THE IMPLICATIONS OF STATE BUSINESS INCENTIVES ON STARTUPS AND INCUMBENT FIRMS

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Abstract

Startups are important vehicles of job creation and productivity growth (Haltiwanger, Jarmin, and Miranda 2013). The most common economic development policies of state governments are taxation and state business incentives. Much has been written about the implications of tax policies but there is a gap in the literature on the implication of incentives and entrepreneurship. 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 startups and incumbent firms (e.g., establishment birth, death, expansion, and contraction) in 33 states, 7 industries over 17 years. Overall, I find little evidence that incentives spur local economic growth. The most striking finding is the persistently negative effect of Investment Tax Credits on both expansion and birth; and the negative effect of R&D Tax Credit on death. These findings suggest the presence of market distortions resulting from incentives that generate corporate welfare to recipient firms but dampen local productivity growth; but, there is also a possibility of incentives providing a life-line to research activities with higher social than private returns. The key underlying policy challenge is how to promote productive over unproductive firms, and productive over unproductive firm behavior.

Key words: state business incentives, entrepreneurship, productivity, corporate political activity, non-market strategy

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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 concerns to the merit and cost effectiveness of economic development incentives. The case in point is Amazon, a technology company with a trillion dollar market cap, which pitted states against states for billions of tax incentive dollars through the Amazon HQ2 bid. In the last two decades, incentives have tripled, costing states $50 billion annually. Yet, systematic evaluations and scholarship remain sparse.

This paper examines the impact of state business incentives on startups and incumbent firms at the state-level.¹ The policy effect on entrepreneurship is rather arbitrary because *the primary target* of incentives are large firms (Buss 2001). State 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 could there be a case to be made for broad-based incentives targeting 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).² Related to this is the question of whether targeting incentives at the largest firms have anti-competitive effect in the market.

At the heart of the debate underlies a basic question of whether incentives enhance or deter the allocative efficiency of capital, and of course who to target to achieve optimal outcome. Theory

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¹ 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)
² 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.
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).³

There is a gap in the literature on the implications of state business incentives on entrepreneurship. Whether state business incentives can have a positive impact on startups therein lies the primary focus of this paper. 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; Tuszyński 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 major 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 paper’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

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³ Low-productivity young firms contract and exit, while high-productivity young firms enter and expand, contributing to innovative activities that further enhance industry productivity.
to conduct policy evaluation. Second, I apply three-way interacted fixed effect regression model, which is the most demanding and 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, I find little evidence that incentives have material impact on their primary objective: establishment expansion (Donegan, Lester, and Lowe 2019; Cahuc et al. 2019; Criscuolo et al. 2019). Most incentives also appear to have no material impact on establishment birth. 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. Instead if anything, incentives may be generating market distortions that slow the process of capital reallocation from less productive to more productive firms (Acemoglu et al. 2018). In my study, one exception might be the negative effect of R&D Tax Credit on death, which suggests that the incentive can possibly be a life-line to research activities with higher social than private returns.

2 State Business Incentives

Incentives are “… a direct reduction, deduction, deferral, or exemption” in tax liability from specific business activities encouraged by the state (Pollard 2015). 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).\textsuperscript{4} Tax credits typically subsidize capital investments, job creation, and research and development activities. Exemptions and deductions typically 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.

State business incentives are usually a part of states’ 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).\textsuperscript{5} Incentives are never the full story to expansion, relocation, and startup decisions.

Incentives can be \textit{discretionary or non-discretionary}.\textsuperscript{6} Discretionary incentives are firm-specific going to large firms in a form of megadeals.\textsuperscript{7} 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.

The state business incentives are a double-edged sword designed (i) to \textit{fight for} relocation and expansion of productive firms in the export-base sectors; (ii) to \textit{fight against} unemployment and the flight of productive firms. The \textit{primary objective} of incentives is to promote local investment and hiring (Bartik 2019). Following the Great Recession, states have become hard-

\textsuperscript{4} Given that my dissertation takes a state-level approach, I choose to omit PTA, which is an incentive determined at the city-level.
\textsuperscript{6} The focus of this dissertation will be on non-discretionary tax incentives and credits. The focus is at the state-level since the policy is determined at the state-level.
\textsuperscript{7} 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.
pressed to create jobs, protect jobs, and reduce high unemployment rates (Neumark and Grijalva 2017; Criscuolo et al. 2019).

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. Slattery and Zidar (2020, 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. 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.

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) that generate desired economic outcomes at a fraction of the cost. However, if the government fails in targeting, the economy is better off with less or no incentives.

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).\(^8\) The credit constraint narrative is tied to the tax literature: incentives lower the effective tax rate, which lessens the credit constraint, permitting firms to put this excess capital to productive use; it could also simply increase the firm survival rate (Criscuolo et al. 2019; Garrett, Ohrn, and Serrato 2019).

Poorly targeted incentives pose serious risks of creating unintended “disincentives” that can potentially distort the market competition and firm behavior. For example, incentives can complicate the tax system by narrowing the tax base or driving up tax rates for ineligible firms, and thereby distort the market and fail to generate economic growth. Incentives are also 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).\(^9\)

The general literature is highly critical of incentives and considers it a wasteful redistribution of taxpayer’s money to large firms (Mattera, Tarczynska, and LeRoy 2014; Tarczynska, Cafcas and LeRoy 2016). It is because the majority of incentives target large incumbents to spur greater investments, increase productivity and protect employment (Buss 2001; Aghion et al. 2015).

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\(^8\) 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).

\(^9\) 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).
Critics question the government’s ability to pick the right winners. The argument is that probability of successfully targeting productive firms with incentives is low, while the associated risks of introducing market distortions too high – meaning that incentives are likely to slow down the allocative efficiency of capital (Acemoglu et al. 2018). A part of the issue is that incentives have become entrenched, often driven by political rather than economic reasons. For example, states with governors facing re-election have been found to offer substantially more incentives to firms, raising concerns to the legitimacy, transparency, and accountability of the policy (Slattery 2020).

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 (See Table 1). 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>
<thead>
<tr>
<th>State Business Incentive</th>
<th>Entrepreneurs/Small businesses</th>
<th>Large Incumbents</th>
</tr>
</thead>
<tbody>
<tr>
<td>Job Creation Tax Credit</td>
<td>Donegan, Lester, and Lowe (2019); Cahuc et al. (2019)</td>
<td></td>
</tr>
<tr>
<td>Investment Tax Credit</td>
<td>Criscuolo et al. (2019)</td>
<td></td>
</tr>
<tr>
<td>Corporate Income Tax Credit</td>
<td></td>
<td></td>
</tr>
<tr>
<td>R&amp;D Credit</td>
<td>Wu (2008); Fazio, Guzman, and Stern (2019); Babina and Howell (2018)</td>
<td>Bloom, Griffith, and Van Reenen (2002); Wilson (2005); Lucking (2019)</td>
</tr>
<tr>
<td>Customized Job Training Subsidy</td>
<td>Bartik (2018)</td>
<td></td>
</tr>
<tr>
<td>Total Incentives</td>
<td></td>
<td>Partridge et al. (2019); Tuszynski and Stansel (2018)</td>
</tr>
</tbody>
</table>

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.¹⁰

¹⁰ See also Neumark, Wall, and Zhang (2011), and Haltiwanger, Jarmin, and Miranda (2013).
3 Data

This dissertation takes advantage of a comprehensive Panel Database on Incentives and Taxes (PDIT) constructed by Bartik (2017). 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.

I merge PDIT data with the Statistics of U.S. Businesses (SUSB) and County Business Patterns. 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.

In Figure 1, a simple scatter diagram of PDIT-CBP merged data plots a startling negative relationship between incentives and employment. While the relationship is just a correlation without all the necessary controls, the rest of the dissertation will explore in greater detail this relationship using rigorous econometric methods.

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11 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.

12 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.

13 CBP program and the SUSB program tabulate the same data in different ways: in the former, the size category is always based on the size of the individual establishment (physical location), whereas in the latter, it is based on enterprise that owns the establishment(s).
Figure 1: Scatterplot of Total Incentives (% of Present Value of Value-Added) on Employment

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).\(^\text{14}\) 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: establishment expansion, contraction, birth, and death. These are the official definitions for SUSB:

- **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.”

\(^{14}\) 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 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.

3.2 Panel Database of Incentives and Taxes

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. 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.

### 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 2).

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.\(^\text{15}\) But none of the seven sectors belong to the

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manufacturing sector, which by definition are all export-base industries and on average have more generous incentives.

Table 2. States, and Industries Included

Panel A: States Included (33 States)

<table>
<thead>
<tr>
<th>State</th>
<th>State</th>
<th>State</th>
</tr>
</thead>
<tbody>
<tr>
<td>New Mexico</td>
<td>Texas</td>
<td>Wisconsin</td>
</tr>
<tr>
<td>Georgia</td>
<td>Colorado</td>
<td>Minnesota</td>
</tr>
<tr>
<td>Maryland</td>
<td>Iowa</td>
<td>Louisiana</td>
</tr>
<tr>
<td>Alabama</td>
<td>Michigan</td>
<td>New Jersey</td>
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<tr>
<td>Massachusetts</td>
<td>Indiana</td>
<td>Nebraska</td>
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<td>Connecticut</td>
<td>Missouri</td>
<td>Pennsylvania</td>
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<tr>
<td>New York</td>
<td>Nevada</td>
<td>Arizona</td>
</tr>
<tr>
<td>North Carolina</td>
<td>California</td>
<td>Oregon</td>
</tr>
<tr>
<td>Illinois</td>
<td>Kentucky</td>
<td>Washington</td>
</tr>
<tr>
<td>Ohio</td>
<td>Tennessee</td>
<td>Virginia</td>
</tr>
<tr>
<td>South Carolina</td>
<td>Florida</td>
<td>District of Columbia</td>
</tr>
</tbody>
</table>

Panel B: Industries Included

<table>
<thead>
<tr>
<th>Industry</th>
<th>NAICS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Construction</td>
<td>23</td>
</tr>
<tr>
<td>Wholesale Trade</td>
<td>42</td>
</tr>
<tr>
<td>Management of companies (holding companies)</td>
<td>55</td>
</tr>
<tr>
<td>Educational Services</td>
<td>61</td>
</tr>
<tr>
<td>Other services</td>
<td>81</td>
</tr>
<tr>
<td>Retail Trade</td>
<td>44-45</td>
</tr>
<tr>
<td>Miscellaneous professional, scientific, and technical services</td>
<td>54</td>
</tr>
</tbody>
</table>

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.
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 3 provides basic descriptive statistics.

Table 3. Descriptive Statistics on all business incentives

<table>
<thead>
<tr>
<th>Variable</th>
<th>N</th>
<th>Mean</th>
<th>Median</th>
<th>St. Dev</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total incentives</td>
<td>4,158</td>
<td>0.5502</td>
<td>0.0850</td>
<td>1.0058</td>
</tr>
<tr>
<td>Job creation tax credit</td>
<td>4,158</td>
<td>0.1846</td>
<td>0.0000</td>
<td>0.5316</td>
</tr>
<tr>
<td>Investment tax credit</td>
<td>4,158</td>
<td>0.0924</td>
<td>0.0000</td>
<td>0.3876</td>
</tr>
<tr>
<td>R&amp;D credit</td>
<td>4,158</td>
<td>0.0042</td>
<td>0.0010</td>
<td>0.0127</td>
</tr>
<tr>
<td>Customized job training subsidy</td>
<td>4,158</td>
<td>0.0545</td>
<td>0.0000</td>
<td>0.1356</td>
</tr>
</tbody>
</table>


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

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 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:

\[
\text{Equation 1}
\]

\[
f(E,I) = pQ - wE - qI
\]

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:

\[
\text{Equation 2}
\]

\[
f(E,I) = pQ - (1 - t)wE - qI
\]

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

\[
\text{Equation 3}
\]

\[
f(E,I) = pQ - wE - (1 - t)qI
\]

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:

\[
\text{Equation 4}
\]

\[
f(E,I) = pQ - (1 - t)wE - (1 - t)qI
\]

With \( t \) being a certain type of tax incentive, the first order condition will look as follows:
No labor tax credit: \[
\frac{\partial E}{\partial I} = \frac{w}{q}
\]
With labor tax credit: \[
\frac{\partial E}{\partial I} = \frac{q}{(1-t)w}
\]
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).

5 Analysis and Methodology Issues

5.1 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. Higher productivity is closely associated with long-term economic growth (Syverson 2011). 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. 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

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16 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.
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). 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).

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

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17 Evidence of relocation in response to tax incentives is not non-existent. Moretti and Wilson (2017) find cross-state relocation within the U.S.

18 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.
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 2 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 2. Evaluating Primary & Secondary Objectives of State Business Incentives

5.2 Analysis

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.

In equation 6, 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 to estimate the following equation:

\[ \ln(Y)_{ist} = \beta_0 + \beta_1(SBI_{ist}) + \delta_{st} + \tau_{it} + \gamma_{is} + \epsilon_{ist} \]

\( Y_{ist} \) 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 regressions are analyzed. Interaction fixed effect regressions include state-by-year (\( \delta_{st} \)), industry-by-year (\( \tau_{it} \)) and industry-by-state (\( \gamma_{is} \)). 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, there is a vast literature discussing slow adjustments, such as the stickiness of wages or labor adjustment costs. I estimate the equation with including lagged dependent variable as controls. The estimation equation looks like this:
\[ \ln(Y)_{ist} = \beta_0 + \beta_1(SBI_{ist}) + \beta_2\ln(Y)_{ist-1} + \delta_{st} + \tau_{it} + \gamma_{is} + \epsilon_{ist} \]

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 (\( \delta_{st} \)), industry-by-year (\( \tau_{it} \)) and industry-by-state (\( \gamma_{is} \)). 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.

### 5.3 Methodology Issues

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 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).

In principle, incentives can result in three outcomes: (i) positive effect; (ii) no effect; (iii) negative effect. A serious empirical 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. 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.

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. These demanding econometric specifications are the best I can do to account for endogenous selection but there is little that can be done empirically to account for unproductive firm behavior. Assuming the absence of such market distortions, scenario (i) would suggest a positive-sum game and scenario (ii) and (iii) would be a zero-sum game of state business incentives.

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. PDIT 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.

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).
6 Results

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

Table 4 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 5 concurrently.
Table 4. Estimated Effects of State Business Incentives (Dummy) on Establishment Birth, Death, Expansion and Contraction, 1998-2015, 33 States, 7 Industries

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Birth</th>
<th>Death</th>
<th>Expansion</th>
<th>Contraction</th>
</tr>
</thead>
<tbody>
<tr>
<td>JCTC Dummy</td>
<td>0.00385</td>
<td>0.00989</td>
<td>0.00248</td>
<td>-0.00449</td>
</tr>
<tr>
<td></td>
<td>(0.0217)</td>
<td>(0.0240)</td>
<td>(0.00811)</td>
<td>(0.00864)</td>
</tr>
<tr>
<td>ITC Dummy</td>
<td>-0.0697**</td>
<td>-0.00606</td>
<td>-0.0280**</td>
<td>-0.0314**</td>
</tr>
<tr>
<td></td>
<td>(0.0318)</td>
<td>(0.0258)</td>
<td>(0.0135)</td>
<td>(0.0141)</td>
</tr>
<tr>
<td>CJTS Dummy</td>
<td>0.0600</td>
<td>0.0533</td>
<td>-0.0176</td>
<td>0.00325</td>
</tr>
<tr>
<td></td>
<td>(0.0472)</td>
<td>(0.0387)</td>
<td>(0.0204)</td>
<td>(0.0176)</td>
</tr>
<tr>
<td>RDTC Dummy</td>
<td>-0.0407</td>
<td>-0.0479**</td>
<td>0.00851</td>
<td>0.00127</td>
</tr>
<tr>
<td></td>
<td>(0.0277)</td>
<td>(0.0215)</td>
<td>(0.0129)</td>
<td>(0.0145)</td>
</tr>
<tr>
<td>Observations</td>
<td>3,927</td>
<td>3,927</td>
<td>3,927</td>
<td>3,927</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.995</td>
<td>0.995</td>
<td>0.999</td>
<td>0.999</td>
</tr>
<tr>
<td>State FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State*Industry</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State*Year</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry*Year</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>
| 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
6.2 Regression Results on Employment and Earnings Effects

Table 5 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 4 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 4. 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 4, 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 4, ITC displays a negative association in earnings in column (5) and (6) of Table 5, though weak in magnitude. CJTS also displays a negative association in earnings per worker, though the magnitude is weak.
### Table 5. Estimated Effects of State Business Incentives (Dummy) on Employment and Earnings per Worker (w/ Adjustment Cost), 1998-2015, 33 States, 7 industries

| VARIABLES | Log(Employment) | | Log(Earnings per Worker) | | |
|-----------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|
|           | (1)             | (2)             | (3)             | (4)             | (5)             | (6)             |
|           | <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          | -0.00767        | -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

### 6.3 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). 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 establishment 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. 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

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19 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.
effects. 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.

7 Conclusion

Using a new and comprehensive database on incentives, this paper 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 incentives appear to have no material impact on establishment birth and establishment expansion. In other words, most incentives generate market distortions and are a zero-sum game. 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. 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

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20 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.  
21 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).
negotiate fair discretionary incentive deals with businesses is often compromised. Perhaps, the most striking finding is the persistently negative effect of Investment Tax Credit on establishment birth and expansion as well as lower average earnings per worker, suggesting that the policy introduces market distortions and dampens local productivity growth.

Overall, Acemoglu et al. (2018) suggest 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 disproportionatelty 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.22

The findings of this study support the claims of Acemoglu et al. (2018) that in most cases, the state governments would be better of cutting down on narrow-base state business incentives and relying on broad-base tax cuts, a much more market-oriented approach that avoids complicating tax laws as do incentives. One exception might be Research and Development Tax

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22 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).
Credits that reduce the establishment deaths. Given the underinvestment in R&D activities in the private sector due to high uncertainty and difficulties of appropriability, incentives that increase local knowledge creation can have merit based on long-term payoff (Fazio, Guzman, and Stern 2019).

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).
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.
REFERENCES


