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Mobility, Skills, and the Michigan Non-Compete Experiment

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Whereas a number of studies have considered the implications of employee mobility, comparatively little research has considered institutional factors governing the ability of employees to move from one firm to another. This paper explores a legal constraint on mobility—employee non-compete agreements—by exploiting Michigan's apparently inadvertent 1985 reversal of its non-compete enforcement policy as a natural experiment. Using a differences-in-differences approach, and controlling for changes in the auto industry central to Michigan's economy, we find that the enforcement of non-competes indeed attenuates mobility. Moreover, non-compete enforcement decreases mobility more sharply for inventors with firm-specific skills and for those who specialize in narrow technical fields. The results speak to the literature on employee mobility while offering a credibly exogenous source of variation that can extend previous research on the implications of such mobility.

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Introduction

Since Arrow's (1962, p. 615) observation that the "mobility of personnel among firms provides a way of spreading information," the implications of interorganizational mobility have received widespread attention. Scholars have examined the connection between mobility and knowledge spillovers (Stolpe 2002, Rosenkopf and Almeida 2003, Agrawal et al. 2006), research and development (R&D) investment (Kim and Marschke 2005, Singh 2007), and entry by spinoffs (Klepper and Sleeper 2005, Gompers et al. 2005). Generally speaking, this literature has treated mobility as exogenous, paying less attention to its antecedents than its implications. To be fair, other scholars have sought to understand antecedents of employee turnover. Psychologists have documented the influence of attitudinal differences (Porter and Steers 1973, Mobley et al. 1979); sociologists have studied the role of organizational demography (Wagner et al. 1984) and social capital (Granovetter 1973, Marsden and Hurlbert 1988); organizational researchers have differentiated between the ease and desirability of turnover (March and Simon 1958); and labor economists have examined contractual conditions favoring the retention of key scientists (Pakes and Nitzan 1982, Anton and Yao 1995).

Few of these studies, however, have taken into account the potential influence on mobility of post-employment covenants not to compete (hereafter, "non-competes"). This omission is particularly puzzling given the prevalence of such contracts among technology companies whose most valuable assets "walk out the door every night" (LaVan 2000). Recent research, mainly in regional policy and entrepreneurship, has begun to investigate non-competes. Gilson (1999) proposed that Silicon Valley's entrepreneurial growth can be attributed to California's proscription of non-competes. While mobility of California inventors does appear to be high (Almeida and Kogut 1999) and more startups appear in regions that do not enforce non-competes (Stuart and Sorenson 2003), causal evidence for these assertions remains thin (Fallick et al. 2006). Furthermore, we know little about which groups of knowledge workers are likely to be more affected by non-competes (but see Garmaise 2007 for evidence that executives are among those affected).

This paper explores the impact of non-competes on interorganizational mobility by exploiting Michigan's apparently inadvertent 1985 reversal of its enforcement policy as a natural experiment. In particular, it argues that the constraint of non-competes will fall more heavily on individuals who have firm-specific skills or who specialize in a narrow range of technologies. We find support for these arguments using several decades of patent data and by employing a differences-in-differences method that ameliorates some of the challenges inherent in tracking mobility of individuals. The job mobility of inventors in Michigan fell 8.1% following the policy reversal compared to inventors in other states that continued to proscribe non-competes, and these effects were amplified for those with particular characteristics. Michigan inventors with skills one standard deviation above the mean in their firm-specificity experienced a decrease in their job mobility of 15.4% following the policy reversal compared to similar inventors in other states. Likewise, having skills one standard deviation above the mean in technical specialization decreased mobility by 16.2%. By comparing the change in the mobility of Michigan inventors relative to inventors in states that did not change their non-compete laws, the paper offers a "research tool" that could help to establish deeper causal evidence on spillovers and other implications of mobility.

Non-Competes: History and Prior Research

Non-competes appear to be nearly universal in employment contracts (LaVan 2000, Kaplan and Stromberg 2001, Stuart and Sorenson 2003), yet the components of non-competition law have not changed materially for centuries. The earliest recorded case was settled in England in 1414, only a few decades after the Bubonic plague had decimated the European labor supply and subsequent to the Ordinance of Labourers that essentially outlawed unemployment in post-medieval England. Thus a plaintiff's request to enjoin one of his former clothes dyers from working in the same town for six months was met with disdain from the judge, who threatened the plaintiff with jail time for having sought to restrict a citizen from practicing his trade (Decker 1993). The principle of keeping skilled labor in the public domain was reinforced during the rise of the craft guilds through the 16th century; not until the decline of the guilds and inception of the Industrial Revolution did the court begin to enforce non-competes entered into voluntarily by employees. The courts typically stipulated a "reasonableness test," limiting the geographic scope and duration of the agreement.

Firms use non-competes to protect their interests: to prevent the disclosure of trade secrets, to honor customer confidentiality, and to guard against competitors appropriating the specialized skills and knowledge of its employees (Valiulis 1985). One might argue that trade secrets are already protected by the nondisclosure agreement (NDA) employees are generally required to sign, but violations of an NDA can be difficult to detect or prove (Hyde 2003). Preventing an ex-employee from joining a competitor reduces the likelihood that an employee will violate the corresponding NDA via so-called "inevitable disclosure" of confidential information at a new job (Whaley 1999).

Although the law of trade secrets is fairly similar across U.S. states (Hyde 2003), enforcement of non-competes varies significantly from state to state. For example, California's Business and Professions Code Section 16600 is reminiscent of early English law: "*Except as provided in this chapter, every contract by which anyone is restrained from engaging in a lawful profession, trade, or business of any kind is to that extent void.*"[1] Gilson (1999) traces the lineage of California's statute back to its inception in 1872 as an "historical accident" of rapid law-making while California sought statehood. Section 16600 has been upheld by the courts and was reaffirmed in August 2008 by the California Supreme Court's ruling in *Edwards v. Arthur Andersen*.[2] Citing the attenuating impact of non-competes on employee mobility, Gilson proposed that this practice is in fact "the causal antecedent" of the high-velocity labor market as well as the unique culture Saxenian (1994) attributes to Silicon Valley. Gilson's hypothesis went untested until Stuart and Sorenson (2003) examined the effect of initial public offerings (IPOs) and acquisitions on founding rates of biotech firms in regions that enforced non-competes compared with those that did not. That proportionally more biotech firms were founded in states that proscribe enforcement of non-competes is consistent with Gilson's hypothesis. However, as the Stuart and Sorenson analysis measures firm foundings, it does not directly track individual mobility.

An individual-level study of mobility was undertaken in the Fallick et al. (2006) examination of the computer industry in Silicon Valley. Using month-by-month data from the Current Population Survey in the top 20 metropolitan areas, they found an increase in intraregional employee mobility for the California computer industry versus other states. The authors caution, however, against interpreting their results

[1] Note that although contracts typically stipulate a "choice of law"—a state under whose laws the agreement is to be governed—in their 1971 *Frame v. Merrill Lynch, Pierce, Fenner & Smith, Inc.* (20 Cal. App. 3d 668 (1971)) ruling the California courts forbade corporations from specifying out-of-state jurisdiction as a means of cherry-picking one's non-compete enforcement regime.

[2] *Edwards v. Arthur Andersen*, 44 Cal. 4th 937 (2008).

as unequivocal evidence linking non-competes and mobility:

> [We] have no direct evidence that the California effect on mobility is due to the absence of enforceable non-compete agreements. As a result we cannot assess the role that other factors (such as local culture) may play in sustaining high rates of employee turnover. (Fallick et al. 2006, p. 481)

Ideally, the impact of non-competes on mobility would be established through a quasi-experiment that randomly reversed the non-compete enforcement policy in one state and compared changes in intraregional mobility rates between that state and those that did not change their non-compete laws. In the next section, we describe why Michigan may afford such an experiment.

Michigan's Reversal of Non-Compete Enforcement

At the turn of the 20th century, the metropolitan area of Detroit, Michigan, in many ways resembled the Silicon Valley of the last few decades. Growth of the nascent auto industry was explosive, with 500 firms entering before 1915 (Klepper 2002). Ten years prior, the Michigan legislature in 1905 had passed statute 445.761 (bearing resemblance to California's prohibition): *"All agreements and contracts by which any person... agrees not to engage in any avocation or employment... are hereby declared to be against public policy and illegal and void."* This law governed non-compete enforcement until March 27, 1985, when the Michigan Antitrust Reform Act (MARA) repealed section 445 and with it the prohibition on enforcing non-compete agreements.

More than 20 pages of legislative analysis of MARA by both House and Senate subcommittees does not mention non-competes as a motivation for the bill (Bullard 1983a, b; 1985). This may be a consequence of MARA having been modeled on the Uniform State Antitrust Act (Lifland 1984), designed to "make uniform the law with respect to the subject of this act among those states that enact similar provisions." Given that the impetus for the change in law appears to have been general antitrust reform and not specifically altering non-compete enforcement, it appears that the 1905 statute prohibiting non-competes was inadvertently repealed as part of the antitrust reform. If so, then Michigan's change in enforcement would be an exogenous event rather than an example of the legislature simply "catching up" with the courts or responding to lobbying efforts. Even if it were the case that behind-the-scenes lobbying by powerful interests contributed to the legislature's move—and we did not uncover any evidence of this—such a change would still be exogenous to the inventors who are the subjects of this study, assuming that they would have been unaware of such efforts.

Additional evidence for the accidental, exogenous interpretation of Michigan's non-compete reversal is found following the enactment of MARA in March 1985. Multiple law review journals in 1985 (Alterman 1985, Levine 1985, Sikkel and Rabaut 1985) drew attention to the change. Given the rise of commercial advertising by law firms in the 1980s, it is likely that news of the change would have disseminated quickly through law firms, which would have then brought the news to their clients in hopes of generating new contractual work and prosecuting cases (Bagley 2006). Furthermore, less than two years later, the Michigan legislature passed MARA section 4(a), effective retroactive to the enactment of MARA. This bill established the "reasonableness" doctrine in Michigan—limiting the scope and duration of non-competes—that is common to many states that enforce non-competes (Decker 1993). Although we would not expect legislative analysis to report that the purpose of this bill was to provide guidance to the judiciary in the wake of an accidentally repealed statute, both House and Senate legislative analyses do state that a motivation for 4(a) was "to fill the statutory void" (Trim 1987a, b).

Interviews with two Michigan labor lawyers, the authors of a *Michigan Bar Journal* article on non-competes that appeared in October 1985, support the interpretation of the MARA repeal of non-compete enforcement as unintentional (Rabaut 2006, Sikkel 2006). Responding to our neutral interview questions in Online Appendix A (provided in the e-companion),[3] Robert Sikkel (2006) reported:

> There was no buildup, discussion, or debate of which I was aware—it was really out of the blue. As I talked to others, this appeared to be a rather uniform reaction.... I have never been able to identify any awareness—and I examined this at the time—that this was a conscious or intentional act. It was part of the antitrust reform and it may have been overlooked.... I am unaware of anyone that lobbied for the change.

Sikkel's report was independently corroborated by Louis Rabaut (2006), another Michigan-based lawyer active at the time of MARA:

> There wasn't an effort to repeal non-competes. We backed our way into it. The original prohibition was contained in an old statute that was revised for other issues... we were not even thinking about non-compete language.... All of a sudden the lawyers saw no proscription of non-competes. We got active and the legislature had to go back and clarify the law.

Like any law, non-competes are subject to interpretation by the courts. The Texas judiciary, for example,

[3] An electronic companion to this paper is available as part of the online version that can be found at http://mansci.journal.informs.org/.

has at times interpreted its non-compete statute leniently (Wood 2000). Garmaise (2007) notes that the states of 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; moreover, although changes in those states either tightened or loosened constraints on enforcement, none fully reversed the previous enforcement policy. Michigan is the only state we know to have clearly and inadvertently reversed its enforcement policy in the past century. Given that Michigan's shift in non-compete enforcement appears to have been exogenous, we propose that Michigan affords a "natural experiment" with which to directly test the impact of non-competes on worker mobility.

Hypothesis 1 (H1). *Relative to other nonenforcing states, the mobility of inventors within Michigan should decrease subsequent to the passage of MARA legislation.*

Although this first claim is admittedly straightforward, its confirmation would yield a reliably exogenous source of variation in the rate at which inventors change jobs and as such could serve as a research tool to aid future work on the implications of mobility. Next, we build on this baseline hypothesis by examining whether subgroups of inventors are impacted differentially by non-compete enforcement. We hypothesize that the effect of non-competes will be amplified both for inventors whose work is more firm-specific and for those who specialize in particular technologies.

Non-competes should have a greater impact on inventors with firm-specific skills, for two reasons. First, organizations place greater value on inventors with firm-specific skills and knowledge (Becker 1962). Such inventors will understand proprietary technologies better and cause a greater disruption of research and development activities if they leave. Those who have developed firm-specific skills over time will not be immediately replaceable from external labor markets. Moreover, departed inventors are more likely to cause the loss of competitive advantage through the inevitable disclosure of trade secrets. Thus we expect that firms will enforce non-competes more aggressively against firm-specific inventors.

Second, inventors with firm-specific skills are more vulnerable to non-competes. To the extent that they have focused on firm-specific tasks or received firm-specific training, their skills may have become less relevant to other organizations. With fewer external opportunities, they will have less bargaining power, whereas those highly valued by other organizations will maintain greater leverage under the threat of litigation. For example, Lamoreaux et al. (2006) found that highly acclaimed or "star" inventors in turn-of-the-century Cleveland were able to extract more favorable terms regarding intellectual property ownership. Lacking external leverage, firm-specific inventors will be more susceptible to the threat of non-competes.

These arguments elaborate March and Simon's reasoning that "[w]hen an individual remains in an organization for a long time, his skills become more and more specific to the organization in question. Consequently, he becomes more and more indispensable to that organization but more and more dispensable to other organizations" (March and Simon 1958, p. 102). Consistent with most research on tenure and mobility, March and Simon assume that firm-specific skills increase with tenure. While this is surely right (and has been modeled empirically, such as in Lane and Parkin 1998; our data also indicate a significant correlation of moderate size), we separate length of employment from accretion of firm-specific skills. We predict that Michigan firms will have capitalized on the sudden enforceability of non-competes to discourage the departure of their most indispensable employees. This implies an additional decrease in the mobility of firm-specific inventors in Michigan following the passage of MARA.

Hypothesis 2 (H2). *Relative to other nonenforcing states, intraregional mobility for Michigan inventors with firm-specific skills should decrease even further subsequent to the passage of MARA.*

Inventors who specialize in narrow technical domains will likewise feel greater pressure from the enforcement of non-competes—even if their skills are not specific to the firm—because non-competes typically list a set of competitors one may not join or a technical field in which one may not work for a period of time following termination of employment (Valiulis 1985). Consider, for example, those with broadly-applicable skills, such as C++ software developers. Their skills are likely to be of use to myriad firms in industries unrelated to their current employer, so the workers will be able to continue to practice their trade at another firm without infringing on the non-compete agreement. In the case of inventors with highly specialized skills, such as speech recognition scientists, the dynamics may be quite different. Although extraorganizational opportunities may also exist for specialists, these are more likely to originate with organizations that compete with their current employer. As such, specialists may perceive fewer (realizable) extraorganizational opportunities and will be less likely to leave their employer.

Not only may the mobility of specialists be impacted because non-competes lead them to perceive fewer external opportunities, but employers will also more aggressively enforce non-competes against those with specialized technical skills. Firms may be particularly vigilant in protecting themselves against the departure of specialists given the threat of competition from unsanctioned spinoffs founded by ex-employees, as documented in several industries, including automotive (Klepper 2002). Even if trade secrets are not an issue, allowing competitors to capture technical specialists will harm the firm because they are rarer and more difficult to replace than those with more generally applicable skills. Thus we expect that the attenuation of mobility by non-compete enforcement will be increasing in the specialization of an inventor's skill set.

Hypothesis 3 (H3). *Relative to other nonenforcing states, intraregional mobility for Michigan inventors with technology-specific skills should decrease even further subsequent to MARA.*

Study Design

If the initiation of non-compete enforcement via the passage of MARA had a measurable impact on worker mobility in Michigan, we would expect the effect to obtain most convincingly in the difference between Michigan's mobility pre-MARA and post-MARA as compared with other states that did not enforce non-competes both pre- and post-MARA. It would not suffice to observe a difference between Michigan's pre-MARA mobility and post-MARA mobility because many factors may have contributed to changes in mobility of Michigan inventors. Rather, we need to establish a baseline ratio of pre-MARA mobility in Michigan versus that of other states that also did not enforce non-competes. If non-competes did attenuate inventor mobility, then we should see a difference between the pre-MARA baseline ratio and the ratio of post-MARA mobility in Michigan versus that of those same nonenforcing states.

In a controlled experimental setting, one observes the same subjects both before and after the treatment. Accordingly, we limited our test population to inventors active before the passage of MARA and tracked their mobility throughout their careers. In addition to being absent pre-stimulus, the inclusion of inventors who joined the labor force post-MARA could conflate the effects of MARA with period and cohort effects (Glenn 2005). We separate the test population into a control group (the set of such inventors in nonenforcing states) and an experimental group (the set of such inventors in Michigan).

Data

We chose to examine inventor mobility using the U.S. patent database for several reasons.[4] First, patents are public documents and thus make the productivity of inventors visible outside of their current employer. Second, because each patent lists both the inventor's hometown and the patent assignee—if not owned by the inventor, in which case the field is blank or lists the inventor, the patent is "assigned," typically to the inventor's employer—we know the inventor's employer and state of residence. Third, by combining the National Bureau of Economic Research (NBER) patent file (Hall et al. 2001) with weekly updates from the U.S. Patent & Trademark Office, we are able to observe these inventors longitudinally from 1975 to 2006. (We also include more limited data from 1963 to 1974 when available.)

Patent data, however, have a variety of documented weaknesses (Griliches 1991), including the fact that many inventors and entire industries do not patent (Levin et al. 1987). Patents routinely take years to process (Jaffe and Lerner 2004), and the optical-character scanning of paper applications by the patent office creates some errors in computer-readable patent files (Miller 2005). Moreover, attempting to detect inventor movement using patents is necessarily inexact for three reasons. First, we may fail to detect moves that occurred between an inventor's patents (e.g., an inventor patented in city A during 1987 and in city C during 1989 but also lived in city B during 1988). Second, even when we observe a move, we do not know precisely when it occurred within the time interval between the two application dates (Song et al. 2003) or whether the employee-employer separation was voluntary or involuntary. Third, and most challenging, patents are not indexed by inventor. Thus our longitudinal analysis of inventor mobility between firms required us to determine which patents belong to which inventor. For this we leveraged and refined existing algorithms (Trajtenberg et al. 2006, Fleming et al. 2007, Singh 2008). Details of the inventor-matching algorithm are given in Online Appendix B (provided in the e-companion).

Of course, no matching algorithm will be completely free of either Type I or Type II errors, where Type I error is the possibility that the algorithm will fail to identify all of an inventor's patents and Type II error is the possibility that an inventor will

[4] In selecting a data set with which to test our hypotheses, we evaluated the strengths and weaknesses of those used in previous mobility studies (Lazear and Oyer 2004). Tracking firm foundings (as in Stuart and Sorenson 2003) does not necessarily capture interorganizational movement of personnel, so we sought a data source focusing on individuals. The Current Population Survey (CPS; used in Fallick et al. 2006) provides month-by-month worker residence and employment information for a wide variety of technical personnel and is ideal for a pooled cross-sectional study; however, its survey method renders it less suitable for a longitudinal study like ours because no one person in the CPS is surveyed for more than 18 months. This limited window is especially problematic given that it may have taken a number of months for news of MARA's passage to diffuse and thus influence inventors' employment choices.

be matched with patents he or she did not invent. Our approach is to design a robust estimation model and conduct sensitivity analyses of the algorithm at various degrees of conservatism. As will be discussed in the results section, we found little variation between running the algorithm at a very conservative level (many Type I, few Type II) and at a very loose level (few Type I, many Type II). We believe this to be indicative that our study design remains mostly insensitive to the algorithm itself because we are drawing conclusions only from the comparison of mobility rates in Michigan and other nonenforcing states. Hence, if mobility rates in Michigan are underrepresented or overrepresented by too conservative an algorithm, they will likewise be underrepresented or overrepresented outside of Michigan.

In this data set, the inventors at risk of moving are those who patented in Michigan or in another nonenforcing state before MARA was passed, including the following: Alaska, California, Connecticut, Minnesota, Montana, North Dakota, Nevada, Oklahoma, Washington, and West Virginia (Malsberger 1996). For example, if an inventor patented in the nonenforcing state of Connecticut in 1983, all of that inventor's patents prior to 2006 would be included. If an inventor never patented in a nonenforcing state or did not do so until after MARA, that inventor's patents would not be included.

Employing a moderate sensitivity setting for our inventor-matching algorithm, the resulting data set contains 98,468 inventors who patented in Michigan or in another nonenforcing state prior to MARA. Following these inventors throughout their careers yields 372,908 patents between 1963 and 2006, for a patent-per-inventor ratio of 3.79.[5] A total of 27,478 intrastate employer changes were detected for those inventors, averaging 0.28 moves per inventor. By comparison, Rosenkopf and Almeida (2003) found that 25% of inventors in their sample had moved, and Stolpe (2002) estimated that 20% of inventors had moved. An inspection of Michigan patents in the same time frame reveals a similar ratio of patents per inventor $(61{,}615/16{,}885 = 3.65)$ but a lower average number of moves per inventor $(3{,}307/16{,}885 = 0.196)$. In terms of assignee matching, we assumed that mergers, acquisitions, and corporate rechristenings would introduce spurious moves. For example, earlier patents for 3M Corporation were assigned to Minnesota Mining & Manufacturing. Thus we identified all pairs of assignee moves and manually checked the moves for all pairs that appeared more than once, using electronic sources.

[5] We find more patents per inventor than Trajtenberg et al. (2006) largely because our sample is restricted to U.S. inventors. Also, this data set includes patents that were applied for prior to 1999 but not granted until after 1999 and thus are not contained in the NBER data set. The dramatic rise in the rate of patenting after 1999 contributes to the larger number as well. Moreover, we invested considerable time in researching the merger and acquisition histories of patent assignees, which uncovered many within-firm matches for inventors with common names.

Variables

We identify inventors as having changed jobs when successive patents have different assignees. The dependent variable, associated with the latter patent, indicates that this has occurred. Because we are studying the effect of non-compete enforcement on inventor mobility, however, we are interested only in moves that are likely to be affected by non-competes; as such, we ignore transitions from self-employment (where the assignee field is empty) to a firm. We do, however, track the transition from employment to self-employment because firms may choose to enforce non-competes against former employees who strike out on their own.

The explanatory variables include a time period indicator, Michigan residence, and measures of the degree to which the inventor had developed firm-specific or technically specialized skills. A time-period indicator indicates a post-MARA patent application date of 1986 or later. Another indicator variable indicates whether the inventor resided in Michigan at the time of patent application. These two are interacted to create an indicator for inventors active in Michigan following the change in the law. We identify inventors with firm-specific skills by measuring the proportion of the citations to a given patent that are from internal as opposed to from external firms. To assess the degree to which an inventor is a technology specialist versus a generalist, we calculate the (logged) concentration of an inventor's inventions with a Herfindahl measure based on the patent technology class. Both the firm-specificity and the technological-specialization measures are interacted with the post-MARA and Michigan dummies in order to explore the effect of MARA firm- and technology-specific workers. Hypothesized (continuous) variables were centered at zero to simplify interpretation of interaction effects.

We used the application year of an inventor's first patent to generate a cohort indicator. This provides a demographic control to distinguish inventors that may have been nearing the end of their career in the early years of the study from inventors whose first patent may have been applied for while they were very young, perhaps as a graduate student, in the closing year of the study window. Yearly indicator variables account for period differences. Because we observe mobility conditional on patenting, we are more likely to miss moves for inventors who patent

Table 1 Summary Statistics and Correlations for Intrastate Employer Mobility (Change in Patent Assignee) of U.S. Inventors with at Least One Patent Prior to MARA in a Nonenforcing State ($n = 372{,}908$ Patents)

Variable	Mean	Std. dev.	Min	Max	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(1) Move	0.074	0.261	0.000	1.000	1.000											
(2) Prior move	0.229	0.420	0.000	1.000	0.131	1.000										
(3) Enforcing state	0.065	0.247	0.000	1.000	0.003	0.075	1.000									
(4) University	0.020	0.139	0.000	1.000	0.009	0.026	0.006	1.000								
(5) No. of patents per firm (*L*)	2.372	2.330	0.000	8.369	−0.150	−0.144	0.039	0.056	1.000							
(6) Technical specialization	0.000	0.689	−2.568	1.367	−0.080	−0.246	−0.073	−0.033	−0.120	1.000						
(7) Michigan	0.165	0.371	0.000	1.000	−0.034	−0.061	−0.118	−0.020	0.066	−0.026	1.000					
(8) Postmara	0.386	0.487	0.000	1.000	0.148	0.504	0.097	0.051	−0.025	−0.263	−0.039	1.000				
(9) Firm-specificity	0.000	0.314	−0.184	0.816	−0.067	−0.030	0.020	−0.010	0.072	−0.059	0.011	0.100	1.000			
(10) Auto industry	0.034	0.182	0.000	1.000	−0.032	−0.057	−0.029	−0.027	0.190	−0.002	0.356	−0.010	−0.006	1.000		
(11) No. of patents per inventor (*L*)	1.188	1.066	0.010	5.063	−0.013	0.172	0.120	−0.030	0.173	−0.412	0.041	−0.046	0.159	−0.011	1.000	
(12) Time since last patent (*L*)	2.725	4.651	−4.605	9.352	0.215	0.246	0.091	0.021	0.073	−0.368	−0.006	0.422	0.140	0.000	0.313	1.000

less frequently. Hence, we control for an inventor's patenting rate with the log of the count of patents before MARA. Six nonexclusive NBER patent categories are used to control for industrial differences, including chemical (74.6% of patents), computers and communication (51.0%), drugs and medical (9.3%), electric and electronic (22.4%), and other (14.1%) (Hall et al. 2001). To control for firm size, we calculated the total number of patents assigned to the inventor's firm that year. An indicator variable was created for patents whose assignees were colleges and universities because employees of such institutions are not bound by non-competes. We entered an indicator for residence in a state that does enforce non-competes because inventors who left a nonenforcing state and subsequently patented in an enforcing state remained in the data set. Finally, we created an indicator variable that becomes and stays 1 after an inventor has first moved, controlling for prior propensity to move.

One obvious concern using Michigan as a natural experiment is the importance of the auto industry in the state's economy. Difficulties in the industry might explain differences in mobility, independent of the reversal of non-compete enforcement. In particular, if layoffs precipitated by automotive downturns drove higher levels of turnover prior to MARA, what might appear as a widening gap between Michigan and other nonenforcing states might be attributable not to non-compete enforcement but to a later recovery by the auto industry. In his review of employment trends in the Michigan auto industry during the 1980s, Singleton (1992) noted that foreign competition caused sharp fluctuations in employment following the oil shocks of 1973 and 1979 and the ensuing demand for more fuel-efficient cars. Some of the most volatile periods—early 1980, late 1981–1982, and late 1990–1992—occurred during NBER-classified national recessions, which did not leave nonauto industries unaffected.

To control for Michigan automotive trends, we developed two measures of whether the inventor patented with an automobile firm. First, we identified auto patents by assignee name according to Plunkett Research, an industrial sector analysis firm.[6] We also classified auto patents by technology class (Online Appendix C of the e-companion lists the classes) and indicated if an inventor's firm had at least one such patent. We developed three additional indicators based on this classification, at firms that received more than 10%, 25%, and 50% auto patents. We included all interactions of the automotive variable with the time periods and Michigan to fully identify automotive versus nonautomotive temporal effects. The different measures did not change the substantive results (though the models consistently demonstrated an *increase* in automotive inventors' mobility in Michigan during the time period, as illustrated below). Table 1 provides summary statistics and correlation tables.

We employ interaction variables to explore the effect of MARA on inventor mobility. The interaction of the Michigan and postmara dummies tells us whether overall inventor mobility was different in Michigan following the passage of MARA. That interaction variable is then interacted with the measures of firm-specificity and technological specialization to explore the effect of MARA on inventors with firm- and technology-specific skills. Requisite two-way interactions are included wherever three-way interactions are used.

Methods

We estimated logit models to assess whether MARA affected the mobility rate of firm-specific and technology-specialized Michigan inventors. The likelihood

[6] http://www.plunkettresearch.com/Industries/Automobiles Trucks/AutomobilesandTrucksIndustryIndex/tabid/91/Default.aspx.

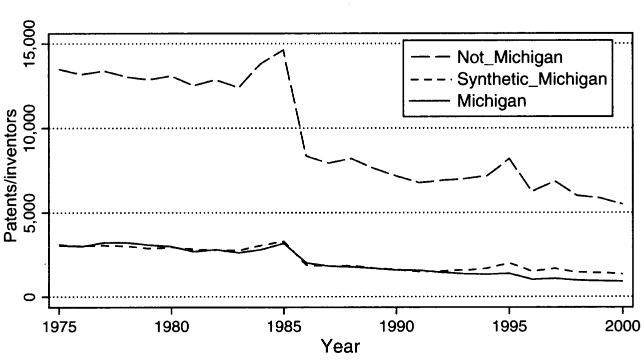
that each inventor's subsequent patent contains a move is estimated utilizing the independent variables described in the previous section. Because at least two patents are necessary to detect a move, inventors with only a single patent are necessarily excluded from the analysis. Thus the 372,908 patents in the original data set are reduced to 274,406 for analysis. Each subsequent patent is an observation, with a move assumed to happen at the midpoint between former and subsequent patent. Assuming the move occurred at the former patent returned similar results and assuming it occurred at the latter patent returned similar though weaker results. Standard errors are clustered by inventor to account for the nonindependence of observations (White 1980).

Results

As a preliminary step, we analyze whether the change of law in Michigan influenced patenting rates at the state level of analysis. Figure 1 illustrates patenting rates of Michigan versus other nonenforcing states from 1975 to 2000 (data after 2000 becomes increasingly thin, as files from the U.S. patent office reflect only granted patents whereas our analysis uses the application date). The patenting rates of both groups are relatively flat before increasing in 1983. The 1986 downturn in both groups reflects our sampling only inventors who applied for their first patent prior to 1986. The non-Michigan rate varies in the mid-1990s, whereas Michigan's rate is more stable.

Figure 1 also includes a "synthetic" Michigan line (Abadie et al. 2007). Prior to and including 1985, this line is a weighted average based on a least squares fit against "real" Michigan using other states that do not enforce non-competes. In other words, the algorithm reconstructs "real" Michigan prior to 1986 from a composite of similar states. In 1986 and later, the synthetic line of Figure 1 is a prediction based on patenting in the control states, multiplied by the same

Figure 1 **Annual Patenting Rates of U.S. Inventors with at Least One Patent Prior to MARA in a Nonenforcing State**



Notes. "Synthetic Michigan" represents predictions of patenting in post-MARA Michigan, based on a weighted average of pre-MARA patenting in other nonenforcing states. MARA passed in 1985.

composite weighting determined before 1986. The motivation for synthetic matching is a better counterfactual for the treated unit, by building from a combination of the most appropriate control units.[7] The algorithm exercises no judgment beyond optimizing the pre-MARA fit of the provided variables based on user-supplied case controls—in this case, states that did not enforce non-competes over the entire time period—and other variables that could influence the outcome. For the patenting-rate analysis, these variables include state population, land in square miles, GDP, number of proprietors, personal income, and total employment.[8] The mobility analysis also includes three patent-count sums of state automobile industry concentration: (1) all patents from firms with at least one automotive technology patent, (2) all patents from firms with at least 50% automotive technology patents, and (3) all patents from firms identified as automotive from the Plunkett industry classification described above.

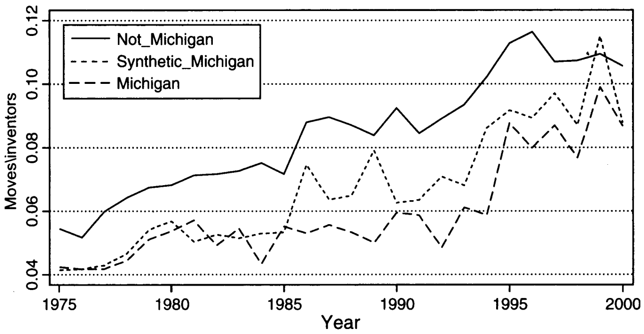
Figure 1 indicates that the rate of patenting in Michigan, relative to a weighted counterfactual Michigan, did not change immediately after the passage of MARA. In 1995, however, synthetic Michigan begins to diverge upward from Michigan's actual rate. Part of the difference arises from the heavy counterfactual weighting of California (0.36) and a substantial rise in that state's patenting in the 1990s. Still, the lack of substantial difference between the real and synthetic data provides some assurance that patenting rates were not greatly affected by any period-specific correlations such as MARA (a graph of the number of inventors over the same time periods looks very similar).

Figure 2 includes analogous lines for the raw mobility of inventors in Michigan and other nonenforcing states, as measured by the percentage of patents in the states that indicate a change in assignee. Non-Michigan states demonstrate a volatile and increasing trend in mobility over the entire time period. Real Michigan mobility increases similarly during the early years, levels off in the 1980s, and jumps in the late 1990s. Overall, it appears that MARA did not cause an absolute decrease in Michigan mobility, though it may have contributed to a decrease relative to other states that continued to proscribe non-competes. Table 2 supports this interpretation, with sharper differences immediately surrounding MARA.

[7] Abadie et al. (2007) provide the STATA routine *synth* to calculate the counterfactual weightings (http://www.people.fas.harvard.edu/~jhainm/software.htm). For the patent analysis, *synth* calculated weights of AK = 0.57, CA = 0.36, and CT = 0.07. Mobility analysis weights were AK = 0.09, CA = 0.26, CT = 0.35, NV = 0.10, and WV = 0.20.

[8] Predictors were variables gathered from the Statistical Abstract of the United States (http://www.census.gov/compendia/statab/) and the U.S. Bureau of Economic Analysis (http://www.bea.gov/).

Figure 2 Annual Mobility Rates of U.S. Inventors with at Least One Patent Prior to MARA in a Nonenforcing State



Notes. "Synthetic Michigan" represents predictions of mobility in post-MARA Michigan, based on a weighted average of pre-MARA mobility in other nonenforcing states. MARA passed in 1985.

The marked upward trend of synthetic Michigan immediately following MARA further supports this interpretation. Rabaut (2006) ascribed the real upturn in the late 1990s to a judicial pendulum swing. Using a scale of 1 to 10, with 1 being complete inability to enforce non-competes and 10 being the opposite, he indicated that Michigan went from a 1 before MARA to an 8 immediately after passage and then back to "…somewhere between 4 and 6. Judges got sick of non-competes. At first they felt they had to enforce them but then they looked harder at being 'reasonable.'" Rabaut further reported that even employers in Michigan became less enamored with non-competes over time, because although they appreciated the use of non-competes as a "hiring shield" they began to realize that it also deprived them of a "hiring sword."

A similar pattern is revealed by modeling an individual inventor's decision to change jobs. We begin with a series of simple logit models. Although we will later control for a variety of factors that, as described above, may substantially influence mobility, the models in Table 3 exclude possibly endogeneous factors, including only indicators for Michigan and post-MARA (and their interaction) as well as annual dummies. The models use data from progressively longer

Table 2 Comparison of Mobility Ratios for U.S. Inventors with at Least One Patent Prior to MARA in a Nonenforcing State

	1980–1989 (5-yr. window) (%)		1975–1995 (10-yr. window) (%)		1963–2006 (all data) (%)	
	Pre-MARA	Post-MARA	Pre-MARA	Post-MARA	Pre-MARA	Post-MARA
Michigan	7.90	5.37	6.21	6.47	5.70	9.18
Non-Michigan	10.16	8.52	8.52	10.38	7.95	12.79
Mobility gap	2.26	3.16	2.31	3.91	2.25	3.61

Notes. Mobility ratios are computed by dividing the number of patents indicating a move by the total number of patents (an inventor's first patent is not considered, as it establishes the first employer and cannot reflect a "move"). Ratios are shown for inventors in Michigan versus other nonenforcing states, pre- and post-MARA. The "mobility gap"—the difference between the mobility ratio of Michigan and other nonenforcing states—grows from the pre- to post-MARA period in each of the three windows.

windows surrounding MARA, initially just 1985 and 1986 and ultimately using all available data. As is visible in the table, the effect of the policy reversal remained strong for several years and then weakened, both in terms of the magnitude and statistical significance of the coefficient on the interaction variable. (Note that although the magnitude of the coefficient on the interaction in Model 1 is similar to that of other models just following the policy reversal, its statistical significance is weak. This could be due either to the smaller number of observations or because news of the policy change—which had not been openly and publicly deliberated—took time to diffuse.) The sharpness of the effect in the years following shortly after MARA, which levels off later, strengthens our confidence in the effectiveness of the natural experiment and differences-in-differences specification.

Table 4 reports multivariate models with additional explanatory and control variables. We adopt the widest possible window (1963–2006) as the most conservative test of our hypotheses. (Unreported regressions of tighter windows yielded similar results.) First considering the control variables, prior mobility has a strong and unsurprisingly positive effect on future movement, indicating heterogeneity in inventor preferences for changing employers. University inventors are more likely to change assignees, which often occurs with the graduation of students into the private or academic sector. Large firms are more likely to retain their employees. Increased frequency of patenting also exhibits a significant effect: Because most patents do not reflect an assignee change, the effect of patent productivity for a given inventor on mobility is negative. However, the longer the time elapsed since the inventor's previous patent, the greater the likelihood that a move has occurred. That the three-way interaction in Model 15 indicated an *increase* in mobility by employees of Michigan auto firms (with substantive results greatly strengthened with the inclusion of the automotive control) indicates that evidence for Hypothesis 1 is not explained by a post-MARA drop in mobility among automotive employees.

Next, we turn to the explanatory variables. Consistent with tenure predictions of prior theory and modeling (Becker 1962, Topel 1991), the first-order measures of firm-specific skills and technology-specialized skills indicate decreased mobility. Models 11–15 step through the various interactions individually, with model fit improving significantly as explanatory variables are added. The negative coefficient on the interaction of the Michigan and post-MARA dummies is, as in Model 10 (the full thirty year window) of Table 3, not statistically significant. But as additional explanatory variables are added in Models 14 and 15, model fit improves, and the interaction of the Michigan and post-MARA dummies

Table 3 Logit Models for Intrastate Employer Mobility of U.S. Inventors with at Least One Patent Prior to MARA in a Nonenforcing State

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9	Model 10
Michigan	−0.3713***	−0.2310***	−0.2747***	−0.3002***	−0.3289***	−0.3322***	−0.3418***	−0.3416***	−0.3416***	−0.3417***
	(0.07686)	(0.04985)	(0.04305)	(0.03941)	(0.03740)	(0.03612)	(0.03566)	(0.03565)	(0.03565)	(0.03565)
Postmara	−1.205766***	−1.2284***	−1.0586***	−0.4786***	−0.2606***	0.4787***	0.5156***	0.4528	1.1094	−0.3505
	(0.0804)	(0.07596)	(0.07402)	(0.06101)	(0.07446)	(0.08194)	(0.1433)	(0.3731)	(1.0197)	(1.1552)
MI * postmara	−0.3381	−0.3654***	−0.2207**	−0.2204***	−0.2026***	−0.1616**	−0.1176*	−0.07585	−0.03967	−0.01716
	(0.2338)	(0.09604)	(0.07078)	(0.06144)	(0.05627)	(0.05248)	(0.04959)	(0.04736)	(0.04611)	(0.04615)
Constant	−1.7183***	−1.5878***	−1.6847***	−2.0236***	−2.2094***	−2.6507***	−2.4877***	−2.3846***	−3.2025**	−1.3235
	(0.03379)	(0.02855)	(0.03088)	(0.03758)	(0.04561)	(0.07116)	(0.1377)	(0.3709)	(1.0177)	(1.1082)
+-year window	1	3	5	7	9	11	13	15	17	All years
No. of observations	22,076	63,206	102,635	140,903	178,795	214,909	241,107	256,422	268,945	274,406

Notes. The "+-year window" indicates how many years of data on either side of the reform were included in that particular regression (e.g., a value of 15 indicates that patents from 1970 to 2000 were included). All models include annual indicators.
$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

achieves statistical significance. This indicates that inventors in Michigan became less mobile following the passage of MARA. The three-way interaction of the Michigan and post-MARA dummies as well as the firm-specificity ratio shows an increased negative and significant effect of MARA on the mobility of firm-specific inventors. Similarly, the interaction of the Michigan and post-MARA dummies as well as the Herfindahl of technical specialization shows a significant negative effect of MARA on the mobility of technology specialists.

Following Hoetker (2007), we illustrate predicted probabilities graphically at the range of values of the hypothesized variables. For this exercise we use Model 16, which relative to Model 15 drops yearly indicators, to visually assess the baseline effect of MARA; the MARA indicator now reflects the average change in mobility of all inventors after 1985. The coefficients, particularly of the hypothesized interactions, are quite similar to those in Model 5, though standard errors increase slightly. Figures 3 and 4 graph predicted probabilities as functions of the treatment groups (pre- and post-MARA, Michigan and non-Michigan inventors) and the two specialization variables (Figure 3 holds the technical specialization measure at zero, and Figure 4 holds the firm-specificity ratio at zero). From Table 2, the baseline predicted probability of mobility for pre-MARA, non-Michigan inventors is 7.95%. Figures 3 and 4 are consistent with the synthetic model in Figure 2 and percentages in Table 2: mobility increased in all nonenforcing states after MARA, but it increased relatively less in Michigan.

We first calculate the magnitude of the two-way interaction in H1. Considering the differences at the means of the centered continuous variables (where the measures of firm-specificity and technical specialization equal 0), the predicted probability of mobility for non-Michigan inventors is 7.95% before MARA and 10.80% thereafter. Similarly, the predicted probability of mobility for Michigan inventors is 7.18% before MARA and 8.98% afterward. Thus the relative risk of post-MARA mobility versus pre-MARA mobility is 1.36 for non-Michigan inventors and 1.25 for Michigan inventors. Comparing these two figures (which themselves represent changes in mobility for each of the two groups over the time periods), the change in relative risk for Michigan inventors is 8.1% less than for non-Michigan inventors.[9]

Next, we turn to the three-way interactions of H2 and H3, utilizing Figures 3 and 4, respectively. Figure 3 graphs the effect of firm-specific skills on mobility in Michigan versus other nonenforcing states. To facilitate interpretation, we consider the predicted probability of movement for an inventor at one standard deviation above the mean of the firm-specificity ratio. If this inventor lived outside of Michigan, the predicted probability of moving would have been 5.88% before MARA and 7.98% thereafter. If this inventor instead lived in Michigan, the predicted probability of moving would have been 5.74% before MARA and 6.59% thereafter. The relative risk of post-MARA mobility versus pre-MARA mobility is 1.36 for non-Michigan inventors and 1.15 for Michigan inventors, a percentage difference of 15.4% for firm-specific inventors in Michigan following the policy reversal. Thus Michigan inventors with more firm-specific skills were less likely to change jobs following

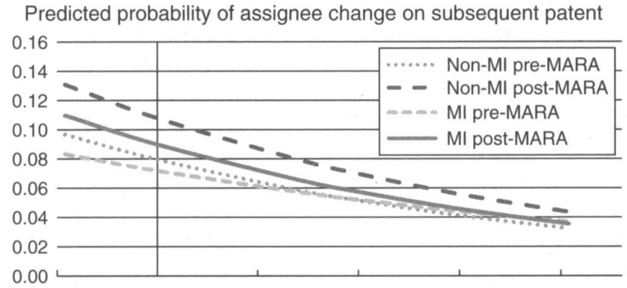
[9] Ai and Norton (2003) argue for the importance of calculating cross-derivatives when interpreting interaction terms for nonlinear models, though their software (STATA's *inteff*) only does so for a simple model with one interaction term. Applying their approach to a model similar to Model 15 (with only one two-way interaction of MARA and Michigan) demonstrated a negative and significant interaction effect at almost all data points (results available from first author). The mean magnitude of the effect was −9.5%.

Table 4 Logit Models for Intrastate Employer Mobility of U.S. Inventors with at Least One Patent Prior to MARA in a Nonenforcing State ($n = 274{,}406$ Patents; 98,468 Inventors; and 27,478 Job Changes)

	Model 11	Model 12	Model 13	Model 14	Model 15	Model 16	Model 17
Chemical industry	0.0593**	0.0580**	0.0580**	0.0578**	0.0578**	0.0751***	0.0578**
	(0.0213)	(0.0213)	(0.0213)	(0.0213)	(0.0213)	(0.0211)	(0.0208)
Computers and communication	0.1353***	0.1342***	0.1344***	0.1341***	0.1343***	0.1432***	0.1343***
	(0.0166)	(0.0166)	(0.0166)	(0.0166)	(0.0166)	(0.0165)	(0.0160)
Drugs and medical	0.0415	0.0414	0.0415	0.0411	0.0412	0.0481+	0.0412
	(0.0281)	(0.0281)	(0.0281)	(0.0281)	(0.0281)	(0.0279)	(0.0254)
Electric and electronic	0.0126	0.013	0.013	0.0126	0.0126	0.0151	0.0126
	(0.0198)	(0.0198)	(0.0198)	(0.0198)	(0.0198)	(0.0197)	(0.0202)
Other industry	−0.1571***	−0.1569***	−0.1570***	−0.1567***	−0.1568***	−0.1509***	−0.1568***
	(0.0186)	(0.0186)	(0.0186)	(0.0186)	(0.0186)	(0.0184)	(0.0191)
Prior move	0.2675***	0.2666***	0.2667***	0.2667***	0.2668***	0.3521***	0.2668***
	(0.0202)	(0.0203)	(0.0203)	(0.0203)	(0.0203)	(0.0186)	(0.0207)
Enforcing state	−0.2315***	−0.2327***	−0.2325***	−0.2330***	−0.2329***	−0.2078***	−0.2329***
	(0.0378)	(0.0378)	(0.0378)	(0.0379)	(0.0379)	(0.0382)	(0.0381)
University	0.3048***	0.3044***	0.3041***	0.3038***	0.3036***	0.3128***	0.3036***
	(0.0493)	(0.0492)	(0.0493)	(0.0493)	(0.0493)	(0.0489)	(0.0508)
No. of patents per firm (L)	−0.2891***	−0.2891***	−0.2891***	−0.2891***	−0.2890***	−0.2898***	−0.2890***
	(0.0044)	(0.0044)	(0.0044)	(0.0044)	(0.0044)	(0.0489)	(0.0043)
Technical specialization	−0.1702***	−0.2121***	−0.2122***	−0.2244***	−0.2242***	−0.2259***	−0.2242***
	(0.0160)	(0.0219)	(0.0219)	(0.0224)	(0.0224)	(0.0221)	(0.0225)
Michigan	−0.1470***	−0.1289***	−0.1165***	−0.1209***	−0.1089**	−0.1099***	−0.1089***
	(0.0327)	(0.0336)	(0.0338)	(0.0335)	(0.0337)	(0.0333)	(0.0322)
Postmara	−0.012	−0.3222	−0.3162	−0.3204	−0.3144	0.3356***	−0.3144
	(1.1839)	(1.1450)	(1.1452)	(1.1482)	(1.1484)	(0.0202)	(8.7470)
Firm-specificity	−1.2053***	−1.2012***	−1.2294***	−1.2014***	−1.2291***	−1.1672***	−1.2291***
	(0.0302)	(0.0454)	(0.0481)	(0.0454)	(0.0481)	(0.0471)	(0.0473)
Postmara ∗ Michigan	−0.0598	−0.0496	−0.0684	−0.0761+	−0.0942*	−0.0926*	−0.0942*
	(0.0443)	(0.0446)	(0.0454)	(0.0461)	(0.0469)	(0.0466)	(0.0475)
Auto industry	0.3787**	0.3800**	0.3793**	0.3802**	0.3795**	0.3858**	0.3795*
	(0.1461)	(0.1464)	(0.1464)	(0.1465)	(0.1465)	(0.1455)	(0.1491)
Postmara ∗ auto industry	−0.0975	−0.101	−0.0994	−0.1016	−0.1001	−0.1040	−0.1001
	(0.1991)	(0.1993)	(0.1993)	(0.1994)	(0.1994)	(0.1983)	(0.2127)
Michigan ∗ auto industry	−1.2787***	−1.2774***	−1.2739***	−1.2776***	−1.2742***	−1.2658***	−1.2742***
	(0.2039)	(0.2041)	(0.2041)	(0.2041)	(0.2041)	(0.0203)	(0.2107)
Postmara ∗ Michigan ∗ auto industry	0.7168**	0.7241**	0.7149**	0.7222**	0.7133**	0.7187**	0.7133**
	(0.2581)	(0.2582)	(0.2583)	(0.2583)	(0.2584)	(0.2575)	(0.2661)
Postmara ∗ firm-specificity		−0.0399	0.0085	−0.0399	0.0077	−0.0288	0.0077
		(0.0593)	(0.0644)	(0.0593)	(0.0644)	(0.0633)	(0.0666)
Michigan ∗ firm-specificity		0.132	0.3047**	0.136	0.3052**	0.3027**	0.3052**
		(0.0860)	(0.1173)	(0.0858)	(0.1171)	(0.1147)	(0.1170)
Postmara ∗ Michigan ∗ firm-specificity			−0.3344*		−0.3289*	−0.3147+	−0.3289+
			(0.1662)		(0.1665)	(0.1636)	(0.1688)
Postmara ∗ technical specialization		0.0538*	0.0538*	0.0738**	0.0735**	0.0619*	0.0735**
		(0.0257)	(0.0257)	(0.0275)	(0.0275)	(0.0272)	(0.0259)
Michigan ∗ technical specialization		0.0764+	0.0781+	0.1602**	0.1603**	0.1523**	0.1603***
		(0.0410)	(0.0408)	(0.0524)	(0.0523)	(0.0517)	(0.0452)
Postmara ∗ Michigan ∗ technical specialization				−0.1505*	−0.1482*	−0.1434+	−0.1482*
				(0.0740)	(0.0741)	(0.0744)	(0.680)
No. of patents per inventor (L)	−0.0758***	−0.0764***	−0.0763***	−0.0767***	−0.0766***	−0.0813***	−0.0766***
	(0.0120)	(0.0121)	(0.0121)	(0.0121)	(0.0120)	(0.0119)	(0.0114)
Time since last patent (L)	0.3674***	0.3669***	0.3669***	0.3669***	0.3669***	0.3712***	0.3669***
	(0.0072)	(0.0072)	(0.0072)	(0.0072)	(0.0072)	(0.0072)	(0.0081)
Constant	−3.1168*	−3.1197*	−3.1236*	−3.1223*	−3.1260*	−3.3926***	−3.1260
	(1.4612)	(1.4777)	(1.4774)	(1.4824)	(1.4820)	(0.7196)	(9.0264)
Log-likelihood	−76,710.738	−76,703.988	−76,701.755	−76,701.54	−76,699.38	−76,920.294	−76,699.388
Annual indicator dummies	Yes	Yes	Yes	Yes	Yes	No	Yes
Block-bootstrapped standard errors	No	No	No	No	No	No	Yes

Note. All models include first-patent-year cohort indicator variables.
$^{+}p < 0.01$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

Figure 3 Interaction Effects for Firm-Specific Skills on Employer Mobility for All Inventors Who Patented in a Nonenforcing State Prior to 1986



Note. The negative impact of firm-specific skills increases in Michigan after the MARA legislation in 1985, as indicated by the steeper slope of the Michigan post MARA line, relative to the Michigan pre MARA line.

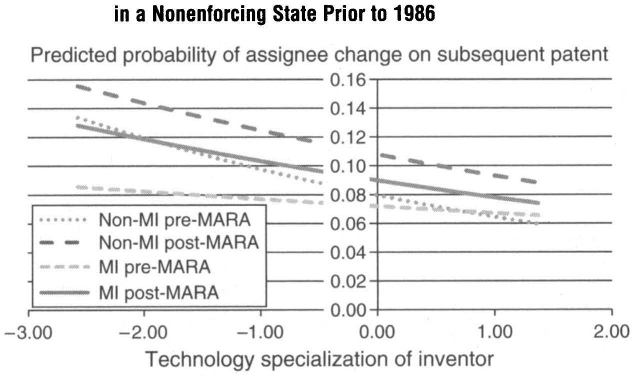
MARA than similarly firm-specific inventors outside the state.

Figure 4 graphs the effect of technology-specialized skills on mobility. As before, we consider the mobility rate of an inventor one standard deviation above the mean. The predicted probability of movement for a technically specialized inventor outside of Michigan was 6.87% prior to MARA and 9.72% thereafter, compared to 6.85% and 8.13%, respectively, for such an inventor within Michigan. The relative risk for the mobility of technically specialized non-Michigan inventors (pre- versus post-MARA) is 1.42, compared to 1.19 for those within Michigan, a percentage difference of 16.2%. Thus technically specialized Michigan inventors were less likely to change jobs following MARA than technical specialists outside the state.

Robustness

We performed a variety of robustness checks. Although the differences-in-differences design should help to ameliorate sensitivities of the matching algorithm, we ran six other trade-off levels between Type I and Type II errors in inventor matching. Neither varying these levels nor ignoring mergers and acquisitions materially affected the results (unreported, but available from the authors). Another concern is that differences-in-differences estimates have been shown to suffer from inflated t-statistics due to serial correlation in data with a large number of periods (Bertrand et al. 2004). Thus we implemented the suggested remedy of Bertrand et al., the block-bootstrap (Efron and Tibshirani 1994), which they argue to be valid when a large number of groups is present. In our study, each of the 98,468 inventors' patent histories represents a "group." The block-bootstrap method samples the patent histories of these inventors with replacement and re-executes the estimation a specified number of times (200, as recommended by Bertrand et al. 2004). As shown in Model 17, significance levels resemble the nonbootstrapped Model 15, suggesting that inference based on this differences-in-differences model is sound.

Figure 4 Interaction Effects for Technology Specialization on Employer Mobility for All Inventors Who Patented in a Nonenforcing State Prior to 1986



Note. The negative impact of technology specialization increases in Michigan after the MARA legislation in 1985, as indicated by the steeper slope of the Michigan post MARA line, relative to the Michigan pre MARA line.

Other unreported models included different ways of identifying patents belonging to the auto industry, as described above, including higher order terms for the time since last patent to account for a nonmonotonic relationship between employment tenure and employer change, omitting moves to self-employment, and substituting a Shannon-Weaver entropy measure for the Herfindahl index. We also decomposed the six NBER sector classifications into 17 subcategories in an attempt to more finely model the various technical fields within which an inventor might work. As an alternative to the logit model, we also estimated the hazard rate of an employer change using a proportional hazard event history model. The disadvantage of rate models is that they assume a move does not occur when a patent is not filed. On the other hand, proportional hazards models avoid assumptions about the relationship between tenure and mobility and should be more sensitive to mobility of extremely productive inventors (because each patent does not contribute a unique data point). Rate models demonstrated similar results.

Discussion

We interpret these results cautiously for several reasons. As noted above, the analysis depends on patent data that, it should be emphasized, enable only imperfect matching of inventors across patents and imperfect observations of job changes. Moreover, we cannot determine whether job changes are voluntary or involuntary, although conversations with employment lawyers and review of specific non-compete agreements indicate that such contracts are typically constructed to survive involuntary separation of employee from employer. Though we have attempted to control for alternative explanations of post-MARA mobility changes in Michigan, the models may be incomplete.[10]

Despite these limitations, we believe that the paper offers at least three contributions. First, the natural experiment identifies non-compete enforcement as a critical institutional determinant of employee mobility. Our models indicated an 8.1% baseline drop in mobility for Michigan inventors that did not work for automobile firms. The effects, both statistically and economically significant, support Gilson's (1999) argument that the "high-velocity labor market" of Silicon Valley can be significantly attributed to California's long-standing proscription of non-compete agreements.

Second, the paper identifies conditions under which non-compete enforcement is more consequential. Workers who have developed firm-specific human capital as opposed to general human capital are 15.4% less likely to change employers when subject to non-compete enforcement. (In obtaining this result, the paper offers a patent-based measure of human capital specificity, complementing the more traditional tenure measure that has often been used as a proxy for firm-specific skills in prior research; see Jovanovic 1979, Lane and Parkin 1998.) Employees who are highly technologically specialized are found to be 16.2% less likely to change jobs. This result in particular may help to explain high rates of spinoff formation in the Silicon Valley's semiconductor and laser industries as well as the early Michigan auto industry, as neither region enforced non-competes (Klepper 2002, Klepper and Sleeper 2005).

Third, and perhaps most useful to mobility researchers, the paper offers a credibly exogenous source of variation in interfirm mobility. Equipped with this "research tool," scholars can revisit questions of causality in the extant literature on mobility, which to date has largely assumed that mobility and its implications are exogenous. The Michigan experiment also enables examination of other phenomena related to non-compete agreements. As just one example, this paper suggests that patents and non-competes are complements and not substitutes, as indicated by the fact that the state-level rate of patenting did not decrease after Michigan enabled enforcement of non-competes.

Building on the themes of this paper, if non-competes inhibit mobility within a region, do they also increase emigration from that region? That specialists are more immobilized by non-competes than other inventors within a region suggests that they may seek career opportunities outside an enforcing state. If so—and notwithstanding the influence of strong research universities, favorable climate, etc.—such incentives and behavior might help explain an agglomeration of talent in nonenforcing areas such as Silicon Valley. These results also open the question of whether non-competes influence the behavior of those who *remain* with their employers. Might those who choose to stay at their current jobs assume less risk and resist experimenting for fear of being terminated while still subject to a non-compete? If individuals cannot extract the full value of their contributions to the company because they are prevented from exploring their market value through external opportunities (as suggested by Motta and Roende 2002), will they in turn be less productive or creative? Will they resist developing firm-specific or technology-specific skills? If collaborations between specialized experts are more likely to invent a breakthrough (Taylor and Greve 2006), and inventors in non-compete regions specialize less, will inventors within non-compete regions invent fewer breakthroughs? Will the value of social capital be less in regions that enforce non-competes because inventors are less free to act on the job opportunity information in their networks (Granovetter 1973, Marsden and Hurlbert 1988)?

Further research is required to understand the organizational and strategic implications of non-competes and inventor mobility. For example, will unsanctioned spinoffs place more strategic distance between themselves and their jilted parent firms where non-competes are enforced? Will this result in less clustering (Audretsch and Feldman 1996) in regions that enforce non-competes? Will firms in non-compete regions invest more heavily in employee training (Becker 1962)? Might large companies in enforcing regions be less aggressive in pursuing new or disruptive markets if their current employees, who best know the "chinks in the armor" of their current strategy, are prevented from competing after leaving, even after being fired? Or will firms in non-enforcing regions (such as Silicon Valley) become more aggressive because they know that their advantage is fleeting? These questions are central to the entrepreneurship, strategy, and regional policy literatures.

Conclusion

This work exploited an inadvertent 1985 change in Michigan non-compete law as a natural experiment,

[10] We do not present evidence concerning the number of court decisions before and after the change in the law, because available databases such as Westlaw typically omit out-of-court settlements and may thus underrepresent the impact of the change in the law. Furthermore, as one employment lawyer put it, "when it comes to non-competes, formal legal action is just the tip of the iceberg; much more impactful is what goes on 'under the surface'—people who don't even try to change jobs because of the threat of legal action" (Bauer 2007). The expectations of legal action can alone serve to deter job mobility, as evidenced in the relationship between reputation for patent enforcement toughness in the semiconductor industry and the mobility of patenting inventors (Agrawal et al. 2009).

comparing the mobility of Michigan inventors relative to similar inventors in other states that did not change their enforcement. Providing direct evidence for the arguments of Gilson (1999) and Stuart and Sorenson (2003), we found a strong decrease in average Michigan mobility once non-competes began to be enforced. By exploiting a natural experiment in a differences-in-differences study design, this study provides stronger identification of the influence of non-competes on mobility (Fallick et al. 2006). Furthermore, the analysis distinguishes the greater effect of non-competes for inventors with firm-specific or technology-specific skills who are not widely marketable beyond direct competitors. The credibly exogenous source of variation in mobility established in this paper can be exploited to extend work on the implications of interorganizational worker mobility.

Ultimately, and as is often the case surrounding issues of sanctioned monopolies, policy planners must decide when the interests of incumbent firms outweigh those of individual careers and possibly regional development. Although much work remains in establishing higher-level connections between, say, non-compete enforcement and economic productivity, we hope that this work contributes both substantively and methodologically to that discussion.

Electronic Companion

An electronic companion to this paper is available as part of the online version that can be found at http://mansci.journal.informs.org/.

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