Taxes and incentives for this new facility are projected for the facility's first 20 years of operation, meaning that tax rules and incentives for year $t = 1$ are assumed to remain unchanged and carry forward to year $t + 20$. To calculate state and local taxes for this new facility, data based on industry averages are used for the firm's balance sheet (including information on value-added, pretax profits, mix of property assets, employment, wages, and R&D spending). State and local taxes and incentives are calculated for each year of the assumed 20 years of operation of the new facility using: (1) the balance sheet information, (2) information on state and local tax rates, and (3) information on rules for how incentives are determined based on firm characteristics.

The PDIT database covers incentives and taxes for 45 industries that compose more than 90 percent of U.S labor compensation in 33 states from 1990 to 2015 (See Table 1). The level of industrial detail is based principally from the 2011 Bureau of Economic Analysis (BEA) industry data, with some Internal Revenue Services (IRS) data merged in on some key variables. This constitutes the most comprehensive database on incentives and taxes to date, including all five major types of incentives: property tax abatements, customized job training subsidies, investment tax credits (ITCs), job creation tax credits (JCTCs), and research and development (R&D) tax credits. The database is uniquely designed whereby each incentive can be turned on or off and also adds up to the total.

Table 1. List of 45 Industries in the Panel Database of Incentives and Taxes

Export-base Industries (Manufacturing)	Export-base Industries (Other)	Non-export-base Industries
Textile mills and textile product mills	Accommodation	Educational Services
Apparel, leather and allied product manufacturing	Waste Management and remediation services	Amusement, gambling, and recreation industries
Wood product manufacturing	Management of companies (holding companies)	Hospitals, nursing, and residential care facilities
Nonmetallic mineral product manufacturing	Broadcasting and Telecommunications	Other services
Primary metal manufacturing	Warehousing and storage	Retail Trade
furniture and related product manufacturing	Computer systems design and related services	Credit Intermediation
Paper manufacturing	Performing arts, spectator sports, museums and entertainment	Food services and drinking places
Computer and electronic product manufacturing	Publishing industries (includes software)	Miscellaneous health care and social assistance
Machinery manufacturing	Information and data processing services	Offices of health practitioners and outpatient care centers
Fabricated metal product manufacturing	Miscellaneous professional, scientific & technical services	Wholesale Trade
Printing and related support activities	Insurance carriers and related activities	Administrative and support services
Plastics and rubber products manufacturing	Securities, commodity contracts, other financial investments	Rental and leasing services and lessors of intangible assets
Motor vehicles, bodies and trailers, and parts		Construction
Electrical equipment, appliance, and component manufacturing		Legal Services
Other transportation equipment		
Food, beverage, and tobacco manufacturing		
Miscellaneous manufacturing		
Chemical manufacturing		
Petroleum and coal products manufacturing		

Source: Bureau of Economic Analysis industry classifications; export-base designation by Bartik (2017)

Another unique feature of PDIT is the availability of a continuous variable estimating the state hiring credit, which is a significant improvement over any other databases that include dummies. The database computes present value of taxes and incentives as a percentage of the present value of the new facility's value-added over the same 20 years using a discount rate of 12 percent based on research on typical discount rates used by corporate executives (Poterba and Summers 1995).[9]

Equation 4

$$Net\ Present\ Value_{ist} = \sum \frac{Value\ added}{(1.12)^{20}} \text{ where } Value\ added = \text{Output} - \text{Material Cost}$$

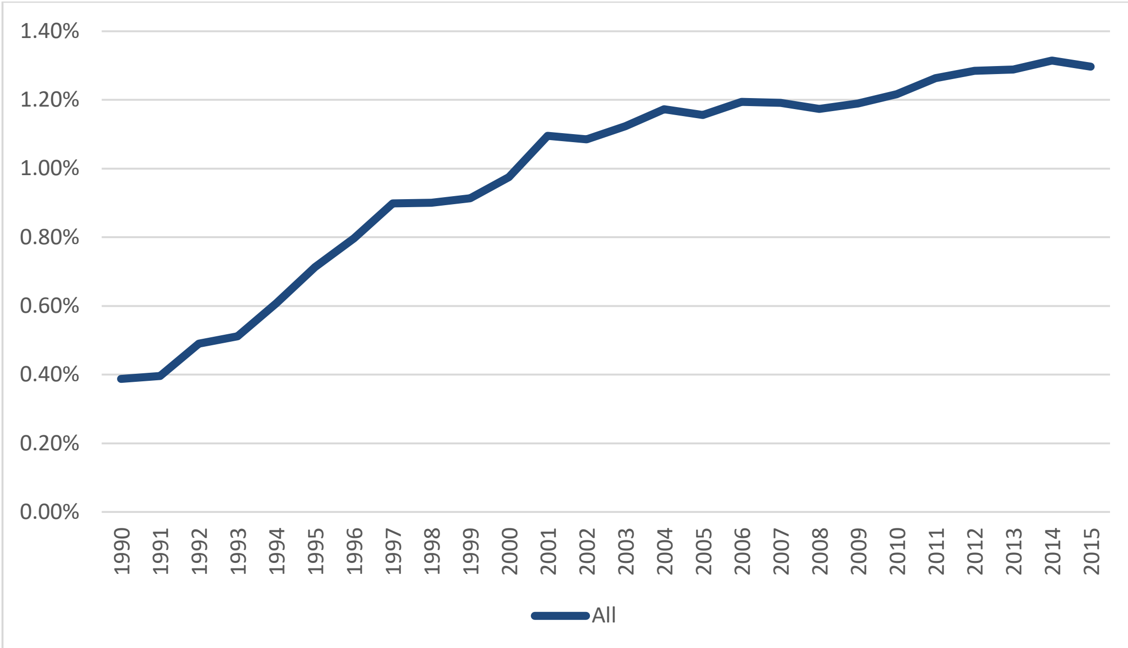
Equation 5

$$\%\ of\ NPV\ of\ Value\ Added_{ist} = \frac{NPV\ of\ tax\ credits\ and\ incentives}{NPV\ of\ total\ value\ added}$$

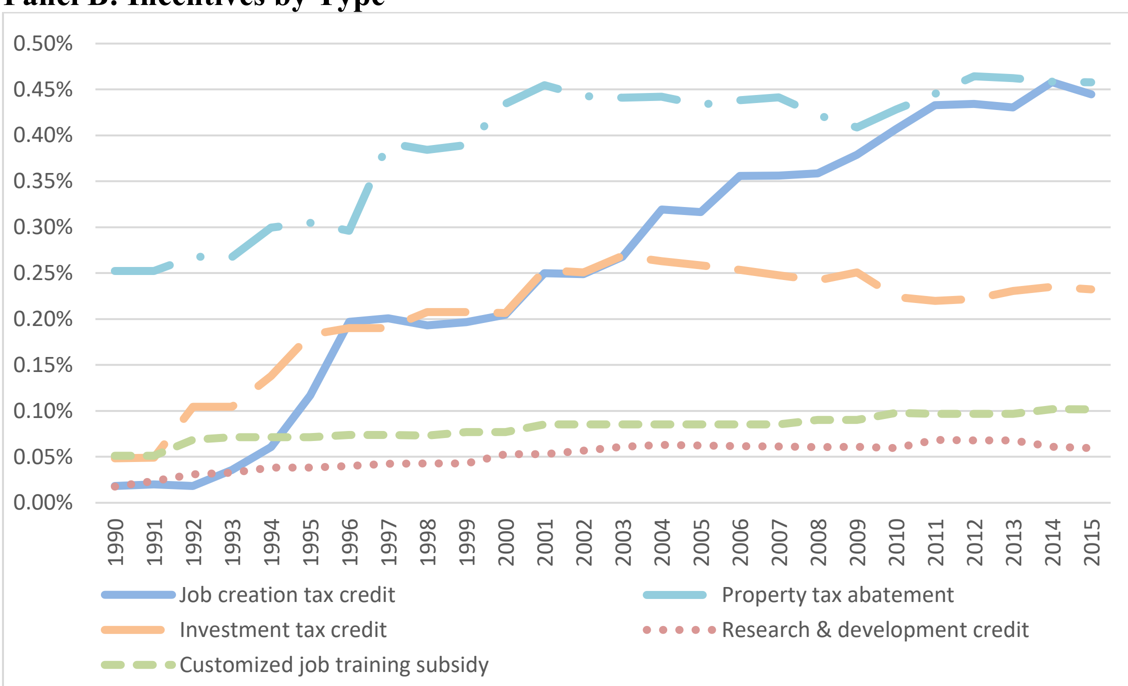
Bartik's (2017) estimates cover two types of JCTCs: those that are dollar per job-year and those that are some percent of wages of workers. Dollar per job-year subsidies are calculated in nominal dollars and adjusted to real 2011 dollars (For 2014-2015, CPI is based on recent inflation trends). State hiring credits differ across a number of dimensions. Perhaps, the only conditions that almost all of them share is creation of new jobs. If state hiring credit is refundable, it can be paid out or else it is written off against corporate tax or payroll tax withholdings. If the credit is non-refundable, carry forward becomes a very important dimension because it determines ow many years the business has to claim all the credits if a company were to max out on write-offs for that fiscal year.

[9] 12 percent reflects the corporate executives' perspective on return of investment, which is a function of future taxes, incentives, and profits. Consider that at 12 percent, a dollar taxes, incentives or profits 10 years from now would be worth only \$0.32 today; 20 years from now, it would be worth only \$0.10 today. Hence, the 20-year model that tracks taxes and incentives will capture most of the present value effects that affect corporate return on investment.

Panel A: Total Incentives



Panel B: Incentives by Type



Source: PDIT, 1990-2015. 33 states, 45 industries.

Figure 3: Economic Development Incentives as % of Business "Value-Added"

State business incentives have tripled between 1990 and 2015 (See Panel A of Figure 3). Total incentives estimates are large at around $50 billion annually for export-based industries, which amounts to about 1.4 percent of all industry value-added and about 30 percent of all state and local taxes (Bartik 2017). JCTCs is the biggest type of incentive, and increased JCTCs made up two-thirds of the 1990-2015 increase in incentive costs (See Panel B of Figure 3).

CBP provides employment, annual payroll and establishment information for 1990-2015 with four-digit Standard Industrial Classification (SIC) for 1990-1997 and North American Industry Classification System (NAICS) for 1998-2015. This series has information on the number of establishments, employment during the week of March 12, first quarter payroll, and annual payroll. Payroll and administrative data are from administrative records for single-unit companies and a combination of administrative records and survey-collected data for multi-unit companies. It also provides 6 million single-unit establishments and 1.8 million multi-unit establishments from the Business Registrar, which includes employer establishments (with paid employees).

For this paper, I merge CBP data with PDIT database. Information on employment, annual payroll, and establishments are obtained by merging the PDIT with CBP. There are several complications that need to be addressed on industry classifications. First, CBP data is available only as four-digit SIC codes years prior to 1998. NAICS two-digit, three-digit and six-digit codes are available between 1998 and 2015. Two industry classifications are substantially difference and such that four-digit SIC codes are not uniquely matched to six-digit NAICS codes. To overcome this complication, I use concordances provided by the

National Bureau of Economic Research (NBER) and U.S. Census Bureau's Center for Economic Studies (CES).[10] I use the provided crosswalks for employment, establishment, and annual payroll information with provided weights for each variable. Second, NAICS classification has been updated a number of times during the panel (e.g. NAICS 1997, NAICS 2002, NAICS 2007, NAICS 2012). I harmonize the industry classification to NAICS 2007 (to match the baseline classification used in PDIT) using the crosswalk provided by the U.S. Census Bureau.[11]

I use data from the Bureau of Labor Statistics (BLS) to adjust for inflation. Rather than applying the national rate of inflation, I use the inflation that varies across regions (See Table 19 in the Appendix). The Consumer Price Index for Census Regions. BLS provides the Consumer Price Index (CPI) available at the Census regions level since 1966. The US is divided into four regions: Northeast, Midwest, West, and South.

2.5.2 Descriptive Statistics

There is significant variation in terms of incentive size by incentive type, and the Job Creation Tax Credit grew in significance over time in size and thus in importance relative to others (See Table 2). In fact, when considering that Property Tax Abatement is a city-specific incentive, JCTC can be considered as the largest type of non-discretionary tax incentive at the state-level.

[10] http://www.nber.org.mutex.gmu.edu/nberces/

[11] https://www.census.gov/eos/www/naics/concordances/concordances.html

Table 2. Descriptive Statistics on all business incentives

Panel A: "All Sample," 1990-2015

Variable	N	Mean	Median	St. Dev
Total incentives	38,610	0.9871	0.3380	1.3784
Job creation tax credit	38,610	0.2570	0.0000	0.5468
Property tax abatement	38,610	0.3966	0.0000	0.8611
Investment tax credit	38,610	0.2016	0.0000	0.5108
R&D credit	38,610	0.0506	0.0000	0.1569
Customized job training subsidy	38,610	0.0813	0.0090	0.1715

Panel B: "Only Treated Sample," 1990-2015

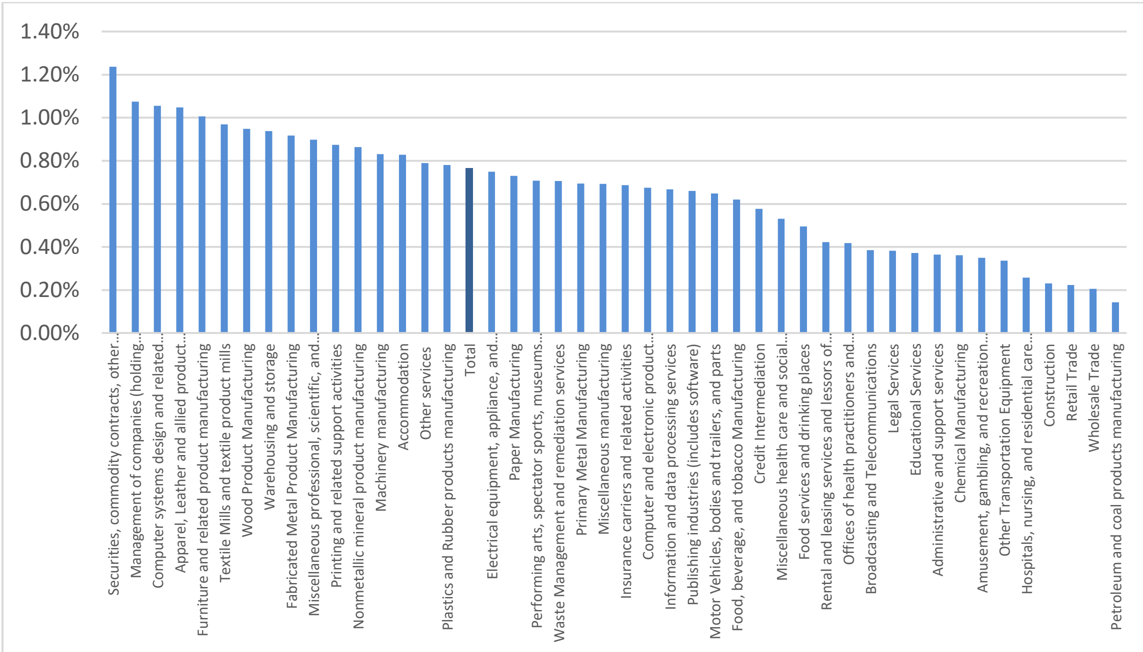
Variable	N	Mean	Median	St. Dev
Total incentives	32,974	1.1558	0.6110	1.4246
Job creation tax credit	11,516	0.7775	0.5620	0.7203
Property tax abatement	11,909	1.2857	0.9600	1.1230
Investment tax credit	10,818	0.7196	0.4555	0.7473
R&D credit	17,578	0.1111	0.0170	0.2176
Customized job training subsidy	19,894	0.1578	0.0800	0.2122

Source: PDIT, 1990-2015.

Notes: The sample consists of 33 states in 45 industries across 26 years. Incentives are calculated as percent of present value of value-added. Values are expressed in percentage terms. Sample excludes AK, NH, SD, WY.

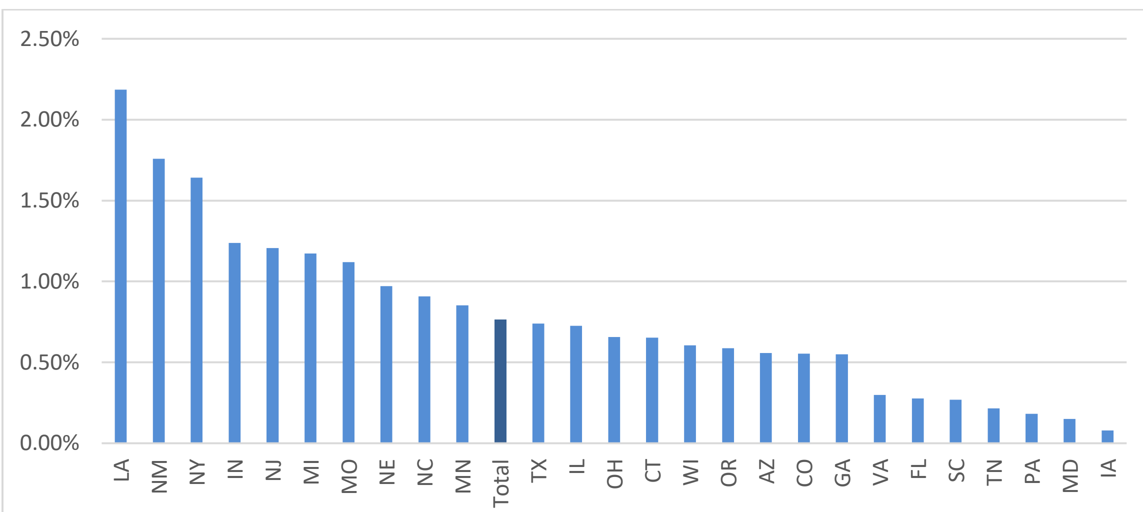
JCTC levels vary greatly across states and industries (Refer to Panel B in Figure 4). For example, the average JCTC is 2.18 percent for Louisiana and 0.08 percent in Iowa. Louisiana and New Mexico are poorer sates but New York, a more affluent state, is also third on the list among states with largest JCTCs. In addition, in Panel A of Figure 4, the average JCTC for "Securities, commodity contracts, other financial investments, and related activities" is 1.2 percent but only 0.1 percent for "Petroleum and coal products manufacturing." Notice that JCTCs target export-base sectors in manufacturing and high value-added professional services.

Panel A: By Industry



Source: PDIT, 1990-2015

Panel B: By State



Source: PDIT, 1990-2015

Figure 4: Average Job Creation Tax Credit

In Table 3, the Analysis of Variance (ANOVA) reveals the total incentives and JCTC variation. The most variation is explained by the state effects. The next is state-by-year and state-by-industry effects. A notable difference is that for JCTC, the second biggest contributor to explaining variation is state-by-year and not state-by-industry as is the case with total incentives. This is because JCTC was virtually non-existent in the 1990s but states widely adopted it over the years.

Table 3: Analysis of Variance (ANOVA)

Panel A: Total Business Incentives

Source of Variation	Partial Sum of Squared	Degrees of freedom	Mean Square	F-statistic
Model	6.7892622	3409	0.0019916	128.41
State	2.2627248	32	0.0707102	4559.04
Industry	1.4560866	44	0.0330929	2133.67
State x Industry	1.9310841	1408	0.0013715	88.43
Year	0.3462355	25	0.0138494	892.94
State x Year	0.6773965	800	0.0008468	54.59
Year x Industry	0.1157348	1100	0.0001052	6.78
Residual	0.5459475	35200	0.0000155	
Total	7.3352097	38609	0.0001900	

Note: The ANOVA explains variance in total incentives. The total number of observations is 38,610. R-squared is 0.9256.
Source: Author' calculations.

Panel B: Job Creation Tax Credit

Source of Variation	Partial Sum of Squared	Degrees of freedom	Mean Square	F-statistic
Model	0.9963894	3401	0.0002930	65.56
State	0.3147353	32	0.0098355	2201.1
Industry	0.1322380	44	0.0030054	672.59
State x Industry	0.1986540	1408	0.0001411	31.57
Year	0.0805439	25	0.0032218	721
State x Year	0.2216151	800	0.0002770	61.99
Year x Industry	0.0420138	1092	0.0000385	8.61
Residual	0.1556716	34838	0.0000045	
Total	1.1520610	38239	0.0000301	

Note: The ANOVA explains variance in job creation tax credit. The total number of observations is 38,240. R-squared is 0.8649.
Source: Author' calculations.

2.6 Empirical Strategy

2.6.1 Summary of Research Limitations

There are a number of limitations to accurately estimating incentive effects. Bartik and Erickcek (2014) illustrate these issues through a simple model:

Equation 6

$$G_{st} = B_0 + B_1 X_{st} + B_2(Incentives_{st}) + e_{st}$$

where G_{st} is the percentage growth in economic activity or employment in a state s over time period t. X_{st} includes a vector of control variables for the state and time period. $Incentives_{st}$ represent the incentives variable for that state and time period. The incentive variable is usually measured in the dollar value of incentives or constructed as a dummy variable and e_{st} is the disturbance term. If the equation is being estimated with firm-level data, then one could add the subscript i to the dependent variable, the incentives variable and the controls, including a firm-specific controls.

The problem with estimating the equation is that incentives are awarded with discretion. Bartik (2018b) reviews 30 different studies and 34 estimates to conclude that typical incentives tip somewhere between 2 percent and 25 percent of incentivized firms to make a decision favoring the location providing the incentives. Put differently, for at least 75 percent of the incentivized firms, their decision on location, expansion or retention will have been the same without the incentive. Estimating ordinary least squares or other similar methods will yield biased estimates since the incentive variable will likely to correlated with the error term. Endogenously determined incentives would bias this study. Consider a scenario where the government officials pass legislature approving incentives anticipating a decline in the state's specific sector. If incentives target a state or industry already experiencing a growth, the estimation will have a positive bias and overstate the policy impact; likewise, if incentives target a state or industry that is shrinking, the estimation will have a negative bias and understate the policy impact.

Lack of a clear counterfactual that would inform what the growth might have been in the absence of the incentive is another important limitation. At the firm-level analysis, propensity score matching is used based on firm size, industry, and other observable characteristics (Donegan, Lester, and Lowe 2019). At the state-level, most studies use an incentive dummy in comparing treatment (adopters) and control (non-adopters) states. Incentives, however, vary considerably across states. One of advantages of Bartik (2017) is that it estimates a value-added percentage of incentives. This variable captures heterogeneity of incentives.

One solution is to control for all possible observable variables. Papke (1994) controls for growth trends (e.g. linear time trend) and location-specific fixed effects in estimating the impact of enterprise zones on local employment and investment.

Another solution is to find an instrumental variable that are correlated with incentives but uncorrelated with growth variables. Holzer et al. (1993) compared incentive recipient firms to those that applied after incentives were exhausted. Greenstone and Moretti (2004) compared counties where new plants located to counties that were runner-ups. Neumark and Grijalva (2017) control for state minimum wages and extended unemployment insurance as among the most policies for job growth that could potentially be confounders to estimating employment growth. They also control for political party assuming that hiring credit policies could vary systematically between democrats and republicans.

2.6.2 Baseline and Interaction Fixed Effect Regressions

Neumark (2013) notes that hiring credits ought to be evaluated based on the net cost per job created. While I cannot directly observe flows, levels of employment across 37 states, 45 industries, and 26 years yields a long panel unlike any other previous studies provides highly granular data that I exploit. While Bartik (2017) database provides information 33 states, Neumark and Grijalva (2017) claim that Alaska, New Hampshire, South Dakota, Wyoming, and Washington state are never adopters of the Job Creation Tax Credit. With an exception of Washington, none of the other four states are in the database but I include them as control state-industries across years. This is an important as 4 states x 45 industries x 26 years yields additional 4,680 observations as a control group, and when considering that by 2015, 24 out of 33 states have adopted one or more state hiring credit.

I estimate the following baseline specification:

Equation 7

$$\mathrm{Ln}(Y)_{ist} = \beta_0 + \beta_1(JCTC_{ist}) + \delta_s + \tau_i + \gamma_t + \varepsilon_{ist}$$

The dependent variable is either the log of employment or the log of earnings per worker in industry *i* in state *s* in time period *t*. state-fixed effects (δ_s), industry fixed-effects (τ_i), and year fixed-effects (γ_t) are incorporated. State fixed-effects control for unobserved, time-invariant state-specific characteristics. For example, the state of New York is substantially larger in population, labor force, and economic output than the state of Alabama. Hence, it is important to control for time-invariant state-specific characteristics. Industry fixed-effects control for unobserved, time-invariant industry-specific characteristics. For example, retail trade industry is very different from legal services. Year fixed-effects control for unobserved, time-varying characteristics, such as the Great Recession between 2007 and 2010.

A preferred specification takes a more sophisticated approach. I argue that it is important to include state-by-year fixed effects control for unobserved, time-varying differences across states; industry-by-year fixed effects control for unobserved, time-varying differences across industries; and industry-by-state fixed effects control for unobserved, time-invariant characteristics of state industries (See Aghion et al. 2008). The state-industry fixed effects control for unobserved, time-varying differences across state-industries. I estimate a series of fixed effect regressions as follows:

Equation 8

$$\text{Ln}(Employment)_{ist} = \beta_0 + \beta_1(JCTC_{ist}) + \delta_{st} + \tau_{it} + \gamma_{is} + \varepsilon_{ist}$$

$Employment_{ist}$ is the log of employment levels, the outcome of interest, in industry *i* in state *s* in time period *t*.[12] The employment variable is from CBP, which is the total mid-March employees with noise. The noise can vary: 0 to 2 percent (low noise), 2 to 5 percent (medium noise), and 5 or more percent (high noise). Some state industry employment figures are withheld to avoid disclosing data for individual companies. The

[12] Given that CBP provides employment data across size groups, equation (2) can be estimated across all size groups separately. The size group includes establishments with 1-4, 5-9, 10-19, 20-49, 50-99, 100-249, 250-499, 500-999, and more than 1000 employees. These decomposed estimates will help answer the question of who creates jobs, small or large establishments, and identify the heterogeneity of JCTC policy effect across size categories. This question will be explored further in the second essay.

variable of interest is $JCTC$, an indicator that equals 1 if state *s* in industry *i* has adopted job creation tax credit by year *t*. In an alternative specification, $JCTC$, equals a ratio of present value of net taxes after Job Creation Tax Credit, to present value of value-added, for a state *s* in industry *i* in year *t*. Interaction fixed effect regressions include state-by-year (δ_{st}), industry-by-year (τ_{it}) and industry-by-state (γ_{is}). The inclusion of these interaction fixed effects ensures that the estimates are robust to many types of unobservable omitted variables that otherwise could confound this analysis. I test for the sensitivity of the estimate across various models and consider which might be more precise.

This demanding specification makes estimating nation-wide employment effects using aggregate data possible and is a particular advantage of my data that compensates for the disadvantages of not being to observe treatment at the individual establishment or firm-level. Industries are composed of mostly 3-digit NAICS. The ε_{ist} is the error term with adjusted standard errors clustered around three-digit state industries (see Bertrand, Mullainathan and Dufflo 2004).

While job creation is at the forefront of interest to policymakers, wage levels of new jobs as well as the nature of jobs created are important too (Courant 1994). Creation of low-wage, part-time jobs create fewer economic benefits than full-time jobs. So, I use CBP data to estimate average payroll effect of business incentives.

Equation 9

$$\text{Ln}\left(\frac{Annual\ Payroll}{Employment}\right)_{ist} = \beta_0 + \beta_1(JCTC) + \delta_{st} + \tau_{it} + \gamma_{is} + \varepsilon_{ist}$$

The $\text{Ln}\left(\frac{Annual\ Payroll}{Employment}\right)$ estimates the both the levels and the growth rate in annual payroll per employee across industry i in state s in time period t. $JCTC$ is the indicator that equals 1 if state s in industry I has adopted any of the Job Creation Tax Credit in year t. State-by-year (δ_{st}), industry-by-year (τ_{it}) and industry-by-state (γ_{is}) fixed effects are included. This specification examines whether the state hiring credit has a trickle-down effect that indirectly raises average earnings per worker. In theory, it is relatively easy for employers (especially multi-establishment firms operating across various states) to benefit from the credit without actually generating net new job creation by frequent job churning, particularly in states that only require full-time equivalents.

2.6.3 Fixed Effect Model (Fully-Saturated with Business Cycle Controls)

Equation (8) tests for the employment effect and Equation (9) tests for the income effect. In the equation (10), I synthesize the first two equations and make the following proposition. I argue that employment and wage effect will largely depend on the business cycle.[13]

I test the hypothesis that JCTC will have a positive employment effect for contracting state-industries and that JCTC will have a positive wage effect for expanding state-industries. During the expansionary cycle, when labor markets tend to be tight and unemployment rates low, hiring credits will have a positive wage effects rather than employment effects. Likewise, during the contractionary cycle, hiring credits will have a positive employment effects rather than wage effects.

[13] See https://www.nber.org/cycles.html. Between 1990 and 2015, there were officially three recorded recessions (July 1990-March 1991; March 2001-November 2001; December 2007-June 2009).

Hypothesis: $\beta_2 < 0$ for $Y = Employment$

$\beta_2 > 0$ for $Y = Earnngs\ per\ worker$

Equation 10

$$Y_{ist} = \beta_0 + \beta_1(JCTC_{ist}) + \beta_2(X_{ist} \cdot JCTC_{ist}) + \delta_{st} + \tau_{it} + \gamma_{is} + \varepsilon_{ist}$$

I use two different variations for the business cycle proxy. The first proxy is state unemployment rate dummy where $UR_{st} = 1$ if $UR_{st} > Median(UR_{st})$ otherwise takes the value of 0. This proxy varies across state and years, and intends to capture state-specific downturns based on the unemployment rate. The second proxy is a state-industry shiftshare model where $\widehat{E_{ist}} = \sum \frac{E_{isb}}{E_{ib}} \cdot \Delta E_{it}$ where $\Delta E_{it} = E_{it} - E_{it-1}$. The base year is 1988. Again, $SS_{ist} = 1$ if $SS_{ist} > Median(SS_{ist})$. In a way, the second proxy is an improvement upon the first since it varies across state-industry-year. In essence, the shift captures the state-industry employment relative to national-industry employment levels. The share refers to the change in industry employment levels from year t and base year (1988). In this framework, state-industry specific downturns are those in which the state-industry employment growth lagged behind national industry employment growth rates.[14]

[14] Some other good proxies for business cycles may be China shock and state GDP. China shock on American localities based on imports is useful in that it is already constructed and readily used by other researchers (Autor, Dorn, and Hanson 2013). Given that Chinese trade impact arises primarily in large importing firms, the China shock may actually affect businesses differently by firm size. China shock could potentially hurt larger manufacturers but not the smaller manufacturers that can nimbly adapt to serve niche markets. Finally, it may be useful to just use BEA's state GDP to determine economic downturns.

2.6.4 Fixed Effect Model (Fully-Saturated with Lagged Dependent Variable Controls)

One important factor that has not yet been discussed is that labor demand adjusts to shocks to product demand and factor prices. Let us assume that factor prices include wages w_t, interest rate r_t, and price p_t. Let us also assume that a firm's cost minimization takes the following functional form:

Equation 11

$$\min_{\{E_t\}} C = a(E_t - E_{t-1})^2 + b(E_t - E_t^*)^2 \text{ where } E_t^* = \delta_0 + \delta_1 \cdot w_t + \delta_2 \cdot r_t + \delta_3 \cdot p_t$$

$$2a(E_t - E_{t-1}) + 2a(E_t - E_t^*) = 0$$

$$2(a+b)E_t = 2a(E_{t-1}) + 2a(E_t - E_t^*) = 0$$

$$E_t = \frac{a}{a+b}(E_{t-1}) + \frac{b}{a+b}(\delta_0 + \delta_1 w_t + \delta_2 r_t + \delta_3 p_t)$$

By rearranging and solving for the equilibrium, we find that E_t is a function of lagged employment (E_{t-1}) and the cost function $(\delta_0 + \delta_1 w_t + \delta_2 r_t + \delta_3 p_t)$. The literature is unclear as to which lag best captures the labor adjustments. Based on my analysis, whichever lags I use make little difference to the estimate. So, I take the more conventional approach of a year lag. I can re-estimate Equations (7), (8), and (9) with lagged employment or earnings per worker variable such that Y_{ist-1} is included as additional control on the right hand side shown in Equation 12 and Equation 13:

Equation 12

$$\mathrm{Ln}(Y)_{ist} = \beta_0 + \beta_1(JCTC_{ist}) + \beta_2(Y_{ist-1}) + \delta_{st} + \tau_{it} + \gamma_{is} + \varepsilon_{ist}$$

Equation 13

$$Y_{ist} = \beta_0 + \beta_1(JCTC_{ist}) + \beta_2(X_{ist} \cdot JCTC_{ist}) + \beta_2(Y_{ist-1}) + \delta_{st} + \tau_{it} + \gamma_{is} + \varepsilon_{ist}$$

2.7 Results

2.7.1 Baseline and Interaction Fixed Effect Models

Table 4 has nine different regression specifications with the log of employment as the dependent variable and the independent variable represented by a JCTC dummy or a JCTC continuous value (e.g., percent of present value of value-added). The estimated effect of the dummy variable ranges from a -15.0 percent to +8.64 percent. While to a lesser extent, the estimated effect of the continuous variable ranges between -0.59 percent to -8.79 percent. At least the signage does not change, though magnitude varies. This wide range of estimates is indicative of the challenges highlighted earlier in Section 2.6.1 but also the importance of finding the right model that best accounts for unobserved heterogeneities, especially since many estimates are statistically significant and even differ in directionalities of signs. The baseline specification in column (1) indicates a negative and statistically significant effect of the JCTC. State industries with the credit are predicted to create an estimated 4.14 percent less employment than state industries without the credit. Adding state-by-industry interacted fixed effects in column (2) more than triples the coefficient to -14.9 percent, while adding the state-by-year interacted fixed effect reverses the coefficient to a +6.33 percent, and adding the industry-by-year fixed effects brings back the coefficient to that of the baseline at roughly -3.45 percent.

These numbers suggest that when controlling for unobserved, time-varying state or industry characteristics, the negative employment effect diminishes. But when controlling for unobserved, time-invariant state-industry characteristics, the negative employment effect increases. These results highlight once more the deeply entrenched state-industry heterogeneities across states in the US, such as the underdevelopment and poverty in the deep south. It is striking that when not controlling for state-industry fixed effects, but only state-year and industry-year, the coefficient is positive and statistically significant at 8.64 percent. It is also worth noting that once state-industry time trends are introduced the coefficient is negative, loses significance, and the magnitude drops to 1.22 percent.

Table 4, Panel C and B present the baseline specifications as well as interaction fixed effects for the log of earnings per worker depicted in Equation (3). Compared to the employment effects, earnings per work effects are visibly weaker with a range of -1.78 percent and +2.41 percent for the dummy variable (Panel C) and a range of -0.69 percent and +0.51 percent (Panel D).

In separate regressions (See Tables 18 and 19 in the Appendix)**,** I run the same regressions but restrict the sample only to export-base industries, which presumably have higher positive externalities (e.g., job multipliers). My sample decreases as I drop 14 non-export industries and keep 31 export-base industries. On average, incentives in export-base industries, which includes both manufacturing and non-manufacturing industries (e.g., professional services), are higher. The negative employment effect persists with a similar magnitude as before but it is no longer statistically significant. This implies that there is a high level of heterogeneity among states that target export-base industries. Clearly, some states are more successful than others at implementing these policies than others. State business incentives appear to be work best not states that offer the most incentives but in those that supplement this policy with investments in skills training, small business services, infrastructure, and land development.

Table 4. Estimated Effects of Job Creation Tax Credits on Employment and Earnings per worker (Levels), Baseline and Interaction Fixed Effect Regressions, 1990-2015, 37 States

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				< Log of Employment >				
Panel A								
JCTC Dummy	-0.0414***	-0.149***	0.0633***	-0.0345***	-0.112***	0.0864***	-0.150***	-0.115***
	(0.0110)	(0.00884)	(0.0121)	(0.0114)	(0.00969)	(0.0122)	(0.00807)	(0.00883)
Panel B								
JCTC (% of PV value-added)	-0.0647***	-0.0879***	-0.0477***	-0.0591***	-0.0774***	-0.0400***	-0.0786***	-0.0590***
	(0.00940)	(0.00773)	(0.0101)	(0.00892)	(0.00861)	(0.00956)	(0.00606)	(0.00641)
				< Log of Earnings per Worker >				
Panel C								
JCTC Dummy	0.00462*	-0.00445**	0.0241***	.0000539	0.0211***	0.0178***	-0.0123***	0.00497*
	(0.00262)	(0.00221)	(0.00311)	(0.00270)	(0.00293)	(0.00331)	(0.00217)	(0.00287)
Panel D								
JCTC (% of PV value-added)	-0.00396	-0.00320	-0.000645	-0.00659***	0.00505*	-0.00497*	-0.00689***	-0.00297
	(0.00250)	(0.00208)	(0.00295)	(0.00245)	(0.00275)	(0.00293)	(0.00192)	(0.00251)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Industry	No	Yes	No	No	Yes	No	Yes	Yes
State*Year	No	No	Yes	No	Yes	Yes	No	Yes
Industry*Year	No	No	No	Yes	No	Yes	Yes	Yes

Note: Data is from County Business Patterns from 1990 to 2015. NBER crosswalk weights are used to convert sic to naics for years 1990 to 1997. All naics codes are harmonized at naics 2007. Data is merged to Bartik (2017) Panel Database of Incentives and Taxes for 37 states, 45 industries, and 26 years. The total number of observations is 41,329. 1,116 observations are missing. The dummy value for the Job Creation Tax Credit takes the value of 1 for the state s, industry i, and year t with the subsidy. The continuous value for the Job Creation Tax Credit is estimated as a ratio of present value of net taxes after Job Creation Tax Credit, to present value of value-added. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

2.7.2 Business Cycles

In this section, I estimate Equation 13 for log of employment and log of earnings per worker. I use two business cycle proxies: one is state unemployment rate and another an employment shiftshare variable (See Table 17 in the Appendix for more information). The state median rate of unemployment for 50 states and D.C. for 26 years (1990-2015) is 5.4 percent. The employment shiftshare median is -0.00004 and the range between -0.1 and 0.18. Initially, I find that controlling for business dynamics does not significantly change the coefficient. When comparing the fully saturated model in column (8) of Table 4 and model with business dynamics in column (1) of Table 5, the coefficient is -11.5 percent and -11.9 percent respectively. Same applies to the earnings per worker variable; the magnitude changes slightly from 0.36 percent in column (8) to 0.45 percent.

The state unemployment rate proxy changes the employment coefficient estimates (-8.86 percent) a lot more than the shiftshare proxy, though the estimates are all negative. This is because as stated earlier, most of the variation occurs at the state-level. Interestingly, the coefficient for the log of earnings per worker is estimated very similarly across these two business cycle proxies at 0.451 percent and 0.415 percent in Panel A and B of column (2) in Figure 5, respectively.

Neumark's economic theory is confirmed by one of the business dynamics proxies on the employment variable; meanwhile, both earnings per worker measures are imprecisely estimated. The one coefficient that confirms the economic theory is the state-industry shiftshare interaction term, which is positive and statistically significant for employment. The interpretation is that state hiring credits have a more positive employment effect during contractionary periods than in expansionary periods. However, the state unemployment interaction term rejects Neumark’s hypothesis whereby employment effect becomes more negative during contractionary periods. These contradicting results raise concerns as to whether the two business cycle proxies capture business dynamism. Perhaps, additional proxies such as state GDP or China import shock or the predicted state employment used in Neumark and Grijalva (2017) would be useful to pursue in the future. But between the two proxies used, the shiftshare is more robust as a measure as it captures state’s industry-specific variations over time while the state unemployment proxy masks industry variation.

Table 5. Estimated Effects of Job Creation Tax Credits on Employment (Levels) and Earnings per Worker (Levels), with Business Cycle proxies, Levels, 1990-2015, 37 States

	Log of Employment	Log of Earnings per Worker
Panel A		
JCTC Dummy	-0.119***	0.00451
	(0.00938)	(0.00319)
JCTC x State-Industry Shiftshare Dummy	0.00840	0.000906
	(0.00944)	(0.00297)
Panel B		
JCTC (% of PV value-added)	-0.0654***	-0.00383
	(0.00667)	(0.00257)
JCTC (% of PV value-added) x State-Industry Shiftshare Dummy	0.0140**	0.00186
	(0.00715)	(0.00250)
Panel C		
JCTC Dummy	-0.0886***	0.00415
	(0.00960)	(0.00301)
JCTC x State Unemployment Rate Dummy	-0.0529***	0.00166
	(0.00839)	(0.00298)
Panel D		
JCTC (% of PV value-added)	-0.0447***	-0.00344
	(0.00829)	(0.00294)
JCTC (% of PV value-added) x State Unemployment Rate Dummy	-0.0204***	0.000671
	(0.00726)	(0.00273)
Observations	41,326	41,326
State FE	Yes	Yes
Industry FE	Yes	Yes
Year FE	Yes	Yes
State*Industry	Yes	Yes
State*Year	Yes	Yes

Note: Data is from County Business Patterns from 1990 to 2015. NBER crosswalk weights are used to convert sic to naics for years 1990 to 1997. All naics codes are harmonized at naics 2007. Data is merged to Bartik (2017) Panel Database of Incentives and Taxes for 37 states, 45 industries, and 26 years. The total number of observations is 41,329. 1,116 observations are missing. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

2.7.3 Regressions with Lagged dependent variables

I contend that including lagged dependent variable as a control is very important given the persistence in employment levels from time t-1 to time t. Also as discussed earlier, all previous models failed to factor in the labor adjustment costs properly. Hence, I run the same regressions discussed in the previous section but with lagged dependent variables as controls. Table 9 introduces lagged dependent variable as controls to estimating log of employment and earnings per worker results from Table 8. Table 11 does the same for Table 10. A rather large change is observed once the adjustment costs are accounted for. For one, coefficients become smaller in magnitude.

In Table 6, the estimated JCTC coefficient drops from -11.5 percent to -3.55 percent, which comes very close to the -3.72 percent estimate of Donegan, Lester and Lowe (2019). Also notice that the range between dummy and continuous variable coefficients becomes more narrow (-3.55 percent and -1.92 percent), suggesting a more precise estimation. Also notice that unlike in Table 8, across nine different specifications only the negative coefficients are now statistically significant (Column 2, 5, 7, and 8). Similar patterns are observed in the estimates for earnings per worker from Table 8 to 9.

In Table 7, I examine state hiring credits while controlling for lagged dependent variable and business dynamics. I find that state industry shiftshare proxy supports Neumark's (2013) argument but state unemployment proxy does not. The shiftshare proxy of business dynamics in panel A suggest that state-industries experiencing a greater employment decline than that of the national average will experience a statistically greater employment levels increase (0.15 percent). And, states experiencing a greater unemployment rate than that of then national average will experience a statistically greater employment levels increase (0.12 percent). While state hiring credits do not reverse the negative employment effects in contractionary periods (-4.3 percent), they do appear to offset the negative employment shocks approximately by one-third (1.5 percent / 4.3 percent) and one-half (1.21 percent / 2.46 percent). These results support the claim that state hiring credits are indeed more effective in alleviating credit constraint and tighter labor markets during contractionary periods. When using state unemployment proxies, however, I find that state hiring credits do not increase the level of employment. These results are consistent across business cycles and even when restricted to just export industries, suggesting that positive externalities and credit constraint arguments do not hold.

As for the earnings per worker variable, the signage is correct for the shiftshare proxy and not for the state unemployment proxy. One interesting observation is that the JCTC Dummy (0.43 percent) yields a positive and statistically significant effect at the 10 percent level, suggesting that while the state hiring credits do not induce higher employment effect, they have a small income effect on the local workers. However, not much can be explained because the magnitudes are generally small and the statistically insignificant across different specifications.

Table 6. Estimated Effects of Job Creation Tax Credits on Employment and Earnings per worker (Levels), With Lagged Controls and a Series of Fixed Effect Regressions, 1990-2015, 37 States

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				< Log of Employment >				
Panel A								
JCTC Dummy	-0.00632	-0.0329***	-0.00700	-0.000945	-0.0385***	0.000824	-0.0392***	-0.0355***
	(0.00427)	(0.00596)	(0.00451)	(0.00431)	(0.00621)	(0.00439)	(0.00636)	(0.00685)
Panel B								
JCTC (% of PV value-added)	-0.00919***	-0.0211***	-0.0114***	-0.00623**	-0.0246***	-0.00775***	-0.0219***	-0.0192***
	(0.00311)	(0.00436)	(0.00298)	(0.00296)	(0.00445)	(0.00269)	(0.00432)	(0.00430)
				< Log of Earnings per Worker >				
Panel C								
JCTC Dummy	0.000772	-0.00247	0.00384**	0.00110	0.00536***	0.00477***	-0.00514***	0.00360
	(0.00134)	(0.00157)	(0.00157)	(0.00145)	(0.00201)	(0.00176)	(0.00171)	(0.00241)
Panel D								
JCTC (% of PV value-added)	-0.000763	-0.00142	-0.000646	-0.000553	0.000571	-0.000325	-0.00245*	-0.000436
	(0.00114)	(0.00141)	(0.00129)	(0.00118)	(0.00178)	(0.00134)	(0.00146)	(0.00191)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Industry	No	Yes	No	No	Yes	No	Yes	Yes
State*Year	No	No	Yes	No	Yes	Yes	No	Yes
Industry*Year	No	No	No	Yes	No	Yes	Yes	Yes

Note: Data is from County Business Patterns from 1990 to 2015. NBER crosswalk weights are used to convert sic to naics for years 1990 to 1997. All naics codes are harmonized at naics 2007. Data is merged to Bartik (2017) Panel Database of Incentives and Taxes for 37 states, 45 industries, and 26 years. The total number of observations is 39,255. The dummy value for the Job Creation Tax Credit takes the value of 1 for the state s, industry i, and year t with the subsidy. The continuous value for the Job Creation Tax Credit is estimated as a ratio of present value of net taxes after Job Creation Tax Credit, to present value of value-added. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Estimated Effects of Job Creation Tax Credits on Employment (Levels) and Earnings per Worker (Levels), with Business Cycle proxies, With Lagged Controls and a Series of Fixed Effect Regressions, 1990-2015, 37 States

	Log of Employment	Log of Earnings per Worker
Panel A		
JCTC Dummy	-0.0430***	0.00430*
	(0.00736)	(0.00260)
JCTC x State-Industry Shiftshare Dummy	0.0150**	-0.00142
	(0.00745)	(0.00230)
Panel B		
JCTC (% of PV value-added)	-0.0246***	-0.000493
	(0.00442)	(0.00190)
JCTC (% of PV value-added) x State-Industry Shiftshare Dummy	0.0121**	0.000125
	(0.00512)	(0.00195)
Panel C		
JCTC Dummy	-0.0258***	0.00173
	(0.00738)	(0.00248)
JCTC x State Unemployment Rate Dummy	-0.0197***	0.00379
	(0.00554)	(0.00240)
Panel D		
JCTC (% of PV value-added)	-0.0133**	-0.00149
	(0.00564)	(0.00230)
JCTC (% of PV value-added) x State Unemployment Rate Dummy	-0.00835*	0.00149
	(0.00469)	(0.00213)
State FE	Yes	Yes
Industry FE	Yes	Yes
Year FE	Yes	Yes
State*Industry	Yes	Yes
State*Year	Yes	Yes

Note: NBER crosswalk weights are used to convert sic to naics for years 1990 to 1997. All naics codes are harmonized at naics 2007. Data is merged to Bartik (2017) Panel Database of Incentives and Taxes for 37 states, 45 industries, and 26 years. The total number of observations is 39,253. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

2.8 Conclusion

Whether state hiring credits actually induce job creation in local economies remains an important but understudied policy issue. Empirical findings in the literature are mixed, but these differences pertain less to the type of data used (aggregate data or micro data), variables included, or how taxes/credits are measured; they pertain to the time period analyzed or industry studied. Using a novel and the most comprehensive database on incentives and taxes, I address this concern and empirically test an important theoretical proposition laid out by Neumark (2013) that a broad-based state hiring credits could be an integral fiscal policy tool to boost job creation, particularly in response to recessions. Unlike most studies on state hiring credits that focus on disadvantaged localities and the question of redistribution, I have focused on examining a nationally-representative sample, across export and non-export base industries, establishment size categories, and business cycles. The large variation in state policies, even among adjacent states, demonstrate that some are better designed and some better implemented than others. Hence, it may be worth examining more in depth states that successfully design and implement state hiring credits than others.

CHAPTER 3. STATE BUSINESS INCENTIVES AND EMPLOYMENT GROWTH: EVIDENCE ACROSS FIRM SIZE AND AGE

3.1 Abstract

A large number of studies examine incentives handed to large firms, finding little supporting evidence, but a dearth of literature has examined the policy effect across the firm size distribution or by incentive type. My study contributes to the literature by studying incentives across the firm size and age distributions, and studying all four major types of incentives at the state-level: (i) job creation tax credit (JCTC); (ii) (iii) R&D tax credit (RDTC); (iv) investment tax credit (ITC); (v) and, customized job training subsidy (CJTS). Merging the panel database on incentives and taxes with public datasets, my findings suggest that ITC is associated with higher employment growth among young firms (Age 6-10), while CJTS is associated with higher employment growth for startups (Age 0-5) and mature firms (Age 11+). I also find supporting evidence that CJTS and ITC spur employment growth among small businesses (<500 Employees). The key takeaway of the study is that incentives targeting young and small businesses appear to be more effective in generating employment growth than incentives targeting mature and large businesses.

3.2 Introduction

States have long used various business incentives to influence business relocation, expansion, and startup decisions (Buss 2001). In general, incentive policies target relocation and expansion decision of large firms, which have the capacity to create large number of jobs at once and make significant investments that generate immediate economic impact (Bruce and Deskins 2012). While a large number of studies examine incentives handed to large firms, a dearth of literature has examined the policy effect across the entire firm size distribution.

Empirical studies are largely in agreement that state business incentives are given to large firms but have little influence over their expansion or relocation decisions. On average, only two percent of a state’s employers have more than 100 employees but they receive between 80 and 90 percent of all incentive dollars (LeRoy et al. 2015). Yet, incentives to such large firms tip less than 25 percent of relocation or expansion decisions (Bartik 2019). In other words, firm expansion and relocation decisions occur largely independent of incentives, and most hiring and investments will have taken place in lieu of incentives in strategically desirable locations.

Critics have called for a complete abandonment of incentive programs, which are wasteful and ineffective. However, the state practice of incentives has been deeply entrenched. In the last two decades, incentives have tripled instead (Bartik 2017). A notable increase in incentives followed the Great Recession, reflecting the desperation of state governments to create jobs. There is evidence that the uptick in incentives coincide well with governor election cycles (Slattery 2020). Voters are more likely to vote for politicians who offer incentives (Bartik 2019). Consequently, incentive reform advocates have been pushing to refocus debate over incentives from a yes-or-no stalemate toward collective deliberation of how to improve incentives as an economic development policy. How can state governments better target and implement incentive programs that achieve economic development goals?

The existing literature on incentives is limited in that it tends to focus on a single type, a single industry, or short time span (Wasylenko 1997).[15] But it is important to recognize that there are four major categories of state business incentives: customized job training subsidies (CJTS), investment tax credits (ITCs), job creation tax credits (JCTCs), and research and development tax credits (RDTC).[16] In practice, firms are bound to consider incentives not in isolation but in combination; hence, examining incentives separately will inevitably lead to an incomplete understanding. Therefore, it is critical that incentives are examined together and in relation to each other.

Another potentially important limitation is the lack of studies that consider question of firm size. As discussed earlier, extant literature finds ineffectiveness of incentives that go to large firms, but what about for small-and-medium firms? Perhaps, if incentives are uniformly prescribed to firms independently of size, there is no concrete reason to think

[15] This study takes a state-level approach and thus Property Tax Abatement (PTA), which is a city-level incentive, is omitted.
[16] CJTS and JCTC are essentially labor subsidies. PTA, ITC, RDTC are capital subsidies.

that incentives given to SMEs should have a material difference in effect to what is observed among large firms. It is a reasonable claim but largely unexplored.

This paper attempts to contribute to the gap in knowledge by addressing the following questions: (i) what is the differential employment effect of four types of state business incentives? (ii) what is the differential effect of these incentives across firm size and firm age? Analyzing the most comprehensive database on incentives and taxes covering 45 industries in 47 cities in 33 states between 1998 and 2015, this paper contributes to the literature by investigating the differential effect of four different types of state business incentives on small vs. large vs. young firms. Specifically, I study employment growth of different incentives by firm size and age categories.

3.3 Motivation and Literature Review

State governments have a number of policy levers with which to spur local economic growth. These include subsidized lending or other business cost subsidies, reduced taxes on equity investments, reduced hiring costs, provision of information or other market-making mechanisms, location-specific or industry-specific subsidies to start a business in a given location or industry, to name a few (Acs et al. 2016). The primary place-based policy in the United States is state and local business tax incentives. Tax credits are a direct reduction in business' tax liability in income, sales and use, property, or other business taxes. These credits can subsidize investments in capital equipment or machinery; job creation in a targeted industry above a certain salary minimum; use or investment in renewable energy; and research and development activities. Exemptions and deductions typically exclude or reduce tax liabilities from specific business activities encouraged by the state, such as the acquisition of land and tax exemptions for building construction, raw materials, sales and use taxes, and inventory taxes.

Few states focus their entire economic development efforts on incentives alone. Rather incentives are embedded within a larger economic development strategy. According to 2018 Area Development's Annual Survey, state and local incentives rank seventh among site selection factors, behind quality of life (6), tax exemptions (5), corporate tax rate (4), highway accessibility (3), labor cost (2), and availability of skilled labor (1).[17] While no study has successfully included all these factors, at the least, it is important to study all types of incentives together and explore how policymakers package them together for negotiating deals.

While a large number of studies examine incentives handed to large firms, a dearth of literature has examined the policy effect across the entire firm size distribution. In the US firms with fewer than 500 employees account for almost two-thirds of net new jobs and generate thirteen times more new patents per employee than do large firms (Acs, Parsons, and Tracy 2008). Consider that empirical studies find that incentives, unlike in large firms, spur job creation and innovation for SMEs? Such findings should have profound implications to policy implementation and practice.

[17] https://www.areadevelopment.com/Corporate-Consultants-Survey-Results/Q1-2019/33nd-annual-corporate-survey-15th-annual-consultants-survey.shtml

3.3.1 Discussion on possibility of differential policy effect

Chatterji, Glaeser, and Kerr (2014) categorize arguments in favor of state business incentives into three general classes: (i) redistribution, (ii) externalities and (iii) credit constraints. The redistributive aim of the policy is best seen in “Empowerment Zones” that incentivize investment in economically disadvantaged, blighted, and underdeveloped micro regions (e.g., neighborhoods, counties). The positive externalities manifest themselves through high job multipliers or knowledge spillovers in export-base industries (e.g., high-tech sector, manufacturing, professional services). Finally, state business incentives lower the effective tax rate, which lessens the credit constraint and allows firms to put excess capital to productive use.

The general literature on incentives is largely agnostic on the question of firm size and overlooks the possibility of differential policy outcome across the firm size distribution. It is possible that the only relevant question is whether state business incentives work for large firms. It is also possible that incentives are universal, thus if they work, they should equally work for all firms, regardless of size. However, these are plausible but untested assumptions. According to a Kauffman Foundation commissioned survey, however, 79 percent of small business owners maintained that government incentives favor established big businesses over small ones, putting them at a disadvantage.[18] Again, such sentiments could merely reflect a bias against large firms or be true. For example, large firms’ superior organizational capabilities in complex legal and

[18] https://www.linkedin.com/pulse/why-does-america-favor-big-old-businesses-over-small-new-victor-hwang/

regulatory conditions, could put them at a natural advantage. This is a clear gap in the incentives literature that is important but understudied.

Returning to Chatterji, Glaeser, and Kerr’s (2014) classification, it is not difficult to relate them to the discourse on differential policy outcome of incentives. Redistribution and credit constraints arguments are better suited as a case for incentives targeting resource-constrained small firms, while externalities argument is better suited to favor capital-abundant large firms. At some level, the debate is about budget constraints, what the most optimal use of public resources is and who to target: large or small firms. At another, there are good policy reasons to support large and small firms alike, and policy objectives need not be mutually exclusive.

Externalities. The externalities argument is well-suited for proponents that want incentives targeted at large firms. According to Bartik (2019) and Moretti (2011), states should target export-oriented industries (e.g., high-tech) with high job multipliers to be able to spur economic growth, innovation, and wage growth. It is large firms with higher productivity, higher-wage premiums, and higher job multipliers that are most likely to generate the greatest positive externalities (Garcia-Mila and McGuire 2002; Henderson, 2003; Greenstone and Moretti 2003; Greenstone, Hornbeck and Moretti 2010). A growing body of literature on welfare effects of incentives finds that gains are almost entirely accrued in incentivized firms and positive externalities are overstated (Kim 2020; Slattery 2020).

Redistribution. The literature on redistribution studies incentives aimed at revitalizing blighted or underdeveloped urban areas. Apart from considerable variation in incentive performance, its main findings suggest that most effective incentives at generating job growth are targeted at small establishments (Wilder and Rubin 1996). Also, some studies find that incentives to large firms effectively discriminates against small firms, citing evidence of displacement and crowd-out effects (Tuszynski and Stansel 2018).

Credit Constraint. The credit constraint narrative also naturally leans toward small businesses (Weber 2007). One possible explanation for why many studies find little policy effect of incentives expansion or relocation decisions of large firms is that they are less capital-constrained and thus incentive dollars less important to their operation. But for small businesses, liquidity constraints are more immediate to their growth trajectory, and labor or capital subsidies enable them to more readily put excess capital to productive use (Criscuolo et al. 2019; Garrett, Ohrn, and Serrato 2019).

3.3.2 Discussion of empirical evidence

State business incentives are largely criticized as wasteful and ineffective, but empirical results remain mixed. Neumark (2013) argues that most negative assessments of the policy are based on evaluation of credits aimed at redistribution by targeting disadvantaged areas. There are many studies that document negative or no effect: Calcagno and Thompson (2004) find a negative relationship between state economic incentives and manufacturing value-added, which suggests inefficient reallocation of state resources. Bingham and Bowen (1994) do not find significant relationship between state spending on economic development and economic growth. But there are also studies that document empirical evidence for effectiveness of: customized job training (Hollenbeck 2008; Holzer et al 1993; Hoyt, Jepsen, and Troske 2008), manufacturing extension services (Jarmin 1998, 1999), economic development (Goss and Phillips 1994, 1997; Faulk 2002; Hicks and LaFaive 2011) and JCTC (Perloff and Wachter 1979; Bishop 1981; Chirinko and Wilson 2016). It becomes clear that incentives might have a differential policy impact by type.

Not all types of incentives are discriminatory toward startups and small businesses. In fact, some studies have found empirical evidence suggesting that certain incentives spur startups and small businesses. Bartik (2019) finds that customized job training and manufacturing extension programs are arguably the most effective incentives that benefit small-and-medium businesses. Other studies document positive impact of research and development credits on new firm entry and spinoffs (Wu 2008; Fazio, Guzman, and Stern 2019; Babina and Howell 2018).

Fazio, Guzman, and Stern (2019) find that the research and development tax credit is associated with a significant long-term impact on both the quality and quality-adjusted quantity of entrepreneurship. Babina and Howell (2018) find that firms that receive research and development credit are more likely to experience “knowledge spillover” and contribute to new venture creation. Wu (2008) finds that research and development credit contributes to overall growth of the high-technology sector. Hence, label incentives dichotomously as “pro-entrepreneurship” or “pro-big-business” is not always accurate.

The more recent studies use firm-level data. Donegan, Lester and Lowe (2019) construct a longitudinal establishment-level data from national databases (Good Jobs First Incentive Database, the National Establishment Time-series database, and the State Economic Development Expenditure Database from the Council for Community and

Economic Research). They identify control establishments for each incentive-receiving treatment establishment based on three-digit SIC code, state, subsidiary status and employment category. They find that incentivized firms fail to create more jobs than matched control establishments. Still, they find that small establishments benefit more from state-level incentives than the large establishments.

Bartik (2018) has recommended that incentives shift their targets from large out-of-state firms to locally-owned small and medium-sized businesses, who are more likely to invest and hire local workers. There is also evidence that incentives benefit small businesses more than large businesses. For example, customized job training subsidy is designed to train local workers with critical skills that many employers would be reluctant to invest themselves. Many small and medium-sized businesses lack the expertise (information barriers), time, or money (financial barriers) to provide these types of trainings, thus, benefit the most from the policy (Bartik 2018). But most other incentives are “more friendly” to large businesses. Job creation tax credit, investment tax credit, and corporate income tax credit are designed to “attract” large businesses, for example.

Table 8. Summary of literature on the benefits to state business incentives, small vs. large

State Business Incentive	Entrepreneurs/Small businesses	Large Incumbents
Job Creation Tax Credit	Donegan, Lester, and Lowe (2018); Cahuc et al. (2019)	
Investment Tax Credit	Criscuolo et al. (2019)	
Corporate Income Tax Credit		
R&D Credit	Wu (2008); Fazio, Guzman, and Stern (2019); Babina and Howell (2018)	Bloom, Griffith, and Van Reenen (2002); Wilson (2005); Lucking (2019)
Customized Job Training Subsidy	Bartik (2018)	
Total Incentives		Tuszynski, Patrick, and Stansel (2018)

Small firm growth is an empirical regularity, independent of incentives, highlighted in studies dating back to Birch (1979). In some sense, small businesses, especially young firms (less than 10 years old) have a greater propensity to grow and add more jobs even without incentives (Davis, Haltiwanger, and Schuh 1996). This growth tendency, of course, is observed in a small cohort of young firms, since most small businesses do not grow or want to grow (Hurst and Puglsey 2011). Hence, targeting well would be important given that only certain firms would respond to policy with growth.[19]

3.3.3 Discussion on Firm Size vs. Firm Age as measure of firm growth

Using the Dun & Bradstreet data, Birch (1987) was instrumental in forming a popular perception that small firms create most jobs. The study suffers from the regression-to-the-mean bias where businesses that experience negative external shocks will more

[19] See also Neumark, Wall, and Zhang (2011), and Haltiwanger, Jarmin, and Miranda (2013).

likely grow and businesses experiencing positive external shocks will more likely shrink. This bias is reinforced by problematic base year measurement that drives the inverse relationship between firm size and growth.

Neumark, Wall & Zhang (2011) is an improved study of Birch by avoiding some of misleading interpretations highlighted by Davis, Haltiwanger and Schuh (1996): lack of suitable data, differentiation of net from gross job creation, size classification, and regression-to-the-mean bias. They use the NETS data, which is an improvement over Dun & Bradstreet data, on US private sector from 1992-2004. Their main finding is that small firms contribute disproportionally to net job growth.

Haltiwanger, Jarmin and Miranda (2013) provided an alternative measurement to firm size. They contend that firm age rather than firm size is a better indicator of employment growth. They conclude that young businesses, not small businesses, create most jobs. They pull state-of-art data from the U.S. Census Bureau's Longitudinal Database (LBD) and its public version, the Business Dynamics Statistics (BDS). The LBD is a valuable data set that overcomes two limitations of most databases: i). information about firm birth ii). information about firms and establishments. In addition, they use an improved the size classification methodology known as current average size (average size in year t and year t-1); this way, they are able to avoid the negative bias found in the base size method (t-1) or the positive bias found in the end size method (t).

They use the growth rate as a dependent variable to examine who creates most jobs, the small, the large, or the young. Independent variables are size and age bins. Growth rate is calculated as employment of year t-1 subtracted from employment of t divided by the average of two employment periods.

Equation 14

$$g_{it} = (E_{it} - E_{it-1})/X_{it} \text{ where } X_{it} = .5 * (E_{it} - E_{it-1})$$

They use 8 size class categories (1–4, 5–9, 10–19, 20–49, 50–99,100–249, 250–499 and 500 and up) and 9 age classes (0,1–2, 3–4, 5–6, 7–8, 9–10, 11–12, 13–15, and 16 and up). Their main findings are: i). When one doesn't control for firm age, inverse relationship confirmed, though sensitive to regression-to-the-mean effects. ii). With firm age controls, there is no systematic inverse relationship between net growth rates and firm size.

3.4 Theory

Consider the following firm's profit maximization problem consisting of just labor and investment decisions (Jorgensen 1963). Let *p* be the price of output, *w* the wage rate, *s* the price of capital goods, Q the quantity of output, *E* the quantity of variable input, and *I* the rate of investment. Without state hiring credit would look as follows:

Equation 15

$$\mathrm{f(E, I)} = pQ - wE - qI$$

The hiring credit lowers the effective tax rate or the labor cost, since it applies only to labor and not capital. Hence, in the presence of the hiring credit, firms in competitive markets would use more labor than capital. Hence, with state hiring credit, the profit maximization function would look as follows:

Equation 16

$$f(E, I) = pQ - (1 - t)wE - qI$$

The state business incentive that lowers the effective cost of capital, the profit maximization function would look as follows:

Equation 17

$$f(E, I) = pQ - wE - (1 - t)qI$$

Finally, when considering that states offer different types of incentives and firms are likely to take advantage of these incentives as a package, it is possible to assume the following profit maximization function:

Equation 18

$$f(E, I) = pQ - (1 - t)wE - (1 - t)qI$$

With t being a certain type of tax incentive, the first order condition will look as follows:

Equation 19

No labor tax credit: $\frac{\partial E}{\partial I} = \frac{w}{q}$

With labor tax credit: $\frac{\partial E}{\partial I} = \frac{(1-t)w}{q}$

With capital tax credit: $\frac{\partial E}{\partial I} = \frac{w}{(1-t)q}$

With labor and capital tax credit: $\frac{\partial E}{\partial I} = \frac{(1-t)w}{(1-t)q}$

These firm's profit maximization functions suggest that labor or capital subsidies lower the effective tax rate, which in turn lowers the cost of labor and capital. Businesses are left with excess capital, which can be deployed to productive use (Criscuolo et al. 2019; Garrett, Ohrn, and Serrato 2019).

The firm's profit maximization functions lead to two relevant hypotheses. First, since state business incentives lower the effective tax rate, *ceteris paribus*, I predict that higher provision of tax incentives should result in higher employment growth for all firms.

Second, I predict that incentives will have a greater employment effect among small firms rather than large firms based on the credit constraint narrative.

3.5 Data

In this paper, I make use of four data sets: Panel Database of Incentives and Taxes (PDIT), the County Business Patterns (CBP), the Statistics of U.S. Businesses (SUSB), and the Quarterly Workforce Indicators (QWI).

Most studies rely on C2ER or Good Jobs First subsidy tracker data, both of which limit what a researcher can do. Some, like Calcagno and Thompson (2004), use the National Association of State Development Officers (NASDA) annual report, but even NASDA themselves admitted their data was messy and incomplete. Finally, some have done the arduous work of collecting data by hand to overcome existing limitations. For example, Slattery (2019) and Slattery and Zidar (2020) use expenditure-based and narrative-based measures for state business incentives. Bartik's (2017) data uses a rule-based measure offered in a state and predicts the incentive level, given estimated activity.

What type of data is used to studying business incentives is of critical importance. Wasylenko (1997) claims that differences in empirical findings are often not due to the type of data used (aggregate or microdata), variables included, or how taxes/credits are measured, but to narrow time periods or industries studied. In other words, many studies look at disparate pieces of the puzzle, and comparing across different empirical findings is not an apples-to-apples comparison.

I use Bartik's (2017) database that measures total incentives and also can be decomposed by different types of incentives. Oftentimes, state policymakers use a combination of economic development incentives to make the package more appealing. In fact, there is a great deal of variation in how states offer business incentives, even across adjacent states. For example, New Mexico offers as much as four times incentives compared to Arizona. What incentives incentivize is a function of all other incentives and thus studying one incentive in isolation can bias estimates. Also, when studies focus on narrow incentives, types, and time span, there is always a concern of generalizability.

3.5.1 Panel Database of Incentives and Taxes

The Panel Database on Incentives and Taxes (PDIT) measures state-provided development incentives for 45 industries across 32 states and the District of Columbia over 26 years (1990-2015) in 45 industries that compose more than 90 percent of U.S labor compensation (Bartik 2017). This constitutes the most comprehensive database on incentives and taxes to date, including all four major types of incentives: customized job training subsidies (CJTS), investment tax credits (ITCs), job creation tax credits (JCTCs), and research and development tax credits (RDTC). The database is uniquely designed whereby each incentive can be turned on or off and also adds up to the total. Following a rule-based simulation, the focus was to estimate non-discretionary incentives received by a typical medium to medium-large export-base firms in a state s, industry i, and time t. Hence, firm-specific discretionary incentives (e.g., megadeals) are excluded from the database.

3.5.2 County Business Patterns

The County Business Patterns (CBP) database provides employment, annual payroll and establishment information for 1990-2015 with four-digit Standard Industrial Classification (SIC) for 1990-1997 and North American Industry Classification System (NAICS) for 1998-2015. I restrict my analysis to NAICS data, so from 1998-2015.

3.5.3 The Statistics of U.S. Businesses

The Statistics of U.S. Businesses (SUSB) program is an annual series providing national, state, metropolitan statistical area and county-level data by enterprise size and industry. SUSB tables (Tabulated data by geographic area, industry, and enterprise size) date back to 1989. But SUSB datasets (Tabulated datasets in comma-delimited text format that allow researchers to analyze data with greater flexibility) are available only from 1997.

3.5.4 Quarterly Workforce Indicators

Quarterly Workforce Indicators (QWI) is the first dataset to provide detailed publicly available county-level information on labor market variables for firms in narrowly defined size and age categories. Firm age and size is obtained from the Business Dynamics Statistics (BDS), which provides aggregated data based on the Longitudinal Business Database (LBD) of the U.S. Census Bureau. Only private sector firms are included in the BDS.

3.5.5 Descriptive Statistics

Merging the PDIT with three public datasets has a number of challenges. The most immediate challenge is with the industry classification. I restrict the sample to 1998-2015 where industry classification is NAICS; pre-1998 classification is in SIC. The CBP data can be harmonized using the NBER SIC-NAICS crosswalk. The QWI reports industry using NAICS codes for all industries, 2-digit sectors, 3-digit sectors, and 4-digit sectors. QWI conveniently has already harmonized SIC-NAICS. SUSB Tables are go back to 1988, but SUSB Annual Datasets by Establishment Industry is available only for 1998 to 2015 (The earliest dataset available is from 1997 based on SIC codes). So, SUSB is the lowest common denominator, forcing the study to be restricted to 1998-2015.

The employment size categories vary across datasets as well. QWI provides local labor market statistics across employment size categories. Firm age and size (only private sector) is obtained from BDS, which is a product of LBD. Firm size categories are 0-19, 20-49, 50-249, 250-499, 500+ employees. Firm age categories are total, 0-1, 2-3, 4-5 6-10, 11+ years. SUSB data provides employment information across enterprise size categories but these bins vary across time periods. The most representative category across time periods is: total, 0-4, 5-9, 10-19, <20, 20-99, 100-499, <500, 500+. The CBP provides employment information across the following establishment size categories: total, 1-4, 5-9, 10-19, 20-49, 50-99, 100-249, 250-499, 500-999, 1000+. Harmonizing across these three datasets, it is possible to have the following size classes: <20, 20-99, 100-499, and 500+. However, note that SUSB provides enterprise categories, CBP provides establishment categories, and QWI provides firm size categories. On one hand, for state officials, the question of job creation across firm size may be less important. After all, a job created by a MacDonald's establishment or locally-owned startup matters little. But on the other hand, to effectively target incentives, it is important to know who creates jobs. I choose to decompose employment in the simplest way: <500 and 500+.

As for incentives, I narrow down the analysis to four major types of incentives: JCTC, ITC, RDTC, and CJTS. Given that my analysis at the state-level, I choose to omit PTA, which is determined at the city-level.

Table 9. Descriptive Statistics on all business incentives

Panel A: "All Sample," 1998-2015

Variable	N	Mean	Median	St. Dev
Total incentives	26,730	1.1635	0.5840	1.4855
Job creation tax credit	26,730	0.3350	0.0000	0.6097
Investment tax credit	26,730	0.2366	0.0000	0.5388
R&D credit	26,730	0.0588	0.0010	0.1674
Customized job training subsidy	26,730	0.0883	0.0190	0.1766

Panel B: "Only Treated Sample," 1998-2015

Variable	N	Mean	Median	St. Dev
Total incentives	24,173	1.2866	0.7410	1.5105
Job creation tax credit	11,516	0.7775	0.5620	0.7203
Investment tax credit	9,122	1.3038	0.9620	1.1659
R&D credit	9,124	0.6931	0.4380	0.7309
Customized job training subsidy	14,167	0.1109	0.0170	0.2170

Source: PDIT, 1998-2015.
Notes: The sample consists of 33 states in 45 industries across 26 years. Incentives are calculated as percent of present value of value-added. Values are expressed in percentage terms.

As shown in Table 9, there is much variation across four types of incentives in 33 states and over 26 years. And at the aggregate level, all four types of incentives are negatively correlated to log of employment. But it is because there is a lot of heterogeneity across states, across industries, and across time.

3.6 Empirical Strategy

The primary research question pertains to the impact of state business incentives on employment growth of small vs. large vs. young businesses. There are several limitations in conducting this research. My contribution to the literature is by examining both firm size and firm age, where I am able to compare small vs. large firms and young vs. mature firms. I categorize young businesses as startups and spinoffs. And, specifically, I study employment growth across firm size and firm age.

3.5.1 Empirical model

I use a three-way interacted fixed effect model, which is the most demanding research design framework given my aggregate data. A series of interacted fixed effect regression include state-by-year (δ_{st}), industry-by-year (τ_{it}) and industry-by-state (γ_{is}). State-by-year fixed effects control for unobserved, time-varying differences across states; industry-by-year fixed effects control for unobserved, time-varying differences across industries; and industry-by-state fixed effects control for unobserved, time-invariant characteristics of state industries (See Aghion et al. 2008). The state-industry fixed effects control for unobserved, time-varying differences across state-industries. The inclusion of these interaction fixed effects ensures that the estimates are robust to many types of unobservable omitted variables that otherwise could confound this analysis. I test for the sensitivity of the estimate across various models and consider which might be more precise. This demanding specification makes estimating nation-wide employment effects using aggregate data possible and is a particular advantage of my data that compensates for the disadvantages of not being to observe treatment at the individual establishment or firm-level. Industries are composed of mostly 3-digit NAICS. The l_{ist} is the error term with adjusted standard errors clustered around three-digit state industries (see Bertrand, Mullainathan and Dufflo 2004).

State-fixed effects, industry fixed-effects, and year fixed-effects are nested in the model. State fixed-effects control for unobserved, time-invariant state-specific characteristics. For example, the state of New York is substantially larger in population, labor force, and economic output than the state of Alabama. Hence, it is important to control for time-invariant state-specific characteristics. Industry fixed-effects control for unobserved, time-invariant industry-specific characteristics. For example, retail trade industry is very different from legal services. Year fixed-effects control for unobserved, time-varying characteristics, such as the Great Recession between 2007 and 2010. Specifically I estimate the following equation:

Equation 20

$$\sum_n \mathrm{Ln}(Employment)_{ist} = \beta_0 + \beta_1(SBI_{ist}) + \delta_{st} + \tau_{it} + \gamma_{is} + \varepsilon_{ist}$$

$Employment_{ist}$ is the log of employment in industry i in state s in time period t for each enterprise size n. The employment variable is from CBP and SUSB. The difference is that when using the SUSB-PDIT data, it is a firm-based measure of employment and when using the CBP-PDIT merged data, it is an establishment-based measure of employment. The variable of interest is SBI, an indicator that equals 1 if state s in industry i has adopted a state business incentive by year t; I examine each of four state business incentives (e.g., job creation tax credit; investment tax credit; customized job training subsidy; and, research and development credit.) separately. In an alternative specification, SBI, equals a ratio of present value of net taxes after the specific state business incentive, to present value of value-added, for a state s in industry i in year t.

To account for the differential policy effect across firm size and firm age distribution, I estimate the regression equation separately for given categories. In the SUSB and CBP data, the regression equation is estimated separately for the firm size categories: 0-19, 20-99, 100-499, and 500+. Additionally, I also examine <500 and 500+ size categories. In the QWI data, the regression equation is estimated separately for the firm age categories: 0-5, 6-10, and 11+.

I also estimate the same equation but while including lagged dependent variable as controls. The estimation equation looks like this:

Equation 21

$$\mathrm{Ln}(Employment)_{ist} = \beta_0 + \beta_1(SBI_{ist}) + \beta_2 Ln(Employment)_{ist-1} + \delta_{st} + \tau_{it} + \gamma_{is} + \varepsilon_{ist}$$

The added control reduces the sample size since t-1 observations are removed. But adding this control is important because it accounts for the adjustment costs. The literature remains unclear as to what might be the precise lag between the time the policy is introduced and when the firms respond. This adjustment could occur as soon as within months or within one or three-year span. While I am unable to account for monthly change that Neumark and Grijalva (2017) implement using the Quarterly Census Employment and Wage (QCEW), I am able to account for a year lags.

3.5.2 Research limitations

There are clear disadvantages and limitations to using aggregate data rather than firm-level data in empirical research. Aggregate data are not subject to sampling error (so this is an advantage) but subject to non-sampling errors. There are several sources to this: "inability to identify all cases that should be in the universe; definition and classification difficulties; errors in recording or coding the data obtained; and other errors of coverage, processing, and estimation for missing or misreported data."[20] Of particular concern are the missing data resulting from data suppression, which refers to various methods or restrictions applied to data by the U.S. Census Bureau to protect the confidentiality of respondent data. In SUSB, for example, employment is either missing or reported as zero, when quarterly payroll is greater than zero, for about 6.5 percent of administrative records.

I use both CBP and SUSB datasets to the effect of incentives across firm size. It is an important nuance that I can capture by comparing these two datasets. CBP provides size categories as follows: 1-4, 5-9, 20-49, 50-99. 100-249, 250-499, 500-999, 1000+. Since the County Business Patterns data includes only employer businesses, non-employer self-employed are outside the scope of my study. CBP does not differentiate between a newly opened small establishment of a large firm and a new firm formation. From a local policy maker's perspective if job creation and economic development is the end goal, both of these new openings can generate jobs and some economic development. However, using SUSB data, I can study exactly whether state business incentives induce new firm formation. I harmonize these two datasets across the following size categories: 0-4, 5-9,

[20] https://www.census.gov/programs-surveys/susb/technical-documentation/methodology.html

10-19, 20-99, 100-499, and 500+. In practice, state business incentives are not targeting businesses that hire less than 10 employees. The high-growth firms are 20-99 and 100-499 categories. I focus my analysis on these categories. Alternatively, I compare the analysis results of the aggregate data and with firm size/establish size categories between <500 and 500+.[21]

3.6 Regression Results

I compare and contrast the impact of four types of state business incentives across firm size, firm age, and establishment size categories. I merge PDIT with CBP, SUSB, and QWI. For compatibility purposes, I narrow the panel to 1998-2015, which circumvents the complexities involved with NAICS-SIC harmonization.

Tables 10 and 11 display results. Each cell in these tables represents a separate three-way fixed effect regression. I do not run regressions including all four incentives at the same time. I also choose not to run a regression with total incentives (summation of all four incentives), which result in an uneven distribution of treatment versus control groups; this problem occurs because most states adopt some form of incentive. The best application of total incentives may be to study the magnitude rather than the effect; hence, restricting the sample to just the treatment group and examining the magnitude of the effect across state-industries and time. Using different sets of data, I run four sets of regressions: (i) estimated effects of state business incentives expressed in a dummy on employment. (ii)

[21] The Small Business Administration tends to use 500 or more employees as a large firm mark, but much of the rest of the world uses 250 employees.

estimated effect of state business incentives expressed as percent of present value of value-added on employment. (iii) estimated effect of state business incentives expressed in a dummy on employment, controlling for adjustment costs (I include a lagged dependent variable as a control). (iv) estimated effect of state business incentives expressed as percent of present value of value-added on employment, controlling for adjustment costs (I include a lagged dependent variable as a control).

Tables 10 draw from QWI data and examine the effect of business incentives on employment growth across different firm age categories. Kauffman Foundation defines firms in the 0-5 age category as startups.[22] Haltiwanger, Jarmin, and Miranda (2013) treat firms in the 0 age category as startups and firms in the age category of less than 10 years old as “Young” or more than 10 years as “Mature.” Following this typology, age category in 6-10 would be considered “Young” but not as startups; mature firms as age category of 11+. For startups, only CJTS is statistically significant and estimates display a positive effect. For young firms (now including age 6-10), ITC is statistically significant and displays a positive effect. When it comes to mature firms, CJTS is the only incentive that displays a statistically significant and positive effect; all others are imprecisely estimated, suggesting that they do not spur employment growth.

[22] https://www.linkedin.com/pulse/why-does-america-favor-big-old-businesses-over-small-new-victor-hwang/

Table 10. Estimated Effects of State Business Incentives (Dummy) on Employment (w/ Adjustment Cost) by Firm Age, 1998-2015, 33 States

	Firm Age			
	(1)	(2)	(3)	(4)
VARIABLES	Total	0-5	6-10	11+
JCTC Dummy	8.23e-06	-0.0123	-0.00638	0.00291
	(0.00284)	(0.0154)	(0.0134)	(0.00353)
ITC Dummy	0.00382	0.0265	0.0394**	-0.00397
	(0.00427)	(0.0227)	(0.0180)	(0.00510)
CJTS Dummy	0.0144*	0.0834**	0.000757	0.0181*
	(0.00766)	(0.0383)	(0.0300)	(0.00937)
RDTC Dummy	0.00444	-0.00998	-0.00851	0.0120
	(0.00778)	(0.0218)	(0.0191)	(0.00928)
Observations	23,397	22,692	22,666	23,372
R-squared	0.998	0.968	0.972	0.997
State FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State*Industry	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes
Industry*Year	Yes	Yes	Yes	Yes

Source: QWI, 1998-2015

Note: Each cell represents a separate regression, totaling 20 separate regressions.

In Table 11, I share the estimated effects of state business incentives on employment by firm/establishment/enterprise size using three different datasets. Understanding the nuances of what each dataset is designed to measure and the way data is categorized is important. The County Business Patterns (CBP) program and the Statistics of U.S. Businesses (SUSB) program tabulate the same data in different ways. The CBP program tabulates establishment-level data by state and industry by the employment size at the individual establishment (physical location), while the SUSB program tabulates establishment-level data by state and industry by the employment size of the enterprise that owns the establishment(s). In both programs, the state and industry are based on the location of the establishment. However, in the SUSB tables the size category is always based on the size of the enterprise that owns the establishment(s), not the size of the establishment itself. QWI integrated firm age and size data from the BDS. Hence, it is most accurate when I am interested in comparing across firm size distribution.

CJTC and ITC are statistically significant and positive across all three data sets for establishments (SUSB and CBP) or firms that would be considered small firms (with employment size category of <500) as well as large firms (employment size category of 500+).

Table 11. Estimated Effects of State Business Incentives (Dummy) on Employment (w/ Adjustment Cost) by Firm Size, 1998-2015, 33 States

	SUSB			CBP			QWI		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
VARIABLES	<500	500+	Total	<500	500+	Total	<500	500+	Total
JCTC Dummy	0.00599	-0.0124	-0.00433	-0.0155	-0.0165	-0.0125**	0.00659	0.0123	7.62e-05
	(0.00481)	(0.00790)	(0.00597)	(0.0107)	(0.0109)	(0.00629)	(0.00517)	(0.00836)	(0.00284)
ITC Dummy	0.0119*	0.0107	0.0146**	0.0215	-0.00665	0.0126*	0.0223***	0.00432	0.00384
	(0.00692)	(0.00832)	(0.00644)	(0.0148)	(0.0166)	(0.00666)	(0.00831)	(0.00994)	(0.00427)
CJTS Dummy	0.0253*	0.0348*	0.0169	0.0417	0.0183	0.0230**	0.0357***	0.000274	0.0146*
	(0.0144)	(0.0189)	(0.0106)	(0.0278)	(0.0249)	(0.0108)	(0.0109)	(0.0195)	(0.00763)
RDTC Dummy	-0.00317	-0.00529	-0.00816	0.00146	0.00789	-0.000809	-0.0176*	0.000881	0.00415
	(0.00614)	(0.0202)	(0.00716)	(0.0162)	(0.0175)	(0.00939)	(0.00929)	(0.0116)	(0.00777)
Observations	21,624	19,965	22,689	24,452	19,631	24,416	23,330	22,993	23,402
R-squared	0.996	0.989	0.996	0.983	0.988	0.993	0.995	0.992	0.998
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Industry	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: CBP, QWI, SUSB, 1998-2015

Note: Each cell represents a separate regression, totaling 45 separate regressions.

3.7 Conclusion

The purpose of this chapter has been to study the effect of incentives on employment growth across firm size and firm age categories. Merging the panel database on incentives and taxes with County Business Patterns (CBP), the Statistics of U.S. Businesses (SUSB), and Quarterly Workforce Indicators (QWI), I found that ITC appears to increase employment among young firms (Age <10), while CJTS appears to increase employment among both young and mature firms (Age 11+). I also find most consistent evidence to suggest that CJTS and ITC spur employment growth among small businesses (<500 Employees). The key takeaway of the study is that incentives targeting young and small businesses has a persistent positive employment effect, while for incentives targeting mature and large businesses there is little supporting evidence.

CHAPTER 4. WHICH STATE BUSINESS INCENTIVE TO IMPLEMENT AND WHERE?

4.1 Abstract

Startups are important vehicles of job creation and productivity growth (Haltiwanger, Jarmin, and Miranda 2013). The most common economic development policies of state and local governments are taxation and state business incentives. A substantial literature on taxation explores its impact on entrepreneurship, but there is a dearth of literature when it comes to state business incentives. I use a novel database on four major types of state-level incentives (e.g., Job Creation Tax Credit; R&D Tax Credit; Investment Tax Credit; and, Customized Job Training Subsidy) to study the implications of state business incentives on business dynamics (e.g., establishment birth, death, expansion, and contraction) in 33 states, 7 industries over 17 years. Overall, I find little evidence in support of incentives' primary objective (expansion) or secondary objective (birth). The most striking finding is the persistently negative effect of Investment Tax Credits on both expansion and birth. These findings suggest that there may be "disincentives" to some incentives that distort markets and dampen local productivity growth.

4.2 Introduction

This chapter examines the impact of state business incentives on startups at the state-level.[23] The policy effect on entrepreneurship is rather arbitrary because most incentives target large firms. State and local policymakers believe that targeting expansion and relocation of large firms is the best use of public resources: higher productivity, more prosperity, and higher tax revenues from other firms and from higher incomes (Garcia-Mila and McGuire 2002; Henderson, 2003; Greenstone and Moretti 2003; Greenstone, Hornbeck and Moretti 2010). But is there a case to be made for broad-based incentives that target startups? When considering ample evidence that entrepreneurship is a major vehicle of job creation and facilitator of technological innovation that lead to productivity growth, the question merits further investigation (Haltiwanger, Jarmin, and Miranda 2013).[24]

At the heart of this debate is the question of whether incentives enhance or deter the allocative efficiency of capital, and of course who to target to achieve optimal outcome. Theory and evidence suggest that a substantial fraction of aggregate productivity growth is accounted for by the reallocation of capital from lower-productivity to higher-productivity firms, which is largely driven by firm entry and firm exit (Syverson 2011; Bartelsman, Haltiwanger and Scarpetta 2013).[25] The existing literature finds that incentives targeting large firms are inefficient, tipping less than 25 percent of expansion and relocation

[23] A substantial literature in taxation explores the question of entrepreneurship (e.g., See William and Hubbard 2000; Cullen and Gordon 2007; Bruce and Deskins 2012)

[24] Business startups account for about 20 percent of US gross (total) job creation while high-growth businesses (which are disproportionately young) account for almost 50 percent of gross job creation.

[25] Low-productivity young firms contract and exit, while high-productivity young firms enter and expand, contributing to innovative activities that further enhance industry productivity.

decisions (Bartik 2019). Less understood are the implications of state business incentives on entrepreneurship. Whether state business incentives can have positive impact on startups therein lies the primary focus of this chapter. The extant literature is sparse and findings mixed. Some studies suggest that certain types of incentives encouraging research and development activities have a positive effect on entrepreneurial activity (Fazio, Guzman, and Stern 2019), while other studies find a crowd-out effect of startups (e.g, higher barriers to entry) and small businesses (e.g., displacement of substitutable businesses) from incentives that favor large firms (Partridge et al. 2019; Tuszynski and Stansel 2018). Still other studies, like Acemoglu et al. (2018), suggest that successfully targeting productive firms with incentives is too challenging, risks of introducing market distortions too large, and favor taxing the continued operations of the incumbents in which case taxes disproportionately fall on less productive firms with higher exit margins.

To explore these issues, I focus on examining the impact of four types of state business incentives and address three related research questions: (i) What is the differential effect of state business incentives on firm expansion and contraction? (ii) What is the differential effect of state business incentives on firm birth and firm death? (iii) Conditional on the provision of state business incentives, is the policy effect accompanied by productivity growth measured in terms of employment and earnings?

This chapter's contribution to the literature is three-fold. First, I take advantage of the most comprehensive data on incentives at the state-level. I construct a nationally-representative sample to conduct policy evaluation. Second, I apply three-way interacted fixed effect regression model, which is the most demanding and yet cleanest way to examine the policy using aggregated data. Thirdly, I propose to study the primary effect (establishment expansion and contraction) as well secondary effects (establishment birth and death) of incentives by examining all firm dynamics.

Consistent with the literature, most incentives appear to have no material impact on establishment birth and establishment expansion. The most striking finding pertains to Investment Tax Credit, which is associated with both lower firm birth and firm expansion. I find that its policy effect is accompanied by, on average, a decline in earnings per workers. These findings cast doubt to state's ability to successfully target incentives to increase allocative efficiency. Also, it merits a deeper investigation on Investment Tax Credit in particular and why it is associated with such a negative effect.

4.3 Data

In this paper, I merge two data sets: Panel Database of Incentives and Taxes (PDIT), and the Statistics of U.S. Businesses (SUSB). I use SUSB data and PDIT data at the state-level.

4.3.1 The Statistics of U.S. Businesses

The U.S. Statistics of Businesses, prepared and managed by the U.S. Census Bureau, is the only public source of annual, complete, and consistent enterprise-level data for U.S. businesses, with industry detail. Drawing on data from the Business Registrar, SUSB program tabulates establishment-level data by county, metropolitan statistical area, or state and industry by the employment size of the enterprise that owns the establishment(s). The data is largely subdivided into SUSB Tables and SUSB Datasets. SUSB Tables are tabulated by geographic area, industry, and enterprise size. SUSB Datasets permit researchers to cross-tabulate data with greater latitude, but data is more carefully suppressed.

In accordance with U.S. Code, Title 13, Section 9, no data is published that would disclose the operations of an individual employer. Hence, information is available selectively across SUSB Tables and SUSB Datasets in compliance with data disclosure rules. Some information, such as establishment birth, establishment death, establishment expansion, and establishment contraction are available in the Employment Change Datasets from 2007-2008. In particular, establishment birth and establishment death data is available across the firm size distribution (total, 1-4 employees, 5-9 employees, 10-19 employees, < 20 employees, 20-99 employees, 100-499 employees, < 500 employees, 500 + employees).[26] In the Employment Change Tables, the panel is longer starting from 1996-1997 (NAICS format data is available only from 1998-1999; prior to this, it is Standard Industrial Classification). However, the tables provide aggregated data without employment size information.

There are four major establishment measures that this chapter will examine: establishment expansion, contraction, birth, and death. Employment growth, which was the focus in Chapter 2 and 3 translates into a measure of "establishment expansion" in Chapter 4. These are the official definitions for SUSB:

[26] SUSB provides information on all U.S. business establishments with paid employees. For 2008-2009 and 2009-2010 employment change datasets, the category is defined as 0-4 employees instead of 1-4 employees. An establishment with 0 employment is an establishment with no paid employees in the mid-March pay period but with paid employees at some time during the year.

- *Establishment expansion* is a measure of "establishments that have positive first quarter employment in both the initial and subsequent years and increase employment during the time period between the first quarter of the initial year and the first quarter of the subsequent year."
- *Establishment contraction* is a measure of "establishments that have positive first quarter employment in both the initial and subsequent years and decrease employment during the time period between the first quarter of the initial year and the first quarter of the subsequent year."
- *Establishment birth* is a measure of "establishments that have zero employment in the first quarter of the initial year and positive employment in the first quarter of the subsequent year." This measure will be an indicator of our interest, entrepreneurship.
- *Establishment death* is a measure of "establishments that have positive employment in the first quarter of the initial year and zero employment in the first quarter of the subsequent year." Establishment death includes both incumbent and startup deaths.

4.3.2 Panel Database of Incentives and Taxes

Bartik's database is constructed using a rules-based method and is a simulation of what a typical firm in a given state, industry, and year would be able to claim. 33 states, 45 industries, and 26 years together comprise more than 90 percent of U.S. private employment, wages, and compensation; 45 industries conduct over 97 percent of R&D, mostly in the manufacturing sector. However, due to data availability issues, my sample becomes more restricted. In particular, this chapter's focus is on entrepreneurship. My measure of entrepreneurship in the chapter is establishment birth.

4.3.3 Merging two Databases

Merging the PDIT with the Statistics of U.S. Businesses (SUSB), I conduct a state and city-level analysis. 33 states, 17 sets of annual changes (1998-2015), and 7 industries (2-digit NAICS sectors) are included in the sample (See Table 12).

I choose not to involve with SIC sectors available in Employment Change Data Tables from 1996-1997. Information on establishment birth, death, expansion, and contraction for states is available only at the 2-digit NAICS sector. Given that most of 45 industries included in Bartik's database are 3-digit NAICS codes, I am forced to omit them. The seven industries included Management of companies (holding companies) (NAICS=55); Educational Services (NAICS=61); other services (NAICS=81); Wholesale Trade (NAICS=42); Construction (NAICS=23); Miscellaneous professional, scientific, and technical services (NAICS=54); Retail Trade (NAICS=44-45). Bartik (2017) separates 5411 and 5415 from NAICS 54, but due to data limitations, this analysis treats them as one sector. Two of the seven industries (NAICS=54 and NAICS=55) are export-base sectors, meaning that they generate the most positive externalities for the local economy (e.g., higher wage, higher productivity, higher job multipliers). In fact, according to the Bureau of Labor Statistics, Management of companies (holding companies) (NAICS=55) is considered a high-tech sector.[27] But none of the seven sectors belong to the manufacturing sector, which by definition are all export-base industries and on average have more generous incentives.

[27] https://www.bls.gov/opub/btn/volume-7/high-tech-industries-an-analysis-of-employment-wages-and-output.htm?view_full

Table 12. States, and Industries Included

Panel A: States Included (33 States)

New Mexico	Texas	Wisconsin
Georgia	Colorado	Minnesota
Maryland	Iowa	Louisiana
Alabama	Michigan	New Jersey
Massachusetts	Indiana	Nebraska
Connecticut	Missouri	Pennsylvania
New York	Nevada	Arizona
North Carolina	California	Oregon
Illinois	Kentucky	Washington
Ohio	Tennessee	Virginia
South Carolina	Florida	District of Columbia

Panel B: Industries Included

Industry	**NAICS**
Construction	23
Wholesale Trade	42
Management of companies (holding companies)	55
Educational Services	61
Other services	81
Retail Trade	44-45
Miscellaneous professional, scientific, and technical services	54

Source: Panel Database of Incentives and Taxes
Note: A total of 46 cities and 33 states are included in the analysis. NAICS Classification is based on 2007. Only 7 two-digit naics sector that could be matched to the U.S. Statistics of Business annual change tables were included. Only two of the seven are export-base sectors, neither in manufacturing. Bartik (2017) separates 5411 and 5415 from NAICS 54, but due to data limitations, this analysis treats them as one sector.

4.3.4 Descriptive Statistics

Incentives have remained constant for most but JCTC has accounted for most growth. Panel database is fairly well-balanced between treatment and control groups. JCTC grows to become the largest incentive, and CJTS and RDTC are the smallest. Table 13 provides basic descriptive statistics.

Table 13. Descriptive Statistics on all business incentives

Panel A: "All Sample," 1998-2015

Variable	N	Mean	Median	St. Dev
Total incentives	4,158	0.5502	0.0850	1.0058
Job creation tax credit	4,158	0.1846	0.0000	0.5316
Investment tax credit	4,158	0.0924	0.0000	0.3876
R&D credit	4,158	0.0042	0.0010	0.0127
Customized job training subsidy	4,158	0.0545	0.0000	0.1356

Panel B: "Only Treated Sample," 1998-2015

Variable	N	Mean	Median	St. Dev
Total incentives	3,300	0.6933	0.1610	1.0842
Job creation tax credit	881	0.8714	0.5730	0.8578
Investment tax credit	618	0.6215	0.1620	0.8262
R&D credit	2,121	0.0082	0.0020	0.0168
Customized job training subsidy	822	1.0849	0.7210	1.0290

Source: PDIT, 1998-2015.
Notes: The sample consists of 33 states in 7 industries across 18 years. Incentives are calculated as percent of present value of value-added. Values are expressed in percentage terms.

4.4 Analysis and Methodology Issues

4.4.1 Hypotheses

The policy evaluation is conducted by probing the following three questions:

(i) What is the differential effect of four state business incentives on firm expansion and contraction?

(ii) What is the differential effect of four state business incentives on firm birth and firm death?

(iii) Conditional on the effectiveness of state business incentives, is the policy effect accompanied growth employment and earnings per worker?

One way to view state business incentives is as policy intervention to spur productivity growth. Then, the effectiveness of incentives depends on how effectively incentives can target (i) birth of firms with high-growth potential; (ii) expansion of more productive firms (measured as employment and earnings growth); (iii) death of firms with lower productivity; (iv) contraction of firms with lower productivity. If incentives lead to higher levels of productivity, whether through increased firm birth or firm expansion or decreased firm death or firm expansion, it would be a good economic development policy. Alternatively, if incentives lead to either no changes or lower levels of productivity, it would suggest that policy is ineffective.

Of particular interest are the policy effects on firm expansion and firm birth. Policies targeting firm death or firm contraction would be politically unpopular and create risks of political cronyism.[28] Therefore, the only tangible pathways in which incentives enhance the reallocation dynamics is by influencing (i) firm birth and (ii) firm expansion.

Influencing firm expansion is the principal objective among local and policy makers. Incentives targeting firm expansion would increase productivity since large firms are endowed with abundant factors of production, scale efficiencies and best managers (Lucas 1978). If large firms generate sufficiently large positive externalities to the local economy, targeting their expansion and relocation decisions with incentives would be the most efficient use of public resources (Bartik 1991, Glaeser 2001, Garcia-Mila and McGuire 2002, Greenstone and Moretti 2003; Greenstone, Hornbeck and Moretti 2010).

The existing evidence suggests that incentives to large firms are costly and inefficient. On average, only two percent of a state's employers have more than 100 employees but they receive between 80 and 90 percent of all incentive dollars (LeRoy et al. 2015). Although a lionshare of incentives go to large firms, they tip less than 25 percent of relocation and expansion decisions (For review, see Bartik 2018).[29] Less explored is a possible secondary impact of incentives on startups. Incentives targeting firm birth could increase productivity since entrepreneurship is a major vehicle of job creation and facilitator of technological innovation that lead to productivity growth (Haltiwanger, Jarmin, and Miranda 2013).[30]

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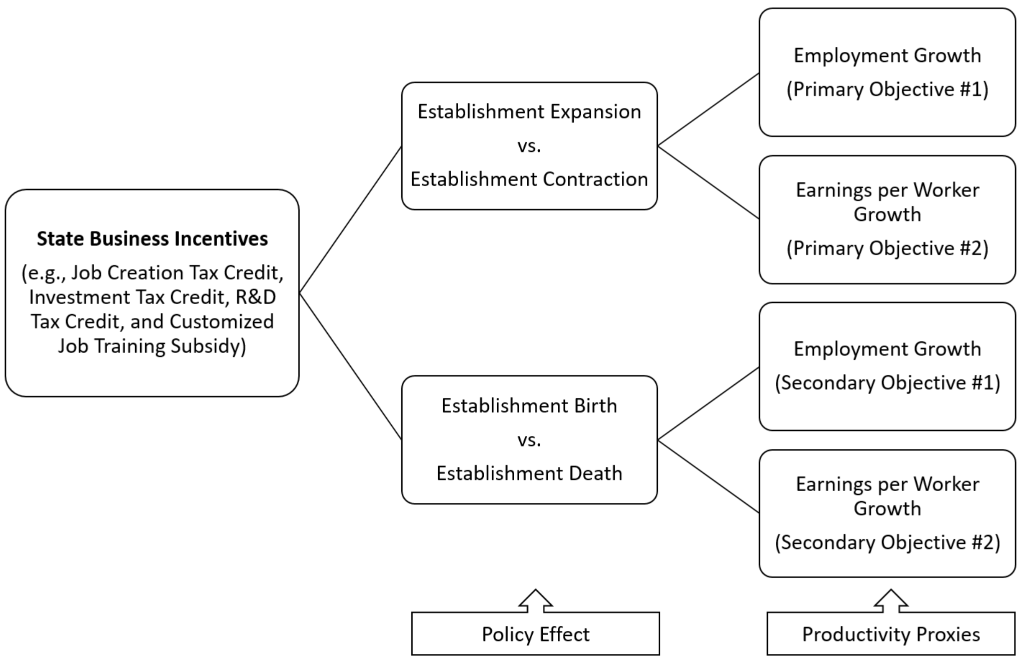
[28] It is possible that some trailing states implement incentives to "slow the bleeding," which would suggest that the policy targets reduction of firm contraction and firm death. But there is no empirical study that confirms this and such objectives would disqualify incentives as "economic development" policies.
[29] Evidence of relocation in response to tax incentives is not non-existent. Moretti and Wilson (2017) find cross-state relocation within the U.S.
[30] Business startups account for about 20 percent of US gross (total) job creation while high-growth businesses (which are disproportionately young) account for almost 50 percent of gross job creation.

In the absence of a clear theory, there are three possible relationships between incentives and entrepreneurship. First, considering that there are no broad-based state business incentive that specifically target startups, there should be no significant relationship. Second, if incentives favoring large firms over startups potentially increase barriers to entry or lead to a crowd-out effects, the relationship would be negative. Conversely, if incentives provision significantly reduces capital constraint for startups, the relationship could be positive (Criscuolo et al. 2019).

I use two proxies for productivity: employment growth and earnings per worker. These are measured using SUSB data at the state-industry level (33 states and 7 industries) and vary across time. Presumably, firms that hire workers are the ones that are on the high-growth trajectory. Also, earnings of workers is a good proxy for their labor productivity. Usually these two proxies would move in the same direction but they need not do so necessarily. A productive firm could partake in labor-capital substitution by investing heavily in capital, for example. In this case, firm's productivity would be masked in the employment growth measure but more likely show up in the earnings per worker measure. Usually firms that make large investments in latest capital demand more high-skilled workforce.

Figure 5 depicts the policy evaluation that I propose to conduct in this paper. If state business incentives induce change in economic behavior of firms, it would result in changes to establishment expansion or contraction as well as establishment birth or death. By definition, establishment expansion should lead to employment growth (primary objective of the incentive). It could be accompanied by earnings growth or not, though from a policymaker's standpoint it would be desirable. Presumably, earnings growth could follow establishment expansion or establishment contraction. Establishment contraction of low-skill workers would naturally increase average earnings. But establishment contraction could also be a signal of more efficient operation.



Note: Four types of incentives can have an impact on establishment expansion and contraction of incumbents. They can also have an impact on establishment birth and death of startups and incumbents. Most studies focus only on the policy effect of establishment expansion (usually measured as employment growth). But, there could also be a policy effect on establishment birth. I not only measure the policy effect, I also examine whether these potential effects are accompanied by productivity growth (e.g., measured as employment and earnings growth).

Figure 5: Evaluating Primary & Secondary Objectives of State Business Incentives

4.4.2 Analysis

In equation 22, I use a three-way fixed effect model. While there may be persistence over time in measuring birth, death, expansion, and contraction, there is less of a case to be made for the use of adjustment cost controls with these variables than employment and earnings. So, I choose I estimate the following equation:

Equation 22

$$\mathrm{Ln}(Y)_{ist} = \beta_0 + \beta_1(SBI_{ist}) + \delta_{st} + \tau_{it} + \gamma_{is} + \varepsilon_{ist}$$

Y_{ist} is the log of establishment birth, establishment death, establishment expansion, and establishment contraction in industry i in state s in time period t. The dependent variables are from SUSB. The variable of interest is SBI, an indicator that equals 1 if state s in industry i has adopted a state business incentive by year t; I examine each of four state business incentives (e.g., job creation tax credit; investment tax credit; customized job training subsidy; and, research and development credit) separately.

A series of interacted fixed effect regression are analyzed. Interaction fixed effect regressions include state-by-year (δ_{st}), industry-by-year (τ_{it}) and industry-by-state (γ_{is}). State-by-year fixed effects control for unobserved, time-varying differences across states; industry-by-year fixed effects control for unobserved, time-varying differences across industries; and industry-by-state fixed effects control for unobserved, time-invariant characteristics of state industries (See Aghion et al. 2008). The state-industry fixed effects control for unobserved, time-varying differences across state-industries. The inclusion of these interaction fixed effects ensures that the estimates are robust to many types of unobservable omitted variables that otherwise could confound this analysis.

For employment, which was the dependent variable in the previous two chapters, there is a vast literature discussing slow adjustments, such as the stickiness of wages or labor adjustment costs. Consistent with previous two chapters, I estimate the equation with including lagged dependent variable as controls. The estimation equation looks like this:

Equation 233

$$\mathrm{Ln}(Y)_{ist} = \beta_0 + \beta_1(SBI_{ist}) + \beta_2 Ln(Y)_{ist-1} + \delta_{st} + \tau_{it} + \gamma_{is} + \varepsilon_{ist}$$

The dependent variable Y is either the log of employment or the log of earnings per worker in industry i in state s in time period t. The variable of interest is SBI, an indicator that equals 1 if state s in industry i has adopted a state business incentive by year t; I examine each of four state business incentives (e.g., job creation tax credit; investment tax credit; customized job training subsidy; and, research and development credit) separately. The added lagged dependent variable control reduces the sample size since t-1 observations are removed. But adding this control is important because it accounts for the adjustment costs. Interaction fixed effect regressions include state-by-year (δ_{st}), industry-by-year (τ_{it}) and industry-by-state (γ_{is}). The inclusion of these interaction fixed effects ensures that the estimates are robust to many types of unobservable omitted variables that otherwise could confound this analysis.

4.4.3 Methodology Issues

While the Panel Database of Incentives and Taxes is the most comprehensive resource, its limitations are also non-trivial. The database follows a rule-based method that simulates what a typical firm in a given city, state, industry, and year would receive. The Council for Community and Economic Research (C2ER) has a database of state's specific incentives but does not provide a numeric value of incentives or incentive details which would allow a compare and contrast. Good Jobs First keeps a database of incentives as well as a numeric value, which is based on what is promised rather than what is actually given. PDIT is a clear improvement on alternative sources as it provides estimated value of incentives by type, state, industry, and year, allowing for a comparison but as a simulation, there is no clear way to know an *explicit counterfactual*. It is impossible to know the take-up rate of the policy or what would have had in the absence of the incentive. Even in PDIT, however, results are highly sensitive to which states, industries, and years are included in the model.

Another serious challenge pertains to *selection bias*. There may be something specific about the firms that opt in for incentives than the firms that do not. Given how easy it is to relabel expenditures to claim credits, or exploit job churning to receive credits, firms that are less productive and more inclined to partake in deviant behavior may be crowding in. In my regression specifications, interacted fixed effects (state-by-year, industry-by-year, and industry-by-state) are included because they are more robust than simply state, industry, and year fixed effects.

Incentive decisions are largely determined at the state-level by governors rather than mayors or local officials. An exception is the Property Tax Abatement (PTA), which are determined at the city-level. But perhaps, then it may be possible to conduct a county-level data to capture more geographic variation. However with aggregate data, information becomes more difficult to obtain. For example, county-level data on employment and earnings are obtainable. Establishment birth, death, expansion and contraction are also available. However, the information becomes greatly suppressed, and sectoral information is not made public (at best is 2-digit NAICS at the state-level but at the county-level no industry variation is available for some of the variables of interest).

4.5 Results

4.5.2 Regression Results on Establishment Birth, Death, Expansion, Contraction

Table 14 presents a three-way interacted fixed effect model (state-industry, state-year, and industry-year). State, industry, and year fixed effects are nested. In addition, lagged dependent variable added as a control for adjustment cost. Each cell represents 20 separate regressions. The panel consists of 17 annual changes starting from 1998-1999 and ending in 2014-2015. A total of 33 states and 7 industries are included. Industries are restricted to just 7 two-digit NAICS codes from a total of 45 industries because firm birth, firm death, firm expansion and firm contraction information is available in SUSB only at the two-digit level.

Most incentives (JCTC, CJTS, RDTC) appear to have no statistically significant impact on establishment expansion. The only exception is ITC, which is associated with decreases in establishment expansion by -2.8 percent. When considering that this is the primary objective of incentives, one could interpret these results as an indication that the policy fails to sufficiently alter firm behavior. With establishment birth, again, it appears that most incentives have no statistically significant impact with the exception of ITC, which is negatively associated (-6.97 percent). These findings are somewhat in support of the narrative that large firms obtain subsidies but instead of increasing investment, earnings, and employment, they artificially slow down another channel of efficiency reallocation:

birth. In effect, incentives may be creating a “disincentive” for productive behavior, either directly or indirectly.

Somewhat puzzling is ITC’s negative association with establishment contraction (-3.14 percent). Major policy objectives is not just job creation but also job protection. This may be especially true for lagging states that want to dissuade firms from relocating away or during recessions, when job protection can potentially be as important as job creation to battling rising unemployment rates. So, ITC’s negative association with establishment contraction is suggestive evidence that it may be more effective as a job protection tool than job creation tool.

Another finding pertains to RDTC’s negative association with establishment death (-4.79 percent). One way to interpret this result is that the policy supports innovative activities and provides sufficient liquidity to firms at the margin, thereby significantly increasing their survival rate. But these results need to be interpreted with caution. It is easy to relabel expenditures to claim credits, or exploit job churning to receive credits. For example, R&D tax credits are evaluated based on the expenditures labeled as “research and development,” but a more comprehensive evaluation would jointly look at outcomes such as patenting, productivity, or jobs. For this reason, I examine results in Table 15 concurrently.

Table 14. Estimated Effects of State Business Incentives (Dummy) on Establishment Birth, Death, Expansion and Contraction, 1998-2015, 33 States, 7 Industries

VARIABLES	(1) Birth	(2) Death	(3) Expansion	(4) Contraction
JCTC Dummy	0.00385	0.00989	0.00248	-0.00449
	(0.0217)	(0.0240)	(0.00811)	(0.00864)
ITC Dummy	-0.0697**	-0.00606	-0.0280**	-0.0314**
	(0.0318)	(0.0258)	(0.0135)	(0.0141)
CJTS Dummy	0.0600	0.0533	-0.0176	0.00325
	(0.0472)	(0.0387)	(0.0204)	(0.0176)
RDTC Dummy	-0.0407	-0.0479**	0.00851	0.00127
	(0.0277)	(0.0215)	(0.0129)	(0.0145)
Observations	3,927	3,927	3,927	3,927
R-squared	0.995	0.995	0.999	0.999
State FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State*Industry	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes
Industry*Year	Yes	Yes	Yes	Yes

Source: Statistics of U.S. Businesses

Note: Each cell represents a separate regression, totaling 20 separate regressions. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

4.5.3 Regression Results on Employment and Earnings Effects

Table 15 present my productivity proxy measures. The log of employment and earnings per worker are estimated using three-way interacted fixed effect model (state-industry, state-year, and industry-year) with lagged dependent variable as a control for adjustment cost. The sample is restricted as close to the one in Table 14 to test whether policy effect of four types of incentives on establishment birth, death, expansion, and contraction, is accompanied by employment and earnings per worker measures. In I expect policy impact to be reflected in at least one of the productivity measures. The most direct measure and targeted priority is employment growth. However, it is possible that the policy impact is observed not in employment growth but in earnings per worker effect. In principle, I expect to see establishment expansion or birth to be accompanied by employment growth.

Overall my results show that most incentives do not have a statistically significant effect on spurring increases in employment or earnings per worker. The employment measure should be compared side-by-side with establishment birth and expansion measures in Table 14. JCTC is the only policy with statistically significant association with employment growth (4.29 percent); it appears that the policy effect is observed only among larger firms (500+). Consistent with results in Table 14, its total effect in column (3) is statistically insignificant. A more direct measure of productivity is earnings per worker. Consistent with negative results captured in Table 14, ITC displays a negative association in earnings in column (5) and (6) of Table 15, though weak in magnitude. CJTS also displays a negative association in earnings per worker, though the magnitude is weak.

Table 15. Estimated Effects of State Business Incentives (Dummy) on Employment and Earnings per Worker (w/ Adjustment Cost), 1998-2015, 33 States, 7 industries

	Log(Employment)			Log(Earnings per Worker)		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	<500	500+	Total	<500	500+	Total
JCTC Dummy	0.0106	0.0429**	0.00442	0.000616	-0.00161	0.00137
	(0.0139)	(0.0185)	(0.0113)	(0.00154)	(0.00194)	(0.00102)
ITC Dummy	-0.0146	0.0389	0.0185	-0.000767	-0.00514**	-0.00239**
	(0.0141)	(0.0285)	(0.0131)	(0.00184)	(0.00217)	(0.00104)
CJTS Dummy	0.0147	0.0274	0.0325	-0.00922**	0.000360	-0.00111
	(0.0242)	(0.0433)	(0.0241)	(0.00403)	(0.00392)	(0.00236)
RDTC Dummy	0.00608	-0.00658	-0.00780	0.000553	-0.000575	0.00145
	(0.0120)	(0.0413)	(0.0265)	(0.00165)	(0.00727)	(0.00173)
Observations	3,603	3,585	3,630	3,603	3,585	3,630
R-squared	0.999	0.993	0.998	0.995	0.970	0.994
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State*Industry	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year	Yes	Yes	Yes	Yes	Yes	Yes

Source: Statistics of U.S. Businesses

Note: Each cell represents a separate regression, totaling 30 separate regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

4.6 Discussion

Three recent studies directly study the impact of state business incentives on entrepreneurship. These studies present the most recent and comprehensive analysis on start-ups and state business incentives. My study contributes to this small but growing literature (Tuszynski and Stansel 2018; Partridge et al. 2019; Fazio, Guzman, and Stern 2019). Like this chapter, all three papers make use of PDIT, which attests to the reliability of this database and the lack of alternatives. The common use of PDIT also permits an easier compare and contrast of this study to theirs.

Partridge et al. (2019) and Tuszynski and Stansel (2018) use aggregate data, and Fazio, Guzma, and Stern (2019) uses proprietary microdata on new business registrations. With the exception of Fazio, Guzma, and Stern (2019) that focuses on RDTC, other two studies consider total incentives. I also use aggregate data, SUSB like Partridge et al. (2019). But only Fazio, Guzman, and Stern (2019) differentiate incentives by types; their study focuses only on the RDTC, but my study looks at all four types. Generally, my findings allow me to analyze differential effect of incentives by type which Partridge et al. (2019) and Tuszynski and Stansel (2018) cannot do. More importantly, unlike other studies, I analyze incentives' primary objective (establishment expansion) along with the secondary objective (establishment birth). Overall, my findings that most incentives have no material impact on startups is consistent with the conclusions of Partridge et al. (2019) and Tuszynski and Stansel (2018). But I am able to identify the particularly negative effect of ITC on establishment expansion and birth. Also, at the city-level, my findings that RDTC reduces establishemtn death and contraction are worth thinking more deeply in relation to the findings of Fazio, Guzma, and Stern (2019).

The main strength of my identification strategy using aggregate data is in the simplicity of the model. Partridge et al. (2019) and Tuszynski and Stansel (2018) have to make numerous assumptions (which are often *ad hoc* and open up many disagreements) about what are the relevant confounding variables.[31] Instead, I use the most demanding specification with the three-way fixed effects. This way, I am able to include not only state and year fixed effects but also industry fixed effects.[32] My results are robust to added control of lagged dependent variable. Fazio, Guzman, and Stern (2019) claim that the RDTC impact on entrepreneurship occurs after 5 years. Neumark and Grijalva (2017) claim to find JCTC impact on employment occurring within 8-12 months; Chirinko and Wilson (2016) find JCTC impact on employment after 3 years. Given that the literature is undecided on the actual adjustment cost period, I find that my time controls as well as lagged dependent variable controls address this uncertainty with best possible means without having to lose so many observations.[33]

4.7 Conclusion

Using a new and comprehensive database on incentives, this chapter has examined in depth the impact of four types of state business incentives on establishment expansion (the primary policy objective) and establishment birth (a possible secondary policy consequence). I determine whether policy impact is beneficial based on whether changes are accompanied by employment or earnings gains. Consistent with the literature, most

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[31] Partridge et al. (2019) control for local demand shocks (e.g., Bartik instrument); job flows measure job-to-job flows at the 2-digit NAICS sector); and, employment shares in specific sectors, shares of adult population with only high school diploma, some college, and at least a Bachelor's degree. Tuszynski and Stansel (2018) control for Economic Freedom of North America (EFNA), percentage of the population that is foreign born, median age, population density, percent of the population over 25 with a bachelor's degree, and percent male, and ideology.

[32] Partridge et al. (2019) use first-differencing, instrumental variable approach, ordinary least squares, and negative binomial analysis. Tuszynski and Stansel (2018) run contemporaneous full-panel regressions as well as 1-, 2-, and 3-year lagged panel models; while year fixed effects are included, state fixed effects are not.

33 Tuszynski and Stansel (2018) use three-year moving averages for dependent variable and independent variable, which significantly reduces their sample (26 years x 33 states = 858 observations).

incentives appear to have no material impact on establishment birth and establishment expansion. Perhaps, the most striking finding is the persistently negative effect of Investment Tax Credit on establishment birth and expansion, accompanied by lower earnings per worker, suggesting that the policy introduces market distortions and dampens local productivity growth.

Acemoglu et al. (2018) had suggested that probability of successfully targeting productive firms with incentives is low, while the associated risks of introducing market distortions too high. Given the persistence in firms' productivity, exiting firms tend to experience several years of failing productivity levels before the actual exit (Carreira and Teixeira 2011). Incentives are a form of capital reallocation among firms that often affect firm exit probabilities. Incentives allocated to relatively inefficient firms could potentially increase their survival likelihood or prevent natural death, and thus slow business dynamism (reallocation process of capital from less to most productive firms). In this view, Acemoglu et al. (2018) argue that taxing the continued operations of the incumbents is a preferred policy prescription to state business incentives, because taxes fall disproportionately on less productive firms, which are more likely to be near the exit margin anyway. They remain agnostic on whether to target small firms and startups or large firms.[34]

[34] Although not to the same extent, tax policies can also broadly target certain economic activities, and indirectly be more favorable to large or small businesses. Bartik (1989) finds that property taxes have a strong negative impact on business startups, because they are paid regardless of profit (which startups rarely have in their beginning years). By contrast, a shift from local labor taxes to business taxes will tend to favor start-ups over mature industries, since profits are rare in startups but those firms still typically have to pay their workers (Chatterji, Glaeser, and Kerr 2014).

Given that evidence of government's ability to pick the "right winners" is rare and state capacity to conduct effective program evaluations low, the findings of this study support the claims of Acemoglu et al. (2018) that taxation may be a more suitable policy prescription given that it promotes a much more market-oriented approach and avoids complicating tax laws as do incentives. Given the underinvestment in R&D activities in the private sector due to high uncertainty and difficulties of appropriability, it appears that only incentives that increase knowledge creation, such as RDTC, may have merit based on long-term payoff (Fazio, Guzman, and Stern 2019).

CHAPTER 5. CONCLUSION

This dissertation took a deep dive on state business incentives using the most comprehensive database on incentives and taxes. The second chapter examined the employment and earnings effects of state hiring credit across business cycles. The third chapter investigated the employment effect of state business incentives by enterprise size and age. The fourth chapter explored policy impact on firm dynamics (births, deaths, expansions, and contractions).

The state business incentives are essentially a double-edged sword designed (i) to *fight for* the relocation and expansion of productive firms in the export-base sectors; (ii) to *fight against* unemployment and the flight of productive firms. To this end, incentives aims to tackle all the most challenging goals to economic development: job creation, job protection, reduction of unemployment, and productivity growth.

The effectiveness of targeted economic development policies, such as incentives, depends on the government's ability to successfully *target* productive over unproductive firms, and successfully *incentivize* productive (e.g., job creation, investment) over unproductive firm behavior (e.g., job churning, displacement effect, relabeling expenses). If the government succeeds in targeting, these incentives (e.g., narrow-base tax reductions) are arguably a preferred policy lever over taxation (e.g., broad-base tax reductions) to achieving the desired economic outcomes all at a fraction of the cost. However, if the government fails in targeting, the economy would be better off without these incentives altogether.

In principle, incentives can result in three outcomes: (i) positive effect; (ii) no effect; (iii) negative effect. The presence of wide-spread unproductive firm behavior will result in a *false positive effect* where there is actually no effect. The presence of endogenous selection will result in a *false negative effect* driven by laggard states where there is actually no effect or even positive effect. My dissertation uses demanding specification to account for endogenous selection but there is little that can be done empirically to account for unproductive firm behavior. But assuming the absence of such market distortions, scenario (i) would be a *positive-sum game* and scenario (ii) and (iii) would be a *zero-sum game*.

The general findings of my dissertation are consistent with the findings of the extant literature: most incentives are a *zero-sum game*. In other words, at best, incentives are ineffective at spurring firms to create jobs or increase worker earnings; at worst, they create "disincentives" that go as far as to dampen employment growth. It is also possible that incentives are simply a weak policy tool due to stigmatization of eligible workers, low participation rate among firms, or low ranking in the site selection criteria, for example.

Based on my empirical results, I consistently find that the Job Creation Tax Credit (JCTC) dampens employment growth. The idea that JCTC designed specifically for the purpose of promoting job creation dampens employment growth is puzzling. JCTC accounted for two-thirds of the increase in total incentives between 1990 and 2015, but is found to be ineffective as a job creation policy. The most immediate explanation is the endogenous selection – that, poorer, lagging states offer more generous incentives to stopgap flight of firms and workers or reduce unemployment rates but to no avail. But my research design should have controlled for selection. Moreover, during the sample period, virtually every state had no JCTC in the beginning and then adopted it afterward. Finally, even the most recent studies using microdata and sophisticated econometrics find consistently negative results (See Donegan, Lester, and Lowe 2019; Neumark and Grijalva 2017). The most plausible explanation is that incentives are highly-politicized and plagued by inter-state competition. As a result, the state government's ability to negotiate fair discretionary incentive deals with businesses is often compromised.

Acemoglu et al. (2018) argue that probability of successfully targeting productive firms with incentives is low, while the associated risks of introducing unproductive behavior too high. Such a position calls for a complete abandonment of incentives as economic development policy in favor of broad-base taxation where taxes fall disproportionately on less productive firms, ensuring that the policy upholds market efficiency. But incentives are politically entrenched and the likelihood of states completely do away with incentives is low. For state governments, incentives remain as one of few policy levers in negotiating deals, and as long as there is pressures for state officials to create jobs, state dependence of incentive policies is likely to stay put. Therefore, a more viable policy recommendation should focus on how to reform incentives for the better.

It is also important to recognize that not all state business incentives are a *zero-sum game*. The literature has overlooked a possibility of differential employment and earnings effects across the firm size distribution, even for the same type of an incentive. In fact, my dissertation found suggestive evidence that a select few incentives may be a *positive-sum* *game*. Unlike other studies that focus on incentives that go to large firms, I studied four major incentives across the firm size distribution, and found differential policy effect for Investment Tax Credit (ITC). ITC is associated with higher employment growth among young, small businesses, though at the aggregate, displays a persistently negative effect on establishment birth and expansion. In general, my findings suggest that small firms might benefit more from incentives than large firms. One plausible explanation is that small businesses are the most credit-constrained entities, and incentives lower the effective tax rates, alleviating a constraint and allowing them to put excess capital to productive use. This then is reflected in employment growth.

The most significant constraint among startups and small businesses, however, might not be a credit constraint but a human capital constraint. Customized Job Training Subsidy (CJTS) is the only incentive in my study which is associated with higher employment growth among all firms (e.g. startups, mature firms, and large firms). In some sense, this is not all that surprising. In today's knowledge economy, the most important commodity is human capital. Large firms have flocked to superstar cities for it, widening the gap between the rich and poor. The shortage of skilled workforce is not just a problem for large firms but also for small firms. The best way to address this problem and promote equitable growth is to incentivize investments in knowledge, skills, and talent. Currently, CJTS makes up a very small proportion of total incentives and the expansion of resources in CJTS would allow states to reap the benefits at a greater scale.

Certainly, one could raise questions to the merits of CJTS instead of more obvious policy alternatives, such as budget increases toward K-12 education or community colleges. To this, it is worth noting that while the highest bang-for-the-buck might be at the K-12 or community colleges, CJTS encompasses many intangible, differentiated benefits, such as learning-by-doing or on-site apprenticeships. In some ways, CJTS can complement other public investments in human capital since many programs involve classroom training and joint-programs with community colleges. CJTS also complements other types of incentives. For instance, it provides employee training assistance specific to business needs for firms that create and retain jobs.

The most pressing policy reform, however, is not in design or implementation, but *evaluation*. Given how much resources are dedicated to incentives, it is critical for state governments to commit sufficient resources for rigorous and continuous program evaluation. The first step in the right direction is the acknowledgement that policy design will remain imperfect and that incentive policies need a reiterative process. Evaluating incentives is the only way to know what works and what does not. Evaluating incentives gives room for policy makers to learn through trial-and-error. This will require an act of fine balance of expanding the administrative costs associated with policy evaluations and ensuring that regulatory hurdles do not create bottlenecks to the system. Continuous evaluation also ensures transparency and accountability of the state government, minimizing the risks of cronyism on one end and minimizing deviant behavior on the other.

The more immediate and timely reforms pertain to *policy design*. First, incentives should target *export-base sectors*. Many incentives target non-export sectors, which are more prone to generate displacement effects. Second, incentives should be *short-term*. Long-term incentives will prevent incumbent governors from overcommitting on incentives, incentivize more productive than unproductive behavior, and will be a lot easier to evaluate. More importantly, in view of that most incentives are ineffective, these policy implementations will significantly reduce wastes associated with incentive offerings, and free up fiscal budgets for investments in public services (e.g., infrastructure, education). Third, some incentives could more specifically target *small businesses*, which face greater credit constraints and appear to benefit more from the policy.

State business incentives, if designed, implemented, and evaluated with excellence, offer a valuable policy lever for state governments to pursue various economic development policy objectives. One recent example of a well-targeted, discretionary state business incentive was the Commonwealth of Virginia's bid on Amazon HQ2. In 2018, Virginia landed Amazon's HQ2 by offering $573 million for 25,000 jobs over ten years, $223 million for transportation improvements, and $1.1 billion over 20 years to expand tech-related higher education. This pales in comparison to Maryland's $8.5 billion, though was larger than the District's $1 billion. Governor Ralph Northam and the State of Virginia put together a reasonably well-designed package. The JCTC package of $22,000 for each job created can be expected to be paid back through increased tax revenue within four years. Investments in transportation infrastructure and education will have been worth doing even without the Amazon deal. Furthermore, the arrival of Virginia Tech's $1 billion campus expansion along with George Mason University's investments will significantly increase the potential for positive externalities in the region. As to how much the Commonwealth will benefit from this incentive will depend on policy implementation and continuous evaluation to ensure transparency and accountability.

APPENDIX

Table 16. The Consumer Price Index for Census Regions

Northeast	**Midwest**	**South**	**West**
Connecticut	Illinois	Alabama	Alaska
Maine	Indiana	Arkansas	Arizona
Massachusetts	Iowa	Delaware	California
New Hampshire	Kansas	District of Columbia	Colorado
New Jersey	Michigan	Florida	Guam
New York	Minnesota	Georgia	Hawaii
Pennsylvania	Missouri	Kentucky	Idaho
Puerto Rico	Nebraska	Louisiana	Montana
Rhode Island	North Dakota	Maryland	Nevada
Vermont	Ohio	Mississippi	New Mexico
Virgin Islands	South Dakota	North Carolina	Oregon
	Wisconsin	Oklahoma	Utah
		South Carolina	Washington
		Tennessee	Wyoming
		Texas	
		Virginia	
		West Virginia	

Source: Bureau of Labor Statistics

Table 17. Detailed Descriptive Statistics on the Business Cycle Variables

	Obs	Mean	p50	Std. Dev.	Min	Max
Panel A: All						
State Unemployment Rate	59132	5.70526	5.4	1.86093	2.3	13.7
Employment Shiftshare	59247	0.00016	0.00004	0.00347	-0.17183	0.1816
Panel B: Treatment Group						
State Unemployment Rate	12950	5.89435	5.4	2.05937	2.3	13.7
Employment Shiftshare	12726	-0.00004	0.00001	0.00475	-0.10658	0.17413
Panel C: Control Group						
State Unemployment Rate	46182	5.65224	5.4	1.79782	2.3	13.5
Employment Shiftshare	46521	0.00022	0.00005	0.00302	-0.17183	0.1816

Source: BLS & CBP, 1990-2015. ALL includes 50 states and DC, thus reflects national trends.

Table 18. Estimated Effects of Job Creation Tax Credits on Employment and Earnings per worker (Levels), Baseline and Interaction Fixed Effect Regressions, 1990-2015, 37 States, Export-base Industries only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				< Log of Employment >				
Panel A								
JCTC Dummy	-0.141***	-0.155***	0.314***	-0.142***	0.104*	0.268***	-0.155***	-0.0401
	(0.0173)	(0.0110)	(0.0517)	(0.0162)	(0.0611)	(0.0481)	(0.00874)	(0.0476)
Panel B								
JCTC (% of PV value-added)	-0.0807***	-0.0794***	-0.0622**	-0.0807***	-0.0484***	-0.0635**	-0.0781***	-0.0436***
	(0.0133)	(0.00842)	(0.0255)	(0.0123)	(0.0179)	(0.0247)	(0.00618)	(0.0145)
				< Log of Earnings per Worker >				
Panel C								
JCTC Dummy	-0.0110***	-0.00967***	-0.000176	-0.0102***	0.0412**	-0.00987	-0.00900***	0.00746
	(0.00370)	(0.00271)	(0.0161)	(0.00346)	(0.0198)	(0.0157)	(0.00235)	(0.0184)
Panel D								
JCTC (% of PV value-added)	-0.0151***	-0.00809***	-0.0337***	-0.0145***	-0.0118**	-0.0349***	-0.00721***	-0.0114**
	(0.00325)	(0.00225)	(0.00663)	(0.00306)	(0.00558)	(0.00656)	(0.00196)	(0.00522)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Industry	No	Yes	No	No	Yes	No	Yes	Yes
State*Year	No	No	Yes	No	Yes	Yes	No	Yes
Industry*Year	No	No	No	Yes	No	Yes	Yes	Yes

Note: Data is from County Business Patterns from 1990 to 2015. NBER crosswalk weights are used to convert sic to naics for years 1990 to 1997. All naics codes are harmonized at naics 2007. Data is merged to Bartik (2017) Panel Database of Incentives and Taxes for 37 states, 45 industries, and 26 years. The total number of observations is 28,285. The dummy value for the Job Creation Tax Credit takes the value of 1 for the state s, industry i, and year t with the subsidy. The continuous value for the Job Creation Tax Credit is estimated as a ratio of present value of net taxes after Job Creation Tax Credit, to present value of value-added. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table 19. Estimated Effects of Job Creation Tax Credits on Employment and Earnings per worker (Levels), With Lagged Controls and a Series of Fixed Effect Regressions, 1990-2015, 37 States, Export-base Industries only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				< Log of Employment >				
Panel A								
JCTC Dummy	-0.00540	-0.0273***	-0.00259	-0.00443	-0.0295	0.00132	-0.0407***	-0.0477
	(0.00704)	(0.00727)	(0.0295)	(0.00651)	(0.0516)	(0.0233)	(0.00688)	(0.0394)
Panel B								
JCTC (% of PV value-added)	-0.00751	-0.0161***	-0.0135*	-0.00685	-0.0153	-0.0128**	-0.0220***	-0.0156
	(0.00467)	(0.00468)	(0.00702)	(0.00446)	(0.0109)	(0.00639)	(0.00442)	(0.00975)
				< Log of Earnings per Worker >				
Panel C								
JCTC Dummy	-0.00155	-0.00328*	0.00536	-0.00160	0.0230	0.00306	-0.00395**	0.00825
	(0.00198)	(0.00195)	(0.00906)	(0.00194)	(0.0198)	(0.00894)	(0.00186)	(0.0191)
Panel D								
JCTC (% of PV value-added)	-0.00240	-0.00253	-0.00687**	-0.00219	-0.00462	-0.00637**	-0.00265*	-0.00359
	(0.00152)	(0.00155)	(0.00310)	(0.00150)	(0.00394)	(0.00314)	(0.00150)	(0.00406)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Industry	No	Yes	No	No	Yes	No	Yes	Yes
State*Year	No	No	Yes	No	Yes	Yes	No	Yes
Industry*Year	No	No	No	Yes	No	Yes	Yes	Yes

Note: Data is from County Business Patterns from 1990 to 2015. NBER crosswalk weights are used to convert sic to naics for years 1990 to 1997. All naics codes are harmonized at naics 2007. Data is merged to Bartik (2017) Panel Database of Incentives and Taxes for 37 states, 45 industries, and 26 years. The total number of observations is 26,781. The dummy value for the Job Creation Tax Credit takes the value of 1 for the state s, industry i, and year t with the subsidy. The continuous value for the Job Creation Tax Credit is estimated as a ratio of present value of net taxes after Job Creation Tax Credit, to present value of value-added. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

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BIOFIGUREY

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