Effects of Non-Compete Agreements on Innovation and Teamwork

By

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ABSTRACT

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This thesis investigates the effects of non-compete agreements on teamwork and innovation. I argue that patents convey information about the productivity of inventors to the labor market. Therefore, firms have an incentive to manipulate the productivity signal revealed by patents to mitigate competition over their inventors. In particular, the firms adjust the size of teams of inventors strategically, often forming larger teams than what is efficient, before applying for a patent. Since signing non-compete agreements with inventors is an alternative way to address competition for current employees, the feasibility of such contractual clauses reduces the firms' incentives to manipulate team size. In the empirical analysis, I investigate the extent to which my prediction is consistent with the data. Specifically, I use patents issued to inventors by the United States Patent and Trademark Office (USPTO) and the reversal of a Michigan law that prevents the enforceability of non-compete agreements in 1985 to employ a difference-in-differences estimation. My results are consistent with the idea that innovative firms reduce their team size when non-compete agreements are feasible.

KEYWORDS: patent, innovation, teamwork, team size, non-compete agreements,
Michigan Antitrust Reformation Act (MARA)

ÖZETÇE

Taylan Alpkaya
Ekonomi Yüksek Lisansı
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Bu tez rekabet yasağı sözleşmelerinin takım çalışması ve inovasyon üzerine etkilerini incelemektedir. Patentler, işgücü piyasasına mucitlerin üretkenliğiyle ilgili bilgi verir. Dolayısıyla firmaların mucitleri üzerinden oluşacak rekabeti önlemek için patentlerin verdiği sinyali manipüle etme motivasyonu vardır. İddiama göre, firmalar patentlerde bulunan mucit sayısını stratejik olarak ayarlar ve bazen bir patent için başvuru yapmadan önce, verimliliği düşürmesi pahasına daha kalabalık takımlar oluşturur. Rekabet yasağı sözleşmeleri firmaların mevcut çalışanlarına yönelik rekabeti engellemek amacıyla kullanabileceği alternatif bir yol olduğu için, bu sözleşmelerin yasal olarak tanınması firmaların takım üyesi sayılarını arttırma motivasyonunu düşürecektir. Tezimde, bu varsayımımın ampirik olarak ne kadar doğru olduğunu araştırıyorum. Bu amaç doğrultusunda, Amerika Birleşik Devletleri Patent ve Marka Ofisi tarafından verilen patentlerin verisini ve 1985 yılında Michigan eyaletinde rekabet yasağı sözleşmelerinin hukuki olarak tanınmayacağını belirten bir kanunun yürürlükten kaldırılmasını bir farkların-farkı analizi için kullanıyorum. Bulduğum sonuçlar, rekabet yasağı sözleşmelerinin hukuki olarak tanındığı durumlarda inovatif firmaların takım sayılarını düşüreceği fikrini destekliyor.

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My mother gave me all her love and support, adapted to my ascents and descents in the best way possible. She managed to be the mother of a five years old striving for love, a fifteen years old that wants the recognition and respect, and a twenty five years old that needed advice and dialogue; often all at the same time. She is not just a caring mother, she is also a great friend and teacher.

My father also provided me with great emotional support and refined counsel, but more importantly, he furnished my intellectual development. With patience and care; evading my poetic desire of a son to be better than his father, he gave to me what he has; his memories, his ideas and his advice. I am very lucky to have recognized his importance for me while I can still talk to him.

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ABBREVIATIONS

DID - Difference-in-Differences

HTS - High-Teamsize Ratio

NCA - Non-Compete Agreement

MARA - Michigan Antitrust Reformation Act

MTS - Mean Teamsize

USPTO - United States Patent and Trademark Office

1 Introduction

Waldman (1984) builds a model in which information about a worker's ability is directly revealed only to the employer, but competitors use the job assignments of workers to infer their abilities. Specifically, promoting a worker sends a positive signal about the productivity of the worker to other employers. This, in turn, results in higher wage offers by the rival firms, and forces the employer to match those offers. Hence, promotion comes at a cost, and if this cost is sufficiently high, employer may decide not to promote some of its workers, willingly making inefficient job allocations.

This thesis uses the main idea of Waldman (1984) to investigate the behavior of innovative firms concerning their inventors. To do so, I use the parallelism between promotions and patents. Patents are similar to promotions in two fundamental aspects. First, patents have the names of their inventors and they are publicly observable. Therefore, just like from promotions, rival firms may gain information from patents. Second, patents yield a positive signal regarding their inventors. This information is perhaps more credible than a promotion, as rival firms do not just observe the assessment of the employer firm regarding their inventor; they can also observe the value of the patent themselves. This means the rather strict assumption of the direct revelation of ability (only) to the employer firm can be relaxed.

If patents are similar to promotions in these two aspects, the results of Waldman (1984) may also apply to patents. This means even if filing a patent is profitable for the firm, there can be some instances where the cost of the patent exceeds its benefit. There is a large literature on alternative strategies to patents, most prominently regarding secrecy, but this thesis intends to focus on another aspect. When it comes to promotions, not promoting an employee does not necessarily mean the higher-level job is going to be vacant; almost surely another employee will fill it. Instead, I will investigate the analog of not promoting an employee for patents. At first glance, not naming some of the inventors may seem like a feasible option, just like not promoting some of the employees, but this is potentially very costly as patent law highly favors the inventor. They may sue for the rights of the patent, which may bring enormous legal expenditures (Gattari, 2005).

The firm, however, may use an alternative strategy to giving less information; it may give more. If the number of inventors of the patent was to be higher, given the value of the innovation is similar, original inventors would receive less attention. Therefore, it would be harder for rival firms to infer the abilities of inventors as the contribution of each inventor is not publicly known. Hence, a firm may profit by arbitrarily increasing the team sizes of patents it files. Unsurprisingly, it cannot include inventors which have none or unsubstantial amount of contribution as this may result in cancellation of the patent (Gattari, 2005). However, it can devise various forms of strategies that would legally allow it to include more inventors to its patents. For example, it may require its inventors to present their ideas in joint meetings, assign revision groups to make inventors contribute to each others' projects, or departmentalize the innovation process to naturally increase the team sizes. Fasse (1991) quotes an earlier work and describe corporate R&D departments as follows: "Identifying inventors' contributions and attributing them to a joint conception becomes increasingly difficult as the number of joint inventors increases".[1] The same paper also cites an amendment to US Code Title 35 regarding patents to suggest that joint application is not necessary as long as the invention is made jointly.[2] Given that (a) it is impossible to determine the real inventor as corporate R&D's make inventors work together; (b) even if one inventor objects to others' contributions, others may still apply for the patent; and (c) the firm has multiple times more influence and resources compared to a single inventor; it is plausible that firms may use the process of joint inventorship in their favor.

In order to test whether firms arbitrarily increase the team size of their patents to prevent information signaling generated by the patents, I use the existence of an alternative approach of preventing competition over employees: non-compete agreements (NCA). These agreements forbid employees to work for rival firms in the same industry for a certain amount of time after leaving their job. Since increasing team sizes often come at organizational costs, there can be firms that are more inclined to sign NCAs if presented the option. Not all states

[1] Witte&Guttag, supra note 16, at 476: "Ideas leading to useful inventions often are the result of 'brainstorming' sessions rather [in which it] is sometimes difficult to determine.., who contributed to the particular invention."

[2] 35 U.S.C. § 116, para. 2 (1988): "If a joint inventor refuses to join in an application for patent.., the application may be made by the other inventor on behalf of himself and the omitted inventor. "

enforce NCAs, however. Until 1985, 11 states were not legally recognizing them, namely Alaska, California, Connecticut, Michigan, Minnesota, Montana, Nevada, North Dakota, Oklahoma, Washington, and West Virginia, which will be called non-enforcing states from now on. Michigan adopted the Michigan Antitrust Reform Act (MARA) in 1985 that changed the law on the non-enforceability of NCAs. Hence, any previous and future NCAs were recognized as legally binding documents inside the state of Michigan.

This means, as far as my theory applies, firms in Michigan have less incentive to increase team sizes in the patents they file after the enactment of MARA, relative to non-enforcing states. Hence, MARA created a viable setting for a difference-in-differences (DID) analysis for me to test my theory. The main results at the firm level show a 3.15 percent decrease in the average team size of firms after the enactment of MARA, relative to non-enforcing states. The results vary between different models but remain significant in almost all robustness tests.

In the next section I will discuss two branches of literature influencing this thesis. The first is a series of papers that uses DID analysis to test the effects of law changes on firm and employee behaviour. The second one is about the role of promotions as a signal of ability. Finally, I will formally introduce my hypotheses. Section 3 is about the empirical strategy, data, and variables. Section 4 present the results, at the state, firm and inventor levels. I conclude in Section 5.

2 Literature Review and Hypotheses

2.1 Using DID to Estimate the Effects of Law Changes

Some of the important work in this literature is about the effects of wrongful discharge laws. The paper by Autor et al. (2006) finds that one of the wrongful discharge laws, the implied contract exception, reduces employment by around 1%. Contrarily, one year later MacLeod and Nakavachara (2007) find that the implied contract exception, as well as good faith policy, enhances employment. Acharya et al. (2014) argue in favor of the wrongful discharge laws and finds a positive relation between their passage and innovation. They also theoretically show that wrongful discharge laws limit employers' ability to act in bad faith after the success of innovation and this encourages workers to put more effort. Ekinci and Wehrheim (2020) further support these findings by showing that increased firing costs through legal costs created by wrongful discharge laws promote employee effort and turnover.

The effects of NCAs are generally regarded more negatively than of wrongful discharge laws. Garmaise (2011) finds that although NCAs increase the firms' investment in its employees, it reduces employees' investments in their human capital and the latter effect is empirically dominating. Stam (2019) shows that NCAs limit new innovative firm formation through limiting inventor mobility. Starr et al. (2020) further support the view that NCAs limit employee mobility by using a labor participation survey with a sample of 11,500 representing the US on a national level. Finally, Johnson et al. (2021) show that NCA enforceability diminishes employee earnings and this effect increases for women and non-whites.

Some papers focus specifically on MARA. Most notably Marx et al. (2009), which I borrow the setting from, show that Michigan's legal recognition of NCAs reduces inventor mobility, and this effect exacerbates for inventors with more firm-specific skills. Marx et al. (2015) further show that enforceability of NCAs drive inventors away from Michigan to states that do not enforce NCAs. Contrary to the previous findings, Carlino (2017) uses MARA to argue that NCAs actually promote startup job creation rate significantly, and it also increases the number of quality-adjusted mechanical patents in Michigan, which are highly important for Michigan's industry.

2.2 The Role of Promotion as a Signal of Ability

Most of the papers in the literature are based on Waldman (1984). This thesis will explain two of these papers in detail. They both investigate the heterogeneity between different types of workers and the differences in firms' promotion decisions regarding these workers.

Milgrom and Oster (1987) extend the theory on signaling role of promotions to explain discrimination and wage differences across different employees. They argue that non-white and female employees are more likely to stay in lower positions not just because of taste-based discrimination, but also indirectly through firm decisions. According to their hypothesis, firms realize that not promoting employees for a long time prevents the effect of prejudice towards them from disappearing, so the firms can retain such employees without increasing their wages. Contrary to these disadvantaged groups, white male workers have less to gain from promotions as prior expectations of their abilities are higher. Therefore, it is less costly to promote them, but since promotions come with wage increases, the difference in wages between disadvantaged and advantaged groups does not converge.

More recently, DeVaro and Waldman (2012) construct a model that takes into account the education levels of the employees, following Bernhardt (1995). Since education level is a signal of ability, all firms in the market have some information about existing workers. But the ability levels of workers are revealed only to the current employer. Because a worker with less education has a lower prior expected ability level, promotion causes a larger increase in their inferred ability level compared to an educated worker. This in turn means they get better wage offers, and the current employer must offer a higher wage increase compared to a more educated worker. Hence, the firm has an incentive to promote more educated workers instead of less-educated ones, ceteris paribus. The authors use internal data from a firm to test their hypotheses and find that the firm distorts high-school workers' promotion decisions more often compared to bachelor's and master's degree holders.

2.3 Theory

This thesis presents two testable hypotheses. The first one constitutes the main subject of the thesis and is discussed extensively throughout the paper. The second hypothesis is an extension of the thesis and explores the heterogeneity in prior expectations of firms regarding workers, deriving its theory from the last section.

I have claimed that a patent is like a promotion in two ways: It is publicly observable by all firms in the market and it gives a positive signal about employees (inventors in my setting) of a firm. In both aspects, a patent is more reliable than a promotion, as it is in written format, accessible, investigated by the USPTO, and can be privately evaluated by all firms in the market. Then, it is possible to apply the framework on promotions to patents. However, there is still an important difference between promotions and patents. While a firm may not promote a *deserving* employee, it cannot hide the name of an inventor, as the patent law strictly prevents such behavior. Therefore, all inventors of a patent must be written on the patent, and the firm may not hide skilled inventors from the market if it decides to file a patent. But it can add additional inventors who did not increase the value of the patent considerably. Then, the signal for each inventor will be lower, and it would be much harder for rival firms to properly assign the credit for the patent. Therefore, a firm may reduce the positive signal generated by a patent by increasing the team size of the patent.

This is only one way for the firm to prevent competition over an inventor and evade the subsequent wage increase. Another way is to make its employees sign NCAs. This can be more profitable for some firms, so incentives for increasing team sizes will be lower if NCAs are legally recognized. With the enactment of MARA, Michigan started to enforce NCAs in courts of law and offered me a chance to test my theory. If firms distort positive signals generated by patents by increasing the team size of patents, and as NCAs are alternatives to this strategy, one would expect a decrease in team sizes of patents in Michigan after the enactment of MARA, relative to non-enforcing states. Let me formally state this.

Hypothesis 1: Relative to non-enforcing states, team sizes of patents in Michigan should decrease after the enactment of MARA.

On top of Waldman (1984), one may further investigate the possibilities of applying the theories of more advanced models by Milgrom and Oster (1987) and DeVaro and Waldman (2012) to patents and inventors. If there is heterogeneity among workers in their prior signals of ability, then the employer firm's decision of whom to promote may reflect this heterogeneity. In my setting, if there is heterogeneity among inventors, some of them will be in higher-sized teams compared to others, as firms will lose more if those inventors' true abilities are revealed to the market. Let me work through an example. An inventor A who has a degree from an Ivy League college with a high GPA is expected to have a better wage than an inventor B from a local university with average grades; not necessarily because of better skills but because of higher expected productivity. Once their true productivity is realized, however, B may turn out to be as good as or better than A. In this case, a patent application by B will send a strong positive signal that wipes out previous ones and a higher wage increase would be necessary to retain B at the firm compared to A. Therefore, I would expect inventors with lower *ex-ante* expectations to be more prone to signing NCAs or signal disruptions. If what I have been discussing up to now is true, then there should be a positive correlation between the value of the signal and the size of the team. I have already stated that inventors like B gain more than inventors like A, and so inventors like B are expected to be in teams with higher sizes. I formally state this idea with the following hypotheses:

Hypothesis 2: Relative to non-enforcing states, skilled inventors with lower prior expected ability levels are more likely to be in lower-sized teams in their patents after the enactment of MARA.

The problem with this setting, however, is in its empirical testing. I can only observe the patents of inventors, not their skill levels, their education, or their age. I can only deduce the experience of an inventor if they file at least two patents. More than a third of the inventors in my data do not. Moreover, innovation is a stochastic process so the experience levels prior to a patent application may vary a lot. Therefore, it is not easy to construct a good proxy to measure experience. I can, however, measure the value of the patent as well as the firm-specific skills of the inventor. This is what I will do when I test for *Hypothesis 2* with inventor-level data.

3 Empirical Framework

3.1 Strategy

If there is a considerable number of firms in Michigan which (a) preferred to hide their promising employees inside teams before the enactment of MARA and (b) preferred signing NCAs over-inflating team sizes after the enactment of MARA; then I would expect to see a decline in team sizes of patents in Michigan sometime after the enactment of MARA. Note that observing such change is not enough as there may be many other regional or national phenomena that occurred around the years MARA was enacted and are not specific to Michigan. Then, to eliminate such confounding effects, one needs to incorporate some form of control. Fortunately, since Michigan is unique among non-enforcing states to reverse its law, other non-enforcing states prove to be a diverse and populous control group that can be compared with Michigan. Hence, it is safe to conclude that the enactment of MARA has created a setting that is suitable for a difference-in-differences (DID) analysis to study the behavior of innovative firms and the effect of NCAs on innovation process. This thesis will investigate whether the change in the team sizes of patents in Michigan is statistically different from the change in the team size of patents in other non-enforcing states after MARA.

As with any DID analysis, parallel trend assumption proves to be crucial. Note that the only identifier of the treatment is time, the year that the enactment of MARA started to affect firm decision making; regardless of which that year may be. In other words, it is not possible to detect if a firm has shifted its policy on patenting because of the change in the enforceability of NCAs; one can only detect if it has shifted its policy after the change. Then even if one controls for other states and conducts a range of robustness tests, it is impossible to show that there is nothing else that happened in Michigan around the years when MARA was enacted that may influence firms' patenting policy. Therefore, after adequate discussion and elimination of likely scenarios, the burden of proof does not lie on the thesis. In other words, unless suggested otherwise with a plausible reason, I assume that team sizes of patents of Michigan and control states would have followed parallel trends if not for the enactment of MARA.

3.2 Data

I use the patent data provided by the USPTO and made easily accessible by Li et al. (2014), which originally comprised 4,243,972 patents. The pioneering work of Hall et al. (2001) also guided me in working with the data. After leaving out enforcing states and years with a very small number of observations, the final data included patents with application dates between 1970 and 2009, in the eleven non-enforcing states. I use state-level, firm-level and inventor-level data. By a variable/data at firm-level, what is meant is that each observation of that variable is unique for a given firm, year, and state. (Note that the same firm often takes patents in different states, in the same year.) In other words, each variable shows information regarding a firm in that given year and state. Although patents are uniquely identified as they are applied for only once, firms and inventors often appear more than once. There are 666,879 observations in the state-level data, which amounts to 199,352 firm-level observations and 835,629 inventor-level observations. Despite the great efforts by the two mentioned studies, information is still missing on some patents like their full list of citations, inventors, and assignees. Therefore, not all the variables have the same number of observations; but most of them are very close to the total number in their respective datasets. Percentages of patents for states are given in Table 3.1.

A patent is a very detailed document that features various information like the inventor(s), assignee(s), technology class(es), citations given to other patents, date applied for and granted, date of expiry, and of course details of the innovation. There are four separate data files used by this thesis; citation file (divided into two) where the level of observation is citation(s) a patent gives (if a patent gives three citations, that patent appears three times in the data), the class file where the level of observation is USPTO technology class(es) of a patent, assignee file where the level of observation is the assignee(s) of a patent, and inventor file where the level of observation is the inventor(s) of a patent. Several decisions need to be made on how to combine these files into a single dataset and how to change the level of observations, which is discussed in detail in Appendix A.

Table 3.1: Firm-Level Observations by State

State	Frequency	Percent
AK	324	0.16
CA	112,369	56.37
CT	18,047	9.05
MI	27,081	13.58
MN	17,358	8.71
MT	946	0.47
ND	621	0.31
NV	2,310	1.16
OK	4,807	2.41
WA	13,668	6.86
WV	1,821	0.91
Total	199,352	100.00

3.3 Variables

In this section a detailed description of the construction, content, and meaning of the variables is given. Most of the results in this thesis are at the firm-level, so all variables discussed in this section are at firm-level. A majority of variables used in other levels of analysis are counterparts of these. Table 3.2 shows the descriptive statistics of the main variables.

3.3.1 Dependent Variables

I use two dependent variables to measure team size. The first one is perhaps the most natural; the average team size in patents (of a firm, given a year and a state), named mean-teamsize (MTS). The second dependent variable used in the analysis is the ratio of patents with five or more inventors to all patents (of a firm, given a year and state), depicted as high-teamsize-ratio (HTS). The main issue with this variable is the arbitrary threshold of five or more inventors. There are some non-conclusive but good reasons for this choice. The first one is the percentile; the team size of five amounts to the 90th percentile. The second is the relative change between the team sizes of four to five; the drop as percentage of all patents is highest when team sizes increase from four to five. The distribution of team sizes is given in Table A1. I also include a robustness check for different thresholds for *HTS* in Table B4, which shows an increase of statistical significance towards the original threshold. Nearly all regressions include both dependent variables as there are often important differences about how they behave. There is more discussion offered about dependent variables in the results section.

3.3.2 Control Variables

Number of Patents: I control firm size through the number of patents taken by a firm. The logarithm of this measure is taken for controlling outliers, resulting in the variable *log(Patnum)*. This is a very simple but highly important control and is included in almost all regressions, as there is a weak but positive correlation between bigger firms and higher team sizes. Also, larger assignees are often located in states with high innovative output, meaning such states often have higher average team sizes. Since Michigan is the second-ranking state in terms of assigned patents, not including firm size as a control will likely result in omitted variable bias.

Value of Patents: I use the average number of citations received by a firm's patents as a measure of patent's influence. The logarithm of one plus influence is taken to produce *log(Influence)*. I also used the tech-and-year-weighted version of this variable as well as both of their interaction with the number of patents of the firm, but the results did not alter much, so only the simplest and most widely used measure is included in the regressions.

Technology HHI Index: The technological composition of a firm is often an important determinant of team sizes. For example, a firm with different technological departments has a better chance to form interdisciplinary teams with multiple inventors. Although the industry ratios catch this effect, they are overall too costly for the regressions. Hence, I make use of the Herfindahl-Hirschman Index calculated for the six NBER categories with the variable *Technology Category HHI*. Since the technology classes are non-exclusive, the HHI ratio is often greater than 1. See Appendix for explanations and Table A3 for descriptive statistics of

Table 3.2: Firm-Level Descriptive Statistics

Variable	Observations	Mean	Std. Dev.	Min	Max
Postmara	199,352	.837	.37	0	1
Michigan*Postmara	199,352	.107	.309	0	1
Mean Teamsize	199,352	2.384	1.632	1	41
High Teamsize Ratio	199,352	.096	.27	0	1
log(Patnum)	199,352	1.006	.625	.693	7.846
log(Influence)	198,472	.466	.479	0	7.051
Technology Category HHI	199,352	1.095	.587	0	5
Enforcing Inventor Primary	199,352	.140	.328	0	5

technology categories of patents.

Ratio of Primary Inventors From Enforcing States: The first listed inventor, or primary inventor, is often regarded higher than others. The USPTO, for example, only includes the states of primary inventors in its data (Hall et al., 2001). It is also likely that the primary inventor lives in the main facility of a firm. Moreover, if a firm has a considerable number of primary inventors in enforcing states, the policy of that firm regarding its patenting and team sizes may vary from the rest of the sample. To control for such effects, I use the variable *Enforcing Inventor Primary* which shows the ratio of patents with primary inventors from enforcing states to all patents. The decision to not exclude all patents which have inventors from enforcing states is discussed in Appendix A.

4 Results

4.1 State-Level

In Table 4.1 the difference-in-differences of average team size of patents between Michigan and other non-enforcing states is shown. As a percentage of Michigan's average team size in the whole sample, the DID estimator amounts to 2.91%. There are two important observations: First, there is a solid decline in the average team size of patents in Michigan after MARA relative to non-enforcing states. Second, the change is not very large. This second result requires further consideration.

In all theoretical models I have considered, the result applies to only a group of inventors. In Waldman (1984) this group are the employees with slightly higher ability than the average of the pool, in Milgrom and Oster (1987) this group are skilled but discriminated workers and finally in DeVaro and Waldman (2012) these are the skilled high-school graduates. In my setting, even though I have not properly defined skilled inventors with lower prior expectations of ability, this constitutes a relatively small subgroup in the sample. Therefore, I do not expect a large change in the average team size of patents in Michigan after MARA.

Secondly, I expect to see the change of behavior in only a subgroup of patents. The incentives change only for medium to large firms, so the team sizes only change for their patents. Contrarily, there are many small firms in the data, which are unaffected by MARA. Moreover, since NCAs may not always be the better option, one may not see a change in the behavior of some firms. There can be more detailed analysis made on the subgroup of inventors and firms that one expects to see a change, but I will leave this as an open question for future policy analyses. Instead, I would like to note that the coefficients of interest should not be taken at face value, but the changes in them between different models are still relevant.

Table 4.1: State-Level Difference in Differences Analysis

Variable	**Average Team Size**
Pre-Mara Michigan	1.610
Post-Mara Michigan	2.513
Difference in Michigan	*0.903*
Pre-Mara Control States	1.700
Post-Mara Control States	2.670
Difference in Control States	*0.970*
Difference-in-Differences	**-0.067**
Diff-in-Diff as Percentage of Michigan	**-2.91 %**

This preliminary state-level DID estimation is further supported by the synthetic control estimation of post-MARA Michigan, which constructs a synthetic Michigan using the rest of the control states.[3] As control variables alongside the dependent variable, I follow Marx et al. (2009) and put the following five variables about states: land size, population, GDP, personal income, and the number of proprietors; and two automotive industry variables, one showing the number of patents of firms with at least one automotive patent ever taken and another one showing the number of patents of firms with at least half of patents are from the automotive industry. I employ various thresholds for the latter automotive industry control (10 percent and 25 percent), but the results do not alter. Figure 4.1 shows results for *MTS* and Figure 4.2 shows results for *HTS*. The dotted line represents would-be Michigan if treatment were not to take effect. The continuous line is real Michigan.

There are two important points to consider when analyzing such a graph. First, in the pre-intervention period which lies at the left side of the vertical line, both treated and synthetic Michigan should display the same behavior. Any difference between them will increase the root mean squared prediction error (RMSPE). In this aspect, both figures behave well, although there are slight deviations. Second, the difference after the intervention should be clear. Again, in both figures, the reader may see the clear separation between the lines.

[3] See Abadie and Gardeazabal (2003) and Abadie et al. (2010) for empirical applications, and Abadie (2021) for the theory of synthetic control methodology.

Figure 4.1: Synthetic Michigan, state-level, dependent variable is MTS.

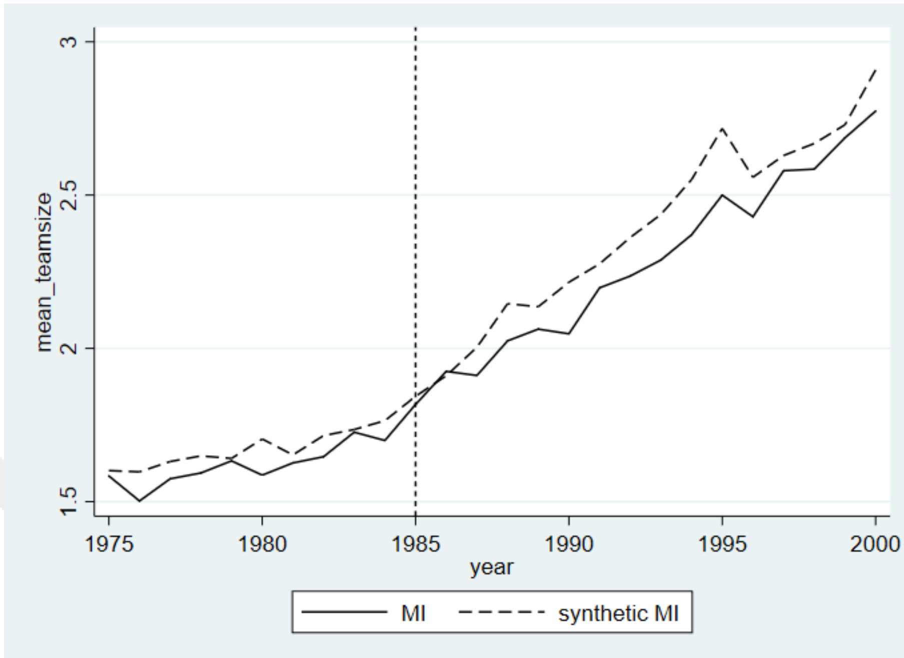
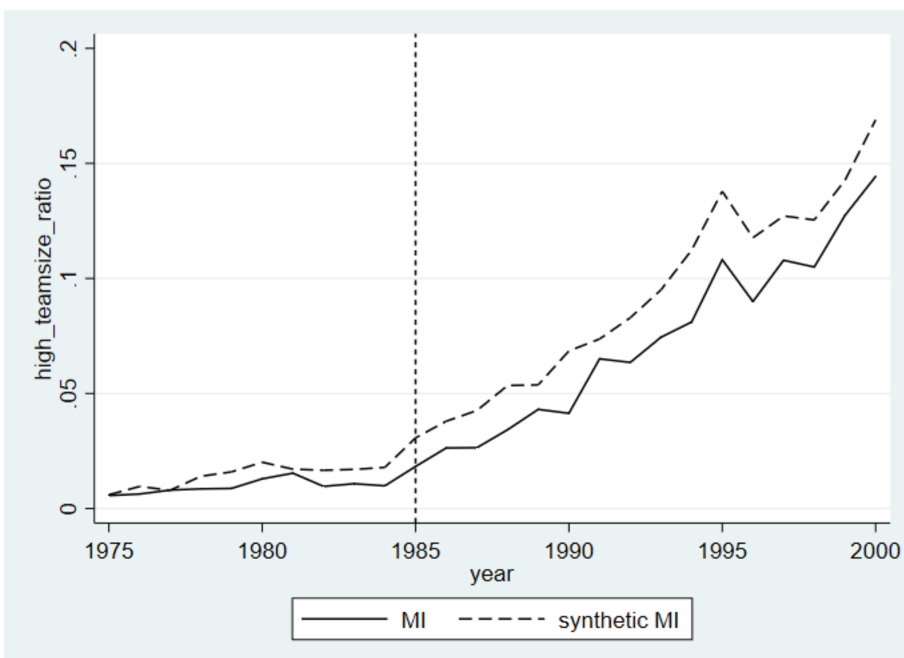


Figure 4.2: Synthetic Michigan, state-level, dependent variable is HTS



Synthetic control methodology is bias-free apart from the choice of which variables it will use. However, such a choice may be regarded as too much of a burden on part of objectivity. Even though the control choice of Marx et al. (2009) is often sound, like the automotive industry or the gross-domestic-product of the state, it is not very intuitive for controls like the land size of the state or number of proprietors living there; as these have no observable *direct* effect on technological industries of states. Hence I opt for allowing a broader range of variables for checking the robustness of the results.

The US Bureau of Economic Analysis offers dozens of variables for US states.[4] I use 121 of them, specifically 42 variables from SAGDP2 and 79 variables from SAINC30. A full list of these variables can be found in Appendix D. 15 other variables already used by the thesis are included in this dataset, making a total of 136 variables. I wrote an algorithm that conducts a synthetic regression for each variable, stores RMSPEs, and permanently includes the variable in the model that has produced the lowest RMSPE. Then it conducts the same process again, stopping only if the RMSPE of the regression can no longer be minimized. An important note is that this process does not give the subset of variables producing the minimum RMSPE as the technical limits do not allow to test for all subsets. Theoretically, for a set of variables consisting of X, Y, and Z, the algorithm may pick X from all three, and given X is picked, it may then choose Y over Z and then stop to choose (X, Y), while the minimum RMSPE belongs to the couple (Y, Z). Nonetheless, it gives the second-best option, as the RMSPE values show in the following paragraph. The results can be found below in Figure 4.3 (when the dependent variable is *MTS*) and Figure 4.4 (when the dependent variable is *HTS*).

[4] See The US Bureau of Economic Analysis; https://apps.bea.gov/itable/iTable.cfm?ReqID=70&step=1

Figure 4.3: Synthetic Michigan with algorithmically picked controls, state-level, dependent variable is MTS

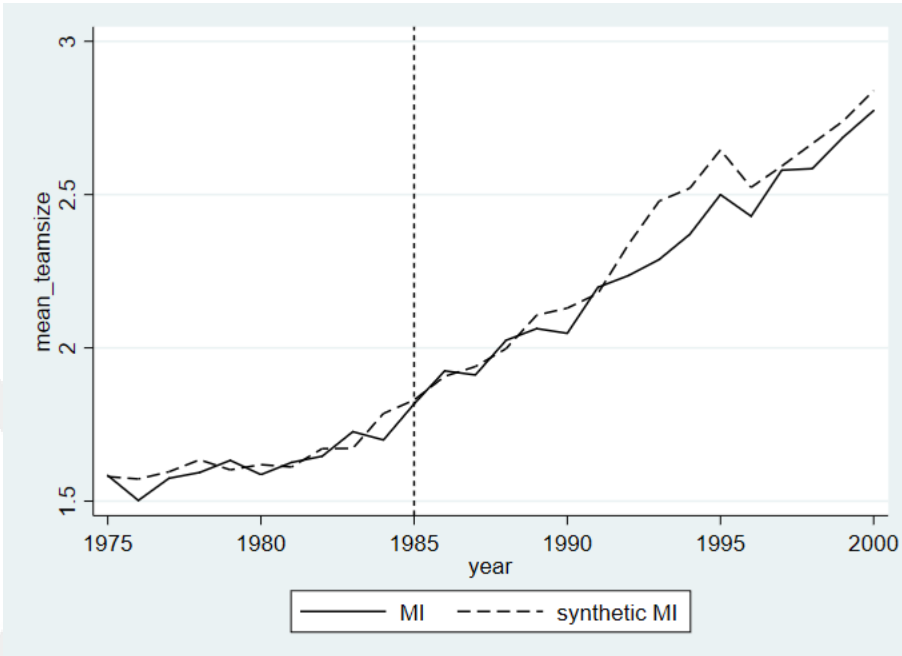
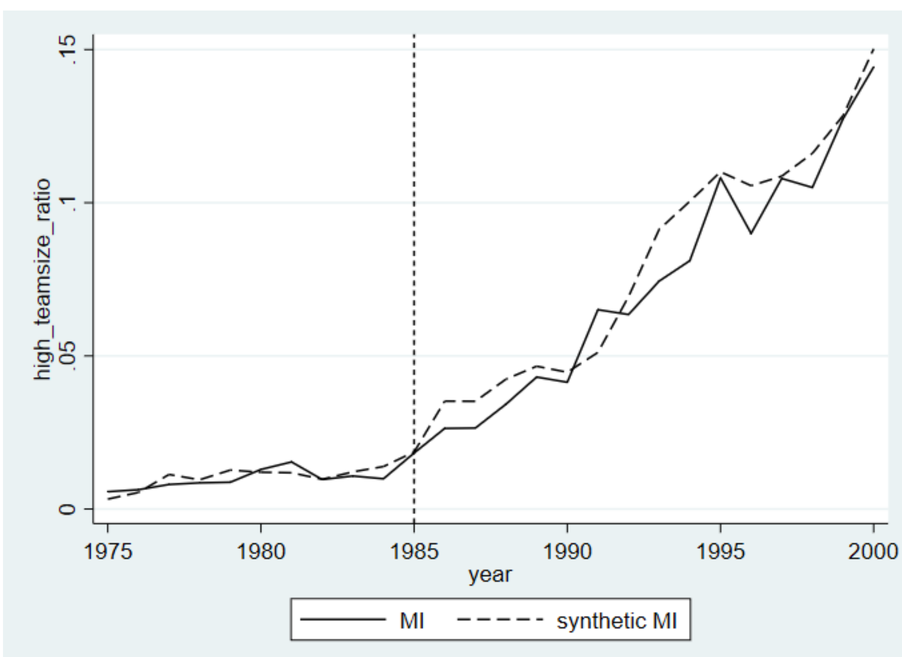


Figure 4.4: Synthetic Michigan with algorithmically picked controls, state-level, dependent variable is HTS



The results for the pre-intervention period are significantly better compared to handpicked controls.[5] Specifically, the RMSPE of the regression drops from .0602 to .0432 when the dependent variable is *MTS* and from .0064 to .0024 when the dependent variable is *HTS*. A brief look at the differences in Figures 4.1 and 4.3 and Figures 4.2 and 4.4 for pre-intervention periods should also be sufficient. Nonetheless, the post-intervention periods are also more closely matched, leaving only a slight increase in *MTS*, and a comparatively smaller increase in *HTS* for synthetic Michigan. One reason for this is the reverse argument that is given for the robustness test: Due to low sample size and high control size, the algorithm may pick controls that are unrelated to the dependent variables but coincidentally mimic the behavior of pre-intervention periods and behave randomly in post-intervention periods. Although this is entirely possible, it must be noted that identifying the real determinants of a state's similarity to other states is very hard. Moreover, if the low sample size argument were to be true, one would expect a random behavior after the optimization (pre-intervention) period, but both regressions display some form of fit. Another reason is that what explains Michigan before MARA and after MARA are different; not necessarily because of MARA but some other factors. The reason that the controls used by Marx et al. (2009) includes auto-industry and not glass products is that there is plausible doubt that the automotive industry explains what happened in Michigan after 1985. Since the algorithm is unaware of this situation, it may fail to incorporate the right variables. Suspecting this, I include GDP, population, and the number of patents of firms with at least one automotive patent ever taken to the synthetic regression and run the algorithm afterward. The results can be found below in Figure 4.5 (when the dependent variable is *MTS*) and Figure 4.6 (when the dependent variable is *HTS*).[6]

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[5] The algorithm picks "Proprietor Number", "Food and Kindred Products", "Industrial Machinery and Equipment", "Miscellaneous Repair Services", "Hotels and Other Lodging Places", "Business Services and Other Services" and "Motor Vehicles and Equipment" as controls for *MTS* and "Transportation Services", "Paper and Allied Products", "Rubber and Miscellaneous Plastics Products", "Military", "Tobacco Products", "Farm Proprietors' Employment" and "Per Capita Personal Income" for *HTS*.

[6] With the exogenous controls, the algorithm now additionally picks "Wage and Salary Employment", "Medical-Ratio", and "Amusement and Recreation Services" for *MTS*, and "Other Services", "Private Households", "Paper and Allied Products", and "Income Maintenance Benefits" for *HTS*.

Figure 4.5: Synthetic Michigan with both exogenous and algorithmically picked controls, state-level, dependent variable is MTS

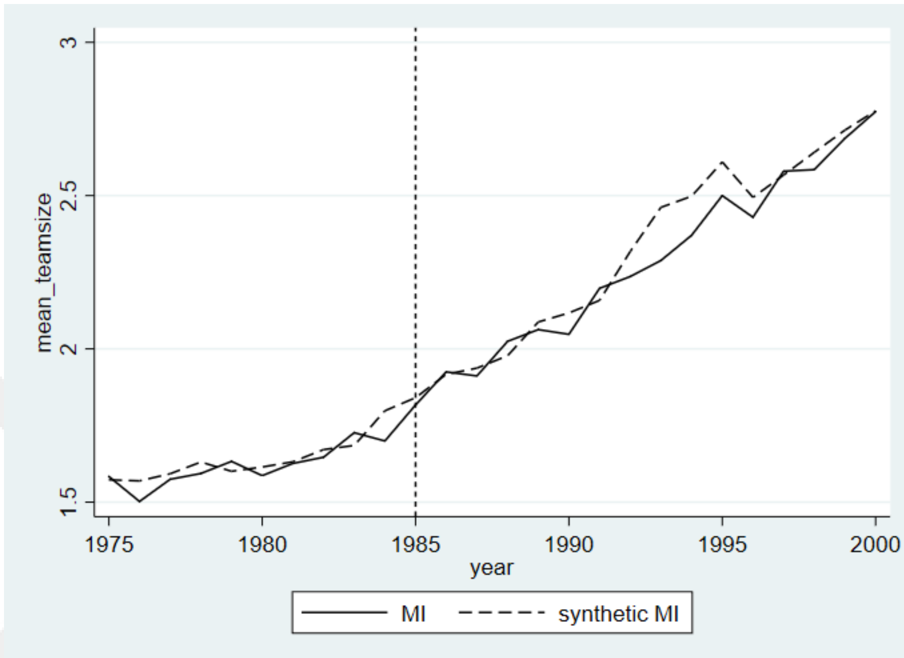
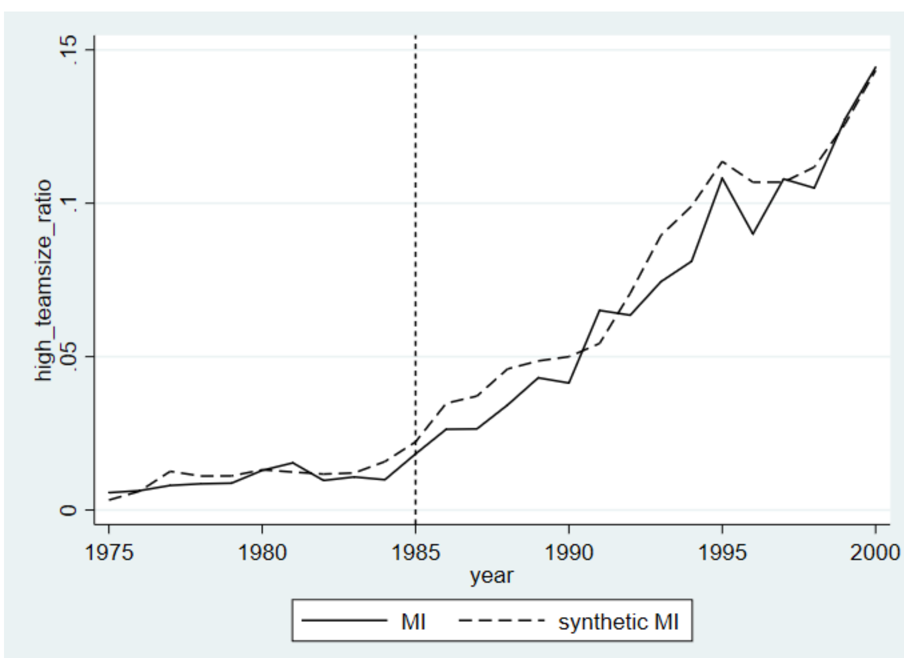


Figure 4.6: Synthetic Michigan with both exogenous and algorithmically picked controls, state-level, dependent variable is HTS



4.2 Firm-Level

Tables 4.2 and 4.3 show firm-level regressions for *MTS* and *HTS* as dependent variables, respectively. In Model 1 *MTS* is regressed only on the DID estimator, *Postmara* and state and year fixed effects. The DID estimator's coefficient, interaction of *Postmara* and *Michigan*, hereafter referred as the treatment, is significant at 5% level. Model 2 introduces a linear time trend which does not alter the results. Model 3 to 6 are the addition of four control variables separately, *log-patnum*, *log-influence*, *tech-category-hhi* and *enforcing-inventor-primary* to the previous model. Model 7 includes all of these controls. This is the baseline model for firm-level as robustness checks are variations of this version.

These basic regressions show that team sizes in Michigan decreased relative to other non-enforcing states after the enactment of MARA. The effect substantially differs for the two dependent variables though. While the economic impact of the treatment is larger for *MTS* in terms of percentage (3.15% compared to 1.6%) , its statistical significance is considerably higher for *HTS*. One explanation for this fact lies in the difference of samples of these two variables. For example, suppose that a firm issues only a single patent in California both in 1982 and in 1983. If the patent in 1982 has a team size of 8 and the patent in 1983 has a team size of 3, then both dependent variables will capture this change. But if the team sizes were to be 16 and 14 respectively, *HTS* would still be 1 for this firm, while *MTS* would change slightly. This creates more variation and allows *MTS* to capture more of the change in team sizes after MARA, but with less precision. Through robustness tests, this property of *MTS* will often make it insignificant, while *HTS* stays quite significant. It is, therefore, important to keep both of them to correctly identify the effect.

Table 4.2: Firm-Level Results, Dependent Variable is MTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.051**	-0.051**	-0.049*	-0.050*	-0.061**	-0.072***	-0.075***
	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.024)	(0.024)
Michigan	-0.126	-0.126	-0.142	-0.136	-0.104	0.000	-0.029
	(0.091)	(0.091)	(0.091)	(0.091)	(0.090)	(0.085)	(0.085)
Postmara	1.031***	1.160	1.067	0.765	-0.624	0.805	-1.564
	(0.161)	(1.683)	(1.682)	(1.690)	(1.677)	(1.574)	(1.572)
Log(Patnum)			0.070***				0.165***
			(0.006)				(0.005)
Log(Influence)				0.088***			0.100***
				(0.008)			(0.007)
Technology Category HHI					0.247***		0.233***
					(0.006)		(0.006)
Enforcing Primary Inventors						1.727***	1.723***
						(0.010)	(0.010)
Time Trend		-0.003	-0.001	0.008	0.046	0.003	0.068*
		(0.044)	(0.044)	(0.044)	(0.044)	(0.041)	(0.041)
Observations	199,352	199,352	199,352	198,472	199,352	199,352	198,472
R-squared	0.065	0.065	0.065	0.066	0.072	0.182	0.192
State&Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Trend	No	Yes	Yes	Yes	Yes	Yes	Yes

Table 4.3: Firm-Level Results, Dependent Variable is HTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.013***	-0.013***	-0.013***	-0.013***	-0.014***	-0.015***	-0.016***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Michigan	-0.012	-0.012	-0.013	-0.013	-0.009	0.003	-0.001
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Postmara	0.122***	0.195	0.185	0.111	-0.015	0.154	-0.160
	(0.027)	(0.283)	(0.283)	(0.284)	(0.282)	(0.274)	(0.275)
log(Patnum)			0.007***				0.018***
			(0.001)				(0.001)
log(Influence)				0.011***			0.012***
				(0.001)			(0.001)
Technology Category HHI					0.029***		0.027***
					(0.001)		(0.001)
Enforcing Primary Inventors						0.197***	0.196***
						(0.002)	(0.002)
Time Trend		-0.002	-0.002	0.000	0.004	-0.001	0.007
		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	199,352	199,352	199,352	198,472	199,352	199,352	198,472
R-squared	0.034	0.034	0.035	0.035	0.038	0.090	0.095
State&Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Trend	No	Yes	Yes	Yes	Yes	Yes	Yes

I conduct several robustness tests at firm-level. In all tests the same setting as in Model 3 of the Firm-Level Results (Tables 4.2 and 4.3) are used, i.e., time trend, state and time fixed effects, *log(Patnum)*, *log(Influence)*, *Tech Category HHI*, and *Enforcing Inventor Primary* are included in these regressions.

First, placebo DID variables are constructed for each state. The results are in Table B1 for *MTS* and Table B2 for *HTS*. For *MTS*, what is most interesting is the coefficient of the placebo treatment for Washington, which has a statistically significant and negative coefficient. I do not know what causes this, but the effect completely disappears for *HTS*, which should not be the case given the setting. So I am inclined to think that random factors cause it. For *HTS* Nevada and Oklahoma show negative statistical significance. Since both states have low sample sizes (only in hundreds in pre-intervention periods) with low statistical significance I again tend to think that randomness determines these results. If Connecticut or Washington were to yield such results for *HTS*, other explanations would have been necessary.

A potential problem arises since observations from California make 57% of the data and may drive the results by themselves. A second robustness test, given in Table B3, addresses this possibility by dropping all California observations in (Models 1 and 2) and including only California as control state (Models 3 and 4). Not including California reduces the significance considerably but the treatment is still significant for both dependent variables. Only including California improves the significance.

A possible objection that has been stated in the *Data and Variables* section is the ultimately arbitrary threshold for the dependent variable *HTS*. In a third robustness test the required number of inventors for a patent to be qualified as "high team size" is changed to three, four, six, seven, eight, and nine. The results are in Table B4. The significance of the results climax towards the choice of five, while six also shows good significance. The effect starts to disappear when the threshold moves to four and seven. There are two separate effects reducing significance while moving towards different poles. When the threshold value decreases, more "ordinary" patents flow in. Contrarily, when the threshold increases, the number of patents that I am interested in drops dramatically, giving almost no observations for some states. Since pre-MARA Michigan observations are 2.1% of the data, this effect destroys any significance. Notice that both increasing and decreasing the threshold still yield negative coefficients, supporting this explanation. Table A1 has information on the distribution of team sizes of patents.

I further test for the effects of through time that may have been occurred after the enactment of MARA. In two separate tests. If there is a state-wide change, for example after the year 1989, that dramatically changed the team sizes, it is possible that fixed time effects fail to capture this ongoing change, so as the time trend which encompasses the entire dataset. Therefore regressions mistakenly show this unrelated change as a part of the treatment. I, therefore, analyze the effect in time window samples, which includes time intervals centered around the enactment of MARA in 1985. The results are in Table B5. The effect of MARA starts to appear with the 5-year window. It is also possible that the regressions capture a change before MARA and unrelated to my theory, or that there are simultaneous efforts by the state of Michigan that are correlated with the enactment of MARA and I observe their impact. To test whether the effect starts before 1985, I construct dynamic effects in Table B6. The results are in favor of my theory; there is no statistically significant effect before the enactment of MARA while there is a significant effect after the enactment of MARA that can be seen in Models 5, 9, and 10.

Technological developments and the shift in industries are an important topic in the literature on innovation. I have previously discussed that Michigan's automotive industry was in decline during the years of the study. There may be other observed and unobserved shifts in industries of Michigan around the enactment year of MARA that the regressions capture as the effect of MARA. In order to check for such possibilities I include the technology groups of patents as controls in Table B7 for *MTS* and Table B8 for *HTS*. More information regarding the technology groups of patents can be found in Appendix A and the distribution of patents across technology categories in Table A3. When I control for technology categories separately through models 1-7, the results are robust, showing that there is no particular effect of an industry shift that drives the results. In these regressions, I use binary variables which indicate whether half or more of firms' patents have the corresponding NBER technology category. In model 8 I combine the variables of models 1-5 into a single regression; which eliminates any significance in *MTS* regression, but treatment in *HTS* regression still holds at 5% confidence level. Technology groups are important determiners of team sizes; different industries have naturally different ways of organizing their inventor teams; therefore it is expected for these variables to soak up most of the variation in the data. In model 9, instead of binary variables, I use variables showing the ratio of patents in an NBER technology category to all patents. This further reduces the significance of *HTS* regression, but the treatment is still statistically meaningful at 10% confidence level.

In Tables B9 and B10 I include additional controls of *generality*, *originality* and *self-reference* used widely by the literature, as well as the variable *foreign-assignee*, which shows whether the assignee is a US firm or not. More explanation regarding this variables can be found in Appendix A. They do not change the results.

I also change how I construct the data in Tables B11 and B12, which again show no dramatic effect that alters the results. The discussion of why and how I run these regressions can be found in Appendix A.

Finally, I conduct synthetic control analyses at the firm-level, which show similar results. The difference in the average team sizes between treated and synthetic Michigan is 1.2 percent. The results of the synthetic regressions are in Figures B1 to B6. Figures displaying *HTS* are especially well-behaving and show the clear-cut difference before and after the enactment of MARA.

4.3 Inventor-Level Results

At inventor-level, I construct running sum counterparts of the variables used at firm-level, which show the average value of a variable up to (and including) the current observation. I include inventors when they file a patent in a non-enforcing state; if they file a patent in an enforcing-state, I do not include that patent. Using running-sum variables, I can focus on not just the individual patents of an inventor, but on the inventor themselves as each observation updates the characteristics of the inventor. The dependent variable is the current team size of the inventor. The results are given in Table 4.4. In the Model 1, I only include the DID treatments and state and year fixed effects. I add the time trend in the next model. The following three models are separate inclusions of; (i) productivity variables of *Patnum* (the number of patents taken by inventor up to this patent) and *Influence-cum* (the average number of year-weighted citations received by inventors patents up to this patent) (ii) citation variables of *Generality-cum* and *Originality-cum* (cumulative versions for inventor-level of the described variables in Appendix A, exactly like *Influence-cum*) and (iii) experience variables *Patexperience* and *Patexperience-squared* showing the years passed since inventor's first patent. Model 6 includes all of the previous variables. The results of this last model are more dramatic compared to state and firm levels, as the cumulative team size of inventors drops by 3.66% relative to other-non enforcing states after the enactment of MARA.

I have discussed two papers in Section 2.2 that some workers can be more prone to not be promoted, even if this is inefficient. Specifically, Milgrom and Oster (1987) builds a model that disadvantaged groups are promoted less often and the empirical foundations of DeVaro and Waldman (2012) show that high-school educated employees are promoted less, both compared to the efficient level. However, I have not specified how this can be interpreted in my setting. I only argued that skilled inventors with lower prior expected ability levels will be in higher-sized teams. In this last part of my thesis, I will discuss *Hypothesis 2*.

One group of such inventors are the less experienced ones. Then, after the enactment of MARA, I expect to see a drop in the team sizes of inexperienced inventors relative to non-enforcing states. The data only includes observations of the patents, therefore I do not know any information regarding the experience of an inventor if they have not taken a patent before the current one. It is possible, indeed plausible, that most of the inventors have years of experience in their fields, and only through the accumulation of that experience they can get their names on a patent. Of course, there are many young and successful inventors too, but it is not possible to observe either the ages or the experiences of inventors.

What I can measure is several proxies for the experience of an inventor. First of these is the amount of time passed since the acquisition of the first patent. Note that many inventors in the data have filed only a single patent, indeed 40 percent of the observations have a value of 1 for this variable (recall that this does not mean 40 percent of inventors have taken only a single patent, the actual number is less than that). Model 1 of Table 4.5 shows the results when current patent's team size is regressed on the interaction of treatment and experience. Interestingly, experience does not affect team sizes, its coefficient is zero. As this result creates suspicion, I use three binary variables to divide up any potential effects that counteract

Table 4.4: Inventor-Level Results

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.119***	-0.119***	-0.121***	-0.107***	-0.119***	-0.100***
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Michigan	0.335***	0.335***	0.324***	0.330***	0.353***	0.332***
	(0.106)	(0.106)	(0.106)	(0.106)	(0.106)	(0.105)
Postmara	1.697***	3.391**	3.979***	3.961***	3.098**	4.897***
	(0.162)	(1.380)	(1.378)	(1.379)	(1.379)	(1.376)
Patnum			0.001***			0.006***
			(0.000)			(0.000)
Influence-cum			0.071***			0.076***
			(0.001)			(0.001)
Generality-cum				-0.208***		-0.252***
				(0.011)		(0.011)
Originality-cum				-0.082***		-0.108***
				(0.011)		(0.011)
Patexperience					0.006***	-0.001
					(0.001)	(0.001)
Patexperience-squared					-0.001***	-0.001***
					(0.000)	(0.000)
Time Trend		-0.044	-0.060*	-0.059	-0.034	-0.082**
		(0.036)	(0.036)	(0.036)	(0.036)	(0.036)
Observations	835,629	835,629	835,629	835,629	835,629	835,629
R-squared	0.093	0.093	0.098	0.095	0.104	0.110
State&Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Trend	No	Yes	Yes	Yes	Yes	Yes

each other.

In Model 2, I use a variable that divides experience into two; the interacted variable becomes 1 if the inventor's first patent was filed in the last two years and 0 otherwise. Surprisingly, the coefficient of the interaction with treatment is not significant but has a positive sign. To test this result, I use an alternative variable that shows whether this observation is the first patent of the inventor, but the coefficient only increases and becomes highly significant, meaning the team sizes of inventors who have no prior patents in Michigan have increased relative to non-enforcing states. Suspecting this result is caused by the first patents only, I use a variable that shows if the inventor has not more than one prior patent before the current observation (including no patents too). This also yields statistically significant and positive coefficients. Evidently, MARA decreased the team sizes of patents for older, experienced inventors, not the younger ones.

How can I explain this result? The first and perhaps simplest explanation is that the theory fails; either the results of Milgrom and Oster (1987) and DeVaro and Waldman (2012), among many others, are not valid (this is highly unlikely) or that my setting is not suitable for their theory. Since the results for *Hypothesis 1* are very robust, I also think the second disjunctive is also not very likely. A second explanation may lie in how I apply the theory to my setting. Maybe, younger inventors are not the ones that become the target of information distortion or NCAs, but older inventors are. If one considers the Schumpeterian nature of innovative industries, where the new wipes out the old, this explanation may carry some merit. Maybe any new inventor who can produce a patent is deemed valuable, but the market does not expect much from the older inventors. Unfortunately, I could not find any way to test this claim. Standing as it is, I still tend to believe younger inventors, who are less known by the market, are more disadvantaged. A final explanation can be found in the earlier discussion; the patent data itself provides no good proxy for the experience of inventors. Patents are not the only tool for protecting intellectual property, in fact, they are not even the most used tool (Hall et al., 2014). Hence, experience variables that I use may not be indicative of experience, but other phenomena. A supporting argument for this can be found in Model 5, where the interacted variable is the number of patents, which does not yield a significant result. This, in turn, may mean that the first model is perhaps the most precise one; the experience variables are not meaningful in this setting.

The second hypothesis does not refer to inventors with lower prior expected ability levels, but also requires them to be skilled. After all, even if an unskilled inventor files a patent, it is unlikely that this event repeats, so the employer firm would not fear losing them. But a skilled inventor has a higher chance of creating innovations, and therefore the employer firm would want to retain them. Hence, such inventors are more likely to be in higher sizes teams before the enactment of MARA, and their team sizes should decrease relatively after the enactment of MARA. To test this, I interact the variable *Influence-cum* with the treatment in Model 6. Recall that *Influence* is a proxy for the value of the patent, and it becomes a proxy for the value of the inventor when transformed to a running-sum variable. Therefore, it can be seen as the value of an inventor. The coefficient of the result is highly significant and negative. This means more valuable inventors in Michigan started to be in lower-sized teams relative to other inventors and inventors in non-enforcing states, after the enactment of MARA, exactly as *Hypothesis 2* predicts.

Finally, I use a proxy for the firm-specific skills of the inventor. *Self-reference*, explained in detail in Appendix A, shows the ratio of citations to the firm's own patents. The running-sum version is therefore shows how often an inventor cites the patents of their firm. Even though I have not discussed the theoretical predictions regarding inventors with firm-specific skills, I still want to explain my reasoning in few sentences and present the result. These inventors are valuable to their firms for two reasons: First, they have important knowledge of the firm's technological background and therefore more productive compared to regular inventors. Second, their knowledge of their firm's current level of technological progress becomes a threat when the inventor works for a competitor. For these two reasons, the firm likely wants to retain such inventors. Therefore, team sizes of inventors with high cumulative self-reference ratios should be higher before the enactment of MARA. Consequently, their team sizes should decrease more relative to other inventors and inventors in non-enforcing states after the enactment of MARA. Model 7 shows the results of regression cumulative average team size to the interaction of treatment and self-reference. The coefficient of the result is very large and significant, providing yet another support for the firm strategy of increasing team sizes to distort the signal created by the patents. The theoretical reasons behind these results are yet to be shown in future studies.

Table 4.5: Interactions with Treatment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.148***	-0.130***	-0.143***	-0.144***	-0.087***	0.090***	-0.067***
	(0.028)	(0.032)	(0.026)	(0.030)	(0.023)	(0.025)	(0.022)
Treatment*Experience	0.007						
	(0.006)						
Treatment*TwoYears		0.042					
		(0.041)					
Treatment*FirstPatent			0.113***				
			(0.040)				
Treatment*SecondPatent				0.079**			
				(0.040)			
Treatment*Patnum					-0.003		
					(0.003)		
Treatment*Influence						-0.209***	
						(0.018)	
Treatment*Self-Reference							-0.472***
							(0.109)
Observations	835,629	835,629	835,629	835,629	835,629	831,527	835,629

Note: State and year fixed effects, time trend and controls are included in all models.

5 Conclusion

In this thesis I extend theory on the role of promotions as signals of ability to explain firm behavior in innovative industries. The fact that patents are publicly available documents and give information about their inventors adds another argument to the decision-making of firms when deciding whether and how to patent their innovations. If a patent is valuable, it generates a positive signal for its inventor(s) and may result in competition over them. Then it can be profitable for the firm to reduce the value of the signal. One way of doing so is to add more inventor names to the patent file, therefore reducing the positive signal generated for each one of them. If this is the case, patent team sizes should be larger than what is optimal.

The Michigan law that makes NCAs enforceable in the state, MARA, provides an opportunity to test this hypothesis. Signing NCAs to forbid them from working for a competitor for a certain amount of time after leaving their firm is an alternative way of preventing competition. Such agreements can be more cost-effective than arbitrarily increasing the team sizes of patents. Therefore, some firms may decide to sign NCAs with their employees instead of putting them in teams to reduce the signal generated by the patent. Hence, if what I have described is true, I would be able to see the change in team sizes of patents in Michigan relative to non-enforcing states after the enactment of MARA in 1985.

My estimations show that the average team size of patents decreased by 2.91% at state-level and 3.15% at firm-level relative to non-enforcing states after the enactment of MARA. According to my theory, the decision-making of changing team sizes occurs at the firm-level, so I focus on that and conduct various robustness checks to test my results. Specifically, I create placebo treatments for control states, look in sub-samples only with and without California which constitute more than half of the data, check for dynamic effects to see any pre-intervention effects, focus on restricted time-intervals around intervention, control for the effects of technology categories of patents and the declining auto-industry of Michigan around the years of the data, construct various other controls that are indirectly related to the estimation but widely used by the literature and change how I construct the data to test for alternative setups. While some of the tests reduce the statistical significance of the treatment, it overall performs well and provides good support for my first hypothesis.

When I focus on patents with 5 or more inventors, the effect sizes drop between half to one percent, but the statistical significance of the results increases dramatically. The second dependent variable that checks for the ratio of high team-sized patents to all patents performs very well, nearly always at the 1% confidence interval. Since larger team sizes are more effective for reducing the value of the signal, this empirical result is expected if my theory is true.

I also employ the synthetic control methodology introduced by Abadie and Gardeazabal (2003), which yields similar results to my other estimations. The distinction of behavior between the synthetic, would-be Michigan if there were no treatment and treated Michigan is apparent in the figures of synthetic control analysis. Realizing that what is controlled in this methodology is perhaps more important than a standard regression, I develop an algorithm that picks the variables that best explains Michigan's pre-intervention team size results, among the 137 variables that I have provided to it. This reduces the potential bias created by the variable selection, and decreases the error terms of the regressions by one-third and two-thirds for the two dependent variables, while still preserving the difference in post-intervention periods.

Finally, I extend the empirical analysis to cover the heterogeneity in prior signals of the ability of workers. In the literature, different papers developed models that investigated the change in firm behavior regarding promotions when workers have different racial or educational backgrounds. In my setting, I cannot observe any properties of inventors except what is written on patents. Therefore, I use time passed between patents, the value of patents, and firm-specific skills of inventors to measure how MARA influenced different subgroups of inventors. I expect that skilled inventors with lower expected levels of ability to be in higher-size teams before MARA; and therefore their team sizes should have decreased after MARA. The results, which can be found in Appendix C are largely in favor of this theory, as more productive inventors and inventors with firm-specific skills have significantly lower team sizes after the enactment of MARA. I find no conclusive evidence on the effect of experience.

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Appendix

A. Data and Variables

The information about patents that this thesis makes use of is their patent id, inventor, assignee, the year applied for, the state of the first listed inventor, type of the assignee, the citations given, and the different technology classes assigned. Using the citation file the variables *forward-citations*, which shows the number of citations a patent has received, and *backward-citations*, which shows the number of citations given by the patent, are produced. There is also an inherent problem with citations caused by the nature of the data; older patents have more citations compared to newer ones as they have more time to be cited. (Hall et al., 2001) To control for this asymmetry, the citations are weighted according to their year's average forward citations, resulting in variable *year-weighted-fc*. I carry this variable to firm-level by taking the mean across the state, year, and firm, and taking one plus the logarithm of it to generate *log(Influence)*.

Moving on to other variables, by combining the citation file with the assignee file, the variable *self-citation-ratio* is produced, which shows the ratio of patents cited that have the same assignee with the patent to backward-citations. The mean of *self-citation-ratio* is taken for a given firm, year, and state to produce *Self-reference*. A patent can seldom have more than one assignee, but these observations are just 1.6% of data and therefore only the primary (first) assignee is used here. Whether a firm is based in the US or in a foreign country may also be an important determinant as the 1980s saw the start of a new wave of globalization. If there is a change in the firm demographics in Michigan this may bias the results, so I construct the variable *foreign-assignee* which shows if the firm is US-based or not. Next, the class file is added to produce *same-cit-class-ratio-b* and *same-cit-class-ratio-f*, which shows the ratio of patents cited that has at least one common technology class with the patent to backward-citations and forward citations, respectively. I then carry these variables to the firm-level. The mean of *same-cit-class-ratio-b* for a given firm, year and state is taken and subtracted from 1 to generate *Generality* and the mean of *-cit-class-ratio-f*, for a given firm, year and state is taken and subtracted from 1 to generate *Originality*. An important thing to note is that a patent can have more than one technology class. There is a maximum of 16 different technology classes assigned in the dataset (only a single patent), and such high numbers of technology classes are outliers. Indeed, only 0.1% of patents have more than 8 technology classes. Therefore, I incorporate up to 8 technology classes to look for a match. Finally, the inventor file is added. The year of application is also in this file.

Since the assignee states almost do not exist before the 1990s in the dataset of Li et al. (2014), using the inventor states is compulsory. However, since different inventors of the same patent may live in different states, this leads to two important decisions to make. One is regarding the inclusion of patents that feature inventors from both enforcing and non-enforcing states. This may pose a potential threat to the difference-in-differences setting. The other is about the repeating observations; a patent with one inventor from California and another from Michigan occurs two times in the state-level data. After a comprehensive discussion, I decided to include all patents which have at least one inventor from a non-enforcing state and include the first listed inventor's firm (if the first inventor is from an enforcing state, I pick the first appearing inventor from a non-enforcing state) from the repeating patents. Nonetheless, I realize that these decisions are ultimately arbitrary. Therefore, I include Tables B11 and B12 display different samples of patents according separated according to the multiple-state differences, for *MTS* and *HTS* respectively. The second model displays where strong results in favor of the treatment while the last models 6 and 7 reduce the significance of the treatment. The rest are more or less the same as the original model. In model 8, I incorporate data of Hall et al. (2001), which have assignee states starting after the 1970s, but this significantly reduces observation numbers due to inconsistencies in datasets.

While choosing between these models, I have made this decision based on *a priori* reasons: I wanted to include all observations for non-enforcing states even if this may create a bias that I cannot observe as not including them may also cause sample selection bias and reduce the number of observations. I also did not want to include the same patent more than once as this causes a great amount of multi-collinearity. If the inventor sequences are distributed randomly across states, then distributing these patents across states according to their inventor sequences would not create a bias. Unfortunately, a comparison of Models 1 and 6 show that inventor sequences are not distributed randomly. I am not sure what causes this non-randomness that in turn affects the treatment's significance. Nonetheless, I moved on with the prior plan and included the patent for the state of the first non-enforcing inventor in the sequence, which seemed like the *simpler* decision.

Technology classes of patents categorized by the USPTO itself provide a useful way to determine the measures presented above, as they are specific and allow for a greater variety. This same property becomes a disadvantage when one wants to understand broader phenomena like the change in the chemical industry. Therefore, I classified 550 technology classes into 6 technology categories defined by NBER (Hall et al., 2001). Since patents can have more than one technology class, they may have more than one technology category. Information on technology categories of patents is given in Table A3.

Table A1: Distribution of Team Sizes of Patents

Team Size	Frequency	Percentage
1	226,283	33.17
2	190,703	27.96
3	121,780	17.85
4	66,172	9.70
5	34,239	5.02
6	18,349	2.69
7	9,965	1.46
8	5,663	0.83
9	3,170	0.46
10	1,965	0.29
11	1,167	0.17
12	739	0.11
13	616	0.09
14	432	0.06
15	923	0.14
Total	682,161	100.00

Table A2: Patent-Level Variables

Variable	Observations	Mean	Std. Dev.	Min	Max
teamsize	682,161	2.543	1.844	1	76
self cit ratio	568,699	.091	.208	0	1
same-cit-class-ratio-b	682,161	.544	.368	0	1
same-cit-class-ratio-f	677,841	.534	.421	0	1
year-weighted-fc	677,389	.931	3.014	0	1153
foreign assignee	682,161	.052	.221	0	1
multi state	682,161	.239	.427	0	1
enforcing inventor	682,161	.209	.407	0	1
enforcing inventor primary	682,161	.104	.306	0	1
repeating patent	682,161	.044	.206	0	1
freshman	682,161	.255	.436	0	1
junior	682,161	.385	.487	0	1
senior	682,161	.615	.487	0	1

Table A3: Distribution of Technology Categories of Patents

Variable	Observations	Mean	Std. Dev.	Min	Max
chemicals	682,161	.204	.403	0	1
computers	682,161	.274	.446	0	1
medical	682,161	.14	.347	0	1
electronics	682,161	.236	.425	0	1
mechanical	682,161	.158	.365	0	1
other category	682,161	.161	.368	0	1
auto	682,161	.054	.225	0	1

Table B1: Placebo Treatments for Control States, Dependent Variable is MTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Alaska	0.067									
	(0.089)									
California		0.068***								
		(0.018)								
Connecticut			0.063**							
			(0.028)							
Minnesota				-0.035						
				(0.031)						
Montana					0.067					
					(0.144)					
North Dakota						-0.142				
						(0.149)				
Nevada							-0.115			
							(0.104)			
Oklahoma								-0.072		
								(0.050)		
Washington									-0.102***	
									(0.039)	
West Virginia										-0.093
										(0.079)

Note: State and year fixed effects, time trend and controls are included in all models.

Table B2: Placebo Treatments for Control States, Dependent Variable is HTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Alaska	0.013									
	(0.016)									
California		0.009***								
		(0.003)								
Connecticut			0.014***							
			(0.005)							
Minnesota				-0.009						
				(0.005)						
Montana					-0.007					
					(0.025)					
North Dakota						-0.010				
						(0.026)				
Nevada							-0.036**			
							(0.018)			
Oklahoma								-0.015*		
								(0.009)		
Washington									-0.006	
									(0.007)	
West Virginia										0.013
										(0.014)

Note: State and year fixed effects, time trend and controls are included in all models.

Table B3: California Robustness Tests

	(1)	(2)	(3)	(4)
Treatment	-0.042*	-0.013***	-0.097***	-0.018***
	(0.025)	(0.005)	(0.025)	(0.004)
Observations	86,653	86,653	138,808	138,808
Dependent Variable	MTS	HTS	MTS	HTS
California Included	No	No	Yes	Yes
Non-California Controls Included	Yes	Yes	No	No

Note: State and year fixed effects, time trend and controls are included in all models.

Table B4: Alternative Thresholds for HTS

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.007	-0.014***	-0.011***	-0.005**	-0.004**	-0.003*
	(0.007)	(0.005)	(0.003)	(0.002)	(0.002)	(0.002)
Team size is greater than:	2	3	5	6	7	8

Note: State and year fixed effects, time trend and controls are included in all models. (N=198,472)

Table B5: Restricted Time Windows

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.022	-0.051*	-0.077***	-0.079***	-0.002	-0.010**	-0.012***	-0.014***
	(0.039)	(0.030)	(0.025)	(0.023)	(0.006)	(0.005)	(0.004)	(0.004)
Observations	16,836	29,185	42,225	56,796	16,836	29,185	42,225	56,796
Dependent Variable	MTS	MTS	MTS	MTS	HTS	HTS	HTS	HTS
Time Window	(-3, +3)	(-5, +5)	(-7, +7)	(-9, +9)	(-3, +3)	(-5, +5)	(-7, +7)	(-9, +9)

Note: State and year fixed effects, time trend and controls are included in all models.

Table B6: Dynamic Effects

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
Treatment 0	0.032	0.032	-0.059	-0.025	-0.024	0.009	0.009	-0.005	-0.005	-0.002
	(0.074)	(0.074)	(0.110)	(0.086)	(0.081)	(0.013)	(0.013)	(0.019)	(0.015)	(0.014)
Treatment -2	0.123					0.014				
	(0.079)					(0.014)				
Treatment -1	0.037					0.016				
	(0.077)					(0.013)				
Treatment +1	0.048					0.013				
	(0.072)					(0.013)				
Treatment +2	0.004					0.002				
	(0.068)					(0.012)				
Treatment (-2, -1)		0.079					0.015			
		(0.056)					(0.010)			
Treatment (+1, +2)		0.025					0.008			
		(0.050)					(0.009)			
Treatment (1970, -1)			-0.026					-0.001		
			(0.085)					(0.015)		
Treatment (+1, 2009)			-0.104					-0.017		
			(0.083)					(0.014)		
Treatment (1970, -2)				0.011					-0.001	
				(0.050)					(0.009)	
Treatment (+2, 2009)				-0.072					-0.017**	
				(0.045)					(0.008)	
Treatment (1970, -3)					0.006					0.002
					(0.041)					(0.007)
Treatment (+3, 2009)					-0.071**					-0.014**
					(0.035)					(0.006)
Dependent Variable	MTS	MTS	MTS	MTS	MTS	HTS	HTS	HTS	HTS	HTS

Note: State and year fixed effects, time trend and controls are included in all models. (N=198,472)
Treatment -2 corresponds to the interaction of Michigan with the dummy of two years before the treatment, i.e. 1983. *Treatment(-2, -1)* corresponds to the interaction of Michigan with the dummy of last two years before the treatment, i.e. year 1983 or 1984. *Treatment (1970, -1)* corresponds to the interaction of Michigan with dummy of years between 1970 and one year before the treatment. The rest follows these rules.

Table B7: Technology and Industry, Dependent Variable is MTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	-0.072***	-0.073***	-0.043*	-0.056**	-0.068***	-0.064***	-0.062***	-0.018	-0.019
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
Chemicals Firm	0.404***							0.446***	
	(0.009)							(0.011)	
Computers Firm		0.032***						0.278***	
		(0.009)						(0.010)	
Medicals Firm			0.475***					0.526***	
			(0.009)					(0.011)	
Electronics Firm				-0.288***				-0.007	
				(0.010)				(0.011)	
Mechanics Firm					-0.213***			0.038***	
					(0.009)			(0.010)	
Other Category Firm						-0.319***			
						(0.009)			
Auto Industry							-0.221***		
							(0.015)		
Chemicals Ratio									0.526***
									(0.012)
Computers Ratio									0.360***
									(0.011)
Medicals Ratio									0.599***
									(0.012)
Electronics Ratio									0.088***
									(0.011)
Mechanics Ratio									0.054***
									(0.012)

Note: State and year fixed effects, time trend and controls are included in all models. (N=198,472)

Table B8: Technology and Industry, Dependent Variable is HTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	-0.015***	-0.016***	-0.012***	-0.014***	-0.015***	-0.015***	-0.014***	-0.009**	-0.009**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Chemicals Firm	0.049***							0.051***	
	(0.002)							(0.002)	
Computers Firm		-0.002						0.027***	
		(0.002)						(0.002)	
Medicals Firm			0.061***					0.064***	
			(0.002)					(0.002)	
Electronics Firm				-0.032***				-0.000	
				(0.002)				(0.002)	
Mechanics Firm					-0.030***			-0.000	
					(0.002)			(0.002)	
Other Category Firm						-0.035***			
						(0.002)			
Auto Industry							-0.025***		
							(0.003)		
Chemicals Ratio									0.060***
									(0.002)
Computers Ratio									0.036***
									(0.002)
Medicals Ratio									0.072***
									(0.002)
Electronics Ratio									0.004**
									(0.002)
Mechanics Ratio									0.006***
									(0.002)

Note: State and year fixed effects, time trend and controls are included in all models. (N=198,472)

Table B9: Other Controls, Dependent Variable is MTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.075***	-0.066***	-0.088***	-0.078***	-0.076***	-0.069***	-0.080***
	(0.024)	(0.024)	(0.029)	(0.024)	(0.024)	(0.024)	(0.029)
Generality	0.151***					0.112***	0.131***
	(0.011)					(0.012)	(0.012)
Originality		0.206***				0.190***	0.201***
		(0.012)				(0.012)	(0.014)
Self-Reference			-0.014				0.006
			(0.025)				(0.025)
Foreign-Assignee				0.182***		0.184***	0.168***
				(0.014)		(0.014)	(0.015)
Observations	197,730	198,472	168,942	198,472	185,686	197,730	168,450
Non-US Firms Included	Yes	Yes	Yes	Yes	No	Yes	Yes

Note: State and year fixed effects, time trend and controls are included in all models.

Table B10: Other Controls, Dependent Variable is HTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.016***	-0.014***	-0.018***	-0.016***	-0.015***	-0.015***	-0.016***
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)
Generality	0.020***					0.013***	0.015***
	(0.002)					(0.002)	(0.002)
Originality		0.037***				0.035***	0.037***
		(0.002)				(0.002)	(0.003)
Self-Reference			-0.002				0.001
			(0.004)				(0.004)
Foreign-Assignee				0.028***		0.028***	0.025***
				(0.002)		(0.002)	(0.003)
				(0.002)		(0.002)	(0.003)
Observations	197,730	198,472	168,942	198,472	185,686	197,730	168,450
Non-US Firms Included	Yes	Yes	Yes	Yes	No	Yes	Yes

Note: State and year fixed effects, time trend and controls are included in all models.

Table B11: Multi State Patent Sub-Samples, Dependent Variable is MTS

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Treatment	-0.075***	-0.121***	-0.056***	-0.039*	-0.063***	-0.030	-0.042*	-0.059**
	(0.024)	(0.022)	(0.020)	(0.021)	(0.023)	(0.024)	(0.025)	(0.025)
Observations	198,472	179,021	152,803	160,439	193,726	199,544	204,400	114,224

Table B12: Multi State Patent Sub-Samples, Dependent Variable is HTS

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Treatment	-0.016***	-0.024***	-0.011***	-0.009***	-0.014***	-0.009**	-0.011***	-0.013***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	198,472	179,021	152,803	160,439	193,726	199,544	204,400	114,224

Note: State&Year Fixed Effects, Time Trend and Controls are included in all models. A patent is named *repeating* if it has at least two inventors from different non-enforcing states. A patent is named *multi-state* if it has at least two inventors from different states. Model 1: Original Sample, only first listed inventor's state is taken for repeating patents. Model 2: Patents with first inventor from non-enforcing state. Model 3: No multi-state patents. Model 4: No patents that has an enforcing-state inventor. Model 5: No repeating patents. Model 6: Only second listed inventor's state is taken for repeating patents. Model 7: All patents included. Model 8: Assignee states used instead of inventor states, control *Enforcing Primary Ratio* excluded.

Figure B1: Synthetic Michigan, firm-level, dependent variable is MTS

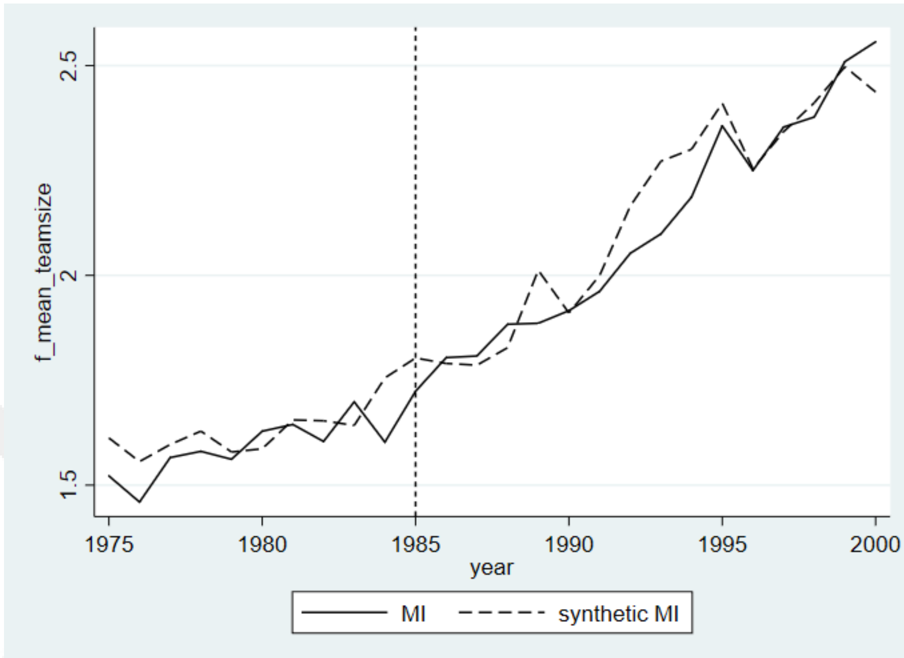


Figure B2: Synthetic Michigan, firm-level, dependent variable is HTS

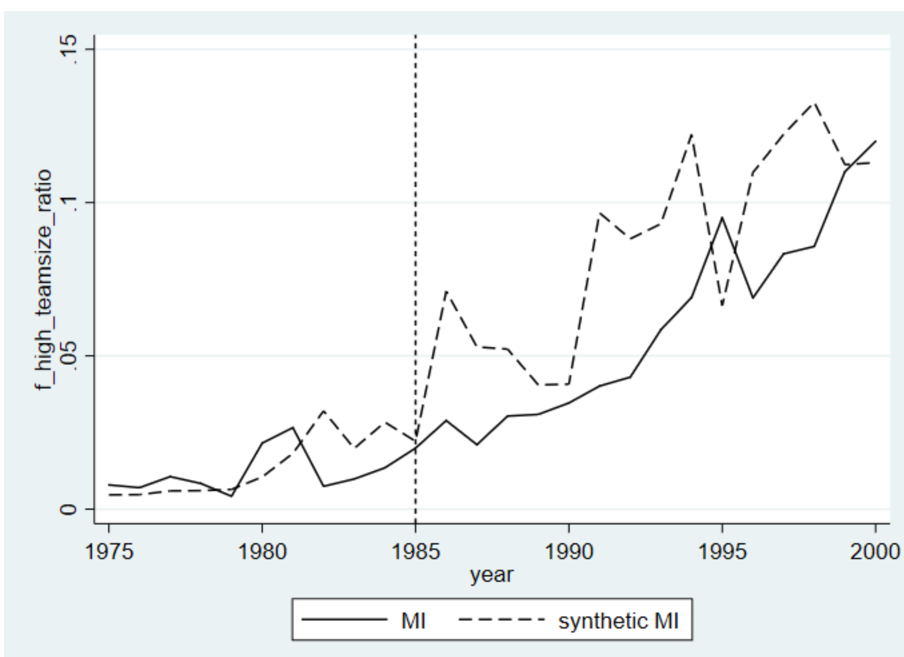


Figure B3: Synthetic Michigan with algorithmically picked controls, firm-level, dependent variable is MTS

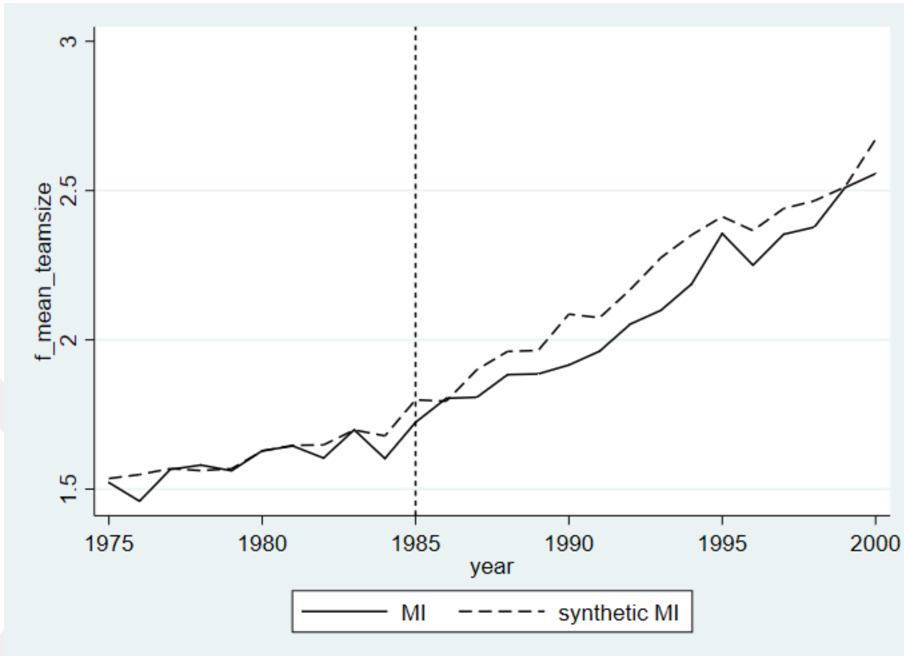


Figure B4: Synthetic Michigan with algorithmically picked controls, firm-level, dependent variable is HTS

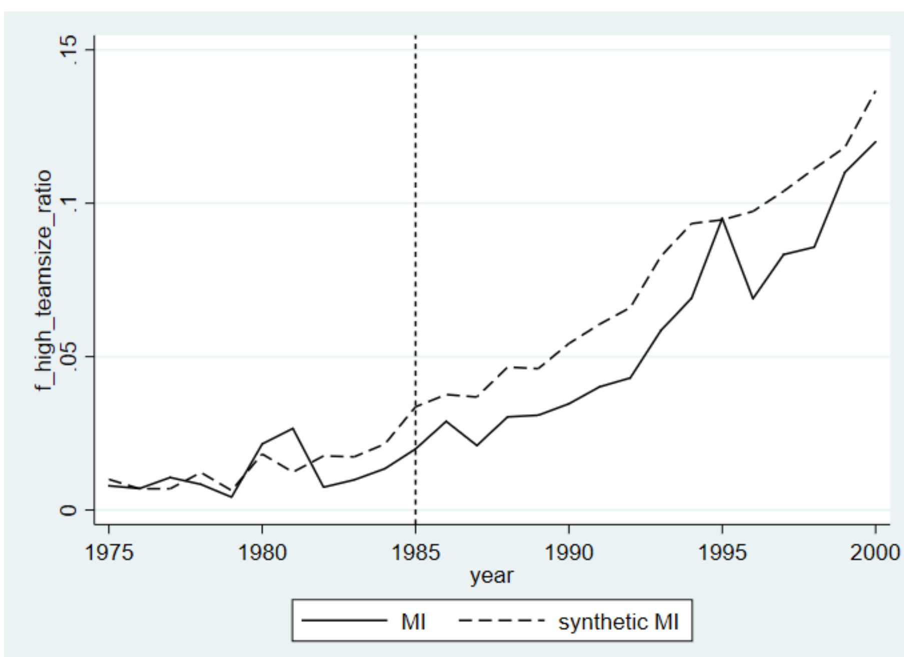


Figure B5: Synthetic Michigan with both exogenous and algorithmically picked controls, firm-level, dependent variable is MTS

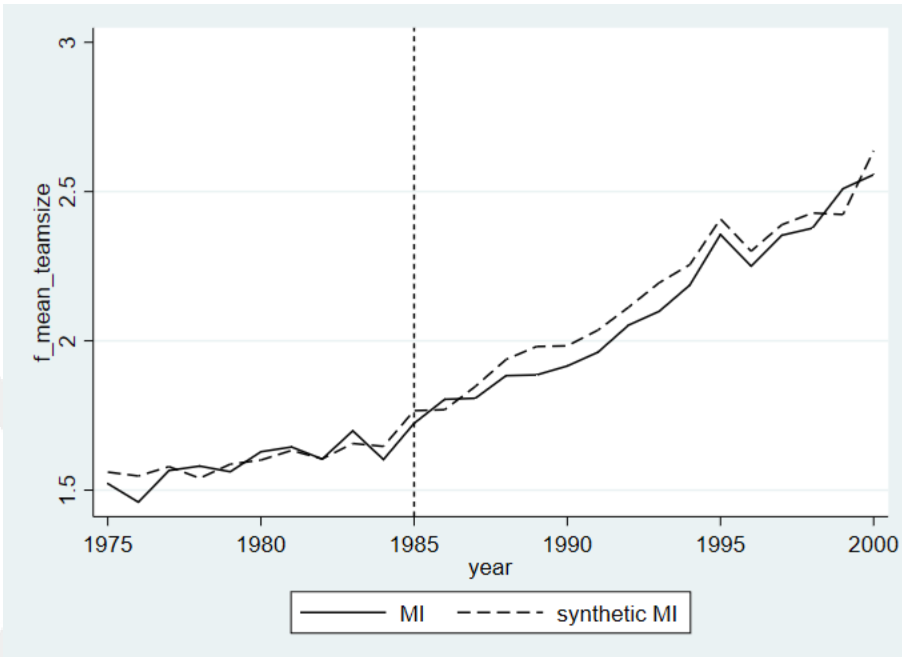
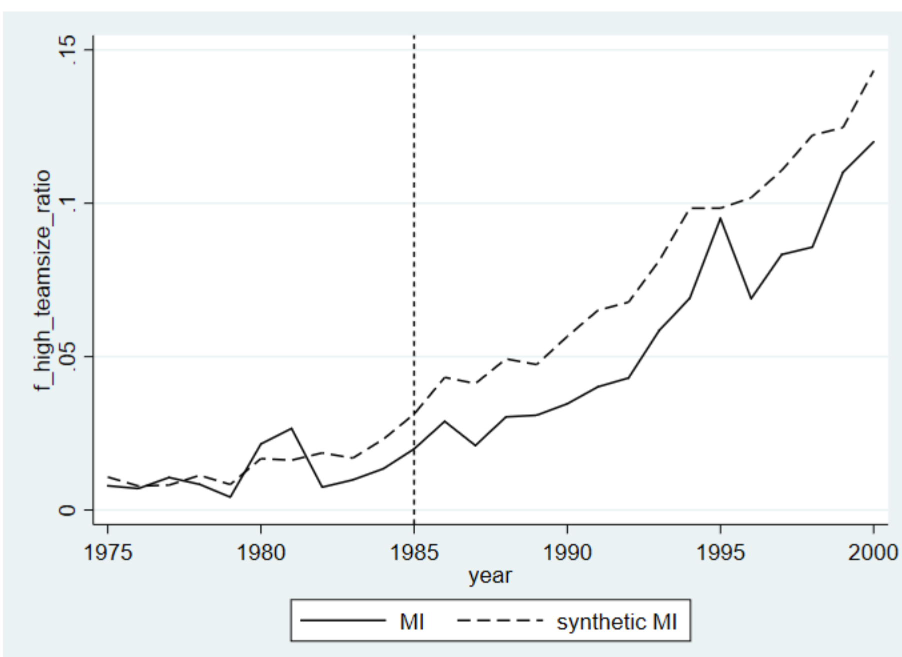


Figure B6: Synthetic Michigan with both exogenous and algorithmically picked controls, firm-level, dependent variable is HTS



C. Variables Used By The Synthetic Control Algorithm

Patent Controls: ”auto-patent-firm-only”, ”auto-industry-m3-only”, ”patnum”, ”influence”, ”chemicals-ratio”, ”computers-ratio”, ”medical-ratio”, ”electronics-ratio”, ”mechanical-ratio”, ”other-category-ratio”, ”tech-category-hhi”, ”self-reference”, ”generality”, ”originality”.

SAINC 30 Controls: ”Employer contributions for employee pension and insurance funds”, ”Employer contributions for government social insurance ”, ”Farm proprietors employment”, ”Farm proprietors’ income”, ”Imputed interest receipts”, ”Imputed rent”, ”Monetary interest receipts”, ”Monetary rent”, ”Nonfarm proprietors employment”, ”Nonfarm proprietors’ income”, ”Average nonfarm proprietors’ income”, ”Average wages and salaries”, ”Income maintenance benefits”, ”Per capita dividends”, ”Per capita income maintenance benefits”, ”Per capita interest”, ”Per capita rent”, ”Per capita retirement and other”, ”Per capita unemployment insurance compensation”, ”Personal dividend income”, ”Personal interest income”, ”Proprietors employment”, ”Proprietors’ income ”, ”Rental income of persons ”, ”Retirement and other ”, ”Supplements to wages and salaries”, ”Unemployment insurance compensation”, ”Wage and salary employment”, ”Wages and salaries”, ”Average earnings per job (dollars)”, ”Derivation of personal income”, ”Dividends, interest, and rent”, ”Earnings by place of work”, ”Net earnings by place of residence”, ”Per capita dividends, interest, and rent”, ”Per capita incomes (dollars)”, ”Per capita net earnings”, ”Per capita personal current transfer receipts”, ”Per capita personal income”, ”Personal current transfer receipts”, ”Personal income (millions of dollars)”, ”Place of residence profile”, ”Place of work profile (millions of dollars)”, ”Population (persons)”, ”Total employment (number of jobs)”.

SAGDP2 Controls: ”Apparel and other textile products”, ”Chemicals and allied products”, ”Electronic and other electric equipment”, ”Fabricated metal products”, ”Food and kindred products”, ”Furniture and fixtures”, ”Industrial machinery and equipment”, ”Instruments and related products”, ”Leather and leather products”, ”Local and interurban passenger transit”,

”Lumber and wood products”, ”Miscellaneous manufacturing industries”, ”Motor vehicles and equipment”, ”Other transportation equipment”, ”Paper and allied products”, ”Petroleum and coal products”, ”Pipelines, except natural gas”, ”Primary metal industries”, ”Printing and publishing”, ”Railroad transportation”, ”Rubber and miscellaneous plastics products”, ”Stone, clay, and glass products”, ”Textile mill products”, ”Tobacco products”, ”Transportation by air”, ”Transportation services”, ”Trucking and warehousing”, ”Water transportation”, ”Agricultural services, forestry, and fishing”, ”Amusement and recreation services”, ”Automotive repair, services, and parking”, ”Business services”, ”Coal mining”, ”Communications”, ”Depository institutions”, ”Durable goods manufacturing”, ”Educational services”, ”Electric, gas, and sanitary services”, ”Farms”, ”Health services”, ”Holding and other investment offices”, ”Hotels and other lodging places”, ”Insurance agents, brokers, and services ”, ”Insurance carriers”, ”Legal services”, ”Membership organizations”, ”Metal mining”, ”Miscellaneous repair services”, ”Motion pictures”, ”Nondepository institutions”, ”Nondurable goods manufacturing”, ”Nonmetallic minerals, except fuels”, ”Oil and gas extraction”, ”Other services”, ”Personal services”, ”Private households”, ”Real estate”, ”Security and commodity brokers”, ”Social services”, ”Transportation”, ”Agriculture, forestry, and fishing”, ”Construction”, ”Federal civilian”, ”Finance, insurance, and real estate”, ”Manufacturing”, “Military”, ”Mining”, ”Retail trade”, ”Services”, ”State and local”, ”Transportation and public utilities”, ”Wholesale trade”, ”Business services and other services”, ”Depository and nondepository institutions”, ”Electronic equipment and instruments”, ”Government and government enterprises”, ”Private industries”, ”Addenda”, “All industry total”