Non-compete agreements, technical expertise, and staffing small firms

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Abstract: An extensive literature on intellectual property examines the relationship between patenting and innovation, but the complementary mechanism of trade-secret protection has received little attention. This article examines the implications of non-compete agreements, which are ostensibly used to protect trade secrets. While prior work has established that non-competes deter individuals from changing jobs, this article examines how non-compete enforcement affects those who nonetheless leave their employers. Exploiting Michigan’s inadvertent 1985 reversal of its non-compete enforcement policy as a natural experiment, I estimate a differences-in-differences model using decades of patent data to establish two aspects of how individuals react when institutional regulation prevents them from practicing their profession. I find that inventors are more likely to change fields—contingent on changing jobs—when non-competes are enforced; and also that non-compete enforcement discourages those who leave their employers from subsequently joining small firms. The results are robust to a variety of specifications and are consistent with the literature regarding the tradeoffs and asymmetric costs of intellectual property protection.

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INTRODUCTION

How does institutional context—and in particular, the enforcement of intellectual property protection—influence innovation and entrepreneurship? An extensive literature documents the relationship between patenting and innovation, but few scholars have examined the complementary mechanism of trade secret protection. In this paper I investigate the implications of post-employment covenants not to compete (hereafter, “non-competes”), which are ostensibly used to protect trade secrets. While previous studies of non-competes have focused primarily on how non-competes bind workers to their current employers, I examine the implications of non-competes for those who nonetheless change jobs.

In order to assess these effects, I exploit an apparently-inadvertent reversal of non-compete enforcement as a natural experiment. As part of a 1985 antitrust reform, the Michigan legislature repealed an 80-year-old law containing a little-noticed provision banning non-competes. I argue that this policy reversal was an exogenous change and use it to build a differences-in-differences model. The results, estimated with work histories constructed from decades of patent data, shed light on how the inventors reacted to the reform in Michigan, as compared with those who changed jobs in states that had no such reform. Marx et al. (2009) use this technique to show that non-competes reduce the likelihood that inventors will change jobs. This study builds upon that as well as other non-compete studies (Stuart and Sorenson 2003; Fallick, Fleishman, and Rebitzer 2005; Garmaise 2007) by documenting two consequences of non-compete enforcement for those who nonetheless decide to leave their employers.

First, individuals subject to non-competes are less likely to work in the same field when they change jobs. Michigan inventors leaving their employers became more likely to change
technical fields after the state began enforcing non-compete agreements, thus creating a
deadweight loss of skills that might have been productively utilized. This result refines key
assumptions of Becker’s theory of human capital and blurs the distinction between “general” and
“specific” expertise. Second, Michigan inventors became less likely to join smaller firms (or to
become self-employed) following the policy change, reflecting the asymmetric costs of
intellectual property protection. As such, non-compete agreements lead individuals to join
larger, more established firms, possibly discouraging entrepreneurial activity. This result adds to
our understanding of institutional factors that encourage or discourage individuals from
participating in entrepreneurial ventures.

The paper proceeds as follows. First, I review the literature on intellectual property and
trade secrets and propose testable hypotheses. Next, I describe Michigan’s inadvertent reversal
of non-compete policy as well as the data used to estimate the differences-in-differences model.
I then interpret the results, discuss their theoretical and practical implications, and conclude.

INTELLECTUAL PROPERTY: PATENTS, TRADE SECRETS, AND NON-COMPETES

Inventors may choose to protect their work in a variety of ways, including patents,
copyrights, trademarks, or trade secrets. Of these, patents are by far the most thoroughly
researched, with hundreds of articles by economists and other social scientists. Given that a
primary motivation for the patent system is to provide incentives for innovation (Schumpeter
1942; Teece 1986), many researchers have examined the optimal design of the patent system,
including breadth of scope (Klemperer 1990; Gallini 1992; Lerner 1994) and duration of
protection (Nordhaus 1969; Kamien and Schwartz 1974; Gilbert and Shapiro 1990). Others have
assessed the connection between patenting and R&D (Hall, Griliches and Hausman 1986; Mansfield 1986; Sakakibara and Branstetter 1999), particularly in the area of fostering cumulative innovation (Merges and Nelson 1990; Heller and Eisenberg 1999; Murray and Stern 2007). In addition to the promise of protecting innovation, scholars have established the threat of litigation as a primary motivation for patenting (Hall and Ziedonis 2001; Schankerman and Scotchmer 2001; Lanjouw and Schankerman 2001).

By comparison, trade-secret protection is a “neglected orphan” in the literature (Friedman Landes, and Posner 1991:62). Formal models (Zabojnik 2002), taxonomies (Liebeskind 1997), and case studies (Dworkin 1980) have been developed, but empirical studies are lacking (for an exception, see Hannah 2005). This gap seems ironic, given that multiple surveys of appropriability mechanisms suggest that patent protection may not be the most effective method of appropriating the returns to innovation. In nine of ten industries surveyed by Cohen, Nelson, and Walsh (2000), secrecy was rated as more effective than patenting. Similarly, members of the Intellectual Property Owners Association indicated that trade secrets were more important than patents for maintaining their competitive advantage (Cockburn and Henderson 2003). Trade secrecy was also found to be more valuable than patents in the European Community Innovation Survey (Arundel 2001). Levin, Klevorick, Nelson, and Winter (1987) note that many firms are reluctant to undertake the public disclosure inherent in the patenting process. Small firms in particular may opt for trade secret protection over patent protection, as suggested by Lerner's (1994) analysis of intellectual property litigation by more than 500 Massachusetts-based firms over a 4 ½ year period.

The paucity of research on trade secrets may be due in part to the difficulty of assembling data, as no national registry is available as with patents. Moreover, the challenge of tracking
trade secrets extends to firms as well. Although employees signing non-disclosure agreements promise not to divulge trade secrets to competitors, violations can be difficult to detect. Given the difficulty of policing trade secret disclosure, many firms adopt non-compete agreements, which place restraints on the ability of ex-employees to join a competitor for a specified period of time after resigning (Hyde 2003). Non-compete enforcement policy is regionally fragmented in the U.S., with several reforms in 2008 alone.¹ That various states continue to come to different conclusions regarding non-compete enforcement suggests a lack of agreement regarding the effect of non-competes on innovation and entrepreneurship.

Legal scholars have long considered the implications of employee non-compete agreements (Blake 1960; Valiulis 1985; Decker 1993). But it was not until Gilson's (1999) assertion that the lack of enforceable non-compete agreements in California was in large part responsible for Silicon Valley’s entrepreneurial growth that empirical work on non-competes commenced. Challenging Saxenian's (1994) claim that the unique “culture” of Silicon Valley was largely responsible for the region's rise to technological prominence, he offered California's long-standing ban on non-competes—dating back to California's incorporation as state—as an alternative explanation for Silicon Valley's abundance of small firms, high rates of interorganizational mobility, and the practice of information sharing between firms. Stuart and Sorenson (2003) were the first to respond, finding that fewer biotech firms were founded following related IPOs and acquisitions in regions that enforce non-competes. Their data, however, were at the firm level and thus did not allow them to explore whether would-be founders simply did not leave their employers due to the non-compete; whether they resigned but

¹ The states of Idaho (Id. SB1393) and Louisiana (La. R.S. 23:921) extended the ability of firms to enforce non-competes, while Oregon (Or. SB248) and New York (Ny. S02393) restricted their ability to do so.
found it necessary to work outside the biotech industry given the non-compete; or whether they continued to work in the industry but elected to join established firms.

Subsequent work has focused on whether non-compete agreements deter individuals from changing jobs, finding that non-competes deter interorganizational mobility both among executives (Garmaise 2007) and technologists (Fallick et al. 2005; Marx et al. 2009). Still, none of these studies would suggest that non-competes fully eliminate interorganizational mobility. To the extent that non-compete agreements exercise post-employment restraints upon those who nonetheless leave their jobs, they may also carry implications for ex-employees. The next section derives two such implications as hypotheses.

**HYPOTHESES**

Extant literature suggests at least two possible implications of post-employment restraints for those who leave their employers. The first of these involves an inherent tradeoff of intellectual property protection. Patents, trademarks, and trade secret protection effectively enable inventors to set a monopoly price for their intellectual property, which would otherwise be available at near-zero cost to consumers given the ease of duplication. While such protection creates an incentive to invest in innovation, it also creates a deadweight loss for consumers whose willingness to pay is greater than marginal cost but lower than the monopoly price (Scotchmer 2004). The deadweight loss is often rationalized *ex ante* in that the good never would have been invented in the first place if not for promise of monopoly pricing.

In the case of non-compete agreements, however, the deadweight loss bears a less direct relationship to the incentive to invest. Most forms of intellectual property protection restrict
access to the output of the innovative process. For example, employees signing a non-disclosure agreement promise not to divulge specific trade secrets. But by forbidding ex-employees to work in the same field, non-compete agreements deny others use not only of the outputs but the inputs as well—namely, the relevant expertise of those who created the trade secrets. Thus the deadweight loss precludes access not only to the trade secrets themselves—which was arguably necessary to provide the original incentive—but also to the relevant skills of individual inventors. Thus non-competes may restrict access to technical expertise that might otherwise be productively deployed for purposes of innovation.

If individuals expect that their non-compete could be enforced, they will subsequently seek jobs in fields different enough from their prior employer to avoid legal confrontation. In a related field study (Marx 2009), one-quarter of randomly selected patent holders in the automatic speech recognition industry reported that they left the field when changing employers, specifically due to the non-compete agreement they had signed. This occurred even for those who had PhDs in the field—or other education or work-related expertise prior to joining the employer where they signed the non-compete—and had been employed in the area by a variety of companies for many years.

**Hypothesis 1a:** Ex-employees will be more likely to change fields when subject to non-competes.

While most workers require some form of income, those with sufficient financial means may be able to “wait out” a non-compete by remaining unemployed for the duration of the agreement.\(^2\) Alternatively, those who wish or need to work but would rather not change fields

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\(^2\) For example, Microsoft executive Vic Gundotra chose not to contest his non-compete when leaving for Google. Instead, he decided to remain unemployed for one year, as described in Google’s official statement: “Mr.
may take steps to avoid detection by former employers. In either scenario, one might expect a
greater (apparent) interval between jobs among those for whom non-competes are enforced.

**Hypothesis 1b:** The interval between spans of employment will be longer for those
subject to non-competes.

A second major implication addresses how non-compete enforcement may exacerbate the
difficulties small firms face in attempting to hire talent with relevant expertise. Once a startup is
incorporated and an initial opportunity has been identified, founders must marshal both financial
and human resources to pursue the opportunity (Hsu 2007). New ventures rely on an influx of
expertise skilled in the art in order to grow (Haveman and Cohen 1994; Klepper 2002; Gompers,
Lerner, and Scharfstein 2005); indeed, the literature on spinoffs demonstrates that entrants who
leverage intra-industry expertise outperform those that do not (Carroll, Bigelow, Seidel, and Tsai
1996; Klepper and Sleeper 2002). Yet small firms face liabilities in attracting key expertise due
to their uncertain life chances and limited resources. To the extent that the cost to litigate or
settle a non-compete suit weighs more heavily on smaller firms, they may be at a greater
disadvantage with respect to recruiting when compared with their more senior corporate siblings.

Several studies have established that the legal system involves asymmetric costs for small
versus large firms. Lanjouw and Schankerman (2004) noted that small firms are disadvantaged
when settling legal disputes regarding intellectual property because they lack large portfolios of
protected intellectual property as well as in-house counsel. As evidence of this advantage,

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*Gundotra has resigned from Microsoft and entered into an agreement with Google. Though the financial
arrangements are confidential, he will not be a Google employee for one year and intends to spend that time on
philanthropic pursuits. We are uncertain what precise role he will play when he begins working for Google, but he
has a broad range of skills and experience which we believe will be valuable to Google.” (Romano 2006)
Lanjouw and Lerner (1996) found that firms with greater financial and legal resources often obtain preliminary injunctions against weaker firms in order to drive quick and favorable settlements. Even when explicit legal action is not taken, the perception of greater facility or resources may suffice: merely a “reputation for toughness” in patent litigation is more effective in stopping knowledge spillovers to firms that are small or young (Agarwal, Ganco, and Ziedonis 2008). Given these asymmetries, smaller companies may avoid legal confrontations. Koen’s (1992) survey revealed that small companies were much more likely than large firms to consider the costs of potential litigation before deciding to initiate a particular type of R&D activity. Likewise, Lerner (1995) demonstrated that biotech firms with higher litigation costs (i.e., small firms) tend to avoid patenting in fields occupied by their competitors.

Given the costs associated with non-compete litigation, similar asymmetries could lead a smaller firm to avoid hiring personnel in the same field who are subject to non-competes, lest it be drawn into a lengthy legal battle. Individuals perceiving such obstacles may likewise steer clear of small companies. Indeed, interviewees and survey respondents in a related field study (Marx 2009) felt that larger firms would be better able to indemnify them against potential non-compete lawsuits—also, that large employers, owing to the diversity of their businesses and organizational slack, more easily offered them non-infringing tasks to work on during the term of the non-compete but with the promise of returning to their field immediately after the restriction expires. Thus large companies also appeared more able to prevent such lawsuits in the first place. Taken together with the lower likelihood of smaller firms trying to hire individuals subject to non-competes, this reasoning suggests the following:

3 For example, in 2005 Nortel Networks paid $11.5 million for the right to hire Motorola COO Mike Zafirovski as its CEO, who was subject to a non-compete agreement (McMillan 2005).
**Hypothesis 2:** Ex-employees subject to non-competes will be less likely to join small firms.

**METHODS**

In 1985 the Michigan legislature repealed an 80-year-old antitrust law, a sub-section of which contained a prohibition on enforcing non-compete agreements. Surprisingly, more than twenty pages of legislative analysis regarding the Michigan Antitrust Reform Act of 1985 (MARA) fails to mention non-competes as a reason for the reform. This suggests that the reversal in non-compete policy may have been an inadvertent consequence of MARA; if so, then Michigan’s change in enforcement would be an exogenous event.

Two additional pieces of evidence suggest that the non-compete enforcement policy reversal may indeed have been unintended. First, two years following MARA the Michigan legislature established the “reasonableness” doctrine—limiting the scope and duration of non-competes as is common in most states that enforce non-competes—possibly indicative that the repeal of the earlier ban had not been fully deliberated. Indeed, legislative analysis of the reasonableness doctrine states that its role was “to fill the statutory void” (Trim 1987). Second, Michigan labor lawyers active at the time of MARA and authors of relevant Michigan Bar Journal articles indicated that the reversal was unexpected. Louis Rabaut (2006) reported “there wasn’t an effort to repeal non-competes. We backed our way into it. All of a sudden the lawyers saw no proscription of non-competes...the legislature had to go back and clarify the law.” Robert Sikkel (2006), another Michigan-based lawyer active at the time of MARA, echoed Rabaut's view: “It was really out of the blue. I have never been able to identify any awareness that this was a conscious or intentional act. I am unaware of anyone that lobbied for the change.”
The above evidence suggests that Michigan’s change in enforcement was an exogenous event rather than an example of the legislature simply “catching up” with the courts or responding to lobbying efforts.\(^4\) Michigan is the only state known to have clearly and inadvertently reversed its enforcement policy in the past century. As such, it is a candidate for use as a natural experiment in assessing the implications of non-compete agreements.

Of course the Michigan Antitrust Reform Act did not merely repeal the prohibition on non-compete agreements; indeed, its stated purpose was to reform antitrust policy. Given that the hypotheses examine the likelihood that individuals will move to competitors or small firms, it is important to consider whether MARA might have affected those outcomes via mechanisms other than allowing non-competes. In addition to repealing Act 329 of 1905, section 1 of which contained the prohibition on non-competes, MARA repealed Act 255 or 1899, Act 229 of 1905, Act 135 of 1913, and Act 282 of 1937. Most of these acts address prohibitions on trusts, price-fixing, and other anti-competitive acts for particular industries including petroleum companies and bakeries.

In his proposal to the legislature, Michigan State Representative Perry Bullard (1983a) cited two motivations for reforming Michigan’s antitrust laws. First, that the prior statutes were too specific, lacking generality: for example, “apply[ing] only to the marketing of goods, not to the provision of services…auto repair shops, for example, have agreed to fix prices and have successfully defended themselves against the operation of Michigan’s law.” Second, that law enforcement officials lacked sufficient means to prosecute such cases: “the bill would greatly

\(^4\) Garmaise (2007) notes that Texas, Louisiana, and Florida amended their non-compete enforcement laws at various points. But each of these was formally deliberated by either judicial or legislative bodies and thus cannot be said to be inadvertent or accidental. Garmaise argues that the policy change is nonetheless exogenous to workers who did not closely follow such deliberations. However, while changes in those states either tightened or loosened constraints on enforcement, none fully reversed the previous enforcement policy as was the case in Michigan.
enhance the effectiveness of antitrust law in Michigan by increasing the penalties for violation
[and] would give Michigan courts access to the case law developed by the federal courts.”

While it is unclear that reforms designed to more closely monitor price-fixing by
automotive mechanics would substantially impact patterns of interorganizational mobility, it is
possible that stricter enforcement of more generally-applicable antitrust provisions might.
Scholars have generally argued that lax antitrust laws and enforcement contribute to increased
merger activity, while stricter antitrust oversight (such as adopted by Michigan) tends to favor
the establishment of rivals and small enterprises (Fligstein 1990; Stearns and Allan 1996; though
see Dobbin and Dowd 1997 for dissenting view). If so, then one would expect MARA to have
discouraged mergers, particularly by like or rival businesses. In such a scenario, it would be
more difficult to find support for the above hypotheses. A greater number of rival firms would
provide more opportunities for ex-employees to take jobs in the same field as their former
employer, and correspondingly less motivation to change fields. Reduced merger activity might
entail the persistence of smaller firms, lessening the likelihood that ex-employees would
subsequently choose large firms. In sum, effects of MARA unrelated to non-competes would
seem to exacerbate the difficulty of finding support for the hypotheses proposed above.

**Data**

Data are U.S. utility patents granted from 1963 through 2006, assembled from the NBER
patent data file (Hall, Jaffe, and Trajtenberg 2001) and augmented via updates from the U.S.
Patent and Trademark Office website. Although patent data have many weaknesses as economic
indicators (Griliches 1991), I utilize them not to measure innovation but rather to establish
occupational patterns over time. These data are attractive for this study as patent holders are sorts of workers whose knowledge of trade secrets firms try to protect using non-competes.\(^5\)

The patent database affords a method of tracking an inventor’s field as each patent is classified according to technical field by patent examiners. By categorizing these classifications and comparing them over time, it is possible to establish when individuals switch fields. This method enables finer-grained assessment than would be possible by using SIC codes or other industry classifications and also avoids the biases of self-reported data. Using the patent database to determine whether non-competes shift ex-employees toward larger firms is attractive both because the population of assignees represents firms involved in creating intellectual property, and also because it is not limited to publicly-traded firms but also includes privately-held firms and unaffiliated inventors.

Although useful in several ways for this study, patent database also has limitations. Because inventors are not required to submit patents at regular intervals, work histories reconstructed from a trail of patents may contain gaps, perhaps even omitting an entire span of employment with a particular firm. I am more likely to detect employer changes for those who patent more frequently, which is controlled for in the models. Moreover, the exact timing of the move is not known; I use the midpoint between the last patent at the former firm and the first patent at the subsequent firm. (Assuming the move occurred at the time of the last patent at the former firm strengthened the results, and assuming it occurred at the first patent at the new firm returned similar though weaker results.) A second limitation, common to most other studies of

\(^5\) If patents and non-competes were substitutes, it would seem inappropriate to use patent data to evaluate the impact of non-competes. Yet Marx et al. (2009) found no change in the rate of patenting when Michigan began to enforce non-competes, suggesting that patents and non-competes are complements, not substitutes.
non-competes (Stuart and Sorenson 2003; Fallick et al. 2006) is that the patent database does not indicate which inventors signed a non-compete and which did not. As a result, findings of this study may understate the impact of non-compete agreements for those who sign them.

A third limitation is that patents are not indexed by Social Security number or other unique identifier. In order to reconstruct work histories, matching algorithms both for inventor and assignee names based on existing algorithms (Trajtenberg, Shiff, and Melamed 2006; Fleming, King, and Juda 2007; Singh, 2008) are used to determine which patents were submitted by a particular individual. For each pair of patents where both the first names are identical as well as the last names, a series of heuristic tests are applied in order to assess the likelihood that the two patents belong to the same inventor. These include assessing the frequency of the name using U.S. Census tables, checking for shared location, employer, technical field, or co-inventors. Of course, any such matching effort of this sort is prone to both Type I (missing patents for a particular inventor) and Type II (matching too many patents for a given inventor) errors. The differences-in-differences method may help to ameliorate this concern.

The population for this study is restricted to the 98,468 inventors who were granted at least one patent in Michigan or another state that did not enforce non-competes prior to MARA. Michigan inventors represent the experimental group, with inventors in states that continued to proscribe non-compete enforcement as the control group. A total of 27,478 employer changes were found for these inventors, 3,307 of which were in Michigan. An employer change is defined as a pair of sequential patents for a single inventor where the assignee for the first patent

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is different than the assignee for the second patent. Because at least two patents are necessary to
detect a move, inventors with only one patent are necessarily excluded from the analysis.
Likewise, inventors with multiple patents all assigned to the same firm (i.e. who never changed
jobs) are excluded. I take unassigned patents—those where the assignee is missing and thus
assigned to the inventor—as indicators of self-employment, I excluding moves from self-
employment to employment as individuals will likely not require themselves to sign a non-
compete. Each employer change is an observation in this analysis, as both hypotheses are
contingent on employees leaving their employer.

**Variables**

I operationalize the dependent variable for Hypothesis 1a by exploiting the classification of patents into technical fields by the U.S. Patent and Trademark Office. While there are hundreds of technical classes, the National Bureau of Economic Research has grouped these into two levels of categories (Hall, Jaffe, and Trajtenberg 2001).\(^7\) Six top-level NBER categories correspond to broad sectors of the economy, whereas the 36 NBER subcategories correspond more closely to individual industries. Each patent receives a primary technical classification, which I use as an indicator of the field a given inventor was working in at the time. Of course an inventor may have multiple patents at a given employer, and these patents may have primary technical classifications in different subcategories, so I compare the mode (most frequent) subcategory for each of the two employers. If the two mode subcategories differ, the dependent

\(^7\) Appendix 1 of Hall, et al. (2001) lists the aggregation of individual patent classes into both subcategories and top-level categories.
variable is set to 1. If more than one mode is found, ties are broken by the highest-numbered subcategory (and, as a robustness check, by the lowest-numbered subcategory).

For Hypothesis 1b, the dependent variable is the interval between the last patent at an inventor’s former employer and the first patent at the subsequent employer. While an inexact measurement, as someone is not required to file a patent on the last day at one firm and the first day at the next firm, it may provide insight into the average interval between jobs. The variable is logged due to skewness.

I proxy for the size of the firm subsequently joined—the dependent variable for Hypothesis 2—using a measure of its patenting frequency. One concern with this approach is that firms may not patent at the same rate each year, particularly small firms. Thus I employ a rolling three-year window of the number of patents granted; too long a window would lead younger or short-lived firms to appear smaller relative to those that were in business for many years. (In order to test the robustness of the three-year assumption, I also produce estimates using one- and five-year windows.) A second concern involves the distribution of firm sizes, particularly in Michigan, as spurious support for Hypothesis 2 could be found if it were the case that large firms dominated the Michigan economy. To address this, I normalize the three-year patent count, dividing by the mean for all firms patenting that year in the same state.\(^8\) For example, a firm that had seven patents in Michigan from 1980-1982 would have its count normalized by the average number of patents filed per firm in Michigan during the same period. Thus the dependent variable indicates the relative size of the firm joined by ex-employees when

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\(^8\) Dividing by the median instead of the meeting yielded similar results, as did calculating the denominator for Michigan versus non-Michigan rather than for each individual state, or calculating the denominator for pre-MARA versus post-MARA instead of each individual year.
compared to other firms in the same state that they might have joined. Finally, the variable is logged in order to address skewness.

Using patenting frequency as a proxy for the dependent variable assumes a relationship between that variable and the actual size of a given firm. In order to test this relationship, I examined the correlation between patent frequency and measures of firm size for a random sample of patent assignees. I gathered revenue and number of employees from three databases: Orbis (Bureau von Dijk), Global Business Information (OneSource), and CorpTech (InfoUSA). As these generally have private-company information only for the last few years, I restricted the sampling frame to assignees with patents after 2000. Of these 3,634 firms, I attempted to look up information for 200 of them and found either revenue or employee base in at least one of the three sources for 154 of those. When information was available for more than one year in a given database, I used the revenue and/or employee-base figures for the year closest to that of the firm’s most recent patent. Where information was found in more than one of the three databases, figures were averaged. The logged number of patents for the firm in a rolling three-year window has a correlation of $r=0.68$ with the logged measure of employee base and $r=0.65$ with logged revenue, both statistically significant at the 0.1% level.9 The measures of employee base and revenue were themselves imperfectly correlated ($r=0.91$).

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9 The above analysis excludes assignees that could not be located in any of the three databases. As an alternative approach, I made the assumption that any firm not covered by these three sources must be very small. This analysis reduced the correlations slightly, to $r=0.67$ for employee base and $r=0.56$ for revenue.
Explanatory Variable

In order to assess the impact of the policy reversal on inventor behavior in both hypotheses, the explanatory variable indicates those Michigan patents applied for following the policy reversal. It is an interaction of two indicators, post-MARA and Michigan. The former is set to 1 if the application date of the patent fell after the passage of MARA. The latter is set to 1 if the inventor’s hometown was listed as being in the state of Michigan.

Control Variables

Control variables include characteristics both of the employers (assignees) and the employees (inventors). As employer changes are detectable conditional on patenting, I control for an individual’s propensity to patent using the (logged) number of days between patents as well as the (logged) number of patents in the pre-MARA period divided by the number of active patenting years. As some inventors moved to states that enforced non-competes, such as Massachusetts, a dummy variable accounts for these observations. Another indicator captures whether the prior employer was a university, as universities are unlikely to ask non-competes of their students, faculty, or staff. Period effects are captured by annual indicators, and cohort effects are controlled for with a dummy variable for each inventor’s first patenting year.

Given the role of the automobile industry in Michigan’s economy, it is important to account for those inventors who leave jobs in the auto industry. As noted by Singleton (1992),

\[ \text{Auto patents are identified by assignee name according to Plunkett Research.} \]
\[ \text{plunkettresearch.com/Industries/AutomobilesTrucks/AutomobilesandTrucksIndustryIndex/tabid/91/Default.aspx} \]
the 1980s were a decade of sharp fluctuations in the auto industry due to foreign competition as well as the oil shocks of the previous decade. Hence, those in the auto industry who changed jobs might be more likely than others to change fields. Moreover, if leaving large auto manufacturers they might have been likely to move to smaller firms. Either of these effects could confound the evaluation of the hypotheses. Thus I control not only for a patent being in the auto industry, but also for the interaction of that indicator with Michigan, post-MARA, and their interaction in order to account for temporal effects.

For Hypothesis 1a, some individuals may simply be more prone to shift fields; thus a dummy variable captures whether a given inventor had previously changed fields. I also include a measure for the size of the former employer, given Sorensen's (2007) finding that those who work for large firms are likely to again join a large firm. Table 1 gives descriptive statistics.

Table 1 about here

Models

For Hypothesis 1a, I estimate logit models in order to assess whether MARA affected the likelihood that an inventor changed fields. The form of the regression equation is

$$\Pr(CF_{ij} = 1) = \frac{e^{\beta X_{ij} + \gamma Z_i + \lambda W_{it}}}{1 + e^{\beta X_{ij} + \gamma Z_i + \lambda W_{it}}}$$

In this equation, $CF_{ij}$ equals 1 if the most frequent subcategory differs between the former and subsequent employers. The vector $X_{ij}$ is a set of characteristics of the job change, including the explanatory variable. The vector $Z_i$ is a set of time invariant individual characteristics,
including year of the first patent. The vector $W_i$ is a vector of potentially time-varying individual characteristics, including whether the inventor had previously switched fields.

For Hypothesis 1b, I estimate an OLS regression on the (logged) duration between the last patent at the inventor’s former firm and the first patent at the inventor’s subsequent firm. The regression equation is similar to that of the logit aside from the inclusion of the error term. I also estimate an OLS model for Hypothesis 2, on the size of the subsequent firm joined by the ex-employee. In all models, standard errors are clustered by inventor to account for the non-independence of observations (White, 1980). All estimates are calculated using the STATA 10 software package.

RESULTS

I first examine support for Hypothesis 1a. Univariate examination of subcategory changes, conditional on employer changes, indeed suggests that Michigan inventors became more likely to shift fields once non-competes were enforced, as compared with other states that continued not to enforce non-competes. As shown in Table 2, although the proportion of inventors changing fields when changing jobs was not statistically distinguishable before Michigan’s policy reversal, it become so afterward, with Michigan experiencing less of a drop than non-enforcing states. Unreported factorial ANOVA analysis shows that the interaction of Michigan and post-MARA is positive and statistically significant at the 5% level ($p<0.026$).

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11 Given that the models use the same data, seemingly-unrelated regression (SUR) might appear appropriate; however, the dependent variables do not share the same form. In unreported results, I estimated SUR both by converting the dependent variable for H1a into a continuous measure and by converting the dependent variable for H2 into a dichotomous outcome. However, Stata 10 does not cluster standard errors for the continuous SUR model and failed to converge for the bivariate probit SUR model unless clustering was omitted.
Figure 1 shows the trend over time regarding field-changing contingent on job changing in Michigan compared with other non-enforcing states. In the years just following the policy reversal, non-enforcing states experience a steep decline in the rate of inventors changing fields when changing jobs. As in the case of job mobility patterns (Marx et al. 2009), Michigan fails to follow the trend of the other non-enforcing states. In fact, aside from 1994 (in which the rates match), the rate of inventors changing fields when changing jobs is consistently higher in Michigan during the post-MARA period, contrary to the pre-MARA period of fluctuation.

Table 2 and Figure 1 about here

Table 3 shows the results of logit regressions on the dichotomous outcome of changing fields, contingent on changing employers. Model 1, which excludes the interaction term for the explanatory variable, confirms that inventors who have already changed fields are more likely to do so again. Inventors who patent more frequently, or who joined larger companies, are less likely to change fields. The duration between patents increases the chance of changing fields. Other variables are not significant. Adding the explanatory interaction variable in Model 2 significantly improves model fit ($p<0.028$ in an unreported likelihood-ratio test) and suggests that the policy reversal indeed raised the likelihood that an inventor would change fields when changing jobs. A marginal-effects calculation of the interaction effect, holding all other variables at their means, yields an interaction effect of 4.4%.

Table 3 about here

However, as noted by Ai and Norton (2003) as well as Hoetker (2007) neither the sign, the magnitude, nor the statistical significance of the interaction effect in a non-linear model is necessarily that of the coefficient on the interaction term. Given that the effect of a change in
any covariate in such models depends on the initial probability of the outcome variable, interpreting interaction effects requires taking into account not only the coefficient on the interaction variable (and those on the interacted variables) but the values of all other variables in the model. Norton, Wong, and Ai (2004) supply the Stata `inteff` routine for computing the cross-partial derivatives necessary to evaluate a single two-way interaction effect for each observation. When applied to Model 2, `inteff` yields an average interaction effect of 4.2%, indicating that the simple marginal-effects calculation overstated the magnitude of the interaction effect. Figure 2 shows that the interaction effect is positive for all observations, ranging between approximately 2.7% and 4.5%. Figure 3 shows that the effect is significant at the 5% level for almost all observations, with a mean z-statistic of 2.23.

Figure 2 and Figure 3 about here

Models 3-5 provide robustness checks. Model 3 executes a block-bootstrap (Efron and Tibshirani 1994), sampling inventors with replacement and re-estimating the model 200 times in order to account for the possible underestimation of standard errors due to serial correlation in differences-in-differences models with a large number of periods (Bertrand, Duflo, and Mullainathan 2004). Standard errors but not coefficients are recalculated, with similar significance on the key explanatory variable. Model 4 is a simple model excluding any control variables that might be endogenous to the policy reversal. The magnitude of the coefficient on the explanatory variable is similar to earlier models, and while statistical significance appears weaker in the table, `inteff` yields a mean z-statistic well within the 5% level. Lastly, Model 5 examines whether a similar effect is also found for top-level categories, as it would seem less likely that non-competes would be enforced across broad sectors of the economy than within individual fields or industries. Indeed, changing the dependent variable from NBER subcategory
(n=36) to top-level NBER category (n=6) multiplies the standard errors by approximately an order of magnitude. This suggests that while inventors subsequently worked in the field different enough to avoid infringing on the non-compete, they avoided shifting to fields that were completely unrelated. In summary, the models offer support for Hypothesis 1a.

Hypothesis 1b receives weaker support, perhaps due to difficulties in identifying the precise intervals between jobs. Although the univariate analysis in Table 4 suggests that the average interval between jobs grew more for Michigan inventors following the policy reversal, no clear trend is evident in Figure 4. Moreover, both factorial ANOVA analysis and multivariate regression are inconclusive: although the coefficient on the interaction of the Michigan dummy and the post-MARA indicator in Model 6 of Table 5 is positive as expected, it fails to reach significance even at the 10% level. Said coefficient is positive and significant only when the variable is not logged, as in Model 7, which seems inappropriate given the skewness of the unlogged variable.

Table 4, Figure 4 and Table 5 about here

I begin assessing Hypothesis 2 with univariate analysis in Table 6 of the (logged) normalized size of firms subsequently joined by inventors who left their employers. Michigan ex-employees joined larger firms on average following the policy change, while the opposite was true for those outside of Michigan. Although those in Michigan joined discernibly smaller firms than those outside the state prior to the reform, following the policy change the difference was smaller by nearly an order of magnitude and only somewhat statistically significant. Factorial ANOVA analysis shows that the interaction of post-MARA and Michigan is positive as well as
significant at the 1% level, lending preliminary support to Hypothesis 2. The trend over time for the size of firms joined by Michigan and non-Michigan inventors is given in Figure 5.

**Table 6 and Figure 5 about here**

In multivariate analysis, I employ the size of the subsequent firm as the dependent variable in an OLS regression. Model 8 of Table 7 shows that Michigan inventors tended to join smaller firms than their peers in other states, although inventors outside of Michigan tended to move to smaller firms following the policy reversal. The larger the prior employer was, the larger the subsequent employer was as well. Inventors who patented more frequently were more likely to join larger firms. Those leaving universities were much more likely to join larger firms. Those inventors who had moved to a non-enforcing state and then changed jobs were also more likely to join a larger firm. Other variables are not significant. Adding the explanatory variable in Model 9 shows that once the state of Michigan began enforcing non-compete agreements, Michigan inventors changing jobs joined larger firms as compared with inventors in other states, with strong statistical significance.

**Table 7 about here**

Models 10-12 provide robustness checks. I test the sensitivity of the model to the choice of a 3-year rolling window in Model 10 using a 5-year window, with similar results. (An unreported model with a one-year window also yields similar coefficients.) In Model 11, I exclude moves to self-employment, reducing the number of observations yet returning similar results. Model 12 estimates a logit model of the likelihood that the ex-employee’s subsequent firm is of above-median size, indicating that Michigan inventors were approximately 6% more likely to join a firm of above-median size following the policy reversal as compared with
inventors in other states. Not shown but available from the author are models demonstrating the robustness of this result to block-bootstrapping the standard errors (as in Model 3) and excluding possibly-endogenous variables (as in Model 4).

**DISCUSSION**

I interpret support for the hypotheses cautiously, given the measurement difficulties involved in individual-level longitudinal studies using patent data. I may fail to capture employer changes and do not know whether they are voluntary.\(^{12}\) Heterogeneity in the firms’ propensity to prosecute non-compete contracts is unobserved as are the contracts themselves.\(^{13}\) Moreover, it may be difficult to generalize these results beyond the technology-focused companies that employ patent-filing inventors; indeed, non-compete agreements may have their strongest effect in technology-based industries where intellectual property is critical. Nevertheless, support for two of the hypotheses offers contributions to both theory and practice.

The first result establishes that non-compete enforcement leads inventors to shift to different fields when leaving their employers. It refines a tenet of human capital analysis: that one “cannot separate a person from his or her knowledge [or] skills…the way it is possible to move physical and financial assets while the owner stays put” (Becker 1962:16). Support for

\(^{12}\) Many non-compete agreements are constructed such that they apply regardless of the reason for separation from the firm. Even if judges are sympathetic in the case of a defendant who was laid off or otherwise involuntarily terminated, individual workers may find it difficult to anticipate the outcome of potential litigation and thus take precautions assuming a worst-case outcome.

\(^{13}\) Another approach to the analysis would assess the number of non-compete lawsuits before and after the policy reversal. I did not pursue this path for two reasons. Westlaw and other databases include only court decisions, eliminating out-of-court settlements and thus not giving the full picture of legal activity. While another database, the Courthouse News Service, maintains a comprehensive list of all cases filed, its records for Michigan commenced after the policy reversal and as such would not be of use in determining any change in trend.
Hypothesis 1a shows that ex-employers are indeed able to separate individuals from the use of their skills, by (temporarily) modifying the property rights associated with expertise. Instead of being “automatically vested” in individuals (Becker 1962:17), property rights under non-compete agreements afford firms some measure of ownership over workers’ skills. This result also blurs Becker’s distinction between “general” and “specific” training, where the former is expertise usable by many firms. As non-competes limit the ability of ex-employees to utilize general skills, firms may be more willing to invest in general training than Becker’s analysis would suggest. However, because non-competes restrict not only the use of skills gained from training by the firm but also prior expertise including education, individuals who expect to sign non-competes may become less willing to invest in their own human capital lest they find themselves constrained from utilizing it.

The second result shows that inventors subject to non-competes are less likely to join a small firm when changing jobs. This informs the literature regarding institutional context and participation in entrepreneurial ventures. Scholars have identified numerous factors affecting entry into entrepreneurship, including risk tolerance (Kihlstrom and Laffont 1979), overconfidence (Landier and Thesmar 2003; Camerer and Lovallo 1999), having a generalist orientation (Lazear 2002), access to capital (Holtz-Eakin, Joulfaian, and Rosen. 1994; Nanda 2008), and social connections to peers with entrepreneurial experience (Stuart and Ding 2006; Nanda and Sorensen 2007). Yet entrepreneurship is not limited to founding a firm; once incorporated, the new venture must mobilize resources—including human resources—in order to pursue the opportunity. This study demonstrates that post-employment restraints exacerbate the difficulty of attracting talent to small firms and helps to answer Sorensen’s (2007:410) call that
scholars consider “the indirect effects of policies not directly related to entrepreneurship [and] that directly or indirectly support and sustain large, established firms.”

More broadly, these findings extend the implications of intellectual property protection from the “market for ideas” (Gans, Hsu, and Stern 2002) to the market for talent. Because non-competes, unlike other forms of intellectual property protection, exclude others' access to the inputs of the innovative process, they restrict not only the mobility of skilled workers but also their ability to utilize their expertise. Moreover, these findings should inform individuals, and in particular, would-be entrepreneurs. Those who have developed—or plan to develop—deep expertise in a particular field may want to seek employment in regions where non-competes are not enforced. Founders should be careful in their choice of location, despite the possible loss of social capital (Sorensen and Dahl 2009), as they may find it more difficult to attract talent in regions where non-competes are enforced.

Policymakers attempting to encourage entrepreneurship may also benefit from these results. The failure of more than 100 attempts to replicate the entrepreneurial culture of Silicon Valley in various locations demonstrates an insufficient understanding of ways in which the state can facilitate the development of new ventures (Goel 2004). Whereas Stuart and Sorenson’s (2003) analysis suggests that non-competes may slow the founding of new firms, this work shows that such contracts may also hinder the growth of small companies as they find it more difficult to attract talent than do larger, more established firms.
CONCLUSION

Building on past work indicating that non-compete agreements bind employees more tightly to their employers, this study established implications of post-employment non-compete agreements for those who nonetheless change jobs. A differences-in-differences model based on an inadvertent 1985 reversal of non-compete enforcement policy in Michigan provided support for two hypotheses using work histories for patenting inventors. First, those subject to non-competes were less likely to continue in the same line of work after leaving their employer. Second, non-competes shift subsequent employment toward larger companies. The results further our understanding of how institutional rules regarding protection of intellectual property, and trade secrets in particular, influences innovation and entrepreneurship.

This study also leaves open several opportunities for follow-on work. First, as the scope of this work is limited to the U.S., a natural next step is to explore how non-competes are handled internationally, including between countries. (A 2008 non-compete reform in China may prove valuable in such a study.) Second, this and other studies of non-competes tend to focus on individual-level behavior, but there may be implications at the firm level as well. Do non-competes lead spinoffs to distance themselves either geographically or strategically from their parent firms? If so, might this migration result in technologies that were invented in enforcing regions becoming commercialized in non-enforcing regions? Third, as yet unanswered are the overall welfare implications of non-competes. Does the abandonment of expertise due to non-competes reduce productivity, or does it contribute to novelty as individuals work in unfamiliar fields? Are the loss of expertise and disadvantaging of small firms offset by benefits that accrue to incumbents? These questions are of interest to scholars, managers, and policymakers alike.
REFERENCES


California (1865). California Business and Professions Code Section 16600.


Schumpeter, J (1942). *Capitalism, Socialism, and Democracy*.


Table 1: Descriptive statistics for inventors who were granted at least one patent in a non-enforcing state in 1985 or earlier and who changed employers. Employer change is determined by two subsequent patents of the same inventor having different assignees; \(n=27,478\).

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Std dev</th>
<th>Min</th>
<th>Max</th>
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<th>2)</th>
<th>3)</th>
<th>4)</th>
<th>5)</th>
<th>6)</th>
<th>7)</th>
<th>8)</th>
<th>9)</th>
<th>10)</th>
<th>11)</th>
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</thead>
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<td>1) change in NBER subcategory</td>
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<td>0.4989</td>
<td>0.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
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<td>1.0000</td>
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<td>2) post-MARA</td>
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<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
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<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
</tr>
<tr>
<td>3) Michigan</td>
<td>0.1204</td>
<td>0.3254</td>
<td>0.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
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<td>4) inventor previously changed subcategory</td>
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<td>0.4896</td>
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<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
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<td>1.0000</td>
<td>1.0000</td>
</tr>
<tr>
<td>5) inventor's pre-MARA patent frequency (L)</td>
<td>-0.5644</td>
<td>0.8641</td>
<td>-2.8230</td>
<td>3.0910</td>
<td>-0.0724</td>
<td>-0.0332</td>
<td>0.2138</td>
<td>0.2138</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
</tr>
<tr>
<td>6) former firm's normalized patents, last 3 years (L)</td>
<td>-1.2664</td>
<td>1.2643</td>
<td>-2.9270</td>
<td>4.0565</td>
<td>0.0185</td>
<td>-0.0697</td>
<td>-0.0153</td>
<td>-0.2115</td>
<td>0.0114</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
</tr>
<tr>
<td>7) subsequent firm's normalized patents, last 3 years (L)</td>
<td>-1.7074</td>
<td>1.0280</td>
<td>-2.3000</td>
<td>4.2250</td>
<td>-0.0156</td>
<td>-0.0746</td>
<td>-0.0396</td>
<td>-0.0523</td>
<td>0.0155</td>
<td>0.1832</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
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</tr>
<tr>
<td>8) prior patent was in auto industry</td>
<td>0.0193</td>
<td>0.1577</td>
<td>0.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
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<td>1.0000</td>
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</tr>
<tr>
<td>9) state enforces non-competes</td>
<td>0.0679</td>
<td>0.2317</td>
<td>0.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
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<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
<td>1.0000</td>
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<tr>
<td>10) duration between patents (L)</td>
<td>6.2634</td>
<td>1.7913</td>
<td>0.0100</td>
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<td>-0.1143</td>
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<td>-0.3149</td>
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<td>0.0272</td>
<td>-0.1118</td>
<td>-0.0284</td>
<td>1.0000</td>
</tr>
</tbody>
</table>

Table 2: Univariate comparison of the proportion of employer changes involving a change in field, as represented by a difference in the most frequent NBER subcategory at the prior versus subsequent employer (for those with a patent in a non-enforcing state prior to 1985); \(n=27,478\). The period prior to the policy reversal is referred to as “pre-MARA.”

<table>
<thead>
<tr>
<th>Variable</th>
<th>pre-MARA</th>
<th>post-MARA</th>
<th>pre- vs. post-MARA t-tests</th>
<th>n</th>
</tr>
</thead>
<tbody>
<tr>
<td>Non-Michigan</td>
<td>0.580</td>
<td>0.503</td>
<td>-0.076***</td>
<td>24,171</td>
</tr>
<tr>
<td>Michigan</td>
<td>0.572</td>
<td>0.540</td>
<td>-0.034*</td>
<td>3,307</td>
</tr>
<tr>
<td>MI vs. non-MI t-tests</td>
<td>0.047</td>
<td>-0.037**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>n</td>
<td>8,838</td>
<td>18,640</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Figure 1: Plot of the proportion of employer changes involving a change in field per year for those with a patent in a non-enforcing state prior to 1985.
Table 3: Logit models of the likelihood that an inventor will change fields. The population is restricted to those inventors who were granted at least one patent in a non-enforcing state in 1985 or earlier and who changed employers. Employer change is determined by two subsequent patents of the same inventor having different assignees; n=27,478.

<table>
<thead>
<tr>
<th>Model</th>
<th>post-MARA</th>
<th>Michigan</th>
<th>post-MARA * Michigan</th>
<th>inventor previously changed subcategory</th>
<th>inventor's pre-MARA patent frequency (L)</th>
<th>former firm's normalized patents, past 3 years (L)</th>
<th>subsequent firm's normalized patents, last 3 years (L)</th>
<th>last patent was in auto industry</th>
<th>last patent was in auto industry * post-MARA</th>
<th>last patent was in auto industry * Michigan</th>
<th>state enforces non-competes</th>
<th>last patent was at university</th>
<th>duration between patents (L)</th>
<th>Constant</th>
<th>log-likelihood</th>
<th>DV represents change in top-level category or sub-category</th>
<th>Block-bootstrapped standard errors?</th>
</tr>
</thead>
<tbody>
<tr>
<td>Model 1</td>
<td>-1.2218 (1.1307)</td>
<td>0.0455 (0.0476)</td>
<td>0.0741* (0.0332)</td>
<td>0.0154 (0.0106)</td>
<td>-0.0514 (0.0216)</td>
<td>0.2418 (0.2192)</td>
<td>-0.1162 (0.3278)</td>
<td>-0.0476 (0.3928)</td>
<td>-0.0797 (0.0651)</td>
<td>-0.0650 (0.0748)</td>
<td>0.1375*** (0.0089)</td>
<td>17.2326***</td>
<td>-18573.898</td>
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<td>no</td>
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<td></td>
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<tr>
<td>Model 2</td>
<td>-1.2626 (1.1240)</td>
<td>-0.0572 (0.0670)</td>
<td>0.0752* (0.0332)</td>
<td>0.0150 (0.0106)</td>
<td>-0.0505* (0.0215)</td>
<td>0.2353 (0.2191)</td>
<td>-0.0675 (0.3279)</td>
<td>-0.1652 (0.3971)</td>
<td>-0.0766 (0.0652)</td>
<td>-0.0653 (0.0748)</td>
<td>0.1374*** (0.0089)</td>
<td>17.2493***</td>
<td>-18571.512</td>
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<td></td>
</tr>
<tr>
<td>Model 3</td>
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<td>-0.0572 (0.0674)</td>
<td>0.0752* (0.0330)</td>
<td>0.0150 (0.0117)</td>
<td>-0.0505* (0.0227)</td>
<td>0.2353 (0.2078)</td>
<td>-0.0675 (0.3203)</td>
<td>-0.1652 (0.3740)</td>
<td>-0.0766 (0.0627)</td>
<td>-0.0653 (0.0838)</td>
<td>0.1374*** (0.0092)</td>
<td>17.4621***</td>
<td>-18571.512</td>
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<td>no</td>
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<td>Model 4</td>
<td>-0.7265 (0.9305)</td>
<td>-0.0336 (0.0644)</td>
<td>0.1461+ (0.0850)</td>
<td>0.0150 (0.0117)</td>
<td>-0.0436* (0.0214)</td>
<td>0.2353 (0.2116)</td>
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<td></td>
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<tr>
<td>Model 5</td>
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<td>0.0790 (0.0878)</td>
<td>0.0150 (0.0117)</td>
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<td>0.1386*** (0.0100)</td>
<td>18.1339***</td>
<td>18571.512</td>
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<td>no</td>
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</tr>
</tbody>
</table>

DV represents change in top-level category or sub-category

Block-bootstrapped standard errors? yes

(Robust standard errors in parentheses)
Figure 2: Per-observation interaction effects for logit model of changing fields conditional on changing employers, n=27,478. Graph constructed using inteff.

Figure 3: Per-observation z-statistics for interaction effects for logit model of changing fields conditional on changing employers, n=27,478. Graph constructed using inteff.
Table 4: Univariate comparison of the (logged) number of days between the last patent at the former employer of an inventor and the first patent at the inventor’s subsequent employer. The population consists of inventors with at least one patent in a non-enforcing state before 1986 who changing jobs; $n=27,478$

<table>
<thead>
<tr>
<th></th>
<th>pre-MARA</th>
<th>post-MARA</th>
<th>$T$-tests</th>
<th>$n$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Non-Michigan</td>
<td>6.12</td>
<td>6.29</td>
<td>0.17***</td>
<td>24,171</td>
</tr>
<tr>
<td>Michigan</td>
<td>6.14</td>
<td>6.48</td>
<td>0.34***</td>
<td>3,307</td>
</tr>
<tr>
<td>MI vs. non-MI t-tests</td>
<td>0.02</td>
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<td>$n$</td>
<td>8,838</td>
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</tbody>
</table>

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Figure 4: Plot of the (logged) intervals between the last patent at an inventor’s former firm and the first patent at the subsequent firm. Population is restricted to those with a patent in a non-enforcing state prior to the MARA policy reversal; $n=27,478$. 

![Graph showing the comparison of intervals between last and first patent before and after MARA policy reversal for Michigan and non-Michigan inventors.](image-url)
Table 5: OLS models for the (logged) number of days between the last patent at the former employer of an inventor and the first patent at the inventor’s subsequent employer; n=27,478.

<table>
<thead>
<tr>
<th>Model 6</th>
<th>Model 7</th>
</tr>
</thead>
<tbody>
<tr>
<td>post-MARA</td>
<td>0.8968</td>
</tr>
<tr>
<td></td>
<td>(0.5509)</td>
</tr>
<tr>
<td>Michigan</td>
<td>-0.0075</td>
</tr>
<tr>
<td></td>
<td>(0.0351)</td>
</tr>
<tr>
<td>post-MARA * Michigan</td>
<td>0.0909</td>
</tr>
<tr>
<td></td>
<td>(0.0634)</td>
</tr>
<tr>
<td># of pre-MARA patents for inventor (L)</td>
<td>-0.7342***</td>
</tr>
<tr>
<td></td>
<td>(0.0185)</td>
</tr>
<tr>
<td>former employer's normalized # patents (L)</td>
<td>0.0585***</td>
</tr>
<tr>
<td></td>
<td>(0.0087)</td>
</tr>
<tr>
<td>last patent was in auto industry</td>
<td>0.2330+</td>
</tr>
<tr>
<td></td>
<td>(0.1197)</td>
</tr>
<tr>
<td>last patent was in auto industry * post-MARA</td>
<td>-0.5304*</td>
</tr>
<tr>
<td></td>
<td>(0.2361)</td>
</tr>
<tr>
<td>last patent was in auto industry * Michigan</td>
<td>0.0877</td>
</tr>
<tr>
<td></td>
<td>(0.1442)</td>
</tr>
<tr>
<td>last patent was in auto industry * post-MARA * Michigan</td>
<td>0.0532</td>
</tr>
<tr>
<td></td>
<td>(0.2902)</td>
</tr>
<tr>
<td>State enforces non-competes</td>
<td>-0.3113**</td>
</tr>
<tr>
<td></td>
<td>(0.1022)</td>
</tr>
<tr>
<td>last patent was at university</td>
<td>-0.2551***</td>
</tr>
<tr>
<td></td>
<td>(0.0595)</td>
</tr>
<tr>
<td>Constant</td>
<td>4.2063***</td>
</tr>
<tr>
<td></td>
<td>(0.5118)</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.254</td>
</tr>
<tr>
<td>Dependent variable logged?</td>
<td>Yes</td>
</tr>
</tbody>
</table>

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

(Robust standard errors in parentheses)
Table 6: Univariate comparison of the (logged) normalized size of firms joined by inventors when changing jobs; n=27,478. The population is restricted to those inventors who were granted at least one patent in a non-enforcing state in 1985 or earlier and who changed employers. Employer change is determined by two subsequent patents of the same inventor having different assignees.

<table>
<thead>
<tr>
<th></th>
<th>pre-MARA</th>
<th>post-MARA</th>
<th>pre- vs. post-MARA t-tests</th>
<th>n</th>
</tr>
</thead>
<tbody>
<tr>
<td>Non-Michigan</td>
<td>-1.56</td>
<td>-1.76</td>
<td>0.20***</td>
<td>24,171</td>
</tr>
<tr>
<td>Michigan</td>
<td>-1.83</td>
<td>-1.79</td>
<td>0.04</td>
<td>3,307</td>
</tr>
<tr>
<td>MI vs. non-MI t-tests</td>
<td>0.27***</td>
<td>0.03+</td>
<td></td>
<td></td>
</tr>
<tr>
<td>n</td>
<td>8,838</td>
<td>18,640</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Figure 5: Plot of the (logged) normalized size of firms joined by inventors (with at least one patent in a non-enforcing state before 1986) when changing jobs. The population is restricted to those inventors who were granted at least one patent in a non-enforcing state.
Table 7: Multivariate regressions for the size of ex-employees’ subsequent firm. The population is inventors granted one or more patents in a non-enforcing state before 1986 and who changed employers. All models have year and cohort indicators.

<table>
<thead>
<tr>
<th>Model 8</th>
<th>Model 9</th>
<th>Model 10</th>
<th>Model 11</th>
<th>Model 12</th>
</tr>
</thead>
<tbody>
<tr>
<td>post-MARA</td>
<td>-1.9159***</td>
<td>-1.9387***</td>
<td>-1.7283***</td>
<td>-1.7066***</td>
</tr>
<tr>
<td>(0.2843)</td>
<td>(0.2821)</td>
<td>(0.2904)</td>
<td>(0.4766)</td>
<td>(1.3264)</td>
</tr>
<tr>
<td>Michigan</td>
<td>-0.0961***</td>
<td>-0.2164***</td>
<td>-0.2148***</td>
<td>-0.2698***</td>
</tr>
<tr>
<td>(0.0187)</td>
<td>(0.0257)</td>
<td>(0.0256)</td>
<td>(0.0336)</td>
<td>(0.0805)</td>
</tr>
<tr>
<td>post-MARA * Michigan</td>
<td>0.2037***</td>
<td>0.1956***</td>
<td>0.2560***</td>
<td>0.3329***</td>
</tr>
<tr>
<td>(0.0359)</td>
<td>(0.0358)</td>
<td>(0.0450)</td>
<td>(0.1048)</td>
<td></td>
</tr>
<tr>
<td># of pre-MARA patents for inventor (L)</td>
<td>0.0216*</td>
<td>0.0213*</td>
<td>0.0213*</td>
<td>0.0273**</td>
</tr>
<tr>
<td>(0.0090)</td>
<td>(0.0090)</td>
<td>(0.0090)</td>
<td>(0.0106)</td>
<td>(0.0257)</td>
</tr>
<tr>
<td>former employer's normalized # patents (L)</td>
<td>0.1389***</td>
<td>0.1382***</td>
<td>0.1348***</td>
<td>0.1708***</td>
</tr>
<tr>
<td>(0.0059)</td>
<td>(0.0059)</td>
<td>(0.0059)</td>
<td>(0.0071)</td>
<td>(0.0116)</td>
</tr>
<tr>
<td>last patent was in auto industry</td>
<td>-0.1438</td>
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<td>-0.1406</td>
<td>-0.1760</td>
</tr>
<tr>
<td>(0.1016)</td>
<td>(0.1015)</td>
<td>(0.1029)</td>
<td>(0.1307)</td>
<td>(0.2576)</td>
</tr>
<tr>
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<td>0.1495</td>
<td>0.1650</td>
<td>0.1564</td>
<td>0.1888</td>
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<tr>
<td>(0.1827)</td>
<td>(0.1827)</td>
<td>(0.1825)</td>
<td>(0.2162)</td>
<td>(0.3994)</td>
</tr>
<tr>
<td>last patent was in auto industry * Michigan</td>
<td>-0.0034</td>
<td>0.0523</td>
<td>0.0491</td>
<td>0.1421</td>
</tr>
<tr>
<td>(0.1256)</td>
<td>(0.1258)</td>
<td>(0.1265)</td>
<td>(0.1626)</td>
<td>(0.3064)</td>
</tr>
<tr>
<td>last patent was in auto industry * post-MARA * Michigan</td>
<td>-0.0601</td>
<td>-0.1974</td>
<td>-0.1914</td>
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<tr>
<td>(0.2146)</td>
<td>(0.2153)</td>
<td>(0.2146)</td>
<td>(0.2583)</td>
<td>(0.4775)</td>
</tr>
<tr>
<td>State enforces non-competes</td>
<td>0.1656***</td>
<td>0.1691***</td>
<td>0.1510***</td>
<td>0.1365***</td>
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<tr>
<td>(0.0274)</td>
<td>(0.0274)</td>
<td>(0.0275)</td>
<td>(0.0304)</td>
<td>(0.0586)</td>
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<tr>
<td>last patent was at university</td>
<td>0.2175***</td>
<td>0.2169***</td>
<td>0.2251***</td>
<td>0.2640***</td>
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<tr>
<td>(0.0440)</td>
<td>(0.0439)</td>
<td>(0.0438)</td>
<td>(0.0518)</td>
<td>(0.0774)</td>
</tr>
<tr>
<td>duration between patents (L)</td>
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<td>0.0030</td>
<td>0.0016</td>
<td>-0.0089+</td>
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<tr>
<td>(0.0039)</td>
<td>(0.0039)</td>
<td>(0.0039)</td>
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<td>(0.0102)</td>
</tr>
<tr>
<td>Constant</td>
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<td>-0.5512*</td>
<td>-0.4834</td>
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<td>(0.2590)</td>
<td>(0.2691)</td>
<td>(0.4599)</td>
<td>(1.3555)</td>
</tr>
<tr>
<td>Observations</td>
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<td>27478</td>
<td>27478</td>
<td>21831</td>
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<tr>
<td>R-squared</td>
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<td>0.050</td>
<td>0.048</td>
<td>0.070</td>
</tr>
<tr>
<td>Model</td>
<td>OLS</td>
<td>OLS</td>
<td>OLS</td>
<td>OLS</td>
</tr>
<tr>
<td>Window for patent counts</td>
<td>3</td>
<td>3</td>
<td>5</td>
<td>3</td>
</tr>
<tr>
<td>Include moves to self-employment</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>no</td>
</tr>
</tbody>
</table>

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1
(Robust standard errors in parentheses)