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

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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. Our findings offer 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. In a difference-in-difference analysis, we find that a 10 percent increase in enforcement led to an increase of about 1 percent to about 3 percent in the startup job creation rate in Michigan and, in general, to essentially no change in the startup entry rate. 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 quality-adjusted 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

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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 percent of fast growers will ultimately contribute disproportionately to job creation in the U.S. 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 from 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 period and within a narrowly defined geographic region.

Even though non-competes represent a restraint on trade, such agreements are common for many types of workers in the U.S. (Stone, 2002, and Starr et al., 2015b).[1] There are no federal laws governing the enforceability of non-competes; enforcement is left to the states, which 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 as a way to safeguard the legitimate business interests of firms. An important issue is whether and to what extent judicial enforcement of non-compete clauses impedes entrepreneurial activity and employment growth.

[1] Starr et al. (2015b) report that 18 percent of all U.S. workers are covered by non-compete agreements and 37 percent indicate that they have been covered by such an agreement during their careers.

Meccheri (2009) shows that non-compete covenants can be justified on efficiency grounds because 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 underinvest) 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.

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. (2015a) 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] 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., 2015a, refer to this channel as an "investment protection effect.")[3] The overall effect of non-compete covenants on startup activity is an open question.[4]

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

[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.
[4] 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.

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 changes in 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 four alternative control groups of states (all U.S. states; states bordering Michigan; the ten non-compete states identified by Marx et al. (2009); and a group consisting of California and North Dakota). In a difference-in-difference (hereafter, DID) analysis, we find that enforcement had a positive and significant effect on the startup job creation rate but little or no effect on the entry rate of new firms. Specifically, depending on the control group, a 10 percent increase in enforcement led to a 1 to 3 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 the 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 do not follow the parallel trends assumption. This leads us to consider a fifth 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 or the ten non-compete states. When using the SCM method, we find that changes in both the startup entry rate and the job creation rate following MARA, while positive, 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.

In the final section of the paper, we extend our analysis to consider the effect of increased enforcement on patent activity. Patent data can be used to study entrepreneurial activity, and patent data are available by various technology classifications. We find that enforcement had a positive and significant effect on the number of mechanical patents and the number of other patents issued to Michigan inventors, while having negative and significant effects on chemical patents and on computer and communications patents. This is an important finding in that it demonstrates 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 (Gilson, 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 support the enforcement of non-competes.

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 interstate 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 the 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 relative to states with less-restrictive enforcement policies. In a study more closely related to ours, Starr et al. (2015a) find that greater enforceability is associated with fewer within-industry spinoffs compared with cross-industry spinoffs, providing evidence for the screening channel.

Our study 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 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 a non-compete agreement can be enforceable to the extent that it is "reasonable as to its duration, geographic area, and the type of employment or line of business." Moreover, if a non-compete clause in the agreement is "found to be unreasonable in any respect, a court may limit the agreement to render it reasonable."[5]

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

[5] Reasonable covenants also may protect trade secrets, confidential information, employer's customers or customer lists.

measureable 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.

Let $s = 1, \ldots, S+1$ for states during the period $t = 1, \ldots, T$. Let s = 1 specify the treatment state and the remaining states are potential control candidates. Suppose the treatment occurs at time $t' + 1$. The pretreatment period is given by $t = 1, \ldots, t'$ and the post-treatment period by $t' + 1, \ldots, T$.

Define $O_{st'}^{R}$ as the average value of the outcomes of interest for Michigan during the pre-treatment period (1977 – 1984), and $O_{st'+1}^{R}$ as the corresponding average for the post-treatment period (1985 – 2013). Similarly, define the averages $O_{st'}^{NR}$ and $O_{st'+1}^{NR}$ for the outcomes for states not receiving treatment. Since $O_{st'}^{NR}$ and $O_{st'+1}^{NR}$ are not observed, they must be estimated. In the DID analysis, we estimated $O_{st'}^{NR}$ and $O_{st'+1}^{NR}$ by alternatively using all U.S. states; the ten non-compete states; the states bordering Michigan; and a group consisting of California and North Dakota, as the counterfactual Michigan.

The DID estimate is given by:

$$DID_{Mich} = (O_{st'+1}^{R} - O_{st'+1}^{NR}) - (O_{st'}^{R} - O_{st'}^{NR}) \tag{1}$$

According to the discussion in the introduction, the sign on DID_{Mich} is indeterminate.

The DID estimator can be written in regression form as:

$$\Delta Y_{st} = \beta_0 + \beta_1 X_{st} + \mu_{st} \tag{2}$$

where $\Delta Y_{s,t}$ is the change in the outcome variable of interest in state *s* in time period *t*; $X_{s,t}$ represents a binary treatment variable; and $\mu_{s,t}$ is random error term. More specifically, $\Delta Y_{s,t}$ represents of the outcome variable for the s^{th} state following the completion of the experiment minus its value before the start of the experiment. The OLS estimator of β_1 is the DID estimator. In terms of our previous notation, the DID estimator is:

$$\hat{\beta}_1 = DID_{Mich} = (O^{R}_{st'+1} - O^{NR}_{st'+1}) - (O^{R}_{st'} - O^{NR}_{st'}) \tag{3}$$

Under the assumption that the treatment is randomly assigned, $E(\mu_{s,t} \mid Y_{s,t}) = 0$, the OLS estimator of β_1 is unbiased.

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 are no excluded variables that are correlated with $X_{s,t}$ and $\mu_{s,t}$.

$$\Delta Y_{st} = \beta_0 + \beta_1 X_{st} + \gamma_j \sum_{j=1}^{J} W_{jst} + \mu_{st} \tag{4}$$

For the additional covariates, we included state-level values for: the nine one-digit SIC industry share of total state employment; the percent of a state's population with a college degree; the percent 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. Even in the absence of non-competes, employers can use other mechanisms, such as patents and trade secrets, to protect their interests. It would be difficult to draw conclusions about the role of non-competes on startup activity without controlling for these other mechanisms. We include patents per 10,000 people as a covariate in the regressions. 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, Duflo and Mullainathan (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, 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. We expect the opposite (that is, a positive) effect on startup activity if enforcement of non-compete clauses leads companies to invest more in Michigan.

2.2 Data.

We use annual state-level data from the U.S. Census Bureau's Business Dynamics Statistics (BDS) for the period 1976–2013 on the entry of new establishments and number of private sector jobs created by these establishments. The BDS consists of longitudinal data covering all private non-farm U.S. establishment 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, as a state industry-level breakdown of the data is not publicly available. The outcome variables used in our analysis are defined as:

$$\text{Establishment Entry Rate}^0_{s,t} = \frac{\text{New Establishments}^0_{s,t}}{1/2(\text{No. of Estabs}_{s,t} + \text{No. of Estabs}_{s,t-1})} \quad (5)$$

$$\text{Job Creation Rate}^0_{s,t} = \frac{\text{Job Created}^0_{s,t}}{1/2(\text{Employment}_{s,t} + \text{Employment}_{s,t-1})} \quad (6)$$

Where the $\textbf{Establishment Entry Rate}^0_{s,t}$ 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 $\text{Job Creation Rate}^0_{s,t}$ refers to the number of jobs created by startups relative to total employment in the state. Following Haltiwanger, Jarmin, and Miranda (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. We also use patent data found in the NBER Patent Data Project (see Section 6.1 for more details on the patent data).

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 Non-enforcing States.

Based on Table 1 in Stuart and Sorenson (2003), Marx et al. (2009) and Marx et al. (2015), we identify 10 states with statutes 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). We use these 10 states as one of our control groups in the analysis that follows. However, Barnett and Sichelman (2016) argue that the Stuart and Sorenson (2003) classification oversimplifies and misstates the dissimilarity in the strength of state-by-state enforcement of non-competes by classifying enforcement strength as a binary variable: “enforcing” or “nonenforcing.” Barnett and Sichelman (2016) argue that the binary classification is highly inaccurate in that, with the exception of California, North Dakota, and Oklahoma (until 1989), all other states have provisions allowing for some form of non-compete enforcement.

3.2 Border States.

A second control group used in the analysis consists of states that border Michigan, as these states may share similar economic and demographic characteristics (Illinois, Indiana, Ohio, Minnesota, and Wisconsin).[6] However, one important drawback with using border states as a control group is that it will not be possible to identify the magnitude of the effect of MARA in Michigan, since it is highly likely that MARA had spillover effects on neighboring states.

[6] Illinois and Minnesota are separated from Michigan by the Great Lakes.

3.3 California and North Dakota.

The misclassification of states identified by Barnett and Sichelman (2016) may not be random and could introduce systematic error. As a result, we also use a control group consisting of two states (California and North Dakota) since only these states can be classified as non-enforcing states in the entire sample period used in our study.

For completeness, we also use all U.S. states, excluding Michigan, as one of our control groups.

One 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 four different control groups (the three groups identified in this section plus a control group based on all U.S. states except Michigan), while Figure 1b shows the trends for the entry rate. As the figures show, each of the control groups closely tracks movements in Michigan during the pre-MARA period.[7]

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

[7] In Section 5, we use the SCM as an alternative approach for selecting a control group.

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 1 provides little evidence that startup activity in Michigan diverged relative to the dynamic path of the other control groups. We turn to the DID analysis to see if this analysis reveals a similar lack of an enforcement effect on startup activity. This visual inspection of the data suggests that MARA had little or no effect on startup activity in Michigan, relative to the control groups.

4. FINDINGS

Our analysis is at the state level, since non-compete laws are determined at the state level, and as such their occurrence and enforcement will vary across states. As indicated, a number of different groupings of the states are used to construct the various control 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*** *affect startup activity in Michigan relative to the control group*

Table 1a presents summary statistics for Michigan, and Table 1b presents these statistics for an average U.S. state when Michigan is excluded from the calculations. 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 the well-known fact 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 our 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 all U.S states. The next two columns in Table 2 give the findings relative to a counterfactual based on the ten non-enforcement states. The third and fourth columns of the table show the findings when the states bordering Michigan constitute the control group, and the final two columns show the results when the control group consists of California and North Dakota.[8]

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 2). Beginning with the job creation rate for Michigan relative to all other U.S. states, the coefficient on the interaction of Michigan and the post-MARA indicator is positive and highly significant. The estimates coefficient suggests that a 10 percent increase in enforcement leads to a 1.1 percent increase in the job creation rate by startups. We also find a positive and significant coefficient for the job creation rate relative to a counterfactual based on the ten non-enforcement states. In this case, the estimates coefficient suggests that a 10 percent increase in enforcement leads also to about a 1.1 percent increase in the job creation rate by startups. Also, we find a positive and significant coefficient for the job creation rate relative to a counterfactual based on the states bordering Michigan. In this instance, the estimates coefficient suggests that a 10 percent increase in judicial enforcement leads to a 0.8 percent increase in the job creation rate by startups. The smaller estimated coefficient may be indicative of a spillover effect in that some jobs that would have been created in Michigan in the absence of MARA were instead created in neighboring states.

[8] The All States and the Limited Non-enforcement regressions shown in Table 2 include regional fixed effects.

Finally, the strongest effect of enforcement on the job creation rate is found when California and North Dakota make up the control group. In this case, a 10 percent increase in enforcement leads to a 2.7 percent increase in the job creation rate by startups in Michigan.

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 only is statistically significant (and only at the 10 percent level of significance) when the control group is based on California and North Dakota. In this case, the estimates coefficient suggests that a 10 percent increase in enforcement leads to a 1.0 percent increase in the startup entry rate.

Taken together, the findings summarized in Table 2 suggest that judicial enforcement of non-compete agreements leads to a modest increase in the number of jobs created by startups, and to a lesser extent to the startup entry rate, if at all. Even using the largest effect found for the job creation rate when California and North Dakota make up the control group suggests that a 10 percent increase in enforcement translates to fewer than 100 new jobs. This is small potatoes in a state where startups typically create about 90,000 jobs annually.

5. SYNTHETIC CONTROLS.

An important requirement of the DID approach is that in the absence of treatment, the outcomes for the treated and control group follows 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 why startup activity in Michigan post-MARA fails to grow relatively faster compared with the control groups. The

SCM developed by Abadie et al. (2010) is an alternative method that, unlike DID, accounts for the effects of confounders changing over time. The SCM is a data-driven search routine 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 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 mean square prediction error (MSPE) in the pre-treated sample period. Importantly, the SCM is used to forecast a counterfactual post-MARA path for Synthetic Michigan.

More specifically, the SCM estimates the unobserved values for $O_{st'}^{NR}$ and $O_{st'+1}^{NR}$ by creating a synthetic control as a weighted combination of the potential controls that best approximates the pre-treatment characteristics of the treated state. Let $W = (w_2, \cdots, w_{S+1})'$ be the weights such that $w_s \geq 0$ and $\sum_{s=2}^{S+1} w_s = 1$. Let X_1 be a $(k \text{ x } 1)$ vector of potential covariates for the treated state, and let X_0 represent a corresponding $(k \text{ x } S)$ matrix for the control states. One objective of the SCM is to search for a linear combination of the columns of X_0 that best approximate X_1. That is, the synthetic controls are formed as a linear combination of the $w_s s$ to minimize the distance between the characteristics of the treated state and those of the control states, where the difference is given by the distance metric:

$$\left\{(X_1 - X_0W)'V(X_1 - X_0W)\right\}^{1/2} \tag{7}$$

where V is chosen algorithmically to minimize the MSPE over the pre-treatment period.[9]

To conduct tests of the statistical significance for the estimated treatment effects, Abadie et al. (2010) implement a placebo (permutation) test in which the treatment is iteratively assigned to the control states that were not exposed to the treatment. In a two-tailed test, either a comparatively large value or comparatively small value for the estimated treatment effect relative to the distribution of placebo effects is taken as support for a causal interpretation of the of treatment effects.

Our objective is the construction of a Synthetic Michigan prior to 1985 based on a composite of all U.S. states. For each outcome variable, we search for a synthetic match using the covariates given in the first column of Table 1. Four states — Illinois, Ohio, Pennsylvania, and West Virginia — receive positive weights in the construction of a Synthetic Michigan when the job creation 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. Table 3 gives the states and their relative weights used to construct the synthetic control groups. 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 pretreatment 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 or the startup entry rate immediately following the passage of MARA, relative to Synthetic Michigan. As already

[9] We use Stata's Synth procedure developed by Abadie et al. (2010), using the default regression-based method to obtain the variable weights for the V matrix.

mentioned, this lack of an immediate effect on startup activity is most likely due to a provision in the legislation in that all statutes that had been repealed by MARA would remain in force. Given these legislative provisions, we expect that startup activity post-MARA would initially continue to closely track Synthetic Michigan. But over time, we also expect that startup activity in Michigan would diverge from the path of its synthetic. This is in fact what we observe. Both the job creation rate (Figure 2a) and the entry rate (Figure 2b) in Michigan increase relative to the synthetic control starting in about the mid-1990s.

Table 4 presents the estimates of the DID analysis both for the startup entry rate and 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. Following Abadie et al. (2010) we use a placebo test (or a falsification test) where the treatment is applied iteratively to each of the states. The fourth column 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 root mean squared prediction error (RMSPE).

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. However, based on Michigan’s placebo ranking shown in the fourth column of Table 4, the DID estimated coefficient is not significantly different from zero. The second 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 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 estimated coefficients are not statistically significant.

6. PATENTS AND PATENT CITATIONS

So far we have looked at 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 startups 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 findings indicate that we need industry-level data to more fully address the questions of whether and to what extent non-compete covenants influence startup activity. Barnett and Sichelman (2016) point out that "no state enforces noncompetes that purport to proscribe employment at non-competing firms." Unfortunately, the Census Bureau does not make industry-level data on startup activity publicly available.

Fortunately, patent data can be used to study entrepreneurial activity, and patent data 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, Jaffe, and Trajtenberg (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*** *affect patents per capita or patent citations per capita in Michigan relative to the control group.*

6.1 Patent Data.

We use data on patent applications obtained from the NBER Patent Data Project. The data span the years 1976–2006.[10] Hall, Jaffe, and Trajtenberg (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 3 shows the path of total patents per capita and for the six technological classifications in Michigan. For total patents, we see that there were fewer than three patents per 10,000 people in Michigan from 1977 through the mid- to late 1980s. Patents per capita started rising in the mid-1990s to reach a peak of 4.4 in 2003 before declining to around 3.6 in 2006. Figure 3 slows 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 one mechanical patent per 10,000 people in Michigan during the period 1977 to 1988. Mechanical patents per capita started rising more rapidly in the late 1990s to reach a peak of 1.7 during the period 2001 to 2004 before declining a bit after that. Mechanical patents accounted for 30 percent of total patents in Michigan from the mid-1970s to the early1980s. That share steadily rose to 42 percent in 2006. During our sample period,

[10] 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.

mechanical patents on average accounted for 35 percent of total Michigan patents, compared with 22 percent for the average state. The chemical and the other patent 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 very highly skewed. Most patents are not worth very much, while a few are very valuable (see, 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 from 1983 to 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 citation-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.

Table 5 summarizes our findings by technology category. The first two columns of Table 5 present the findings for total private patents per capita and for the citation-weighted version. Subsequent columns in Table 5 show the results for the six major technological categories we investigate. The coefficient of interest (the estimated coefficient on interaction of Michigan and the post-MARA indicator) is positive for total patents but negative for citation-weighted patents. However, neither of these coefficients is significantly different from zero. Regarding the subcategories, the coefficient of interest is negative in four cases and negative and significant in two cases (chemicals, and computers and communications). We also find that the coefficient of interest is positive for three classifications and positive and significant for two subcategories (for mechanical and for the “others” category).

Table 6 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 associated *p*-values associated with the placebo tests are shown in the fourth column of Table 6.

The most statistically significant results are found for mechanical patents. For mechanical patents, Table 6 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. Based on Michigan’s placebo ranking for mechanical patents shown in the fourth column of Table 6, the DID estimated coefficient is significantly different from zero. The *p*-values associated with Michigan’s relative rank in the distribution of placebo states shows that 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 accounted for more than 40 percent of all patent activity in Michigan in 2006.

7. 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 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 10 percent increase in enforcement led to an increase of about 1 percent to 3 percent in the startup job creation rate in Michigan and to essentially no change the startup entry rate. Extending our analysis to consider the effect of increased enforcement of non-compete covenants on patent activity, we find that enforcement had differential effects across technological classifications. Importantly, increased enforcement had a positive and significant effect on mechanical patents in Michigan. The mechanical category is an important technological classification in Michigan in that it accounted for over 40 percent of total patent applications in Michigan in 2006.

While our findings suggest that increased enforcement may promote job creation, there is obviously room for future research. Our findings for patents suggest that enforcement may have differential effects on startup activity across industries. The consideration of aggregate versus industry-level analysis also has relevance for understanding the productivity advantages of cities. For example, how does enforceability affect localization economies (externalities that tend to increase with increases in the number of firms or workers in a given industry in a given city)? How does enforceability affect urbanization economies (externalities that tend to increase as the total number of firms or total number of workers increase in a given within a city)? While we do not find compelling evidence that enforcement affects entry and job creation by new firms, cities may, nonetheless, be less productive and less innovative if enforcement limits employees' outside options, even if enforcement does not limit industry or city size. 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 Buzzard et al., 2016). Evidence that increased enforcement limits knowledge spillovers would be provided if patent citations are less likely to come from the same city post-MARA compared with pre-MARA citations (an exercise we did not undertake). Using patent citation data, Sing 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 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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TABLE 1a: Summary Statistics for Michigan											
	Michigan, 1977 – 1984						Michigan, 1985 – 2013				
	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 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								
	All States†		Limited Non-Enforcement†		States Bordering Michigan†		California and North Dakota	
	Job Rate	Entry Rate	Job Rate	Entry Rate	Job Rate	Entry Rate	Job Rate	Entry Rate
Michigan	0.0294 (0.0280)	0.0949 (0.0261)***	-0.0324 (0.0949)	-0.0035 (0.0822)	0.1120 (0.0607)	0.1093 (0.0307)***	-0.4796 (0.2674)	-0.2160 (0.0822)
Post-MARA	-1.0938 (0.1122)***	-0.6791 (0.0938)***	-0.6710 (0.1792)***	-02797 (0.1358)**	-0.6996 (0.3961)	-0.8175 (0.2843)***	-1.0320 (0.0866)***	-0.3592 (0.1994)
Mich.*Post-MARA	0.1147 (0.0193)***	-0.0140 (0.0158)	0.1181 (0.0383)***	0.03382 (0.0243)	0.0755 (0.0364)*	-0.0168 (0.0310)	0.2774 (0.0755)***	0.10252 (0.0339)*
Patents Per Capita	-0.0250 (0.0195)	0.0021 (0.0145)	-0.0196 (0.0185)	0.0519 (0.0247)*	-0.1019 (0.0247)**	-0.0628 (0.0250)	-0.0446 (0.0561)	0.1115 (0.0399)*
Percent College Grad	-0.0553 (0.0942)	-0.0616 (0.0690)	0.3615 (0.2681)	-0.0375 (0.2086)	0.2162 (0.3288)	-0.0532 (0.2002)	-0.1364 (0.0960)	-0.4242 (0.1292)
Unemployment Rate	0.0838 (0.0376)**	0.0970 (0.0318)***	-0.0101 (0.0424)	0.0003 (0.0287)	0.0109 (0.0503)	0.0069 (0.0639)	0.1368 (0.0412)***	0.0420 (0.0331)
Labor Force Part. Rate	0.1247 (0.2959)	0.4921 (0.2188)**	-0.7934 (0.4852)	0.0786 (0.3189)	-0.3004 (0.4731)	0.1947 (0.2388)	-0.3742 (0.6587)	-011606 (0.2197)
Real Per Capita Income (levels)	0.1023 (0.1967)	0.1270 (0.1448)	0.2377 (0.1117)*	-0.0683 (0.1277)	-0.3668 (0.4550)	0.0108 (0.2243)	0.1132 (0.1302)	-0.0719 (0.1296)
Share Pop age 16 to 64	-1.3308 (0.4172)***	-0.6464 (0.3921)*	-2.6690 (0.8335)***	-1.1764 (0.7405)	-3.9705 (1.7862)*	1.0964 (1.4478)	0.1240 (1.1145)	0.8956 (1.1929)
Pop Growth (levels)	6.6124 (0.8880)***	7.5317 (0.9215)***	3.8736 (0. 4962)***	4.0283 (0.7902)***	1.7673 (2.5222)	4.6064 (1.4045)***	-2.4576 (2.9633)	3.3907 (0.7902)*
Constant	3.7819 (1.5150)**	-0.1108 (1.4765)	12.4425 (3.0419)	0.7563 (2.6429)	12.7011 (8.5630)	-9.2279 (7.0142)	-1.7645 (2.2977)	-2.6246 (4.0180)
No. of Obs.	1,850	1,850	407	407	222	222	111	111
R^2	0.8771	0.9046	0.9297	0.9575	0.9545	0.9752	0.9698	0.9840

†Robust standard errors in parentheses.

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

Limited non-enforcement states are AR, CA, CT; MI, MT, NV; ND, OK, WA, and WV. Border states are IL, IN. MI, MN, OH, and WI. All states excludes Michigan.

Table 3: States Receiving Positive Weights for the Synthetic Control Group	
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 Startup 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**
Job Creation Rate	0.0671	0.1245	0.0575	14/50	0.0925
Entry Rate	0.0873	0.0872	0.0001	24/50	0.0877

Table 5: Estimated Effects of the Michigan Antitrust Reform Act on Private Patent and Patent Citations, 1977– 2006†								
	Total Private Patents Per Capita		Private Chemical Patents Per Capita		Private Computer Patents Per Capita		Private Drugs Patents Per Capita	
	Patents[a]	Citations[a]	Patents[a]	Citations[b]	Patents[a]	Citations[b]	Patents[a]	Citations[b]
Michigan	0.0034 (0.0159)	0.1001 (0.1901)	0.0106 (0.0058)*	0.1399 (0.0535)***	-0.0014 (0.0039)	-0.0636 (0.0727)	-0.0093 (0.0048)**	-0.2277 (0.0721)***
Post-MARA	0.1293 (0.0922)	0.2647 (0.1990)	-0.0386 (0.0194)**	0.0782 (0.0677)	0.0596 (0.0260)**	-0.0620 (0.0972)	0.0022 (0.0161)	0.0285 (0.0715)
Mich.*Post-MARA	0.0131 (0.0088)	-0.0409 (0.1223)	-0.0210 (0.0035)***	-0.1503 (0.0303)***	-0.0075 (0.0038)**	-0.0772 (0.0422)*	-0.0077 (0.0021)***	0.03190 (0.0405)
Total Patents Per Capita/Or Total Citations Per Capita	0.0620 (0.0033)***	0.0662 (0.0060)***	0.0222 (0.0017)***	0.0228 (0.0334)***	0.0068 (0.0016)***	0.0078 (0.0034)**	0.0051 (0.0006)***	0.0065 (0.0015)***
Percent College Grad	0.0045 (0.0623)	-0.0433 (0.0295)	-0.0309 (0.0301)	-0.0276 (0.0103)***	0.0355 (0.0185)*	0.0254 (0.0130)**	0.0330 (0.0172)*	0.0217 (0.0105)**
Unemployment Rate	0.0462 (0.0294)	0.0391 (0.0201)**	-0.0056 (0.0082)	-0.0277 (0.0112)***	0.0172 (0.0089)*	0.0247 (0.0130)*	0.0083 (0.0050)*	0.0133 (0.0099)
Labor Force Part. Rate	0.0079 (0.1304)	0.0176 (0.0189)	0.0562 (0.0755)	0.0116 (0.0067)*	-0.0919 (0.0482)*	-0.0180 (0.0099)*	-0.0083 (0.0347)	0.0004 (0.0062)
Real Per Capita Income (levels)	0.4113 (0.2224)*	0.9700 (0.7092)	0.0546 (0.0498)	-1.0570 (0.7956)	0.1097 (0.0594)*	0.8933 (0.4502)**	0.0818 (0.0491)*	0.3997 (0.2815)
Share Pop age 16 to 64	0.1947 (0.2464)	0.0292 (0.0365)	0.1666 (0.1206)	0.0090 (0.0102)	-0.0377 (0.1056)	0.0154 (0.0142)	-0.0329 (0.0352)	-0.0037 (0.0079)
Pop Growth (levels)	0.2916 (0.4576)	-0.5068 (3.4724)	-0.3540 (0. 2007)*	-4.7571 (1.7741)***	0.2567 (0.2111)	0.6309 (1.7527)	0.0818 (0.4914)*	1.7865 1.4022)
Constant	-1.3385 (0.8861)	-6.1843 (6.1843)	-0.9482 (0.5443)	-0.5793 (1.6780)	0.4115 (8.5630)	-0.5361 (3.3347)	0.0200 (0.1627)	0.1850 (1.7886)
No. of Obs.	1,500	1,500	1500	1500	1500	1500	1500	1500
R^2	0.8603	0.7575	0.8503	0.8473	0.5213	0.5847	0.6528	0.5809

Table 5: Continued						
	Private Electrical Patents Per Capita		Private Mechanical Patents Per Capita		Private Other Patents Per Capita	
	Patents[a]	Citations[b]	Patents[a]	Citations[b]	Patents[a]	Citations[b]
Michigan	-0.0045 (0.0823)	0.0579 (0.0836)	0.0179 (0.0063)***	0.2250 (0.0561)***	-0.0099 (0.0054)*	-0.0213 (0.0568)
Post-MARA	0.0789 (0.0384)**	0.1690 (0.1472)	0.0052 (0.0162)	0.0178 (0.0395)	0.0220 (0.0157)	0.0332 (0.0497)
Mich.*Post-MARA	0.0010 (0.0060)	-0.0507 (0.0884)	0.0357 (0.0027)***	0.1544 (0.0290)***	0.0127 (0.0025)***	0.0508 (0.0313)*
Total Patents Per Capita/Or Total Citations Per Capita	0.0110 (0.0042)***	0.0134 (0.0057)	0.0078 (0.0004)***	0.0068 (0.0009)***	0.0091 (0.0008)***	0.0089 (0.0011)***
Percent College Grad	-0.0216 (0.0208)	-0.0371 (0.0189)**	0.0007 (0.0116)	-0.0097 (0.0043)**	-0.0212 (0.0109)*	-0.0160 (0.0057)***
Unemployment Rate	0.0245 (0.0171)	0.0276 (0.0184)	0.0019 (0.0036)	0.0063 (0.0035)*	-0.0014 (0.0037)	-0.0053 (0.0067)
Labor Force Part. Rate	-0.0554 (0.0566)	0.0041 (0.0104)	0.0263 (0.0225)	0.0069 (0.0033)**	0.0811 (0.0262)***	0.0126 (0.0043)***
Real Per Capita Income (levels)	0.0703 (0.0427)*	0.4429 (0.4005)	0.0493 (0.0272)*	0.3574 (0.1669)**	0.0456 (0.0334)	-0.0664 (0.2829)
Share Pop age 16 to 64	0.027 (0.1623)	-0.0626 (0.0308)	0.1700 (0.0347)	0.0045 (0.0052)	0.0545 (0.0385)	0.0103 (0.0048)**
Pop Growth (levels)	0.4873 (0.4363)	3.1671 (3.2460)	-0.1084 (0. 1024)	-0.5121 (0.9173)	-0.0875 (0.0993)	-0.8221 (1.0241)
Constant	-0.0229 (0.5327)	-0.7193 (2.4363)	-0.2530 (0.1535)*	-1.3440 (0.5497)**	-0.5458 (0.1711)***	-3.1635 (-0.9325)***
No. of Obs.	1,500	1,500	1500	1500	1500	1500
R^2	0.4483	0.4694	0.8131	0.8113	0.8064	0.8242

[†]Robust standard errors in parentheses.
*,**,*** Represent statistical significance at the 10 percent, 5 percent; and 1 percent levels, respectively.
[a]Regression includes total patents per capita.
[b]Regression includes total citations per capita.
Note: All regressions include year effects and census region fixed effects. Control group based on all states excluding Michigan.

Table 6: Estimated Effects of Change in Michigan's Non-compete Enforcement on Total Patent Citations, and by Technology Classifications, 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 (*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

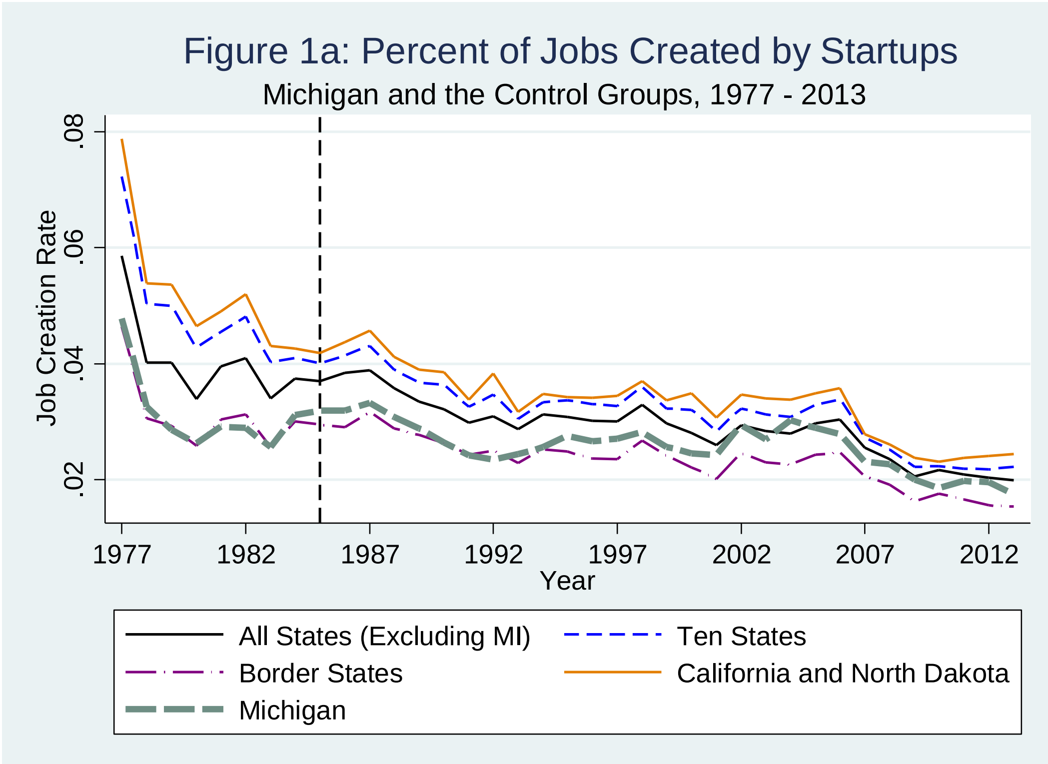


Figure 1a: Percent of Jobs Created by Startups
Michigan and the Control Groups, 1977 - 2013
Job Creation Rate
.02
.04
.06
.08
1977
1982
1987
1992
1997
2002
2007
2012
Year
All States (Excluding MI)
Ten States
Border States
California and North Dakota
Michigan

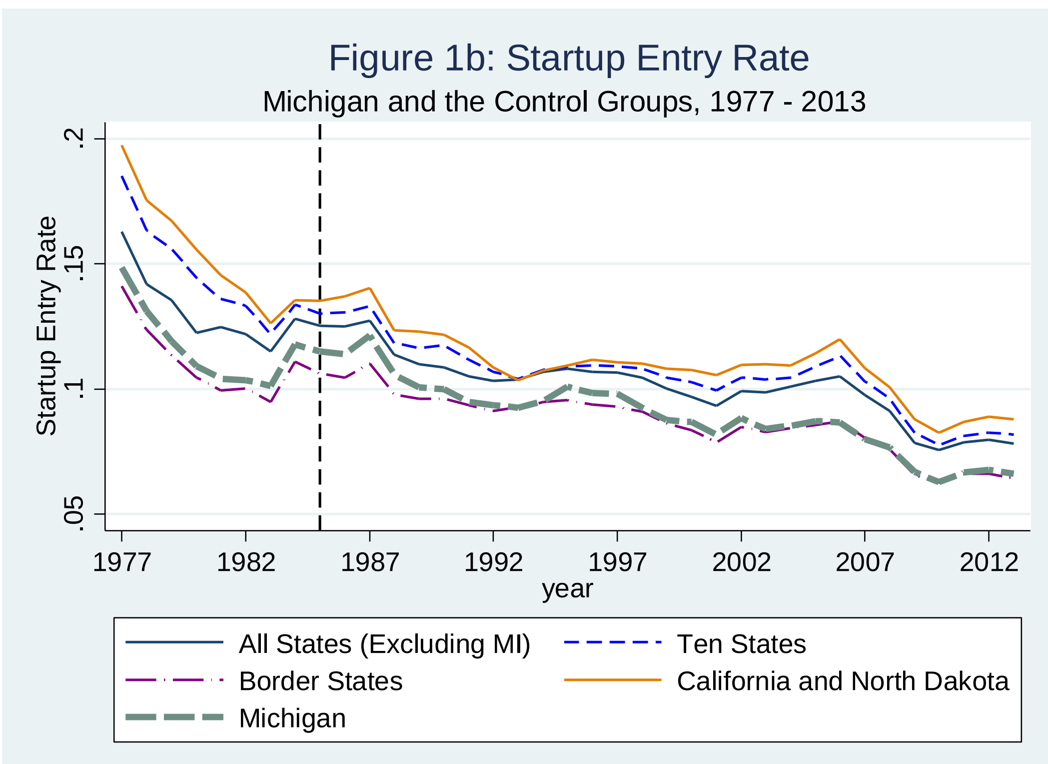
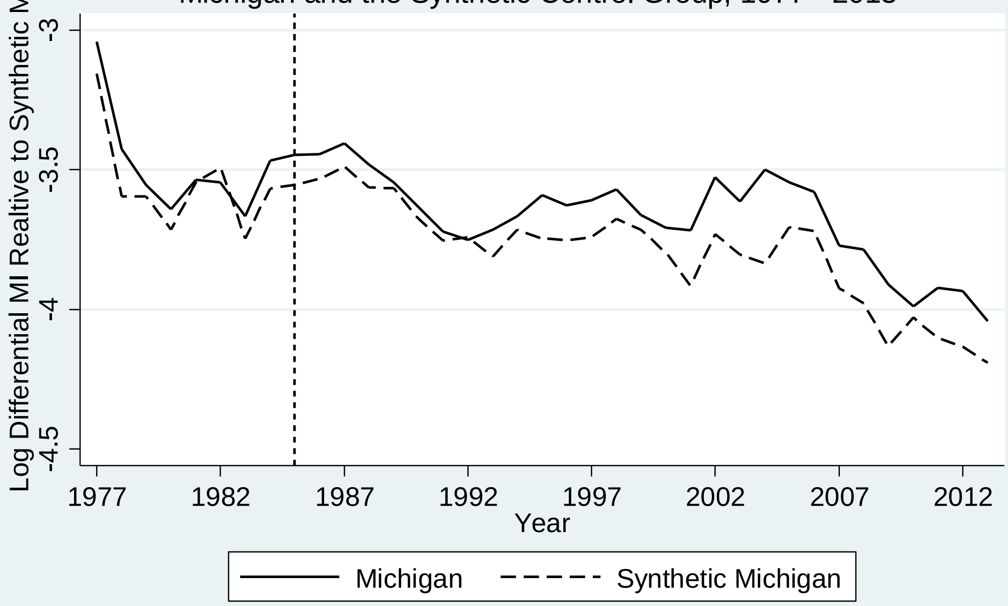


Figure 1b: Startup Entry Rate
Michigan and the Control Groups, 1977 - 2013
Startup Entry Rate
.05
.1
.15
.2
1977
1982
1987
1992
1997
2002
2007
2012
year
All States (Excluding MI)
Ten States
Border States
California and North Dakota
Michigan

Figure 2a: Percent of Jobs Created by Startups

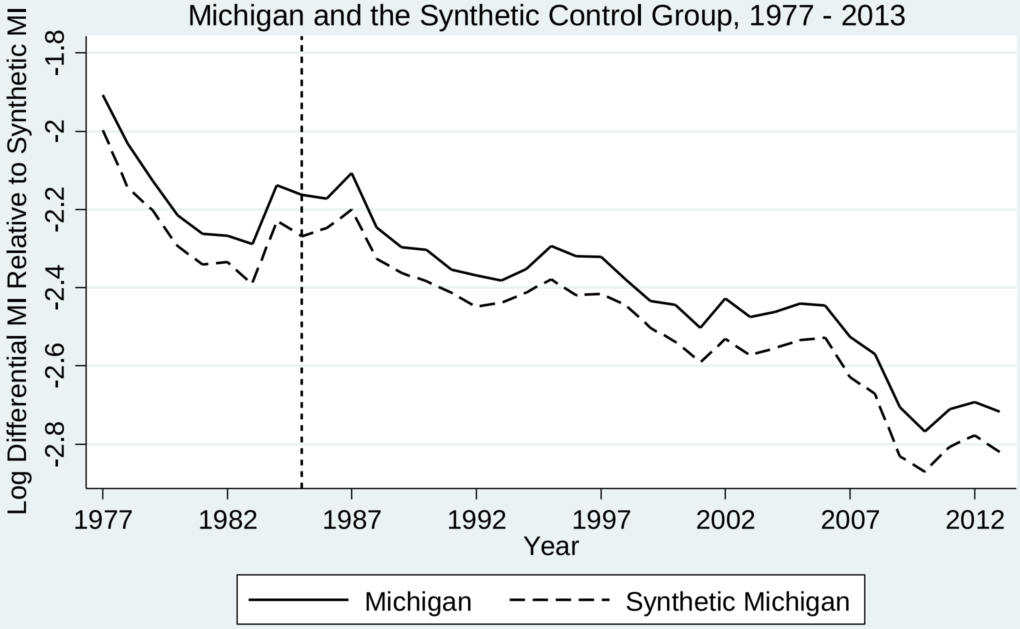
Michigan and the Synthetic Control Group, 1977 - 2013



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

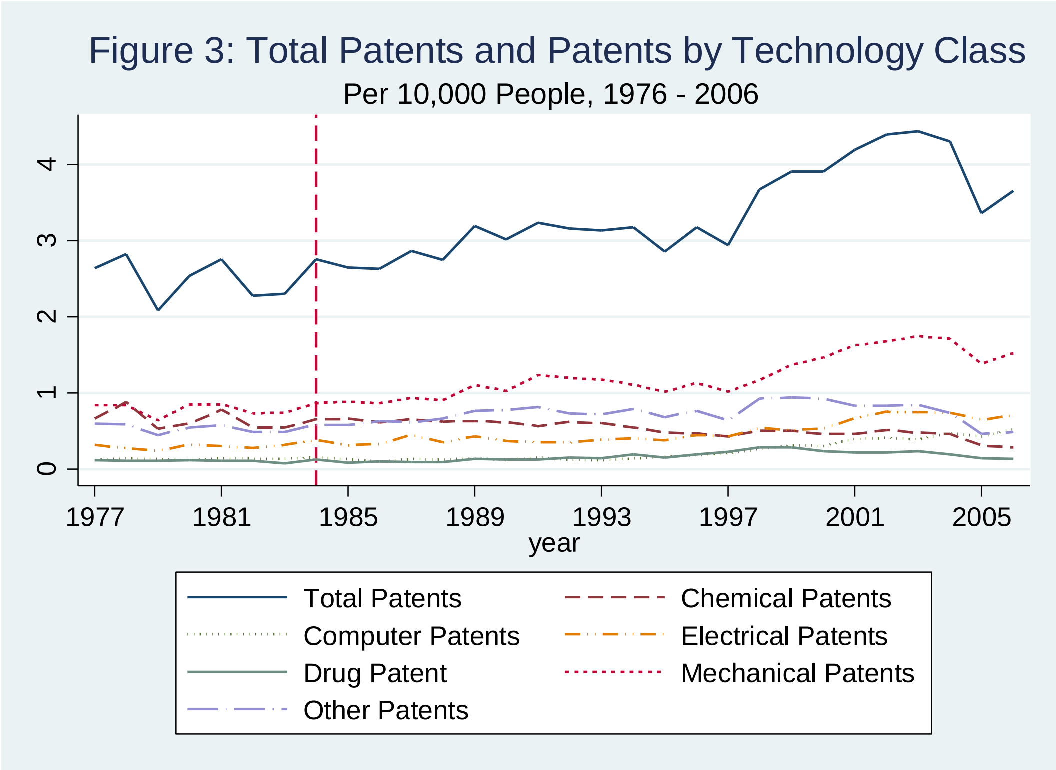


Figure 3: Total Patents and Patents by Technology Class
Per 10,000 People, 1976 - 2006
0
1
2
3
4
1977
1981
1985
1989
1993
1997
2001
2005
year
Total Patents
Chemical Patents
Computer Patents
Electrical Patents
Drug Patent
Mechanical Patents
Other Patents