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Do Non-Compete Covenants Influence State Startup Activity? Evidence from the Michigan Experiment

Gerald A. Carlino
Emeritus Economist, Federal Reserve Bank of Philadelphia Research Department

"DO NON-COMPETE COVENANTS INFLUENCE STATE STARTUP ACTIVITY? EVIDENCE FROM THE MICHIGAN EXPERIMENT"

Gerald A. Carlino*

Federal Reserve Bank of Philadelphia

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Abstract

This paper examines how the enforceability of employee non-compete agreements affects the entry of new establishments and jobs created by these new firms. We use a panel of startup activity for the U.S. states for the period 1977 to 2013. We exploit Michigan's inadvertent policy reversal in 1985 that transformed the state from a non-enforcing to an enforcing state as a quasi-natural experiment to estimate the causal effect of enforcement on startup activity. In a difference-in-difference framework, we find little support for the widely held view that enforcement of non-compete agreements negatively affects the entry rate of new firms or the rate of jobs created by new firms. We find that increased enforcement had no effect on the entry rate of startups, but a positive effect on jobs created by these startups in Michigan relative to a counterfactual of states that did not enforce such covenants pre- and post-treatment. Specifically, we find that a doubling of enforcement led to an increase of about 8 percent in the startup job creation rate in Michigan. We also find evidence that enforcing non-competes positively affected the number of high-tech establishments and the level of high-tech employment in Michigan. Extending our analysis to consider the effect of increased enforcement on patent activity, we find that enforcement had differential effects across technological classifications. Importantly, increased enforcement had a positive and significant effect on the number of Mechanical patents in Michigan, the most important patenting classification in that state.

Keywords: Startup activity, Non-compete agreements, Regional economic growth.

JEL Codes: O30, O38, R11

1. INTRODUCTION

Business startups play an important role in job creation. For example, on average, startups created almost 4 million jobs over the past four decades in the U.S. economy. As Haltiwanger, Jarmin, and Miranda (2013) show, while many of these startups will fail within a few years, a small percentage of fast growers will ultimately contribute disproportionately to job creation in the U.S. Recent examples of fast-growing startups include Google, Amazon, and Microsoft. One channel for growth of startup activity and entrepreneurship is through employees' leaving their current employers to form new establishments. Concerned about competition with former employees, many employers require their employees to sign non-compete covenants. In contract law, a post-employment non-compete covenant is a clause whereby one party (typically an employee) agrees not to start or join a similar business that would be in competition with another party (usually the employer). Typically, non-competes restrict an employee's job mobility for a limited time and within a narrowly defined geographic region.

An important consideration is that non-competes may hinder knowledge flows among workers and firms in states that enforce such agreements. Marx, et al. (2015) find that employee non-compete agreements are responsible for the migration of knowledge workers from states enforcing these contracts to non-enforcing states. Further, workers covered by such agreements may feel constrained about sharing information with outsiders, further limiting an important source of knowledge spillovers.

Despite the fact that non-competes represent a restraint on trade and may limit knowledge spillovers, such agreements are common for many types of workers in the U.S. (Stone, 2002, and

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Starr, et al., 2020).[1] There are no federal laws governing the enforceability of non-competes; enforcement is left to the states, and states differ in the manner and the extent of non-compete enforcement. The courts in many U.S. states tend to enforce employee non-compete agreements because they recognize them as a way to safeguard the legitimate business interest of firms. An important issue is whether and to what extend does judicial enforcement of non-compete clauses impede entrepreneurial activity and employment growth.

The impact of enforceability on entry and employment of new firms is theoretically ambiguous. The literature has identified two channels in which the enforcement of non-compete agreements could affect startup activity. Starr, et al. (2015) identify a negative channel, referred to as a "screening effect," in which greater enforcement lowers the expected returns to spinoff activity by raising the probability of losing a lawsuit for violating the terms of a non-compete agreement.[2] Kang and Fleming (2020) point out that startups could avoid states with stronger non-compete laws, since the entrepreneurs typically lack the resources necessary to educate and train employees and may prefer to hire experienced employees from nearby competitors.

On the other hand, to the extent that non-compete clauses help companies protect their investments, this protection may stimulate startup activity and employment growth (Starr, et al., 2015, refer to this channel as an "investment-protection effect").[3] Kang and Fleming (2020) point out that startups tend to be small, having few assets other than their ideas and intellectual

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[1] Starr et al. (2020) report that 18 percent of all U.S. workers are covered by non-compete agreements and that 37 percent say they have been covered by such an agreement during their career.
[2] These costs would include any payments an employee (or a third party) makes to his parent firm to be released from a non-compete agreement.
[3] The higher expected profits associated with the investment channel will be reduced if firms have to pay a wage premium to entice potential workers to move to enforcing states.

property, and may be attracted to states with relatively stronger enforcement that may deter employees from departing.[4]

The overall effect of non-compete covenants on startup activity is an open question given the competing forces of the screening effect and the investment-protection effect.[5]

The purpose of this paper is to provide evidence on the effect of judicial enforcement of non-compete covenants on the rate of entry of startups and the job creation rate of new firms. In the main analysis, we use a panel of startup activity in U.S. states for the period 1977 to 2013 and exploit the Michigan Antitrust Reform Act (MARA) of 1985 (which inadvertently "legalized" non-compete agreements) as a quasi-natural experiment to estimate the causal effect of enforcement on startup activity. To evaluate whether the observed changes in startup activity is being driven by a response to changed enforcement policy, we need to identify a comparison state or states that trace the counterfactual path of startup trends for Michigan. The quality of our analysis is obviously tied to how well we estimate the comparison group. There are a number of strategies for constructing a comparison group, and we start by identifying three alternative control groups of states (states sharing a land border with Michigan; states sharing a land border or a water boundary with Michigan; and the 10 "non-compete" states identified by Marx, et al., 2009). In a difference-in-difference (hereafter, DID) analysis, we find that enforcement had a positive and, in some cases, significant effect on the startup job creation rate but little or no

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[4] In general, Meccheri (2009) shows that non-compete covenants can be justified on efficiency ground as they attempt to solve a "hold-up" problem. *Ex ante*, both the employee and the employer benefit from worker training and the sharing of trade secrets. But e*x post*, the employee has an incentive to "hold up" his employer for additional compensation by threating to divulge confidential information. Forward-looking employers would be unwilling to invest (or would under-invest) in education and training and be less willing to share trade secrets with employees unless they had some form of legal recourse provided by non-compete agreements.
[5] Also, strict enforcement may limit agglomeration economies, by limiting knowledge spillovers, and the benefits associated with labor market matching and pooling. An analysis of the effect of enforcement on agglomeration economies is beyond the scope of this paper.

effect on the entry rate of new firms. Specifically, depending on the control group, a doubling of enforcement led to a 6 percent to 8 percent increase in the startup job creation rate in Michigan and to essentially no change in the startup entry rate.

A crucial assumption underlying the DID strategy is that the outcome in treatment and control groups would follow the same time trend or a parallel trend in the absence of the treatment. A standard DID analysis would result in biased estimates if the treatment and control groups did not meet the parallel-trends assumption. This leads us to consider a fourth alternative control group, identified using a data-driven search routine: the Synthetic Control Method (SCM). The basic idea underlying the SCM is that often a combination of states produces a better control group than any single state or arbitrary group of states, such as states bordering Michigan. When using the SCM method, we find that changes in both the startup entry rate and the job creation rate, while positive, following MARA, are not significantly different from pre-MARA findings. Taken together, these findings offer little support for the view that enforcement of non-compete agreements negatively affects the entry rate of new firms or the rate of jobs created by new firms.

We extend our analysis to consider the effect of increased enforcement on high-tech establishments and to employment by these establishments.[6] A common view since the work of Saxenian (1994) is that employee non-compete covenants may serve to suppress high-tech development in states such as Massachusetts, where historically, non-competes tend to be enforced by the courts, but did not hold back the tech boom in states such as California, where such covenants are much less likely to be enforced. As with startup activity, we found some evidence that enforcement of non-competes in Michigan after 1985 positively affected both high-

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[6] We thank Enrico Moretti for suggesting the extension of the analysis to high-tech activity.

tech establishments and employment at these establishments, but only for one of the four control groups considered.

Finally, patent data can be used to study entrepreneurial activity and are available for total patent activity and for six technology classifications. We find that enforcement had a significant positive effect on the total number of patents issued to Michigan inventors, and in the Mechanical category and in the other classifications. We find the number of patents issued in the drug classifications is negatively and significantly affected by enforcement. The patent findings are important in that they demonstrate the importance of considering subcategories, something we are not able to do with the publicly available startup data.

Previous studies have found mixed evidence regarding the importance of non-compete clauses on worker and inventor mobility. Most state courts enforce some form of non-compete clauses, with California being an important exception. Thus, worker mobility, or "job hopping," could be unusually high in California because of the unenforceability of non-compete clauses under California law (Gibson, 1999). Fallick, et al. (2006) find much greater mobility of college-educated males employed in the computer industry in Silicon Valley compared with the inter-firm mobility of similarly educated workers in the computer industry in other areas outside of California. Part of this turnover could be induced as firms and workers seek better matches. It is important to note that Fallick, et al. (2006) find that employee turnover in other industries is no higher in California than in other locations, suggesting that a lack of enforcement of non-compete clauses is not the primary reason for the job-hopping observed in California. Still, a number of other studies offer evidence that tends to suggest the enforcement of non-competes limits worker mobility. Balasubramanian, et al. (2019) find that increased enforcement is positively associated with longer job tenure in high-tech industries, without increased wages.

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Garmaise (2011) finds that stronger state enforcement tends to reduce mobility of U.S. executives and lowers their compensation. Marx, et al. (2009) find that the 1985 policy reversal that transformed Michigan from a non-enforcing state to an enforcing state resulted in an 8 percent decrease in within-state mobility of inventors, and Marx, et al. (2015) find that Michigan's policy reversal not only restricted within-state mobility but also led to increased inter-state mobility of inventors (a "brain drain"). Bozkaya and Kerr (2014) more broadly show how rigid employment law can hinder the development of innovative sectors that rely on rapid labor turnover. Samila and Sorenson (2011) find that local supply of venture capital in states that limit the scope of non-compete agreements positively influences innovative activity, firm entry, and job creation. Conti (2014) finds that firms are more likely to undertake riskier, potentially path-breaking, R&D projects in states that tend to enforce non-competes than in states with less-restrictive enforcement policies. Starr, et al. (2015) find that greater enforceability is associated with fewer within-industry spinoffs compared with cross-industry spinoffs, providing evidence for the screening channel.

In a study more closely related to ours, Kang and Fleming (2020) use Florida's 1996 legislative change that eased restrictions on their enforcement to study the effects of increased enforcement both on large firms and on small firms. The researchers find that following the enforcement change, large firms were more likely to establish businesses in Florida, while small firms were not. It's important to note that we look at startup activity as opposed to focusing on small firms, as do Kang and Fleming (2020).

Our study also differs from past research in that we focus on how non-competes affect startup activity rather than how they affect worker mobility or investment activity. While knowledge of how non-competes limit mobility and investment is useful, this research does not inform us

about the effects of non-competes on firm entry or about the employment created by new firms in states enforcing such agreements.

2. EMPIRICAL METHODOLOGY AND DATA

Michigan had a long history of prohibiting the enforcement of non-compete agreements. Section 1 of Act No. 329 of the Public Acts of 1905 prohibited the enforcement of non-compete covenants. The act states:

> "All agreements and contracts by which any person, copartnership or corporation promises or agrees not to engage in any avocation, employment, pursuit, trade, profession or business, whether reasonable or unreasonable, partial or general, limited or unlimited, are hereby declared to be against public policy and illegal and void."

This act governed the enforcement of non-compete clauses until March 1985, when Michigan's legislature inadvertently eliminated the statute when it passed MARA. While the main purpose of MARA was to consolidate Michigan's antitrust statutes, in doing so, the legislature unintentionally repealed numerous statutes, including Public Act No. 329. According to Marx, et al., (2009): "More than 20 pages of legislative analysis of MARA by both House and Senate subcommittees does not mention non-competes as a motivation for the bill." A number of researchers conclude that the repeal of Public Act 329 was unintentional given that antitrust reform was the main motivation for MARA. Marx, et al. (2009) persuasively argue that changes in Michigan's enforcement policy can be viewed as an exogenous event allowing one to test for causal influence of non-competes on startup activity.

In December 1987, the Michigan Legislature reversed course and passed MARA Section 4a adopting a "reasonableness standard" in that non-compete agreements can be enforceable to the extent that they are "reasonable as to its duration, geographic area, and the type of employment

or line of business." Moreover, if the non-compete clauses in the agreement are "found to be unreasonable in any respect, a court may limit the agreement to render it reasonable."[7]

2.1 Empirical Methodology

In the research described below, Michigan's seeming unintended reversal of its non-compete enforcement policy is used as a quasi-natural experiment in a DID analysis. If the judicial enforcement of non-compete agreements initiated by the passage of MARA (the treatment) had a measurable effect on startup activity (the outcome) in Michigan, we expect to observe differences between startup activity pre- and post-treatment compared with a control group of other states. The DID estimation can be expressed in regression form as:

$$Y_{st} = \beta_0 + \beta_1 T_s + \beta_2 P_t + \beta_3 P_t * T_s + \mu_{st},$$

where Y_{st} represents the outcome of interest for state s in time period t. T_s is a dummy variable equal to one for Michigan observations, the treatment state, and zero otherwise. P_t represent a dummy variable equal to unity beginning in 1985 and zero otherwise. The interaction term $P_t * T_s$ is essentially an indicator variable equal to unity for Michigan observations post-treatment. $\hat{\beta}_3$ is an estimate of the average differential change in Y from the pre-treatment period 1977 to 1984 to the post-treatment period 1985 to 2013 for Michigan relative to the control group. Under the assumption that the treatment is randomly assigned, $E(\mu_{s,t} | Y_{s,t}) = 0$, the OLS estimator of β_3 is unbiased.

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[7] Reasonable covenants also may protect trade secrets, confidential information, and employers' customers or customer lists.

One concern is that other state characteristics, such as state income growth and state population growth, may be important for determining the outcome of the experiment. Including these additional covariates helps to ensure that there is no omitted variable bias:

$$Y_{st} = \beta_0 + \beta_1 T_s + \beta_2 P_t + \beta_3 P_t * T_s + \gamma_j \sum_{j=1}^{J} X_{jst} + \mu_{st}$$

For the additional covariates, denoted j, we included state level values for: the nine one-digit SIC industry share of total state employment; the percentage of a state's population with a college degree; the percentage of a state's population aged 15 to 64 years old; the state's unemployment rate; the state's labor force participation rate; real per capita state income growth; and state population growth. We include year fixed effects to control for common aggregate sources of variation in startup activity. The variables are in logs, with the exception of real per capita income growth and population growth, which are in levels. Bertrand, et al. (2004) demonstrate the importance of using cluster-robust standard errors in a DID framework. We adopt this approach and cluster the standard errors at the level of treatment, which is the state.

As already noted, the sign on β_3 is uncertain. To the extent that the enforcement of non-compete agreements impedes entrepreneurial activity, there could have been a decrease in entry and job creation in Michigan compared with the control group following the passage of MARA. Alternatively, the opposite (a positive) effect on startup activity is anticipated if enforcement of non-compete clauses leads companies to invest more in Michigan. How these opposing forces net-out is an empirical issue central to the analysis considered in this paper.

2.2 Data

In this section, we will discuss the data and sources for startup activity. We will discuss the data

for high-tech activity and patents in subsequent sections. We use annual state-level data from the

U.S. Census Bureau's Business Dynamics Statistics (BDS) for the period 1977–2013 on the

entry of new establishments and the number of private sector jobs created by these

establishments. The BDS consists of longitudinal data covering all private non-farm U.S.

establishments and firms. For the 50 U.S. states, this gives a panel consisting of 1,850

observations. The BDS provides annual measures of business dynamics (such as the number of

startups, firm closures, and job creation and destruction) for states, aggregated by establishment

and firm characteristics. We are limited to looking at aggregate state-level startup activity

because a state-industry-level breakdown of the data is not publicly available. The outcome

variables used in our analysis are defined as:

$$\text{Establishment Entry Rate}_{s,t}^{0} = \frac{\text{New Establishments}_{s,t}^{0}}{1/2(\text{No. of Estabs}_{s,t} + \text{No. of Estabs}_{s,t-1})} \qquad (1)$$

$$\text{Job Creation Rate}_{s,t}^{0} = \frac{\text{Job Created}_{s,t}^{0}}{1/2(\text{Employment}_{s,t} + \text{Employment}_{s,t-1})}, \qquad (2)$$

where the Establishment Entry Rate$_{s,t}^{0}$ refers to the number of startups in state s in time t by

age zero establishments relative to the total number of establishments in state s. Similarly, the

Job Creation Rate$_{s,t}^{0}$ refers to the number of jobs created by startups relative to total

employment in the state. Following Haltiwanger, et al. (2013), we define rates relative to a

denominator that averages employment of the number of firms in the current and previous year.

We supplement these data with additional covariates predictive of startup activity, such as

economic and demographic characteristics of states. The share of a state's employment by the one-digit industry, state unemployment rates, and state labor-force participation rates are obtained from the Bureau of Labor Statistics. Data for state population, state population share aged 15-64, and the share of state population with a college degree are obtained from the Census Bureau. State-level GDP is obtained from the Bureau of Economic Analysis Regional Economic Accounts.

3. IDENTIFYING THE CONTROL GROUPS

To evaluate whether startup activity is responsive to changed enforcement policy, we need to identify a comparison state or states that trace the counterfactual path of the outcome variables of interest. There are a number of strategies for constructing a comparison group, all of which have merit, but also concerns.

3.1 States Sharing a Land Border with Michigan

One control group used in the analysis consists of U.S. states that border Michigan, as these states may have similar economic, demographic, and social characteristics (Indiana, Ohio, and Wisconsin).[8] It is likely that states sharing a border with Michigan share many unobservable characteristics, helping to ensure the parallel trend requirement for this control. We refer to this group of states as Border States.

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[8] Michigan borders the Canadian province of Ontario, but Ontario is excluded from the control group to maintain consistency of the included data.

3.2 States Sharing a Land or Water Boundary with Michigan

Two states that share a water boundary with Michigan are added to give us five states in this control group (Illinois, Indiana, Ohio, Minnesota, and Wisconsin). For brevity, we refer to this control group as Expanded Border States.

3.3 Non-Enforcing States

Based on Table 1 in Stuart and Sorenson (2003), Marx, et al. (2009) and Marx, et al. (2015) identify 10 states with statutes that claimed to limit the enforcement of non-compete agreements both pre- and post-MARA. These 10 states (Alaska, California, Connecticut, Minnesota, Montana, Nevada, North Dakota, Oklahoma, Washington, and West Virginia) constitute the control group used by Marx, et al. (2015) and Marx, et al. (2009) to study the migration of knowledge workers from Michigan to other states. Given Michigan's switch from a non-enforcing state pre-MARA to an enforcing state post-MARA, other states that did not enforce such agreements both pre- and post-MARA constitute an appropriate comparison group. We use these 10 states, referred to as Non-Enforcement States, as the main control groups in the analysis to follow. The dynamic consistency in non-compete legislation is a main reason we prefer using the Non-Enforcement States to chart the counterfactual path for the variables of interest.

3.4 Parallel Trends

Another condition for a good control group is that the group should display similar or parallel trends during the pre-treatment period compared with the treatment state (Michigan in our case). Figure 1a illustrates the trends in the job creation rate for the states sharing only a land border with Michigan, while Figure 1b shows the trends for the entry rate. As the figures show, each state in this control group closely tracks movements in Michigan both for the job creation rate

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and for the entry of startups during the pre-MARA period. The levels for these variables for Michigan and the states in this control group are similar, too, strengthening the assumption that these untreated states provide an appropriate "counterfactual Michigan."

Figure 1c shows the trends in the job creation rate for the states in the potential control group called Expanded Border States, while Figure 1d shows the trends in the job creation rate for the 10 states that make up the potential control group called Non-Enforcement States. As the Figure 1c and Figure 1d reveal, the trends in job creation rate in the pre-treatment period for either of these potential control groups do not as closely parallel those for Michigan during this period as do the trends for the Border States. Still, the Non-Enforcement States is the preferred comparison group given that these states did not enforce non-competes pre- and post-MARA. For consistency, we will conduct a DID analysis using these three alternative control groups, both for completeness and for comparison to other studies, such as Marx, et al. (2015). [9, 10]

Before proceeding to the formal analysis, it is important to consider what happened to startup activity in Michigan just after the 1985 legislation. There is little evidence of a sharp break in Michigan's trend either in the job creation rate (Figure 1a) or in the entry rate (Figure 1b) immediately after 1985. This lack of an immediate effect on startup activity is most likely due to a provision in the legislation that non-competes in effect at the time of repeal remained unenforceable. Because of this, the number of employees in Michigan who were actually subject to enforcement was relatively small for a significant period of time following the passage of MARA. In December 1987, the reasonableness standard was made retroactive to the 1985

[9] In Section 5, we use the SCM as an alternative approach for selecting a control group.
[10] To conserve space, figures for the entry rate are not presented for either the Expanded Border States or the Non-Enforcement States. The trends for the entry rate are similar to those shown for the job creation rate. These additional figures are available upon request.

passage of MARA. Given these legislative provisions, it seems reasonable to expect that startup

activity post-MARA would initially continue to closely track that of the other control groups, but

eventually start to diverge from counterfactual Michigan. However, Figure 1a provides little

evidence that startup activity in Michigan diverged relative to the dynamic path of the other

control groups. This visual inspection of the data suggests that MARA had little or no effect on

startup activity in Michigan, relative to the control groups.

 We turn to the DID analysis to see if it reveals a similar lack of an enforcement effect on startup

activity.

FINDINGS

Our analysis is at the state level, since non-compete legislation is determined at the state level,

and as such, non-competes' occurrence and enforcement will vary across states. As indicated, a

number of different groupings of the states are used to construct the various comparison groups.

The alternative control groups are used in a DID framework during the period 1977 to 2013 to

estimate the causal effects of enforcement on startup activity. The null hypothesis we test is:

H_0 : *The Passage of MARA did not affect startup activity in Michigan relative to the control group*

H_A : *The passage of MARA did have an effect on startup activity in Michigan relative to the control group*

Table 1a presents summary statistics for Michigan, and Table 1b presents these statistics for

states other than Michigan. The panel on the left side of the tables shows the summary statistics

for the pre-MARA period, while the panel on the right shows these statistics for the post-MARA

period. The tables show that startup activity has been declining over time. In Michigan, the mean

job creation rate fell from just over 3 percent pre-MARA to about 2.5 percent post-MARA. In the nation, the average job creation rate fell from 4.4 percent pre-MARA to just under 3 percent post-MARA. As the tables show, the entry rate also fell post-MARA relative to the pre-MARA period in both Michigan and the nation.

Table 2 summarizes the findings of the DID analysis. The first two columns of Table 2 present the findings for job creation and the startup entry rate, respectively, when the control group consists of the states sharing a land border with Michigan. The next two columns in Table 2 give the findings relative to a counterfactual based on the Expanded Border States, while the final two columns show the results for the Non-Enforcement States.

The results of interest are given by the interaction of the Michigan dummy variable and a dummy variable for the post-MARA period (shown in the third row of Table 2). Recall that pre-MARA refers to the period 1977 – 1984 and the post-MARA period is 1985 – 2013. Beginning with the job creation rate for Michigan relative to its land neighbors, the coefficient on the interaction of Michigan and the post Michigan indicator is positive for the job creation rate, but it's not statistically significant. Using the Expanded Border States as the control group, we find a positive and statistically significant effect for the job creation rate, suggesting that a doubling in enforcement results in a 6.2 percent increase in job creation in Michigan, relative to counterfactual Michigan. Similarly, we find that a doubling in enforcement leads to a 7.8 percent increase in the job creation rate by startups relative to a counterfactual based on the Non-Enforcement States.

Turning to the startup entry rate, the estimated coefficient on the interaction between the Michigan dummy variable and the dummy variable for the post-MARA period is not statistically significant for any of the alternative control groups considered.

Taken together, the findings summarized in Table 2 suggest that increased enforcement of non-competes had no effect on the entry rate of startups, but had a positive effect on jobs created by these startups in Michigan relative to a counterfactual of Non-Enforcement States. Importantly, we find little support for the widely held view that enforcement of non-compete agreements negatively affects the entry rate of new firms or the rate of jobs created by new firms.

SYNTHETIC CONTROLS

An important requirement of the DID approach is that, in the absence of treatment, the outcomes for the treated and control groups follow parallel trends through time (i.e., the effects of the unobserved variables are fixed over time). Thus, the parallel-trends requirement implies that without treatment, the outcomes of interest for the treated and control groups are expected to evolve at the same rate. However, it is likely that many of the unobserved variables may have time-varying effects on the outcomes of interest. This could be one reason that startup activity in Michigan post-MARA fails to grow faster than that in the states sharing a land border with Michigan, for example. The SCM developed by Abadie, et al. (2010) is an alternative method that accounts for the effects of confounders changing over time. The SCM is a data-driven search routine designed to construct a comparison group based on pre-treatment economic and demographic trends. In our application, the SCM is a technique for constructing a counterfactual or "Synthetic Michigan" based on a linear combination of algorithmically derived weights assigned to the most representative or most similar states (using all 49 other states) that did not

receive the treatment. For our purposes, the SCM matches Michigan to potential candidate states having comparable pre-treatment-period predictor variables. The weighting assigned to each predictor variable is determined by regression analysis that minimizes the root mean square prediction error (RMSPE) in the pre-treated sample period. Importantly, the SCM is used to forecast a counterfactual post-MARA path for Synthetic Michigan.

Our objective is the construction of a Synthetic Michigan prior to 1985 based on a composite of all U.S. states.[11] For each outcome variable, we search for a synthetic match using the covariates given in first column of Table 1. Table 3 gives the states and their relative weights used to construct the synthetic control groups. Four states — Illinois, Ohio, Pennsylvania, and West Virginia — received positive weights in the construction of a Synthetic Michigan when the jobcreation rate is the outcome of interest. Two states — Ohio and Pennsylvania — contribute to the synthetic control group when the startup entry rate is the outcome of interest. As Table 3 shows, for example, Ohio receives the highest weight for both outcome variables, ranging from 0.58 to 0.87. Figure 2 shows a graph of outcome variables for Synthetic Michigan (the broken line) juxtaposed with the graph of actual outcomes in Michigan. The figure reveals that Michigan and its synthetic track one another tightly during the pre-treatment period both for the job creation rate (Figure 2a) and for the startup entry rate (Figure 2b).

Figure 2 shows there was not much change in either the job creation rate (Figure 2a) or the startup entry rate (Figure 2b) immediately following the passage of MARA, relative to Synthetic Michigan. As already mentioned, this lack of an immediate effect on startup activity is most likely due to a provision in the legislation that all statutes that were repealed by MARA would

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[11] We use STATA Synth's procedure developed by Abadie, et al. (2010) to conduct the synthetic control analysis.

remain in force. Given these legislative provisions, we expect that startup activity post-MARA would initially continue to closely track Synthetic Michigan. But we also expect that, over time, startup activity in Michigan would diverge from the path of its synthetic. This is in fact what we observe. Both the job rate (Figure 2a) and the entry rate (Figure 2b) in Michigan increase relative to the synthetic control starting in about the mid-1990s.

The Synthetic Control Method uses a simple DID estimator:

$$DID_{MI} = (\overline{Y}_{Post}^{MI} - \overline{Y}_{Pre}^{MI}) - (\overline{Y}_{Post}^{C} - \overline{Y}_{Pre}^{C}) = \Delta\overline{Y}_{treatment} - \Delta\overline{Y}_{control},$$

where $\overline{Y}_{Pre}^{MI}$ represents the sample average of Y in Michigan, *MI*, before treatment, *Pre*. Similarly, $\overline{Y}_{Post}^{MI}$ is the sample average for *MI* after treatment, *Post*. Correspondingly, $\overline{Y}_{Pre}^{C}$ and $\overline{Y}_{Post}^{C}$ represent averages of Y pre- and post-treatment for the control group, C.

Table 4 presents the estimates of the DID analysis both for the startup entry rate and for the startup job creation rate using Synthetic Michigan as the control group. For each of the outcomes considered, the first column presents the average difference between Michigan and Synthetic Michigan during the pre-treatment period. The second column gives the mean difference during the post-treatment period, while the third column presents the DID estimates of the effect of MARA on startup activity.

Starting with the job creation rate, the first column of Table 4 shows a pre-MARA difference between Michigan and its counterfactual of 0.067 percent. The gap between Michigan and Synthetic Michigan widens to 0.125 percent during the post-MARA period, producing a DID estimate of 0.0575 percent. This result is similar to what we found for the regression-based DID analysis reported in Table 2. Specifically, a doubling of enforcement leads approximately to a 6

percent increase jobs created by startups. The last row of Table 4 shows the findings for the entry rate. Once again, we find that increased enforcement had essentially no effect on the entry rate of startups in Michigan.

To formally test the significance of the DID estimates, we follow Abadie, et al. (2010) and use a placebo test (or a falsification test) where the treatment is applied iteratively to each of the states. The fourth column of Table 4 shows the rank of the estimates and the *p*-values of the post-treatment change for Michigan relative to the distribution of all other U.S. states taken from the placebo tests. The final column in Table 4 presents the pre-treatment RMSPE. Based on Michigan's placebo ranking shown in the fourth column of Table 4, the DID-estimated coefficient for the job creation rate is not significantly different from zero. The last row of Table 4 shows that the DID estimate for the startup entry rate is small and not significantly different from zero.

To summarize, our findings based on the SCM also offer little support for the widely held view that enforcement of non-compete agreements negatively affects startup activity. If anything, increased enforcement appears to have had positive effects on the job creation rate of startups in Michigan, although placebo tests are not statistically significant.

6. HIGH-TECH EMPLOYMENT AND ESTABLISHMENTS

High-technology businesses are often considered pillars of growth for regions and the nation. High-tech activity is associated with high-value-added production, highly skilled workers, and relatively high wages. Many cities and states view high-tech activity as a source of growth and have offered generous subsidies to attract high-tech firms to their locations. A prominent

example is the 238 city leaders who offered subsidies, some quite substantial, for Amazon's second headquarters.

Many occupations in high-tech firms tend to be disproportionately covered by non-compete agreements. For example, Starr, et al. (2020) reported that more than 40 percent of electrical and electronics engineers were covered by non-competes. Because of both the increased coverage of non-competes for workers in the high-tech industry and policy considerations, it seems reasonable to consider the effect of non-compete agreements on high-tech activity.

Unfortunately, official statistics identifying high-tech activity do not exist and must be estimated. According to Goldschlag and Miranda (2020), "[a]n interagency workshop held by U.S. statistical agencies in 2004 identified a set of important factors that contribute to the concept of High Tech. These include disproportionately high employment of STEM workers, disproportionately high employment of R&D workers and capital, the production of High Tech products, and the use of High Tech production methods, including the use of High Tech capital goods and services." Implementing this definition in practice requires identifying economic activity based on the use of high-tech workers, or the production of high-tech goods or services.

Most attempts to estimate high-tech industries are based on input rather than on output.[12] For the nation, Hecker (1999) identified a list of "High-Tech" three-digit SIC industries using employment in both R&D and technology-oriented occupations as reported in the Occupational Employment Statistics (OES) surveys (now referred to as STEM occupations). Based on OES surveys, Hecker identified 29 three-digit SIC industries in which the number of R&D workers and technology-oriented occupations accounted for a proportion of employment that was at least

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[12] See Goldschlag and Miranda (2020) for a review of literature identifying high-tech activity based on the industry's use of input versus industrial output.

twice the average for all industries surveyed. We apply Hecker's 29 industries to state-level employment and establishment data found in County Business Patterns for the period 1977–2013.[13] Figure 3a shows the series for high-tech employment in Michigan and the 10 Non-Enforcement States, while Figure 3b shows the time series for establishments. As the figures show, other than the level, each state in this control group more or less tracks movements in Michigan for both the jobs in high-tech and the number of high-tech establishments during the pre-MARA period, indicating these untreated states provide an appropriate "counterfactual Michigan."[14]

High-tech is an important source of employment in Michigan, averaging almost 400,000 jobs during the period 1977-2013, second only to California among the 10 Non-Enforcement States. In terms of the share of total employment accounted for by high-tech jobs, this share averaged 11.8 percent in Michigan, compared with an average rate of 7.5 percent for the nation. The share of employment accounted for by high-tech is almost 60 percent greater in Michigan than the average state.

Table 5 summarizes our findings of the regression-based DID analysis. The first two columns of Table 5 present the findings for high-tech employment and high-tech establishment, respectively, when the control group consists of the states sharing a land border with Michigan. The next two columns in Table 5 give the findings relative to a counterfactual based on the Expanded Border States, while the final two columns show the results for the Non-Enforcement States.

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[13] See Table 1 in Hecker (1999) for a list of the 29 three-digit SIC code industries designated high-tech activity used in this study.

[14] The 29 SIC codes used in this study were converted to the 1997 edition of NAICS by the Office of Technology Policy and the Census Bureau. These NAICS codes are reported in Table Appendix B in Goldschlag and Miranda (2016). We found no appreciable difference in the findings when the NAICS definitions are used instead of the SIC code definitions. The NAICS findings are available upon request.

The results of interest are given by the interaction of a Michigan dummy variable and a dummy variable for the post-MARA period (shown in the third row of Table 5). Beginning with the creation of high-tech jobs in Michigan relative to the states sharing a land border with Michigan, the coefficient on the interaction of Michigan and the post-Michigan indicator is negative, but insignificant for high-tech employment and high-tech establishments. Using the Expanded Border States as the control, we find a positive effect for both the job creation rate and for the startup entry rate, but neither coefficient is statistically significant. We find positive and statistically significant effects of enforcement on both high-tech jobs and establishments when counterfactual Michigan is based on the Non-Enforcement States.

Taken together, the findings summarized in Table 5 suggest that increased enforcement of non-competes had no effect on either high-tech jobs or high-tech establishments in Michigan relative to a counterfactual of states sharing borders with Michigan. Only when we use the alternative definitions of counterfactual Michigan based on the Non-Enforcement States do we find any evidence of a positive and statistically significant effect of increased enforcement on high-tech activity. Specifically, we find that a 10 percent increase in enforcement led to a 5 percent increase in high-tech jobs and to a 4 percent increase in the number of high-tech firms.

Table 6 presents the states and relative weights used to construct "Synthetic Michigan" for high-tech activity. Figures 3c and 3d show a graph of high-tech variables for Synthetic Michigan (the broken line) juxtaposed with the graph of actual outcomes in Michigan. The figures reveal that Michigan and its synthetic track one another tightly during the pre-treatment period, both for employment (Figure 3c) and for establishments (Figure 3d).

Table 7 presents the estimates of the DID analysis for the high-tech employment and high-tech establishments using Synthetic Michigan as the control group. The second column of the table gives the mean difference during the post-treatment period, while the third column presents the DID estimates of the effect of MARA on high-tech activity. The rank and *p*-values associated with the placebo tests are shown in the fourth column of Table 7. Based on Michigan's placebo ranking shown in the fourth column of Table 7, the DID-estimated coefficient is not significantly different from zero.

7. PATENTS AND PATENT CITATIONS

So far, we have considered the effects of enforcement on aggregate startup activity. However, enforcement may have differential effects on startups across industries, and these effects may get muted in the aggregation of industry-level startup to total startup activity. Employers are much more likely to be concerned with mobility of their employees to other firms in the same industry than they are with employees changing industries altogether. In interviews with 52 randomly sampled patent holders in a single industry, Marx (2011) found that 25 percent of those who signed non-compete agreements changed industries when changing jobs. In comparison, individuals not covered by such covenants were significantly less likely to change industries when changing jobs. Marx's finding indicates that we need industrial-level data to more fully address this question of whether and to what extent non-compete covenants influence startup activity. Barnett and Sichelman (2016) point out that "no state enforces non-competes that

purport to proscribe employment at non-competing firms." Unfortunately, the Census Bureau does not make industry-level data on startup activity publicly available.[15]

Fortunately, patent data can be used to study entrepreneurial activity and are available by technology classifications. In this section, we consider the effects of enforcement of patent activity in Michigan for total patents per 10,000 people (referred to as patents per capita) and for six patent technology classifications identified by Hall, et al. (2001). The null hypothesis we test is:

> H_0 : *The Passage of MARA did not affect patents per capita or patent citations per capita in Michigan relative to the control group*
>
> H_A : *The passage of MARA did have an effect on patents per capita or patent citations per capita in Michigan relative to the control group.*

7.1 Patent Data

We use data on patent applications obtained from the NBER Patent Data Project. The data span the years 1976–2006.[16] Hall, et al. (2001) aggregated various patent classes into six main technological categories: Chemical (excluding Drugs); Computers and Communications; Drugs and Medical; Electrical and Electronics; Mechanical; and Others. Figure 4 shows the path of total patents per capita and for the six technological classifications in Michigan. For total patents, we see that there were less than three patents per 10,000 people in Michigan between 1977 through the late 1980s. Patents per capita started rising in the mid-1990s to reach a peak of 4.4

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[15] Starr, et al. (2020) report the results for a 2014 survey they conducted finding that incidence of non-compete varies across industries: The percentage of workers covered by such covenants ranged from 9 percent in agriculture and hunting, to over 30 percent in information; mining and extraction; and professional and scientific.
[16] U.S. Patent and Trademark Office. Overview of the U.S. Patent Classification System (USPC). Washington, D.C. (2012), http://www.uspto.gov/patents/resources/classification/overview.pdf.

patents per capita in 2003 before declining to a value around 3.6 in 2006. Figure 4 shows that Mechanical patents were a major contributor to the run-up in total patents per capita observed since the late 1990s. There was less than 1 Mechanical patent per 10,000 people in Michigan during the period 1977–1988. Mechanical patents per capita started rising more rapidly in the late 1990s and reach a peak of 1.7 patents per capita during the period 2001–2004 before declining a bit after that. Mechanical patents accounted for 30 percent of total patents in Michigan in the mid-1970s to the early 1980s. That share steadily rose to account for 42 percent of all Michigan's patents in 2006. During our sample period, Mechanical patents on average accounted for 35 percent of total Michigan patents, compared with 22 percent for the average state. The Chemical and the Others categories accounted for 19 percent and 22 percent of Michigan patents, respectively, during our sample period, whereas the Drugs category accounted for only 5 percent.

One concern about using patents as an innovation indicator is that the value of patents is highly skewed. Most patents are not worth much, while a few are valuable (e.g., Harhoff, et al., 1999). If a patent has value, we would expect it to be renewed before the patent expires. Serrano (2010) calculates that 78 percent of U.S. patents granted during 1983–2001 were not renewed, indicating that most patents are of low value. Fortunately, researchers can adjust for patent quality in their innovation metrics by weighting patents by the number of citations they receive. In the analysis, we exclude self-citations from these counts (i.e., a Microsoft patent that cites another Microsoft patent). In the analysis that follows, we look at both patents per capita and citations-weighted patents per capita. The regressions we use in the patent analysis are similar to the regressions used for startup activity as summarized in Table 2, except patents (citation-weighted patents) replace the startup job (entry) rate as the dependent variable.

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Table 8 summarizes our findings by technology category when Non-Enforcement States is the control group.[17] The first two columns of Table 8 present the findings for total private patents per capita and for the citation-weighted version. Subsequent columns in Table 8 show the results for the six major technological categories we investigate. Starting with total private patents per capita, the coefficient of interest (the estimated coefficient on interaction of Michigan and the post-MARA indicator) is positive and significant for total patents. Regarding the subcategories, the coefficient on patents per capita is negative in three cases and negative and significant in one case (Drugs). We also find that the estimated coefficient on patents per capita is positive for three categories and positive and significant for two classifications (Mechanical and Other).

Considering citations, we find that citation-weighted patents per capita are negative but insignificant for total patents per capita. For the subcategories, the coefficient on citations per capita is negative in four cases and significantly negative for Computer patents. The coefficient is positive in two cases and significantly positive for Mechanical patents.

Table 9 presents the estimates of the DID analysis for the citation-weighted patents using the SCM to form the control group. For each of the technological categories considered, the first column presents the average difference between Michigan and Synthetic Michigan during the pre-treatment period. The second column gives the mean difference during the post-treatment period, while the third column presents the DID estimates of the effect of MARA for the various technological categories considered. The rank and *p*-values associated with the placebo tests are shown in the fourth column of Table 9.

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[17] To conserve space, we present only tables with Non-Enforcement States as the comparison group.

The most statistically significant results are found for Mechanical patents. Table 9 shows a pre-MARA difference between Michigan and its counterfactual of 0.2176 percent. The gap between Michigan and Synthetic Michigan widens to 0.3747 percent during the post-MARA period, producing a DID estimate of 0.1570 percent. This estimate is quite similar to the 0.1899 estimate reported in Table 8 for Mechanical patents. Based on Michigan's placebo ranking for Mechanical patents shown in the fourth column of Table 9, the DID-estimated coefficient is marginally significantly different from zero. The *p*-values associated with Michigan's relative rank in the distribution of placebo states shows, the DID estimate is not significantly different from zero for the other five technology categories, as well as for the total category. Still, the finding of a positive and significant effect of increased enforcement on Mechanical patents is important given that Mechanical patents in 2006 accounted for more than 40 percent of all patent activity in Michigan.

8. CONCLUSION

In this study, we considered how state-level enforcement of non-compete agreements affects the entry of new establishments and the jobs created by these new firms at the state level. Exploiting Michigan's inadvertent reversal of its enforcement policy as a quasi-natural experiment, we find no evidence that increased enforcement negatively affected the aggregate startup entry rate or the overall job creation rate of new firms. If anything, increased enforcement appears to have had positive effects on the job creation rate of startups in Michigan. In a standard DID analysis, we find that a doubling in enforcement led to a 6 percent to 8 percent increase in the startup job creation rate in Michigan and to essentially no change the startup entry rate.

We also find evidence that enforcing non-competes positively affected the number of high-tech establishments and the level of high-tech employment in Michigan.

Extending our analysis to consider the effect of increased enforcement on patent activity, we find that enforcement had differential effects across technological classification. Importantly, increased enforcement has a positive and significant effect on Mechanical patents in Michigan. The Mechanical category is an important technological classification in Michigan: In 2006, it accounted for over 40 percent of the state's total patent applications.

While our findings suggest that increased enforcement may promote job creation, there is room for future research. Our findings for patents suggest that enforcement may have differential effects on startup activity across industries. Yet, only researchers with projects approved by the Census Bureau can access the industry-level data. This is one area for future research.

An interesting question is how does enforceability affect productivity of cities and ultimately of the nation? Cities may be less productive and less innovative if enforcement limits employees' outside options, even if enforcement does not limit startup activity. For example, knowledge spillovers may be limited if employees feel constrained by non-compete agreements from exchanging information with outsiders.

Patent data could be used to shed light on this issue. Often, patent citations are used to measure the extent of localized knowledge spillovers (see Jaffe, et al., 1993, and Buzard, et al., 2017). Using patent-citation data, Singh and Marx (2013) provide tantalizing evidence that knowledge diffusion is subdued in regions where non-competes are enforceable. Still, more work needs to be done.

A related issue is whether the ability of firms and workers to form better matches is constrained in local labor markets that enforce non-competes. Reduced "job-hopping" resulting from non-competes is a concern if reduced churning lowers labor productivity through less-efficient matching among firms and workers. Davis and Haltiwanger (2015) report that job seekers have fewer opportunities to meet prospective employers if startup activity is less fluid. Similarly, non-competes also may limit labor market pooling by tying workers to their current employers and by giving rise to a "brain-drain" from enforcing to non-enforcing states (for a review of the evidence, see Marx, et al., 2015). Policymakers need to not only balance the interest of firms and workers, but also consider the broader issues associated with the effects of non-competes on the productivity and the growth of cities.

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	OBS.	MEAN	SD	MIN	MAX		OBS.	MEAN	SD	MIN	MAX
Job Creation Rate	8	0.0313	0.0071	0.0256	0.0478		29	0.0258	0.0042	0.0176	0.0332
Est. Entry Rate	8	0.1169	0.0163	0.1013	0.1486		29	0.0895	0.0150	0.0628	0.1215
Share Agri.	8	0.0024	0.0003	0.0020	0.0027		29	0.0053	0.0010	0.0030	0.0066
Share Mining/Extraction	8	0.0440	0.0048	0.0375	0.0497		29	0.0457	0.0124	0.0258	0.0720
Share Light Mfg.	8	0.0742	0.0023	0.0723	0.0793		29	0.0568	0.0094	0.0439	0.0702
Share Heavy Mfg.	8	0.2754	0.0304	0.2359	0.3136		29	0.1915	0.0248	0.1418	0.2436
Share Trans./Communications	8	0.0516	0.0012	0.0493	0.0527		29	0.0254	0.0214	0.0048	0.0511
Share Trade	8	0.2629	0.0078	0.2533	0.2745		29	0.3551	0.1020	0.2146	0.4641
Share Depository Inst.	8	0.0577	0.0044	0.0526	0.0636		29	0.0523	0.0099	0.0362	0.0671
Share Services	8	0.0802	0.0058	0.0750	0.0907		29	0.0846	0.0317	0.0468	0.1450
Share Health/Legal/Ed. Services	8	0.1516	0.0182	0.1291	0.1728		29	0.1834	0.0290	0.1338	0.2286
Share Pop Aged 15 - 64	8	0.6428	0.0271	0.6101	0.6625		29	0.6612	0.0034	0.6590	0.6691
Percent College Grad	8	0.1247	0.0252	0.0943	0.1430		29	0.1942	0.0362	0.1430	0.2520
Unemployment Rate	8	0.1109	0.0312	0.0696	0.1537		29	0.0742	0.0024	0.0366	0.1378
Labor Force Part. Rate	8	0.6363	0.0046	0.6281	0.6433		29	0.6505	0.0230	0.6001	0.6871
Real Per Capita GSP Growth	8	0.0153	0.0350	-0.0343	0.0654		29	0.0138	0.0226	-0.0465	0.0489
Pop Growth	8	-0.0009	0.0059	-0.0102	0.0051		29	0.0031	0.0040	-0.0054	0.0095
Patents Per 10,000 People	8	2.2039	0.2911	1.8488	2.5161		29	3.3178	0.6826	2.2763	5.1695
Citations per 10,000 People	8	23.6368	2.4554	19.6388	28.5580		21	25.106	11.7822	0.1335	35.9402

TABLE 1a: Summary Statistics for Michigan

Michigan, 1977−1984 — Michigan, 1985−2013

	TABLE 1b: Summary Statistics for All Other, Excluding Michigan									
	States Other than Michigan, 1977–1984						States Other than Michigan, 1985–2013			
	OBS.	MEAN	SD	MIN	MAX		OBS.	MEAN	SD	MIN	MAX
Job Creation Rate	392	0.0445	0.0160	0.0218	0.1145		1421	0.0295	0.0085	0.0128	0.0989
Est. Entry Rate	392	0.1332	0.0285	0.0885	0.2423		1421	0.0981	0.0293	0.0508	0.1802
Share Agri.	392	0.0044	0.0019	0.0018	0.0104		1421	0.0067	0.0020	0.0027	0.0177
Share Mining/Extraction	392	0.0866	0.0489	0.0331	0.3248		1421	0.0705	0.0332	0.0230	0.2558
Share Light Mfg.	392	0.1122	0.0589	0.0194	0.3326		1421	0.0712	0.0368	0.0118	0.2621
Share Heavy Mfg.	392	0.1350	0.0682	0.0057	0.3184		1421	0.1003	0.0457	0.0054	0.2497
Share Trans./Communications	392	0.0639	0.01538	0.0368	0.1458		1421	0.0341	0.0286	0.0034	0.1378
Share Trade	392	0.2867	0.0345	0.2264	0.4042		1421	0.3688	0.1108	0.1621	0.5703
Share Depository Inst.	392	0.0677	0.0136	0.0439	0.1221		1421	0.0612	0.0189	0.0256	0.1683
Share Services	392	0.0981	0.0509	0.0529	0.4254		1421	0.0956	0.0556	0.0369	0.4279
Share Health/Legal/Ed. Services	392	0.1455	0.0297	0.0728	0.2266		1421	0.1917	0.0399	0.0660	0.3154
Share Pop Aged 15 - 64	392	0.6403	0.2850	0.5825	0.7018		1421	0.6594	0.0175	0.6012	0.7034
Percent College Grad	392	0.1406	0.0379	0.0662	0.2300		1421	0.2187	0.0533	0.1040	0.3900
Unemployment Rate	392	0.0709	0.0221	0.0276	0.1779		1421	0.0574	0.0190	0.0230	0.1353
Labor Force Part. Rate	392	0.6451	0.0381	0.5097	0.7332		1421	0.6687	0.0401	0.5136	0.7537
Real Per Capita GSP Growth	392	0.0200	0.0299	-0.1383	0.1926		1421	0.0185	0.0238	-0.1022	0.1344
Pop Growth	392	0.0122	0.0134	-0.0098	0.0845		1421	0.0099	0.0097	-0.0598	0.0739
Patents Per 10,000 People	392	1.4950	2.7115	0.1173	21.0235		1029	2.2927	3.7465	0.0930	30.4258
Citations Per 10,000 People	392	14.6505	25.3367	0.6961	197.7697		1029	18.0755	30.3086	0	260.4539

	Table 2: Estimated Effects of the Michigan Antitrust Reform Act on Startup Entry Rate and the Job Creation Rate, 1977–2013[†]					
	States Sharing a Land Border with Michigan[††]		States Sharing a Land or Water Border with Michigan[†††]		Non-Enforcement States[††††]	
	Job Rate	Entry Rate	Job Rate	Entry Rate	Job Rate	Entry Rate
Michigan	0.1691 (0.0405)***	0.0842 (0.0175)***	0.0933 (0.0301)***	0.0432 (0.0156)***	0.1118 (0.0593)*	0.0673 (0.0410)*
Post-MARA	-0.3815 (0.2973)	-0.5477 (0.1071)***	-0.5682 (0.1785)***	-0.9266 (0.1040)***	-0.9465 (0.1043)***	-0.5570 (0.0825)***
Mich.*Post-MARA	**0.0172 (0.0331)**	**-0.0074 (0.0163)**	**0.0623 (0.0281)****	**0.0005 (0.0139)**	**0.0780 (0.0405)****	**0.0054 (0.0265)**
Percent College Grad	-0.0075 (0.0156)	0.0013 (0.0068)	0.0143 (0.0060)**	0.0212 (0.0035)***	0.0029 (0.0047)	-0.0018 (0.0037)
Unemployment Rate	0.0016 (0.0094)	0.0076 (0.0045)*	-0.0007 (0.0078)	0.0139 (0.0038)	0.0095 (0.0066)	0.0177 (0.0048)***
Labor Force Part. Rate	-0.2000 (0.0062)***	-0.0104 (0.0029)***	-0.0105 (0.0037)***	-0.0041 (0.0021)*	-0.0065 (0.0027)**	0.0026 (0.0018)
Real Per Capita Income (levels)	0.2581 (0.6967)	-0.0605 (0.2857)	-0.3172 (0.5026)	-0.0124 (0.2729)	0.1530 (0.2240)	-0.0778 (0.1627)
Share Pop age 15 to 64	-0.0944 (0.0205)***	-0.0122 (0.0076)	-0.0564 (0.0146)***	0.0111 (0.3893)	-0.0527 (0.0069)***	-0.0196 (0.0049)***
Pop Growth (levels)	2.5810 (2.8308)	3.5853 (1.5780)**	3.5763 (2.2605)	5.1755 (1. 1003)***	5.4376 (0. 7615)***	5.4120 (0.5667)***
Constant	2.60 (4.0200)	-3.83 (1.8791)**	3.85 (1.8259)**	-3.71 (0.7242)	4.35 (0.8405)***	0.44 (0.6187)
No. of Obs.	148	148	222	222	406	406
R^2	0.9712	0.9929	0.9523	0.9793	0.9006	0.9092

[†]Robust standard errors in parentheses.

*,**,*** Represent statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

[††]States Sharing a Land Border with Michigan are IN, OH, and WI.

[†††]States sharing a land border or a water border with Michigan are IN, OH, WI, IL, and MN.

[††††]Limited Non-Enforcement States are AK, CA, CT, MN, MT, NV, ND, OK, WA, and WV.

All regressions include one-digit SIC industry share of total employment and year fixed-effects.

Table 3: States Receiving Positive Weights for the Synthetic Control Group Startup Activity	
Job Rate	**Entry Rate**
Illinois = 0.264	
Ohio = 0.576	Ohio = 0.865
Pennsylvania = 0.126	Pennsylvania = 0.135
W. Virginia = 0.034	

Table 4: Estimated Effects of Change in Michigan's Non-Compete Enforcement on the Startup Entry Rate and the Startup Job Creation Rate Relative to Synthetic Michigan

	Average Difference Relative to Control Group Pre-Treatment Period	**Average Difference Relative to Control Group Post-Treatment Period**	**Change Post-Treatment**	**Rank**	**Pre-Treatment Period RMSPE**
High-Tech Jobs	0.0671	0.1245	0.0575	14/50 $p = 0.28$	0.0925
High-Tech Firms	0.0873	0.0872	0.0001	12/50 $p = 0.48$	0.0877

	States Sharing a Land Border with Michigan[††]		States Sharing a Land or Water Border with Michigan[†††]		Non-Enforcement States[††††]	
	JOBS	FIRMS	JOBS	FIRMS	JOBS	FIRMS
Michigan	0.0063 (0.0601)	-0. 1365 (0.0661)**	-0.1582 (0.0851)*	-0.2732 (0.0963)***	-0. 2565 (0.3764)	0.1437 (0. 3190)
Post-MARA	-2.2494 (0.4585)***	-1.2834 (0.5292)**	-2.4802 (0.3988)***	-2.1529 (0.4060)***	1.9780 (0.5079)***	-2.5906 (0.4867)***
Mich.*Post-MARA	**-0.0714 (0.0516)**	**-0.0127 (0.0536)**	**0.0651 (0.0911)**	**0.0692 (0.1009)**	**0.4952 (0.2181)****	**0.4032 (0.1257)****
Percent College Grad	0.0836 (0.0255)***	0.1069 (0.0280)***	0.0171 (0.0143)	0.0391 (0.0153)**	-0.0384 (0.0252)	-0.0124 (0.0246)
Unemployment Rate	-0.0185 (0.0144)	-0.0230 (0.0158)	0.0181 (0.0186)	0.0178 (0.0210)	0.1491 (0.0350)***	0.1316 (0.0320)***
Labor Force Part. Rate	0.3778 (0.0098)***	0.0480 (0.0107)***	-0.0622 (0.0111)***	-0.0654 (0.0119)***	0.0170 (0.0158)	-0.0070 (0.0146)
Real Per Capita Income Growth (Levels)	0.2657 (0.9062)	0.4382 (1.0681)	0.3389 (1.1932)	0.7927 (1.3388)	-1.0653 (1.2300)	-1.2964 (1.1968)
Share Pop age 15 to 64	0.0238 (0.0345)	0.0365 (0.0387)	0.2113 (0.0350)***	0.2001 (0.0361)***	0.0144 (0,0328)	-0.0244 (0.0294)
Pop Growth (levels)	-5.2326 (7.6608)	-7.1501 (4.8486)	-14.7027 (5.9348)**	-22.2128 (6.5415)***	24.4059 (4.7466)***	23.0728 (4.5025)***
Constant	14.30 (5,6513)**	23.89 (6.6384)***	10.21 (4.3542)**	8.48 (4.6874)*	-16.37 (3.9803)***	-8.16 (3.7040)**
No. of Obs.	148	148	222	222	406	406
R^2	0.9755	0.9867	0.9129	0.9529	0.8260	0.7755

Table 5: Estimated Effects of the Michigan Antitrust Reform Act on High-Tech Employment and the High-Tech Firms, 1977–2013[†]

[†]Robust standard errors in parentheses.

*,**,*** Represent Statistical Significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

[††]Shares a land border with Michigan: IN, OH, WI.

[†††]Shares a land or a water border: IL, IN, OH, MN, WI.

[††††]Non-Enforcement States are AK, CA, CT, MN, MT, NV, ND, OK, WA, and WV.

All regressions include one-digit SIC industry share of total employment and year fixed-effects.

Table 6: States Receiving Positive Weights for the Synthetic Control Group	
High-Tech Activity	
Job Rate	**Entry Rate**
Indiana = 0.156	Indiana = 0.119
Ohio = 0.828	Ohio = 0.805
W. Virginia = 0.016	W. Virginia = 0.076

Table 7: Estimated Effects of Change in Michigan's Non-Compete Enforcement on High-Tech Entry Rate and Job Creation Rate Relative to Synthetic Michigan

	Average Difference Relative to Control Group Pre-Treatment Period	**Average Difference Relative to Control Group Post-Treatment Period**	**Change Post-Treatment**	**Rank**	**Pre-Treatment Period RMSPE**
High-Tech Jobs	0.1797	0.1514	-0.0283	22/50 $p = 0.44$	0.1892
High-Tech Firms	0.0871	0.1395	0.0524	21/50 $p = 0.42$	0.0875

	Total Private Patents Per Capita		Private Chemical Patents Per Capita		Private Computer Patents Per Capita		Private Drugs Patents Per Capita	
	Patents	Citations	Patents	Citations	Patents	Citations	Patents	Citations
Michigan	-0.0460 (0.0386)	-0.7063 (0.3882)*	-0.0171 (0.0105)*	-0.1792 (0.0868)**	-0.0104 (0.0117)	0.0799 (0.1348)	-0.0195 (0.0072)***	-0.4118 (0.1060)***
Post-MARA	0.0232 (0.0796)	-0.7564 (0.8233)	-0.0183 (0.0226)	-0.0653 (0.2001)	-0.0536 (0.0258)	-0.5262 (0.2673)**	0.0024 (0.0171)	-0.1691 (0.1818)
Mich.*Post-MARA	**0.0537 (0.0249)****	**-0.2094 (0.2425)**	**-0.0041 (0.0077)**	**-0.0696 (0.0651)**	**-0.0056 (0.0064)**	**-0.2635 (0.0957)****	**-0.0076 (0.0039)****	**-0.1364 (0.0708)**
Share of High-Tech Jobs	0.0003 (0.0091)	0.0893 (0.0827)	-0.0100 (0.0017)***	-0.0588 (0.0161)***	0.0150 (0.0039)***	0.0948 (0.0383)**	0.0072 (0.0013)***	0.1455 (0.0224)***
Percent College Grad	0.0173 (0.0041)***	-0.0641 (0.0354)*	-0.0039 (0.0012)***	-0.0136 (0.0088)	0.0085 (0.0014)***	0.0126 (0.0128)	0.0027 (0.0007)***	0.0017 (0.0090)
Unemployment Rate	-0.0092 (0.0044)**	-0.1014 (0.0443)**	-0.0037 (0.0014)**	-0.0351 (0.0122)**	0.0003 (0.0011)	-0.0087 (0.0136)	0.0004 (0.0006)	-0.053 (0.0104)
Labor Force Part. Rate	-0.0033 (0.0022)	0.0683 (0.0211)***	-0.0014 (0.0006)**	0.0089 (0.0045)**	-0.0034 (0.0007)***	-0.0131 (0.0075)*	0.0014 (0.0006)**	0.0381 (0.0073)***
Real Per Capita Income (levels)	0.1762 (0.1570)	2.9400 (1.7308)*	0.0400 (0.0365)	0.5564 (0.3448)	0.0462 (0.0456)	0.6155 (0.5535)	0.0137 (0.0414)	0.9758 (0.6446)
Share Pop age 15 to 64	0.0152 (0.0055)**	0.3647 (0.0484)***	0.0042 (0.0014)***	0.0719 (0.0128)***	0.0008 (0.0016)	0.0804 (0.0174)***	-0.0007 (0.0009)	0.0129 (0.0114)
Pop Growth (levels)	-0.5240 (0.3913)	-10.8540 (5.2508)**	-0.1769 (0.1204)	-2.6602 (1.1868)**	-0.1370 (0.1246)	-6.2048 (2.2102)**	0.1275 (0.0876)	1.096 (1.2278)
Constant	-1.64 (0.6560)**	-49.03 (21.4671)	-0.1065 (0.1792)	-7.07 (1.5427)***	0.3351 (0.2057)**	-9.77 (2.3063)	-0.2898 (0.1279)**	-7.09 (1.6116)***
No. of Obs.	330	330	330	330	330	330	330	330
R^2	0.8400	0.8336	0.8414	0.8184	0.6619	0.6622	0.5927	0.6369

Table 8: Estimated Effects of the Michigan Antitrust Reform Act on Private Patent and Patent Citations, 1976– 2006[†, ††]

	Private Electrical Patents Per Capita		Private Mechanical Patents Per Capita		Private Other Patents Per Capita	
	Patents	Citations	Patents	Citations	Patents	Citations
Michigan	0.0036 (0.0099)	-0.0434 (0.0939)	0.0218 (0.0076)***	0.1231 (0.0767)	-0.0234 (0.0085)***	-0.2748 (0.0983)**
Post-MARA	0.0127 (0.0171)	-0.1617 (0.1669)	0.0230 (0.0145)	0.0315 (0.1354)	0.0570 (0.0053)	0.1344 (0.1829)
Mich.*Post-MARA	**0.0066 (0.0063)**	**-0.01780 (0.0578)**	**0.0430 (0.0094)*****	**0.1899 (0.0668)*****	**0.0214 (0.0053)*****	**0.0881 (0.0609)**
Share of High-Tech Jobs	0.0012 (0.0028)	0.0007 (0.0206)	-0.0106 (0.0012)***	-0.802 (0.0128)***	-0.0026 (0.0015)**	-0.0127 (0.0125)
Percent College Grad	0.0023 (0.0008)**	-0.0141 (0.0077)*	0.0003 (0.0006)	-0.0252 (0.0056)***	-0.0005 (0.0008)	-0.0256 (0.0070)***
Unemployment Rate	-0.0019 (0.0010)**	-0.0231 (0.0098)**	-0.0023 (0.0009)**	-0.0137 (0.0079)*	-0.0019 (0.0008)**	-0.0155 (0.0089)*
Labor Force Part. Rate	-0.0016 (0.0004)***	-0.0020 (0.0041)	0.0002 (0.0003)	0.0147 (0.0030)***	0.0014 (0.0004)***	0.0217 (0.0039)***
Real Per Capita Income (levels)	0.0459 (0.0301)	0.6840 (0.3020)**	0.0252 (0.0299)	0.6778 (0.2647)	0.0053 (0.0301)	0.3205 (0.3351)
Share Pop age 15 to 64	0.0051 (0.0011)	0.0991 (0.0102)***	0.0055 (0.0010)***	0.0765 (0.0079)***	0.0003 (0.0009)	0.0239 (0.0087)**
Pop Growth (levels)	-0.1884 (0.0867)**	-3.2678 (1.1549)**	-0.1067 (0.671)	-0.9804 (0.7786)	-0.0424 (0.0764)	1.1632 (1.2422)
Constant	-0.5049 (0.1296)*	-10.33 (1.2826)***	-0.6568 (0.1095)***	-10.06 (0.9419)***	-0.1166 (0.1108)	-4.71 (1.2897)***
No. of Obs.	330	330	330	330	330	330
R^2	0.7948	0.8186	0.8665	0.8594	0.7831	0.7539

[†]Robust standard errors in parentheses.
*,**,*** Represent Statistical Significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

[††]Control Group is Non-Enforcement States (AK, CA, CT, MN, MT, NV, ND, OK, WA, and WV).

All regressions include year effects and one-digit industry controls.

	Average Difference Relative to Control Group Pre-Treatment Period	Average Difference Relative to Control Group Post-Treatment Period	Change Post-Treatment	Rank (*p*-value)	Pre-Treatment Period RMSPE
Total Patent Citations	0.2290	0.1705	0.0586	28/50 (0.5490)	0.2582
Chemical Citations	-0.1005	-0.1019	-0.0014	24/50 (0.4706)	0.1293
Computer Citations	0.0615	-0.0470	-0.1085	25/50 (0.4902)	0.0903
Drugs Citations	-0.0279	-0.1058	-0.0778	45/50 (0.8824)	0.0493
Electrical Citations	0.0343	-0.0177	-0.0520	38/50 (0.7451)	0.0454
Mechanical Citations	0.2176	0.3747	0.1570	4/50 (0.0784)	0.2210
Other Citations	-0.0077	0.0487	0.0564	7/50 (0.1373)	0.0429

Table 9: Estimated Effects of Change in Michigan's Non-Compete Enforcement on Total Patent Citations, and by Technology Classifications, Relative to Synthetic Michigan

Figure 1a: Percent of Jobs Created by Startups
Michigan and the Border States,1977 - 2013
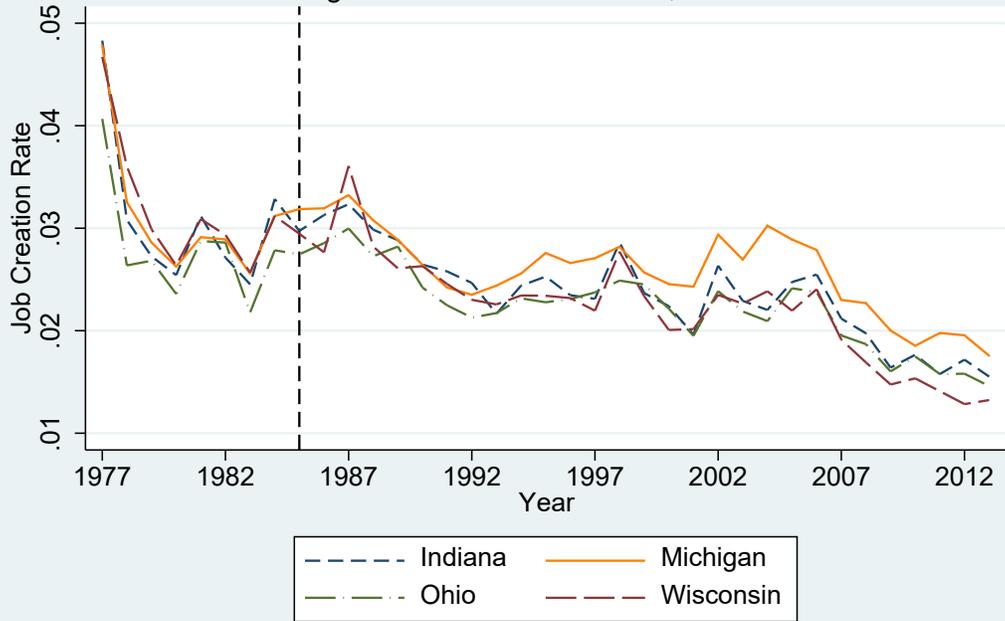
Figure 1b: Startup Entry Rate
Michigan and the Border States, 1977 - 2013

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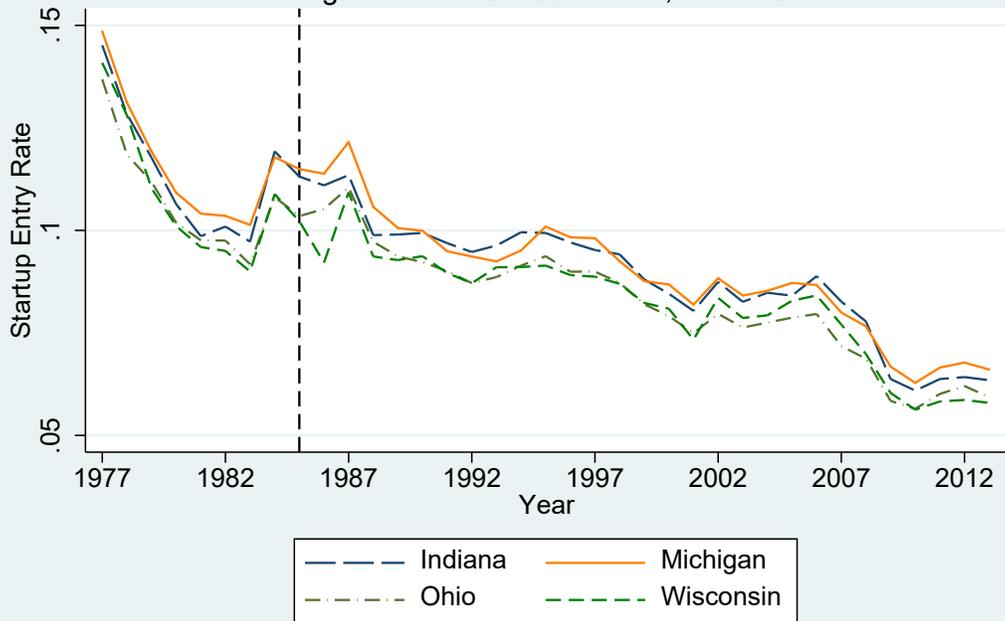
Figure 1c: Percent of Jobs Created by Startups
Michigan and the Expanded Border States, 1977 - 2013
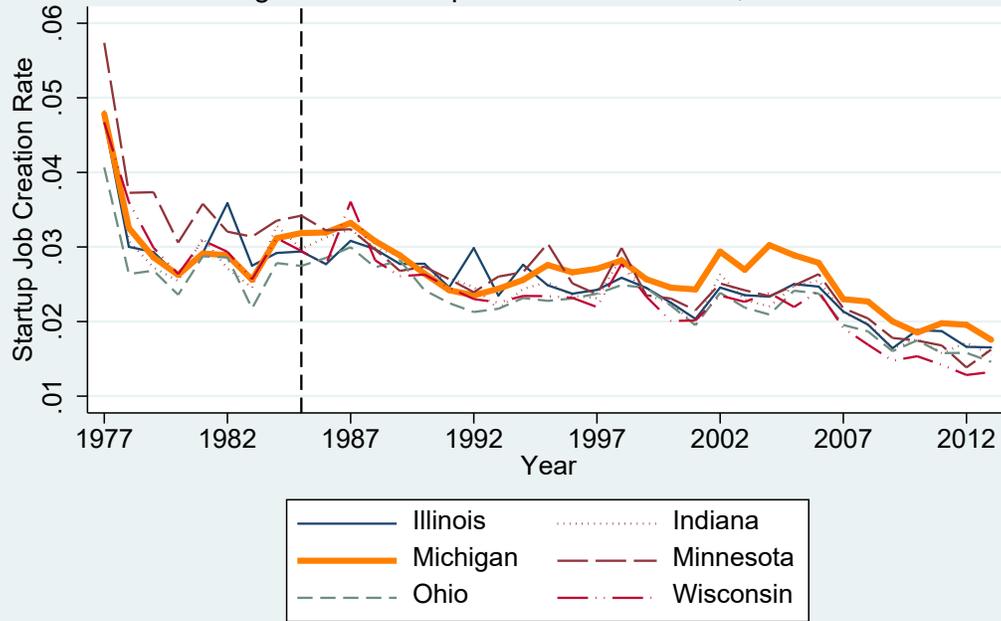
Figure 1d: Percent of Jobs Created by Startups
Michigan and the Non-Enforcement States, 1977 - 2013
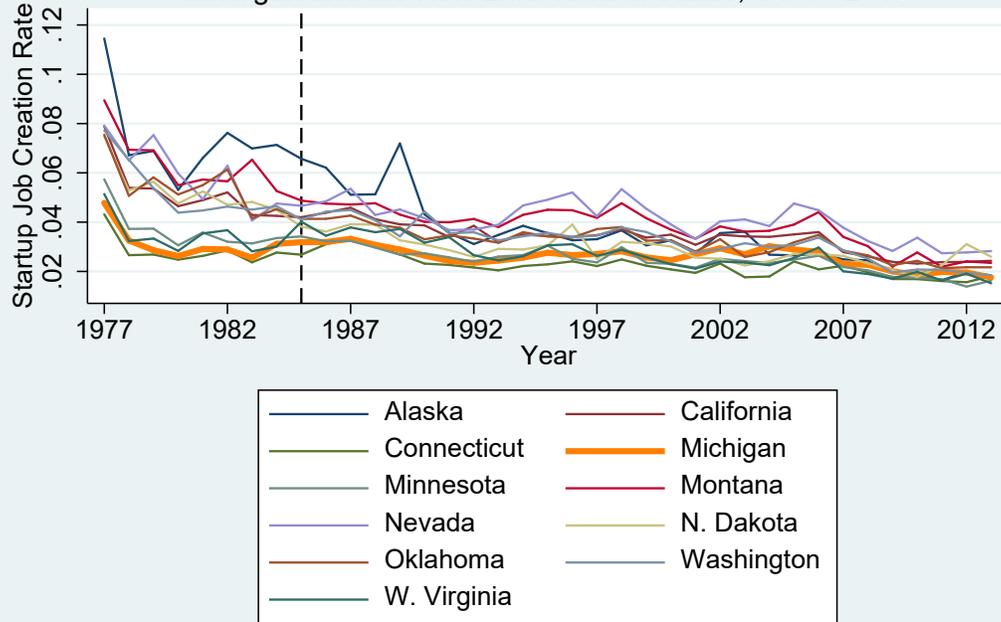
Figure 2a: Percent of Jobs Created by Startups
Michigan and the Synthetic Control Group, 1977 - 2013

Synthetic Control Group is a Combination of IL = .26, OH = .58, PA = .13, and WV = .034

Figure 2b: Startup Entry as a Percent of All Establishments
Michigan and the Synthetic Control Group, 1977 - 2013
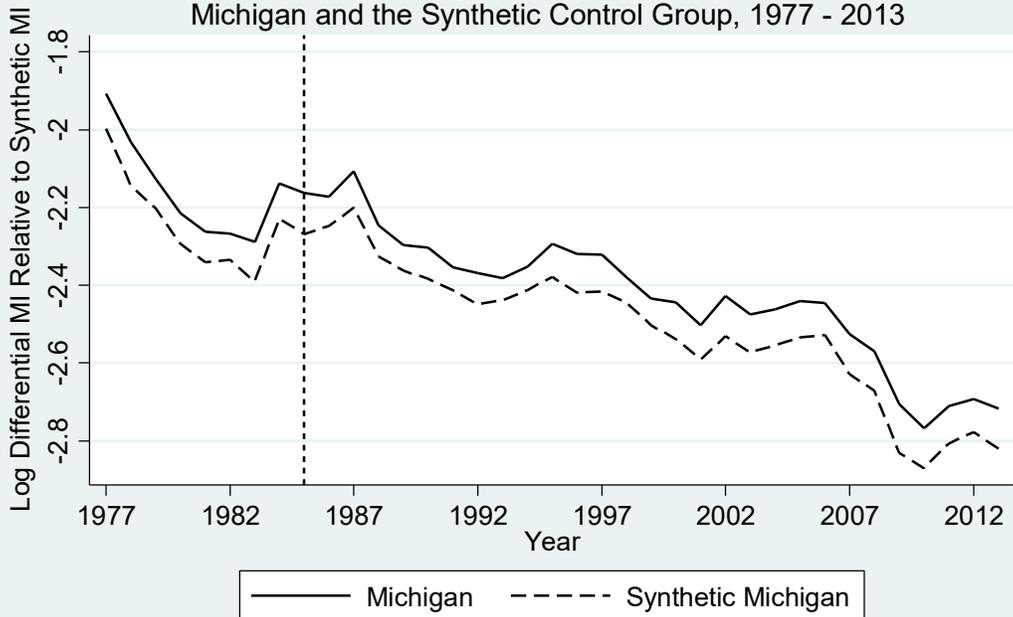
Synthetic Control Group is a Combination of OH = .87, and PA = .13

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Figure 3a: High-Tech Employment
Michigan and the Non-Enforcement States, 1977 - 2013
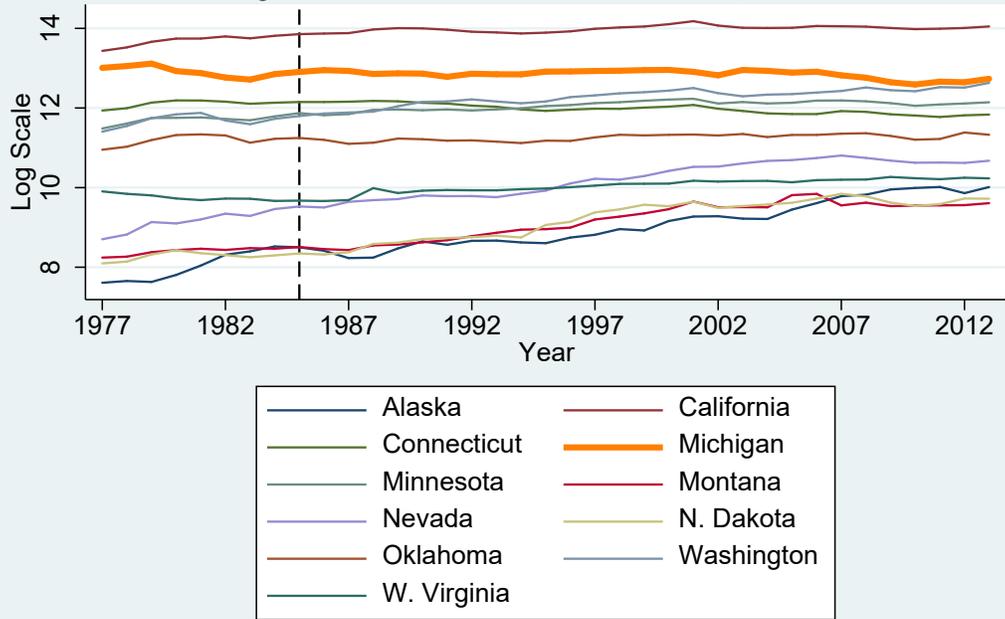
Figure 3b: High-Tech Establishments
Michigan and the Non-Enforcement States, 1977 - 2013
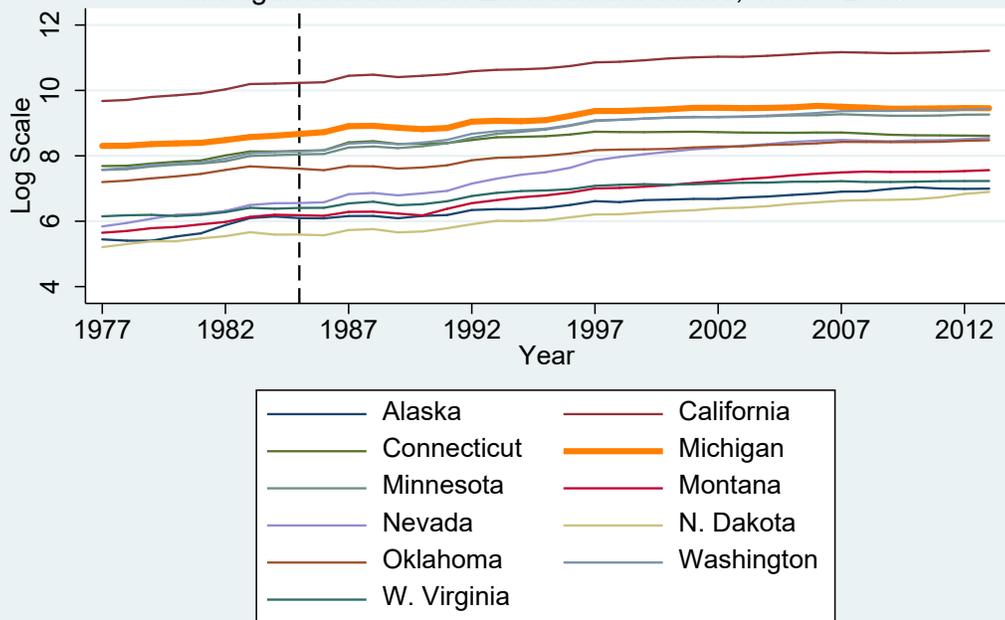
Figure 3c: High-Tech Employment
Michigan and Synthetic Michigan, 1977 - 2013*
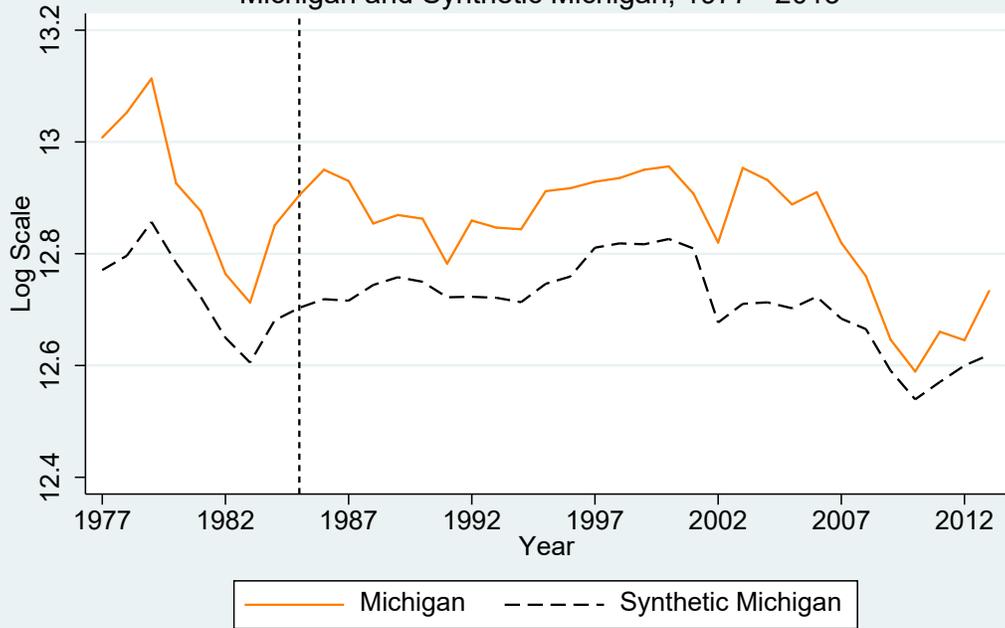
Figure 3d: High-Tech Establishments
Michigan and Synthetic Michigan, 1977 - 2013*
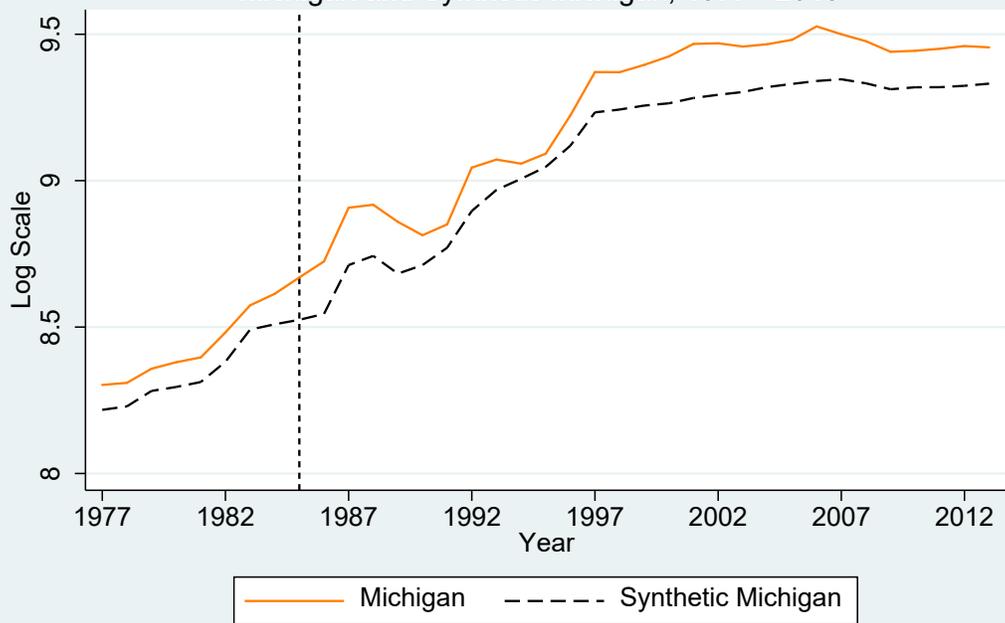
Figure 4: Total Patents and Patents by Technology Class

Per 10,000 People, 1976 - 2006