Mobility, Skills, and the Michigan Noncompete Experiment

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Abstract: While prior research has considered the desirability and implications of employee mobility, less research has considered factors affecting the ease of mobility. This paper explores a legal constraint on mobility —employee noncompete agreements—by exploiting Michigan's apparently-inadvertent 1985 reversal of its 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 noncompetes indeed attenuates mobility. Moreover, noncompete enforcement decreases mobility most sharply for inventors with firm-specific skills, and for those who specialize in narrow technical fields. The results speak to the literature on mobility constraints while offering a credibly exogenous source of variation that can extend previous research.

INTRODUCTION

Employee mobility has received substantial attention from scholars and has recently gained greater managerial attention with the proliferation of high-technology companies whose most valuable assets "walk out the door every night." Research in the organizational and labor economics traditions can be categorized according to March and Simon's (1958) theory concerning the antecedents of turnover, which posits both the *desirability of movement* and *ease of movement* as key determinants of an individual's decision to leave an organization. Individuals' desire to move has been measured using attitudinal differences in areas such as job satisfaction, perception of fit, and commitment to the organization (Porter and Steers 1973, Mobley and Griffeth et al. 1979). Perhaps motivated by criticism that such studies explained a low percentage of observed variance (Jackofsky and Peters 1983), sociologists reframed the question by moving the unit of analysis from the individual to the team. Organizational demographers thus examined not the propensity of an individual person to leave the group but rather the characteristics of teams that make them more likely to lose members (McCain O'Reilly et al. 1983, Wagner, Pfeffer et al. 1984, Bantel and Jackson 1989). Labor economists have developed the contractual conditions under which scientists and other inventors are most likely to be retained (Pakes and Nitzan 1982, Anton and Yao 1995). Both economists and organizational researchers have highlighted the desirability of matching an employee with the most productive employer and the influence of tenure upon the probability of turnover (Jovanic, 1979; Topel, 1991; Lane and Parkin, 1998; Mitchell and Holtom, 2001). Sociologists have investigated how social capital influences the matching process by making individuals more aware of opportunities (Granovetter, 1973; Marsden and Hurlbert, 1988). Recent work has integrated psychological and sociological approaches into an "unfolding" model of turnover (Lee, Mitchell, et al. 1999) in which external shocks such as mergers or changes in marital status often drive employees to reconsider their satisfaction with their current employment.

Taken together, these studies have mainly focused on the *desirability* of movement. Less research has considered factors affecting the ease of movement from one organization to another. One exception is found in Trevor (2001), which demonstrated that the connection between desirability of movement (i.e., job satisfaction) and mobility was moderated by ease of movement as represented by the availability of external opportunities.

Since Arrow's (1962) observation that the "mobility of personnel among firms provides a way of spreading information", researchers in strategy traditions have tended to focus on the *implications* of interorganizational worker mobility. Several scholars have examined the connection between mobility and spillovers (Stolpe 2002; Agrawal, Cockburn et al. 2006; Breschi and Lissoni 2003; Song, Almeida et al. 2003), noting that such employer-to-employer moves may facilitate knowledge transfer both locally (Almeida and Kogut 1999) and over great distances (Rosenkopf and Almeida 2003; Singh 2006a). In addition to infusing the hiring firm with knowledge, employee mobility has been shown to be associated with changes in strategic direction (Boeker 1997), organizational structure (Klette, Moen et al. 2000), the compensation of R&D staff (Moen 2005), innovation and patenting (Kim and Marschke, 2005; Singh 2006b), though not necessarily with performance (Groysberg, Lee, and Nanda, forthcoming). The growth of industries (Franco and Filson 2000; Klepper 2002; Klepper and Sleeper 2002) and even regions (Rosengrant and Lampe 1992; Saxenian 1994) has been attributed in part to the movement of technical personnel between firms.

While these research efforts have significantly advanced our understanding of the implications of mobility to strategic advantage at both the firm and regional level, scholars have cautioned readers against drawing overly strong causal links between mobility and its implications (for example, see Rosenkopf and Almeida 2003 for an assessment of endogeneity and Azoulay et al 2007 for a causal research design on spillovers, based on unexpected deaths of researchers.) The concerns have largely been due to a lack of exogenous variation in mobility, leaving lingering questions about omitted variables and/or reverse causality. A credibly exogenous source of variation in employee mobility would enable more sound causal inferences regarding mobility and its implications.

This paper explores a legal constraint on mobility—post-employment covenants not to compete (hereafter, "noncompetes")—by exploiting Michigan's apparently-inadvertent 1985 reversal of its enforcement policy as a natural experiment. Building upon organizational and labor economics perspectives, it contributes a better understanding of how the ease of movement influences a particular individual's mobility. In particular, it argues that the constraint of noncompetes will fall more heavily upon individuals who specialize in firm-specific skills or who specialize in a narrow range of technologies. Support is found for the arguments using several decades of patent data and by employing a differences-in-differences method that ameliorates some of the inherent challenges in tracking mobility of individuals. Speaking to more strategic perspectives, the research establishes a credibly exogenous source of variation in mobility. By comparing the change in the mobility of Michigan inventors relative to inventors in other states that did not change their noncompete laws, it offers a research tool that could help to establish deeper causal evidence on spillovers and other implications of mobility.

NONCOMPETES: HISTORY AND PRIOR RESEARCH

Noncompetes 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 himself 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 sixteenth century; not until the decline of the guilds and inception of the Industrial Revolution did the court begin to enforce noncompetes entered into voluntarily by employees. The courts typically stipulated a "reasonableness test," including the geographic scope and duration of the agreement.

Firms use noncompetes to protect their interests: to prevent the disclosure of trade secrets, to honor customer confidentiality, and to prevent competitors from appropriating the specialized skills and knowledge of its employees (Valiulis 1985). One might argue that trade secrets are already protected by the non-disclosure 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 via a noncompete 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 noncompetes varies significantly from state to state. For example, California's Business and Professions Code section 16600 (California 1865) 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 1865 as a "historical accident" of rapid law-making as California sought statehood. Yet section 16600 has been upheld by the courts and not overturned by the legislature. Citing the attenuating impact of noncompetes 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 attributes to Silicon Valley. Gilson's hypothesis went untested until 2003, when Stuart and Sorenson (2003) examined the effect of initial public offerings (IPOs) and acquisitions on founding rates of biotech firms in regions that enforce noncompetes versus those that did not. That proportionally more biotech firms were founded in states that proscribe enforcement of noncompetes 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 Fallick, Fleischman, and Rebitzer's (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 vs. other states. The authors caution, however, against interpreting their results as unequivocal evidence linking noncompetes and mobility:

[1] Note that although contracts typically stipulate a "choice of law"—a state under whose laws the agreement is to be governed—in *Frame v. Merrill Lynch* (1971) the California courts forbade corporations from specifying out-of-state jurisdiction as a means of cherry-picking one's noncompete enforcement regime.

> "[W]hile there appears to be a 'California' effect on mobility in information technology clusters, we have no direct evidence that this is due to the absence of enforceable noncompete agreements. As a result we cannot rule out the role that other factors (such as local culture) may play in sustaining high rates of employee turnover."

Ideally, differences in mobility would be established not through cross-sectional analysis but through a controlled experiment: by randomly reversing the noncompete enforcement policy in one state, and comparing changes in intraregional mobility rates between that state and those that did not change their noncompete laws. In the next section, we describe why Michigan may afford such an experiment.

MICHIGAN'S REVERSAL OF NONCOMPETE 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 §16600): *"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 noncompete enforcement until 27 March 1985, when the Michigan Antitrust Reform Act (MARA) repealed MCL 445 and with it the prohibition on enforcing noncompete agreements.

More than twenty pages of legislative analysis of MARA by both House and Senate subcommittees does not mention noncompetes as a motivation for the bill (Bullard 1983a; Bullard 1983b; Bullard 1983c; Bullard 1985). This may be a consequence of MARA having been modeled on the Uniform State Antitrust Act (1985), 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 noncompete enforcement, it appears that the 1905 statute prohibiting noncompetes was repealed as part of the anti-trust 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 general business practice, 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 have yet to 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 noncompete reversal is found following the enactment of MARA in March 1985. Multiple law review journals in 1985 (Alterman 1985; Levin 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, who brought the news to their clients in hopes of generating new contractual work and prosecuting cases (Bagley 2006). Further, 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 noncompetes—that is common to many states that enforce noncompetes (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; Trim 1987b; Trim 1987c).

Interviews with Michigan labor lawyers (authors of a Michigan Bar Journal article on noncompetes that appeared in October of 1985) support the interpretation of the MARA repeal of noncompete enforcement as unintentional (Rabaut 2006; Sikkel 2006). Responding to our neutral interview questions in Appendix A, Robert Sikkel 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 anti-trust 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, another Michigan-based lawyer active at the time of MARA:

> "There wasn't an effort to repeal noncompetes. 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 noncompete language…All of a sudden the lawyers saw no proscription of noncompetes. We got active and the legislature had to go back and clarify the law."

Like any law, noncompetes are subject to interpretation by the courts. The Texas judiciary, for example, has at times interpreted its noncompete statute leniently (Wood 2000). Nonetheless, Michigan is the only state we know of to have clearly and inadvertently changed its enforcement policy in the past century.[2] Given that Michigan's shift in noncompete enforcement appears to have been exogenous, we propose that Michigan affords a "natural experiment" with which to directly test the impact of noncompetes on worker mobility.

Hypothesis 1: Relative to other non-enforcing states, the mobility of inventors within Michigan should decrease subsequent to the passage of MARA legislation.

While 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 upon this baseline hypothesis by examining whether subgroups of inventors are impacted differentially by noncompete enforcement. We hypothesize that the effect of noncompetes will be amplified both for inventors whose work is more firm-specific and for those who specialize in particular technologies.

Noncompetes should have a greater impact on inventors with firm-specific skills, for two reasons. First, organizations place greater value upon 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 can cause the loss of competitive advantage through the "inevitable disclosure" of trade secrets. Thus we expect that firms will enforce noncompetes more aggressively against firm-specific inventors.

[2] The Florida legislatures made a series of changes to its noncompete enforcement policies in the 1990s, but these were fully and openly debated prior to passage and thus cannot be used as an experiment.

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 such external leverage, firm-specific inventors will be more susceptible to the threat of noncompetes.

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). March and Simon assume that firm-specific skills increase with tenure. While this is surely right (and has been modeled empirically, see Lane and Parkin, 1998; our data also indicate a significant correlation of moderate size), we focus explicitly on firm-specific skills. Hence, we predict that Michigan firms will have capitalized on the sudden enforceability of noncompetes 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: Relative to other non-enforcing states, Michigan intraregional mobility for 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 noncompetes—even if their skills are not specific to the firm—because noncompetes do not proscribe the practice of a trade but instead typically list a set of competitors one may not join 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 they will be able to continue to practice their trade at another firm without infringing upon the noncompete agreement. In the case of inventors with highly specialized skills, such as a speech recognition scientist, the dynamics may be quite different. Although extraorganizational opportunities may also exist for the specialist, these are more likely to originate with organizations that compete with their current employer. As such, specialists may perceive fewer (realizable) extraorganizational opportunities (March and Simon, 1958).

Not only may the mobility of specialists be impacted because noncompetes lead them to perceive fewer external opportunities, but employers will also more aggressively enforce noncompetes against those with specialized technical skills. 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 noncompete enforcement will be increasing in the specialization of an inventor's skill set.

Hypothesis 3: Relative to other non-enforcing states, the Michigan intraregional mobility for inventors with technology-specific skills should decrease even further subsequent to MARA.

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STUDY DESIGN

If the initiation of noncompete 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 a difference between Michigan's mobility pre-MARA and post-MARA versus other states that did not enforce noncompetes both pre and post-MARA. It would not suffice to observe a difference between Michigan's pre-MARA mobility and post-MARA mobility, for many factors may have contributed to changes in mobility of inventors, both within and outside of Michigan. Rather, we need to establish a baseline ratio of pre-MARA mobility in Michigan vs. that of other states which also did not enforce noncompetes. If noncompetes did attenuate inventor mobility, then we should see a difference between the baseline ratio and the ratio of post-MARA mobility in Michigan vs. that of those same 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 non-enforcing 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.[3] First, patents are public documents and thus make the productivity of inventors visible outside of their current employer. Second, since 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 NBER patent file (Hall, Jaffe, and Trajtenberg 2001) with weekly updates from the US Patent & Trademark Office, we are able to observe these inventors longitudinally from 1975 through 2006 (we also include the more limited NBER data from 1960-1974).

Patent data, however, have a variety of documented weaknesses (Griliches 1991; Alcacer and Gittelman 2006) including the fact that many inventors and entire industries do not patent (Levin, Klevorick 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

[3] In selecting a dataset 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 (used in Fallick, Fleischman, and Rebitzer 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 as 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.

1988). Second, even when we observe a move, we do not know precisely when it occurred within the time interval of the two application dates (Song, Almeida et al. 2003) and 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, Shiff et al. 2006; Fleming, King et al. 2007; Singh, 2006b). Details of the inventor-matching algorithm are given in Appendix B.

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 be matched with patents they 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 very 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—comparing relative mobility rates across regions—remains mostly insensitive to the algorithm itself since we are not drawing conclusions except from the comparison of mobility rates in Michigan and other non-enforcing 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 dataset, the inventors at risk of moving are those who patented in Michigan or in another non-enforcing 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 non-enforcing state of Connecticut in 1983, all of that inventor's patents from 1960 through 2006 would be included. If an inventor never patented in a non-enforcing state or did not do so until after MARA, that inventor's patents would not be included.

Employing a middle-of-the-road sensitivity setting for our inventor-matching algorithm, the resulting dataset contains 98,468 inventors who patented in Michigan or in another non-enforcing state prior to MARA. Following these inventors throughout their careers yields 372,908 patents between 1960 and 2006, for a patent-per-inventor ratio of 3.79.[4] A total of 27,478 intrastate employer changes were detected for those inventors, averaging .28 moves per inventor. By comparison, Almeida and Rosenkopf (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 timeframe reveals a similar ratio of patents per inventor (61,615/16,885=3.65) but a significantly lower average number of moves per inventor (3,307/16,885=.196). In terms of assignee matching, we assumed that mergers, acquisitions, and corporate rechristening 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.

Variables

We identify an inventor as having changed jobs when successive patents have different assignees. The dependent variable, *move*, indicates that this has occurred. Since we are studying the effect of noncompete enforcement on inventor mobility, however, we are interested only in moves which are likely to be affected by noncompetes; 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 as firms may choose to enforce 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. The time-period indicator *postmara* indicates a patent application date of 1986 or later. The indicator variable *Michigan* indicates whether the inventor resided in Michigan at the time of patent application. The

[4] We find more patents per inventor than Trajtenberg, Shiff, et al. (2006), largely because our sample is restricted to US inventors. Also, this data set includes patents that were applied for prior to 1999, but not granted until after 1999, 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.

variable *firmspecratio* identifies inventors with firm-specific skills by measuring the proportion of the inventor's citations that are to patents held by the firm. In order to assess the degree to which an inventor is a technology specialist vs. a generalist, we calculate the (logged) concentration of an inventor's inventions (*linv_herf)* with a Herfindahl measure based on the patent technology class. These measures of specialization are in addition to an inventor's tenure with the firm (modeled implicitly in non-parametric rate models and explicitly in logit models), which has often been used as a proxy for firm-specific skills in prior research (Jovanic, 1979; Lane and Parkin, 1998). The hypothesized and continuous variables were centered at zero to simplify interpretation of the 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 less frequently. Hence, we control for an inventor's patenting rate before MARA with the log of the count of patents (*lpremarapatrate*) and interactions with Michigan residence and the post-MARA time period. This control should also identify more important or "star" inventors (Zucker and Darby, 1998; Groysberg, Lee, and Nanda, forthcoming). Six non-exclusive NBER patent categories are used to control for industrial differences, including Chemical (74.6% of patents), Computers & Communication (51.0%), Drugs & Medical (9.3%), Electric & Electronic (22.4%), and Other (14.1%) (Hall, Jaffe, and Trajtenberg 2001). To control for firm size we calculated the total number of patents assigned to the inventor's firm that year (*lnfirmpats*). An indicator variable was created for patents whose assignees were colleges and universities (*university)* as employees of such institutions are not bound by noncompetes. We entered an indicator for residence in a state that does enforce noncompetes (*enforce*) as inventors who left a non-enforcing state and subsequently patented in an enforcing state remained in the risk set. Finally, *priormove* becomes and stays 1 in the time periods after an inventor has first moved, controlling for prior propensity to move.

One obvious concern of 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 noncompete 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 non-enforcing states might be attributable not to noncompete 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 through 1982, and late 1990 through 1992—occurred during NBER-classified national recessions, which did not leave non-auto industries unaffected.

In order to control for Michigan automotive trends, we developed two measures of whether the inventor patented with an automobile firm. We first classified auto patents by technology class (Appendix C 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 also identified auto patents by assignee name according to Plunkett Research, an industrial sector analysis firm.[5] 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.

Insert Table 1 about here.

We employ a variety of interactions in order to explore the effect of MARA on inventor mobility. The interaction of *Michigan* and *postmara* tells us whether overall inventor mobility was different in Michigan following the passage of MARA. That interaction variable is then interacted with *firmspecratio*

[5] http://www.plunkettresearch.com/Industries/AutomobilesTrucks/AutomobilesandTrucksIndustryIndex/tabid/91/Default.aspx

and *linv_herf* in order 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 hazard models to assess whether MARA changed the mobility rate of firm-specific and technology-specialized Michigan inventors. We used proportional hazard models (equation 1) to avoid making parametric assumptions about the form of duration dependence in the underlying mobility rate (Cox, 1972). This avoids specifying the relationship between tenure and mobility (different relationships have been proposed, see Jovanic, 1979; Lane and Parkin, 1998), and thus supports our substantive focus upon the influence of specific and specialized skills. The rate for each inventor is the product of an unspecified baseline rate $r_o(t)$ and an exponential term specifying the multiplier effects of macro controls (M_t) and individual variables (X_{it}) on the baseline rate. Because the addition of factors within an exponent can be separated multiplicatively ($e^{(x+y)} = e^x e^y$), the effect of a change in each term on the overall rate can be interpreted independently. Interpretation of Cox model coefficients also does not depend on the particular value of the explanatory or control variables, or the cross-derivative of a non-linear function (for an alternate interpretation, see Ai and Norton, 2003).

$$r_i(t) = r_o(t)e^{(\alpha M_t + \beta_i X_{it} + \varepsilon_{it})} \qquad (1)$$

Each patent is an observation; to account for the non-independence of observations, standard errors are clustered by inventor (White, 1980). Inventors enter the risk set with their first patent, with repeated "failures" or moves possible during their career. Spells are calculated from the filing date of the inventor's first patent at a new employer, rendering the data in Conditional-B format (Hosmer & Lemeshow, 1999). Since the dataset does not indicate when an inventor is no longer at risk of patenting, all inventors are "uninformatively" right-censored (Singer & Willett 2003). As a robustness check, we also estimate the likelihood of mobility as a dichotomous outcome using a logit specification. Whereas the Cox model implicitly accounts for duration, in the logit model we include a measure of tenure with the current employer (*lfirmtenure*).

RESULTS

Figure 1 illustrates patenting rates of Michigan vs. other non-enforcing states from 1975 to 2000 (data after 2000 become increasingly thin, as files from the US 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 while Michigan's rate is more stable.

Figure 1 also includes a "synthetic" Michigan line (Abadie, Diamond, and Hainmueller, 2007). Prior to and including 1985, this line is a weighted average based on a least squares fit against "real" Michigan of states besides Michigan that do not enforce noncompetes. In 1986 and later, the synthetic line is a prediction based on patenting in the control states, multiplied by the weighted average 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.[6] 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 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

[6] Abadie, Diamond, and Hainmueller (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=.57, CA=.36, and CT=. 07. Mobility analysis weights were: AK=.09, CA=.26, CT=. 35, NV=.10, and WV=.20. Predictor variables for both were state populations, land in square miles, GDP, number of proprietors, personal income, and total employment, 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/).

by any time-specific trends 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 non-enforcing states, as measured by the percentage of patents that indicate a change in assignee. Non-Michigan states demonstrate an erratically monotonic increase in mobility over the entire time period. Real Michigan mobility increases similarly during the early years, levels off in the 1980s, and jumps radically 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 noncompetes as shown in Table 2. The marked upward trend of synthetic Michigan immediately following MARA supports this interpretation. Rabaut (2006) ascribed the real upturn in the late 1990s to a judicial pendulum swing. On a scale of 1 to 10, with 1 being complete inability to enforce noncompetes 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 noncompetes. At first they felt they had to enforce them but then they looked harder at being 'reasonable.'" Rabaut (2006) further reported that even employers in Michigan became less enamored with noncompetes over time, because while they appreciated the use of noncompetes as a "hiring shield" they began to realize that it also deprived them of a "hiring sword."

Insert Figures 1 and 2 and Table 2 about here.

Table 3 reports multivariate models. Considering the control variables first, 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 assumedly occurs most often with the graduation of students into the private or academic sector. Large firms are more likely to retain their employees, perhaps indicating greater financial stability. Both yearly and cohort indicator variables (not shown) demonstrate increased mobility over time. Industrial controls (also not shown) indicate that Drugs and Medical inventors moved approximately 29% more than the NBER baseline "other category." Chemical inventors moved approximately 18% more, Computers and

Communication inventors 21% more, Electric and Electronic inventors 8% more, and Mechanical inventors did not move differently from the baseline category. Consistent with tenure predictions of prior theory and modeling (Becker, 1962; Topel, 1991), the first order measures for firm-specific skills and technology specialized skills indicate decreased mobility. That the three-way interaction in model 5 indicated a 26.4% *increase* in mobility by employees of Michigan auto firms (with substantive results greatly strengthened with the inclusion of the automotive control) indicates that confirmation of Hypothesis 1 is not explained by a post-MARA drop in mobility among automotive employees.

Insert Table 3 about here

Models 1-4 step through the various interactions individually. The consistently negative coefficient on the interaction of *Michigan* and *postmara* indicates that inventors in Michigan became less mobile following the passage of MARA. This establishes the baseline hypothesis (H1) that the mobility of Michigan inventors decreased following MARA. The three-way interaction of *Michigan*, *postmara,* and *firmspecratio* shows an increased negative and significant effect of MARA on the mobility of firm-specific inventors as predicted by H2. The interaction of *Michigan, postmara,* and *linv_herf* shows a significant negative effect of MARA on the mobility of technology specialists. Interpreting the strength of the effects from the full Model 5, the basic MARA effect was a decrease in turnover of 19.8%, for inventors that did not work for auto firms and relative to inventors in non-enforcing states (calculated by exponentiating the coefficient on *postmara * Michigan)*. A one-standard-deviation increase in the proportion of firm self-citations implies an 11.0% lower hazard rate in Michigan after MARA, and a similar increase in inventor specialization implied a 12.4% lower hazard rate in Michigan after MARA. [7]

Robustness

We tested robustness in a variety of ways. As an alternative to the Cox rate model we also estimated the likelihood of a move as a dichotomous outcome with each patent as a unit of analysis.

[7] For the continuous variables, we calculate the effect of a one standard deviation change. Given the exponential form of the hazard model estimation, this is not dependent on where it is calculated. For the example of firm-specific skills: 11.0% =100(1- $e^{(-0.3677*0.314)}$) where -0.3677 is the coefficient and the standard deviation is 0.314.

Coefficients in the logit specification of Model 6— which also includes the *ltimelapsed* variable indicating the duration since the previous patent as well as the *lfirmtenure* variable indicating how long an inventor had been with the current employer—are similar to those in Model 5. Unreported models that included higher order terms for *lfirmtenure,* to account for a non-monotonic relationship between tenure and leaving (Jovanic, 1979; Lane and Parkin, 1998), demonstrated substantively similar results (as did the corresponding parametric specifications in rate models).

The differences-in-differences design of the study should help to ameliorate sensitivities of the matching algorithm. Neither six different tradeoff levels between type I and type II errors in inventor matching nor ignoring mergers and acquisitions materially affected the results (unreported, but available from the authors). However, differences-in-differences estimates have been shown to suffer from inflated standard errors due to serial correlation with data from a large number of periods (Bertrand, Duflo, and Mullainathan 2004). Thus we implemented Bertrand et. al.'s suggested remedy of 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 (as recommended by Bertrand et al, *200*). As shown in Model 7**,** significance for all hypotheses resembles the non-bootstrapped Model 5, suggesting that inference based on this differences-in-differences model is sound.

Other unreported though confirming robustness checks include further decomposing the six NBER industry classifications into 17 categories, omitting moves to self-employment, substituting a Shannon-Weaver entropy measure for the Herfindahl index, and interacting the size of firm variable (*lnfirmpats*) with the MARA time period and Michigan indicators (the interactions were insignificant). To address the issue that we cannot observe the exact date of movement, we ran the earliest possible move (that the move occurred immediately after the last patent at the old firm), the latest possible move, and at the midpoint. All hypotheses continued to receive support at the 5% level.

DISCUSSION

These results improve our understanding of the legal constraints on mobility, and in particular, which types of inventors are most affected by noncompetes. The models indicated a 19.8% baseline drop in mobility for Michigan inventors that did not work for automobile firms (inventors that worked for auto firms were 26.4% more likely to move). Inventors that had developed firm-specific and technology-specific skills experienced additional decreases of 11.0% and 12.4%. These results depend on patent data which, it should be emphasized, require imperfect matching and identification of inventors and do not observe moves directly. Furthermore, we cannot determine if the mobility is voluntary or involuntary, though noncompetes remain in force either way. The research does afford the opportunity, however, to revisit the large literature on mobility which has heretofore assumed that mobility and the implications of mobility are exogenous.

Building upon the themes of this paper, if noncompetes inhibit mobility within a region, do they also increase emigration from that region? That specialists are more immobilized by noncompetes 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 non-enforcing areas such as Silicon Valley.

These results also open the question of whether noncompetes 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 noncompete? If individuals cannot extract the full value of their contributions to the company since they are prevented from exploring their market value through external opportunities, will they in turn be less productive or creative? Will they avoid investing in firm-specific or technology-specific skills (Becker, 1962)? If collaborations of specialized experts are more likely to invent a breakthrough (Taylor and Greve, 2006), and inventors in noncompete regions specialize less, then inventors within noncompete regions might invent fewer breakthroughs. Will the value of social capital be less in regions that enforce noncompetes, because inventors are less free to act upon 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 noncompetes and inventor mobility. For example, will unsanctioned spinoffs place more strategic distance between themselves and their jilted parent firms where noncompetes are enforced? Will this result in less clustering (Audretsch and Feldman, 1996) in regions that enforce noncompetes? Will firms in noncompete 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 enforcing regions become more aggressive, because they know that their advantage was fleeting? These questions are central to the organizational, strategy, and regional policy literatures.

CONCLUSION

This work exploited an inadvertent 1985 change in Michigan noncompete law as a natural experiment, comparing the mobility of Michigan inventors relative to similar inventors in other states that did not change their enforcement. Providing the first direct evidence for the mobility arguments of Gilson (1999) and Stuart and Sorenson (2003), we found a strong decrease in average Michigan mobility once noncompetes began to be enforced. This paper is the first to our knowledge to apply longitudinal analysis to the question of noncompete enforcement, and the differences-in-differences study design based on Michigan’s reversal of enforcement lessens causality concerns. Further, the analysis distinguishes the greater effect of noncompetes 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 in order to extend work on the implications of interorganizational worker mobility.

Through our study of this topic, we also became aware of anecdotal evidence of what we call "involuntary sabbaticals" as a response to noncompetes. For example, JetBlue founder David Neeleman was unable to found the now-prominent airline for five years after being dismissed from Southwest Airlines, which refused to reduce the term of the five-year noncompete agreement he had signed (Wells 2002).[8] Following last year's legal wrangling over Kai-Fu Lee's defection from Microsoft to Google, industry evangelist Vic Gundotra chose not to contest his noncompete when leaving Microsoft for Google. Instead, he decided to take a year off as described in Google's official statement:

> *"Mr. 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)

Although abandoning employment for the term of one's noncompete is one method of avoiding legal sanction when changing jobs, this option is available only to those with substantial financial means.

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. While much work remains in establishing higher-level connections between, say, noncompete enforcement and economic productivity, we hope that this work contributes both substantively and methodologically to that discussion.

[8] Noncompete agreements are generally not nullified in the case of involuntary termination; whether departing employees resign or are fired, they are still bound by the agreement.

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Appendix A: Interview questions for Michigan labor lawyers

Before describing our results or the importance of the natural experiment, we asked:
1) When and how did you become aware of the effort to change the Michigan non-compete laws?
2) When and how did inventors and engineers become aware?
3) How aware was the legislature that non-compete laws were being changed as part of the anti-trust legislation?
4) Did the law change the mobility of inventors and engineers? Was there any highly publicized litigation? Did your practice change?
5) Who wanted to change the non-compete laws? Did they actively lobby for it?

After describing our results:
6) What else was happening in Michigan that might have caused this change in mobility?

Appendix B: Inventor Identification and Matching Algorithms

Our algorithm builds on work by Fleming, King, and Juda (2006), Singh (2006b), and (Trajtenberg, Shiff, and Melamed 2006), with a major difference the absence of the Soundex transforms of inventor names. The Soundex algorithm is useful when errors introduced are errors based auditory confusability, such as the names Geoffrey and Jeffrey. However, patents are submitted on paper and scanned using optical recognition software (Miller 2005), which introduces errors based not on auditory but visual confusability: an 'e' mistaken as a 'c', an R transformed to a K, and so on.

Prior to running the matching algorithm, a pre-matching data set is constructed with extensive cleaning of names and locations. Every city's spelling is checked, each city-state pair is verified, and all state abbreviations are confirmed. Inventors occasionally use a county designation, rather than a city when listing their residence, so each US inventor using a county name was researched individually to attempt to identify whether the inventor has other patents that provide a city designation. Inventors often use nicknames such as Dan instead of Daniel (which would also not be detected by Soundex), so inventors using nicknames listed in the top 200 rank of the US Census Bureau's Frequently Occurring First Names and Surnames from the 1990 Census have been manually researched to see if the same inventor appears under his or her full name. Punctuation and foreign characters introduce additional errors and have been transformed, such as ø is replaced by 'O SLASHED' as in the case of JøRGENSEN showing up as J.OSLASHED.RGENSEN. All spaces, accents and punctuation are removed from names.

Assignee names are also cleaned. Assignees frequently appear under a number of variations (for example, AT&T INC, AT&T CORPORATION and AT AND T CORPORATION) and have been researched manually and canonized where appropriate. We also extracted inventor moves from one assignee to a different assignee and researched each firm pair to determine if mergers or acquisitions occurred that may indicate a move or exit where a change did not actually occur. Companies having undergone a merger or acquisition appear under the name of the acquiring firm as of the date of the merger according to the Worldwide Mergers, Acquisitions, and Alliances Databases in SDC Platinum.

Prior to executing the matching algorithm, the "commonness" of each name is noted. For US and Canada the US Census Bureau's *Frequently Occurring First Names and Surnames from the 1990 Census* is used to establish the expected frequency of an inventor's names. If a name is present in our dataset yet not in the Census Bureau name lists, it is assumed to be as uncommon as the least frequently occurring name in the Census Bureau's data set (which covers the 90% most frequently occurring surnames and given names). Middle name frequencies are based on the frequency of middle names in the data set itself.

The size of the inventor's hometown also influences the likelihood of a match. Prior to executing the algorithm, the population decile for each zip code is computed using data from ZIPCodeWorld. The contents of the cleaned, assembled pre-matching data set are as follows for each patent: 1) the inventor's given name, middle name, and surname, all with frequency scores 2) the inventor's city (with decile score), state or province, country, and ZIP code, 3) the primary technology class on the patent, 4) the assignee name, and 5) the list of the co-inventors.

For each pair of patents whether the surnames and given names match, a score is calculated using several factors regarding whether the two patents share the same inventor. The first component of the match score is an index for the uniqueness of the name, computed using cumulative frequencies from the Census Bureau tables for surnames, given names, and middle names. The match score is incremented if the two patents contain the same assignees, if they have identical technology classes, and if they share cities or zip codes[9] (and if not, to a lesser extent if they share states). If the cities do not match, a reverse-index of the city size is added (i.e., a larger increment for smaller cities) based on the larger of the two. Finally, the score is incremented by a scaled factor of the percentage of co-inventors that are identical between the two patents.

Appendix C: USPTO classes used to identify auto patents.

180	Class	180 MOTOR VEHICLES
188	Class	188 BRAKES
152	Class	152 RESILIENT TIRES AND WHEELS
191	Class	191 ELECTRICITY: TRANSMISSION TO VEHICLES
296	Class	296 LAND VEHICLES: BODIES AND TOPS
298	Class	298 LAND VEHICLES: DUMPING
301	Class	301 LAND VEHICLES: WHEELS AND AXLES
303	Class	303 FLUID-PRESSURE AND ANALOGOUS BRAKE SYSTEMS
305	Class	305 WHEEL SUBSTITUTES FOR LAND VEHICLES
903	Class	903 HYBRID ELECTRIC VEHICLES (HEVS)
307	Class	307 ELECTRICAL TRANSMISSION OR INTERCONNECTION SYSTEMS
310	Class	310 ELECTRICAL GENERATOR OR MOTOR STRUCTURE
91	Class	91 MOTORS: EXPANSIBLE CHAMBER TYPE
92	Class	92 EXPANSIBLE CHAMBER DEVICES
192	Class	192 CLUTCHES AND POWER-STOP CONTROL
280	Class	280 LAND VEHICLES
123	Class	123 INTERNAL-COMBUSTION ENGINES
D12	Class	D12 TRANSPORTATION

[9] This alternative match is to control for cities such as New York City, NY, is one city with many zipcodes. There are also cities such as Los Altos, CA and Los Altos Hills, CA that are often used interchangeably under these two different names, but share the same zip code.

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 non-enforcing state (n=372,908).

	Variable	Mean	Stdev	Min	Max	1)	2)	3)	4)	5)	6)	7)	8)	9)	10)	11)	12)	13)
1)	move	0.074	0.261	0.000	1.000	1.000												
2)	priormove	0.279	0.449	0.000	1.000	0.091	1.000											
3)	enforce	0.065	0.247	0.000	1.000	0.003	0.064	1.000										
4)	university	0.020	0.139	0.000	1.000	0.026	0.023	0.006	1.000									
5)	lnfirmpats	2.372	2.330	0.000	8.369	-0.082	-0.183	0.039	0.056	1.000								
6)	linv_herf	0.000	0.689	-2.568	1.367	-0.080	-0.243	-0.073	-0.033	-0.120	1.000							
7)	michigan	0.165	0.371	0.000	1.000	-0.034	-0.064	-0.118	-0.020	0.066	-0.026	1.000						
8)	postmara	0.421	0.494	0.000	1.000	0.055	0.493	0.095	0.051	-0.028	-0.262	-0.038	1.000					
9)	firmspecratio	0.000	0.314	-0.184	0.816	-0.067	-0.062	0.022	-0.010	0.084	-0.084	0.012	0.090	1.000				
10)	lpremarapatrate	-0.446	0.776	-1.861	2.692	-0.005	0.173	0.100	0.004	0.159	-0.365	0.019	0.118	0.189	1.000			
11)	ltimelapsed	5.312	1.537	0.010	9.352	0.175	-0.171	-0.061	0.005	-0.066	0.072	-0.002	-0.190	-0.074	-0.257	1.000		
12)	lfirmtenure	6.914	1.449	0.010	9.589	0.090	-0.092	0.066	0.018	0.152	-0.259	0.021	0.332	0.225	0.113	0.303	1.000	
13)	autoindustry	0.598	0.490	0.000	1.000	-0.130	-0.280	-0.073	0.041	0.356	0.032	0.118	-0.174	0.071	-0.057	0.058	0.040	1.000

Table 2: Comparison of mobility ratios for U.S. inventors with at least one patent prior to MARA in a non-enforcing state. Mobility ratios are computed by dividing the number of patents indicating a move by the total number of patents. Ratios are shown for inventors in Michigan vs. other non-enforcing states, pre- and post-MARA. The "mobility gap"—the difference between the mobility ratio of Michigan and other non-enforcing states—grows from the pre- to post-MARA period in each of the three windows.

Years	1980-89 (5-yr window)		1975-95 (10-yr window)		1960-2006 (all data)	
pre/post-MARA	pre	post	pre	post	pre	post
Michigan	5.22%	5.42%	4.84%	5.80%	4.69%	6.49%
other	7.16%	8.71%	6.56%	9.19%	6.47%	9.49%
"mobility gap"	*1.94%*	*3.29%*	*1.71%*	*3.39%*	*1.79%*	*3.00%*

Figure 1: Annual patenting rates of U.S. inventors with at least one patent prior to MARA in a non-enforcing state. "Synthetic Michigan" represents predictions of patenting in post-MARA Michigan, based on a weighted average of pre-MARA patenting in other non-enforcing states. MARA passed in 1985.

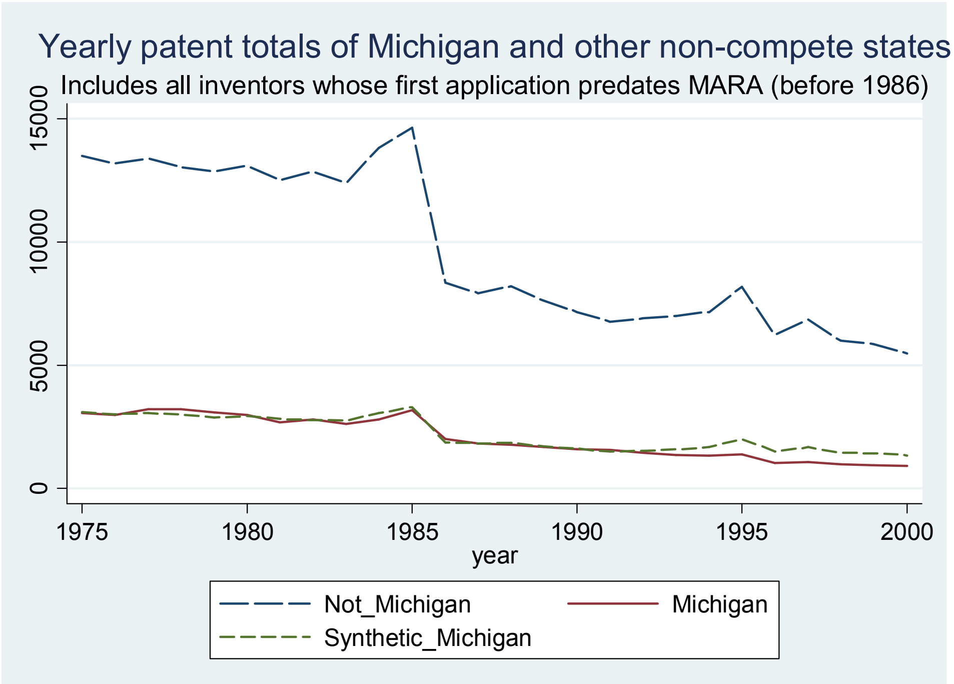

Figure 2: Annual mobility rates of U.S. inventors with at least one patent prior to MARA in a non-enforcing state. "Synthetic Michigan" represents predictions of mobility in post-MARA Michigan, based on a weighted average of pre-MARA mobility in other non-enforcing states. MARA passed in 1985.

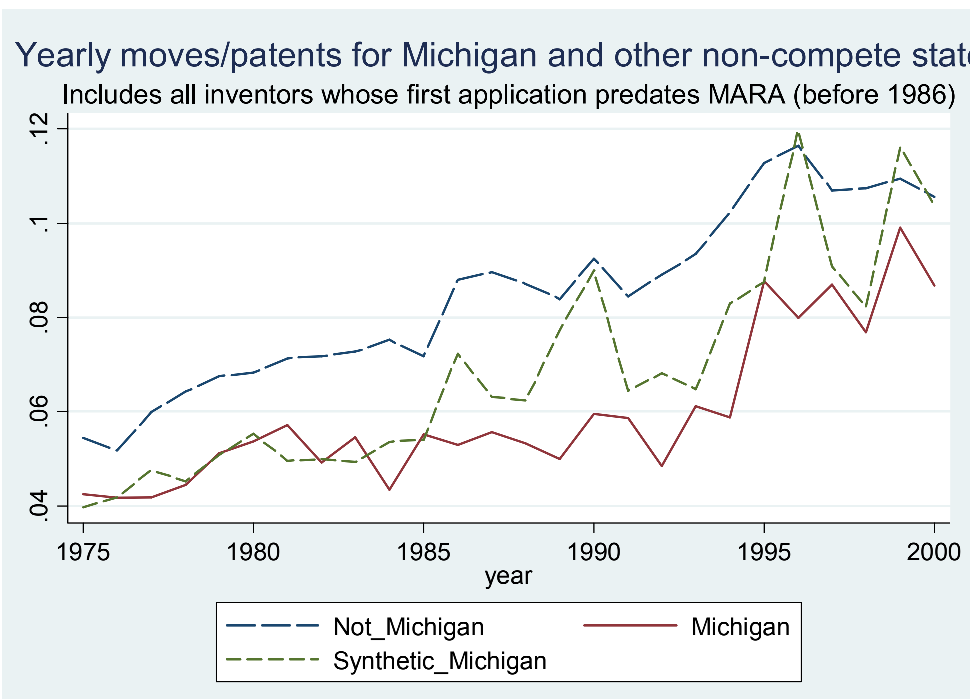

Table 3: Cox event-history models for intrastate employer mobility of U.S. inventors with at least one patent prior to MARA in a non-enforcing state (n=372,908 spells, 98,468 inventors, and 27,478 job changes). All models include annual period indicator variables, first-patent-year cohort indicator variables, and industry indicators for six NBER-defined industries.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
priormove	0.2303***	0.2366***	0.2367***	0.2367***	0.2368***	0.3648***	0.2368***
	(0.0180)	(0.0181)	(0.0181)	(0.0181)	(0.0181)	(0.0235)	(0.0218)
enforce	-0.0428	-0.0412	-0.0410	-0.0417	-0.0416	-0.0806*	-0.0415
	(0.0420)	(0.0420)	(0.0420)	(0.0420)	(0.0420)	(0.0406)	(0.0405)
university	0.4991***	0.4998***	0.5000***	0.4989***	0.4992***	0.6437***	0.4991***
	(0.0399)	(0.0399)	(0.0399)	(0.0400)	(0.0399)	(0.0440)	(0.0590)
lnfirmpats	-0.0025	-0.0027	-0.0027	-0.0026	-0.0026	-0.0486***	-0.0026
	(0.0034)	(0.0034)	(0.0034)	(0.0034)	(0.0034)	(0.0038)	(0.0032)
linv_herf	-0.1117***	-0.0885***	-0.0888***	-0.1007***	-0.1003***	-0.4262***	-0.1003***
	(0.0153)	(0.0197)	(0.0197)	(0.0198)	(0.0198)	(0.0194)	(0.0162)
michigan	-0.0802	-0.0721	-0.0611	-0.0562	-0.0472	-0.1071*	-0.0471
	(0.0490)	(0.0495)	(0.0492)	(0.0487)	(0.0485)	(0.0505)	(0.0393)
postmara	-5.1427***	-21.6624***	-21.6878***	-21.6407***	-21.6705***	-3.3454***	-21.662
	(0.7241)	(0.7318)	(0.7330)	(0.7323)	(0.7334)	(1.0093)	(0.0000)
firm-specialization ratio	-0.6553***	-0.8063***	-0.8334***	-0.8077***	-0.8326***	-0.9064***	-0.8326***
	(0.0268)	(0.0384)	(0.0405)	(0.0385)	(0.0405)	(0.0418)	(0.0162)
lpremarapatrate	0.8597***	0.8740***	0.8755***	0.8717***	0.8731***	0.0138	0.8731***
	(0.0203)	(0.0205)	(0.0205)	(0.0206)	(0.0206)	(0.0218)	(0.0516)
postmara * Michigan	-0.1409*	-0.1460*	-0.1699*	-0.2014**	-0.2202**	-0.1634*	-0.2202***
	(0.0715)	(0.0709)	(0.0705)	(0.0701)	(0.0700)	(0.0752)	(0.0468)
postmara * lpreparapatrate	-0.6311***	-0.6487***	-0.6502***	-0.6454***	-0.6470***	0.0762*	-0.6470***
	(0.0298)	(0.0302)	(0.0302)	(0.0302)	(0.0302)	(0.0311)	(0.0175)
Michigan * lpreparapatrate	-0.0564	-0.0632	-0.0726+	-0.0415	-0.0514	-0.0822+	-0.0514
	(0.0382)	(0.0390)	(0.0393)	(0.0388)	(0.0390)	(0.0455)	(0.0377)
postmara * Michigan * lpreparapatrate	-0.0236	-0.0197	-0.0090	-0.0580	-0.0460	0.0008	-0.0459
	(0.0802)	(0.0810)	(0.0814)	(0.0760)	(0.0764)	(0.0803)	(0.0688)
autoindustry	-0.8396***	-0.8326***	-0.8316***	-0.8329***	-0.8320***	-0.9519***	-0.8320***
	(0.0206)	(0.0207)	(0.0207)	(0.0207)	(0.0207)	(0.0210)	(0.0099)
postmara * autoindustry	0.1264***	0.1121***	0.1094***	0.1126***	0.1101***	0.2060***	0.1101**
	(0.0297)	(0.0300)	(0.0300)	(0.0300)	(0.0300)	(0.0316)	(0.0392)
Michigan * autoindustry	-0.2243***	-0.2275***	-0.2308***	-0.2188***	-0.2223***	-0.1723**	-0.2223***
	(0.0536)	(0.0539)	(0.0538)	(0.0540)	(0.0539)	(0.0556)	(0.0383)
postmara * Michigan * autoindustry	0.2392**	0.2417**	0.2531**	0.2231**	0.2346**	0.1728*	0.2345**
	(0.0800)	(0.0802)	(0.0803)	(0.0803)	(0.0804)	(0.0843)	(0.0927)
postmara * firm-specialization ratio		0.2869***	0.3408***	0.2889***	0.3385***	0.0534	0.3384***
		(0.0535)	(0.0577)	(0.0535)	(0.0577)	(0.0628)	(0.0729)
michigan * firm-specialization ratio		0.1227	0.2966**	0.1266	0.2859**	0.2728**	0.2859**
		(0.0788)	(0.1013)	(0.0786)	(0.1013)	(0.1028)	(0.1353)
postmara * Michigan * firm-specialization ratio			-0.3984**		-0.3677*	-0.3348*	-0.3677*
			(0.1542)		(0.1544)	(0.1681)	(0.1044)
postmara * linv_herf		-0.0453+	-0.0448+	-0.0215	-0.0223	0.2429***	-0.0223
		(0.0257)	(0.0257)	(0.0270)	(0.0271)	(0.0281)	(0.0265)
Michigan * linv_herf		-0.0038	-0.0024	0.0881+	0.0841+	0.0714	0.0841+
		(0.0422)	(0.0420)	(0.0472)	(0.0472)	(0.0450)	(0.0503)
postmara * Michigan * linv_herf				-0.2040*	-0.1923*	-0.1628+	-0.1922*
				(0.0842)	(0.0843)	(0.0848)	(0.0628)
ltimelapsed						0.5667***	
						(0.0096)	
lfirmtenure						0.0503***	
						(0.0125)	
constant						-5.5049***	
						(1.3417)	
Block-bootstrapped standard errors	No	No	No	No	No	No	Yes
Model specification	Cox	Cox	Cox	Cox	Cox	Logit	Cox
log pseudolikelihood	-259809.19	-259791.11	-259787.6	-259786.42	-259783.45	-83932.926	-259783.45