ESSAYS ON EMPLOYEE NON-COMPETE AGREEMENTS

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by

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ABSTRACT

Employee non-compete agreements are an integral part of the professional experience for many knowledge workers and may have pronounced effects on innovation, entrepreneurship, and the occupational trajectories of individuals. Yet scholars have rarely considered non-competes in their analysis of related phenomena. For instance, although hundreds of articles have been written on turnover, few if any of these have considered the potential implications of non-competes for interorganizational mobility. Similarly, studies of occupational change have neglected to account for how such contracts might affect individuals' ability to determine their career paths. More broadly, despite a broad intellectual property literature regarding patents, very few have considered the complementary mechanism of trade-secret protection, which non-competes are ostensibly designed to protect.

In this dissertation, I begin to fill this gap with a multi-method study of the implications of employee non-compete agreements. I collect original field data via randomly-sampled interviews and a survey conducted in conjunction with the IEEE engineering society, which help to ground intuitions regarding how non-compete agreements shape occupational decision-making. The resulting propositions are then

tested using patent data, which facilitates tracking the work histories of individuals over several decades. Key to this analysis is the discovery of an apparently-inadvertent non-compete policy reversal in Michigan during the mid-1980s, which I use as a natural experiment in the specification of a differences-in-differences model.

The results show that non-competes affect both the decision to leave one's employer as well as post-employment choices. Individuals subject to non-competes are less likely to change jobs, particularly when their skills are less transferable to other firms or industries. When workers nonetheless change jobs, they change fields—or refrain from working entirely—in order to avoid violating the terms of the non-compete. Those who do not honor the non-compete often seek protection from potential lawsuits by joining a large firm. That non-competes prevent individuals from exercising their expertise, or that they discourage them from joining small companies, illustrates the potential impact of non-competes on broader outcomes including innovative activity and participation in entrepreneurial ventures. These findings contribute to the literature regarding intellectual property, interorganizational mobility, and occupational change.

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ACKNOWLEDGEMENTS

Employment law is an unusual topic to study in a business school. Indeed, the origin of this idea, as well as its execution, owes to a wide number of people, only a subset of whom I can adequately acknowledge here.

I was trained as an engineer and used to harbor some resentment toward managers and other business-related folk. Steve Wheelwright helped me to get beyond this prejudice and encouraged me to apply to Harvard Business School more than ten years ago. His subsequent (and surprising) support of my decision to take a leave of absence from the MBA program to join a Silicon Valley startup was instrumental to my experiencing the contrast in how non-competes are handled in Massachusetts and California. Accordingly, I thank Angus Davis, Mike McCue, and Hadi Partovi for recruiting me to Tellme Networks. If not for that chain of events, I doubt I ever would have come to study this topic.

Upon entering the doctoral program, I immediately began working with Lee Fleming, who would become my dissertation committee chair. Lee was remarkably patient with me as I scaled the learning curve of academic research. He treated me more like a colleague than an apprentice, which was particularly admirable considering that I arrived with a potentially toxic combination of overconfidence coupled with ignorance of what it takes to produce quality research. I count myself lucky to have been taken under his wing and begun working on research from day one. Moreover, he is a person of extraordinarily high integrity.

Rakesh Khurana, my second committee member, introduced me to the world of organizational theory in his first-semester survey course. Both then and subsequently, he showed me the value in situating research within a theoretical tradition as opposed to focusing solely on managerial implications. Moreover, he pushed me to think of this work in the context of the seemingly-inevitable wave of regulation by the state, which promises to broaden both its appeal and impact. I suspect that Rakesh will contribute to a fundamental rethinking of the purpose not just of management education but of the objective function of business itself; I count myself lucky to "have known him when…"

My third committee member, Josh Lerner, was among the first to see promise in my unconventional topic. He helped me identify a potential contribution to the literature on intellectual property and, perhaps more importantly, was a reliable source of telegraphic feedback. The academic world needs more of his bluntness! While I doubt that I will ever approach his level of throughput, I am grateful for his example and coaching. I hope that this work has been worthy of his time.

In addition to my dissertation committee, a number of others have had direct impact on this work. Deborah Strumsky, a co-author of the second chapter, compiled the patent database used for the hypothesis testing in this paper and invested dozens if not hundreds of hours in marking individual exceptions to text-processing rules. Jason Qiang also worked on an earlier version of the name-matching algorithm. Harlan Piper located the Michigan legislative discussion regarding the reversal of non-compete policy in the Harvard law library. Steven Klepper provided extraordinary feedback on multiple drafts of the second chapter. Bill Simpson answered endless questions regarding three-way interactions and other statistical topics; he has taught me most of what I know about

econometrics. I am deeply grateful to the IEEE engineering society, and in particular to Russ Harrison, Erica Wissolik, and Marc Beebe for collaborating on the non-compete survey. I am likewise indebted to the sixty-five individuals who agreed to be interviewed. Finally, several employment lawyers including Jonathan Keselenko and John Bauer gave freely of their time and wisdom.

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One of the benefits of marrying a writer is a having an ever-present editor. Much more than just coaching me on word choice and structure, however, Heather grilled my conceptual framing and on multiple occasions was the one who helped me see when it was time to give up on a particular project. Much more than that, she has so many times reassured me that it was not crazy to quit my job and move the family across the country to spend four years preparing to take a line of work that would never pay as well as what I used to do. She and my boys were also extraordinarily kind while I was physically unable to work for a few months last year. Henry and Ian have both somehow managed to conceal their amusement that their dad is "still in college" while brightening each day with their enthusiasm.

I'm pretty sure that my father always wanted me to be a professor. He must have cringed when I joined a six-person startup instead of entering the Ph. D. program at MIT in the mid-1990s. It took me a dozen extra years to figure out that this is what I want to do, but he has been kind enough not to say "I told you so." This dissertation is dedicated both to him and to the memory of my mother.

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1: INTRODUCTION

Economic activity occurs within an institutional context which circumscribes the choices of organizations and individuals alike (North 1990; Acemoglu, Johnson, and Robinson 2002). Including but not limited to the enforcement of laws and contracts, actions by the state profoundly influence societal outcomes of interest. Thus an account of such factors is essential to a full understanding of a variety of phenomena. In this dissertation, I examine a widespread yet rarely studied aspect of institutional context: the use of employee non-compete agreements (hereafter, non-competes). These components of many employment contracts—the use of which is sanctioned by the state—forbid employees from working at a competitor for a certain length of time following separation from their employers.

Ostensibly used to help guard against the leakage of trade secrets (Decker 1993; Whaley 1999), non-competes may hold unintended consequences not only for the individuals who sign them but for broader outcomes of interest including innovation and participation in entrepreneurial activity. Although non-competes have been used since the 15th century (see Chapter 2 for a fuller historical account), policymakers and managers alike continue to wrestle with their appropriate use—as evidence by the fact that in 2008 alone, four U.S. states as well as China undertook substantial non-compete reforms. More importantly, that these various legislative agencies have come to opposing conclusions regarding the appropriate use of non-competes suggests and absence of grounded, rigorous research on their implications. One contribution of this study is to inform such debates, not only among policymakers but also for firm managers who decide whether or not to ask employees to sign such contracts.

Yet employee non-compete agreements do not merely represent an interesting phenomenon. The study of non-competes promises to refine and extend several areas of theory, including human capital, interorganizational mobility, occupational change, and intellectual property. Becker (1962:16) claimed that one "cannot separate a person from his or her knowledge [or] skills," but non-competes are designed to do exactly that—at least temporarily, for those who leave their employers. Hundreds of articles have been written about the antecedents of turnover (Trevor 2003), and although many studies have documented the positive spillovers of interorganizational mobility, (Rosenkopf and Almeida 2003; Kim and Marschke), few scholars have considered how institutional factors might constrain or shape the movement of workers between firms and organizational fields. Related findings could have broad implications for the literature on occupational change and mobility, questioning the notion of the "boundaryless career" (Arthur and Rousseau 1996). In the intellectual property literature, patents have received the attention of many economists and other social scientists while the complementary method of trade-secret protection remains undertheorized and virtually devoid of empirical work. Non-competes, ostensibly used to guard against the leakage of trade secrets, offer an opportunity to gain empirical traction on the topic and possibly link intellectual property to human capital.

In this dissertation, I focus on the implications of non-competes both for current and ex-employees. (Antecedent factors explaining variation in the use of non-competes across industries or firms, and also the willingness of individuals to sign such contracts when asked, are also of interest but beyond the scope of this work.) The question of whether non-competes discourage workers from changing jobs has received some scholarly attention, but questions regarding causality remain. The issue of whether non-competes shape post-employment trajectories has received even less attention, prompting me to take a grounded-theory approach (Glaser and Strauss 1967). Tying this fieldwork—the first fieldwork on non-competes to my knowledge—together with large-sample analysis from an inadvertent policy reversal in Michigan helps to triangulate on the results via both qualitative and quantitative methods (Jick 1979).

In Chapter 2, coauthored with Deborah Strumsky and Lee Fleming, I investigate whether non-competes bind employees to their employers. While prior scholars have investigated this question, causality had been difficult to pin down both due to the limitation of cross-sectional analysis (Fallick, Fleischman, and Rebitzer 2006) and because firm level data could not examine the mobility of individuals (Stuart and Sorenson 2003). This study exploits a little-known policy reversal during the mid-1980s following which the state of Michigan began to enforce non-competes. Both archival and interview data indicate that the reversal was not deliberately considered by either the judiciary or the legislature but appears to have been inadvertent, suggesting its suitability for use as a natural experiment. The resulting differences-in-differences model, estimated using decades of work histories reconstructed from the U.S. patent database, indicates a sharp drop in interorganizational mobility for Michigan-based inventors following the policy reversal when compared with those in other states that continued not to enforce non-competes. Moreover, non-competes had nearly twice as strong an effect on those whose skills had limited transferability beyond their firm or industry. The results are robust to a variety of specifications and also control for the automobile industry so important to Michigan's economy.

In Chapter 3, I move to post-employment implications of non-competes. Given that this topic has not received prior attention in the academic literature, I attempt to ground my insights using fieldwork. More than 60 interviews, coupled with results from a survey conducted in conjunction with the IEEE engineering society, reveal four main responses for those constrained from practicing their profession by non-compete agreements. Those who honor the agreement do so most often by taking *occupational detours* in that they change fields when changing jobs; a smaller number of wealthy individuals avoid working entirely, taking *unpaid sabbaticals*. Of those who decide not to honor the non-compete—often those who are were senior or who have less transferable skills—most do so by *seeking shelter* with a large company they feel can indemnify them against possible lawsuits; those who persist in working at small companies attempt to avoid detection by their prior employer by *lying low*. Those interviewed indicate that each of these choices holds negative implications for them personally, including reduced wages for the duration of the non-compete, the atrophy of specialized skills, and the loss of professional ties.

Chapter 4 returns to the Michigan experiment in order to test the propositions of the fieldwork using large sample analysis. I find evidence of the *occupational detour* construct in that Michigan inventors following the policy reversal were more likely to change fields when changing jobs than were inventors in other states that continued not to enforce non-competes. I also find that Michigan inventors who changed jobs joined larger firms after the policy change, offering evidence for the *seeking shelter* construct and showing how the enforcement of non-competes can discourage participation in entrepreneurial activity.

This dissertation substantially extends our understanding non-competes: that they discourage individuals from changing jobs; that those subject to such contracts are more likely to change fields when they change jobs; and that ex-employees are less likely to join small companies when non-competes are enforced. It moreover identifies characteristics that moderate the impact of non-competes, including the transferability of one's skills, personal wealth, and seniority. The study addresses previous concerns regarding the causal impact of non-competes via the Michigan natural experiment and also provides a "research tool" for others studying non-competes or who require exogenous variation in one or more of the outcome variables from this study.

This study also opens several avenues for future work. Given the career limitations placed on individuals by non-competes, will regions that enforce non-competes experience "brain drain" of their top talent to regions that do not enforce these agreements? Will those who remain in enforcing regions find themselves less motivated to develop firm specific or industry-specific expertise, lest it not be portable to their next job? Will professional networks and other forms of social capital prove less useful where non-competes are enforced, approximately providing another motivation for skilled workers to leave the region? Similar questions exist at the firm or industry level, including whether non-compete affect the formation and location of clusters. In addition, although I have not specifically framed non-competes as a determinant of social stratification or a barrier to attainment, the application of these results to such issues appears straightforward.

To be clear, my aim is not to perform an overall welfare calculation by weighing the costs to individuals or society against the benefits to firms of being able to prevent ex-employees from joining competitors. Although this study highlights negative implications for individuals and small firms, established companies certainly appear to benefit from the use of non-competes in that they are more easily able to retain key employees and prevent them from aiding competitors. Ultimately workers, executives, and policymakers must weigh the benefits and costs of non-competes at the individual, firm, and regional levels. My hope is that this work will better inform the participants in that debate.

2: MOBILITY, SKILLS, AND THE MICHIGAN NON-COMPETE EXPERIMENT

ABSTRACT

While 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.

INTRODUCTION

Since Arrow's (1962) 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, Cockburn, and McHale 2006), R&D investment (Kim and Marschke 2005; Singh 2007), and entry by spinoffs (Klepper 2005; Gompers, Scharfstein, and Lerner 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, Griffith, Hand and Meglino 1979); sociologists have studied the role of organizational demography (Wagner, Pfeffer, and O'Reilly 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); 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, Fleischman, and Rebitzer 2006). Further, we know little about which groups of knowledge workers are likely to be more affected by non-competes (though 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 upon 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 not to enforce 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 sixteenth 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 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 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 (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 1872 as a "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*. 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 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 enforce 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.

[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* ruling the California courts forbade corporations from specifying out-of-state jurisdiction as a means of cherry-picking one's non-compete enforcement regime.

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 non-competes and mobility:

> *"[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 non-compete 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, 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 27 March 1985, when the Michigan Antitrust Reform Act (hereafter, MARA) repealed section 445 and with it the prohibition on enforcing non-compete agreements.

More than twenty 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; 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 non-compete enforcement, it appears that the 1905 statute prohibiting non-competes was inadvertently 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 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, who would have then 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 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; Trim 1987b; Trim 1987c).

Interviews with two Michigan labor lawyers, the authors of a Michigan Bar Journal article on non-competes that appeared in October of 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 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 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, has at times interpreted its non-compete statute leniently (Wood 2000). Garmaise (2007) notes that 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, while 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: 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 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 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 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, Levenstein, and Sokoloff (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 1958:102). 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, see 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: Relative to other non-enforcing 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 they will be able to continue to practice their trade at another firm without infringing upon 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: Relative to other non-enforcing 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, for 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 vs. that of other states which 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 vs. that of those same non-enforcing 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.[2] First, patents are public documents and thus make the productivity of inventors

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[2] 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

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 more limited data from 1963-1974 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, Klevorick, Nelson, and Winter 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, Almeida, and Wu 2003) or whether the employee-employer separation was voluntary or involuntary. Third, and most challenging, patents are not indexed by

data source focusing on individuals. The Current Population Survey (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 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.

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, and Melamed 2006; Fleming, King, and Juda 2007; Singh, 2008). 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 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 since we are drawing conclusions only 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 prior to

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 moderate 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 1963 and 2006, for a patent-per-inventor ratio of 3.79.[3] 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 lower average number of moves per inventor (3,307/16,885=.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.

[3] 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, 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. Since 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 as 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 internal as opposed to from external firms. 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 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 less frequently. Hence, we control for an inventor's patenting rate with the log of the count of patents before MARA. 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 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 as employees of such institutions are not bound by non-competes. We entered an indicator for residence in a state that does enforce non-competes as inventors who left a non-enforcing state and subsequently patented in an enforcing state remained in the data set. Finally, we create an indicator variable that becomes and stays 1 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 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 non-enforcing 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 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. First, we identified auto patents by assignee name according to Plunkett Research, an industrial sector analysis firm.[4] We also 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 included all interactions of the automotive variable with the time periods and Michigan in order to fully identify automotive vs. non-automotive 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.

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

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=274,406 patents).

	Variable	Mean	Stdev	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)	# 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)	# 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

We employ interaction variables in order 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 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 logit models to assess whether MARA affected the mobility rate of firm-specific and technology-specialized Michigan inventors. The likelihood 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 non-independence 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 vs. other non-enforcing states from 1975 to 2000 (data after 2000 becomes 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: 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.

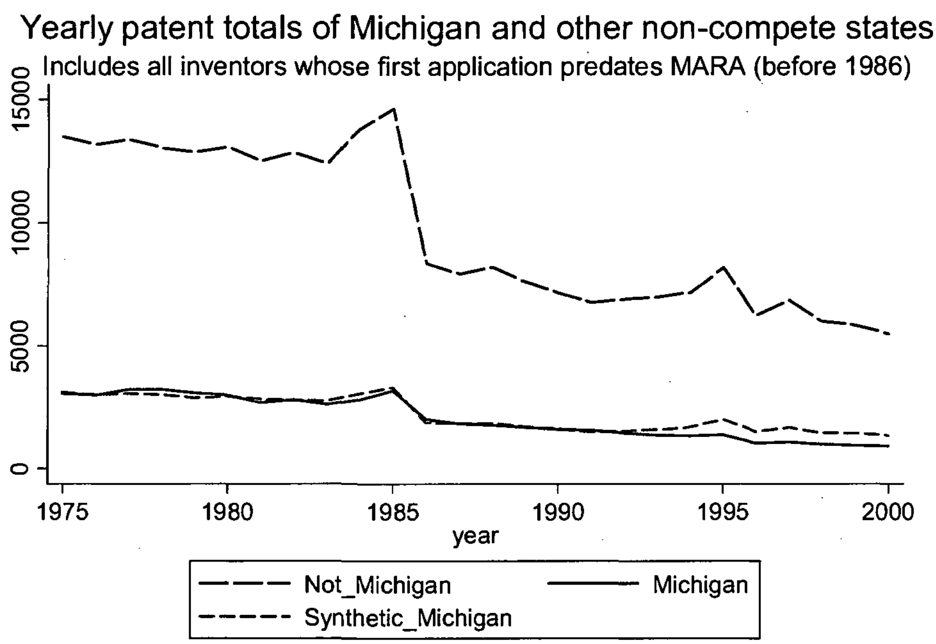


Figure 1 also includes a "synthetic" Michigan line (Abadie, Diamond, and Hainmueller, 2007). Prior to and including 1985, this line is a weighted average based on a least squares fit against "real" Michigan using other states that do not enforce non-competes. In other words, the algorithm reconstructs "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 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.[5] 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.[6] 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).

[5] 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.

[6] Predictor variables were 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 includes analogous lines for the raw mobility of inventors in Michigan and other non-enforcing 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.

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.

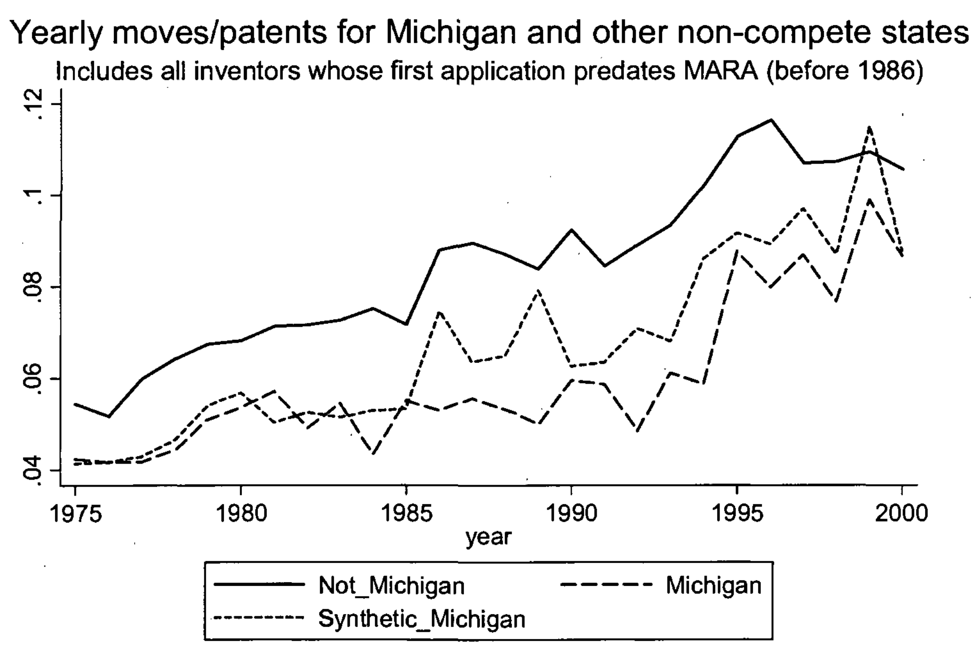


Table 2: Comparison of mobility ratios for U.S. inventors with at least one patent prior to MARA in a non-enforcing state. Mobility ratios are computed by dividing the number of patents indicating a move by the total number of patents (an inventor's first patent is not considered, as it establishes the first employer and cannot reflect a "move".) Ratios are shown for inventors in Michigan 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.

	1980-89 (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%

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. On 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 while 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. While we will later control for a variety of factors which, as described above, may substantially influence mobility, the models in Table 3 excludes 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 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 3: Logit models for intrastate employer mobility of U.S. inventors with at least one patent prior to MARA in a non-enforcing state. 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 through 2000 were included). All models include annual indicators.

	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
# observations	22076	63206	102635	140903	178795	214909	241107	256422	268945	274406

Table 4 reports multivariate models with additional explanatory and control variables. We adopt the widest possible window (1960-2006) 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.

Table 4: Logit models for intrastate employer mobility of U.S. inventors with at least one patent prior to MARA in a non-enforcing state (n=274,406 patents, 98,468 inventors, and 27,478 job changes). All models include first-patent-year cohort indicator variables.

	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 & 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 & 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 & 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)
# 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)	(0.0680)
# 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	-76710.738	-76703.988	-76701.755	-76701.54	-76699.38	-76920.294	-76699.388
annual indicator dummies	Yes	Yes	Yes	Yes	Yes	No	Yes
block-bootstrapped standard errors	No	No	No	No	No	No	Yes

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 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, in order 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. Figure 3 and Figure 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 non-enforcing 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% afterwards. Thus the relative risk of post-MARA mobility versus pre-MARA mobility is 1.36 for non-Michigan inventors and 1.25 for Michigan. 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.[7]

Next we turn to the three-way interactions of H2 and H3, utilizing Figure 3 and Figure 4 respectively. Figure 3 graphs the effect of firm-specific skills upon mobility in Michigan vs. other non-enforcing 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

[7] Ai and Norton (2003) argue for the importance of calculating cross-derivatives when interpreting interaction terms for non-linear 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%.

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, 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 MARA than similarly firm-specific inventors outside the state.

Figure 3: Interaction effects for firm-specific skills on employer mobility, for all inventors who patented in a non-enforcing state prior to 1986. 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.

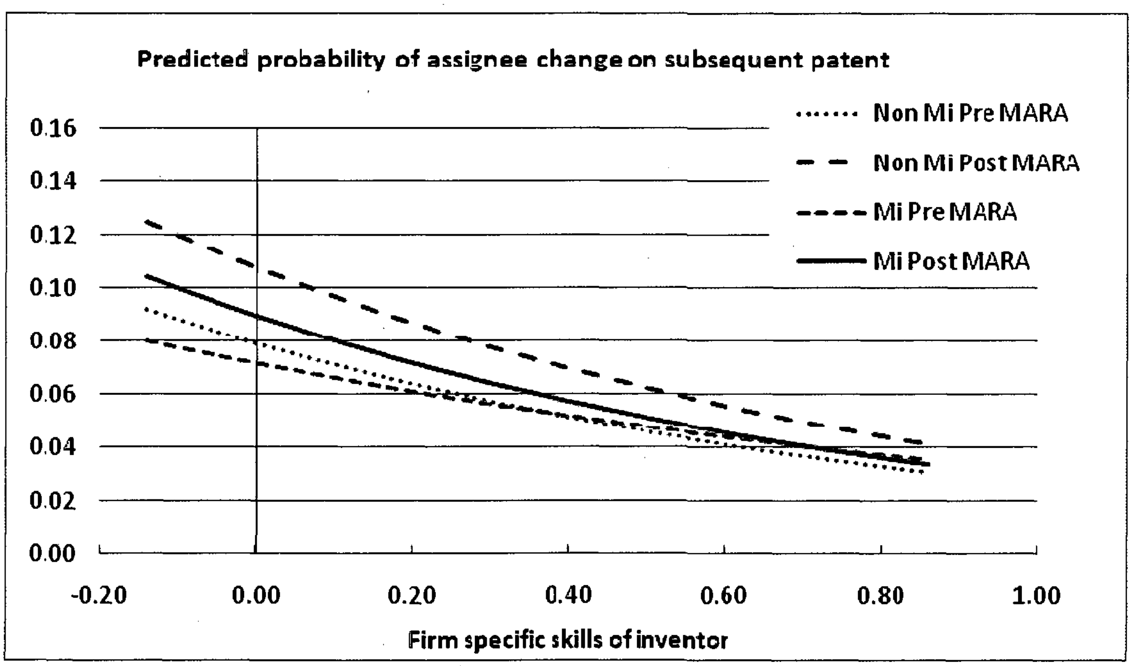
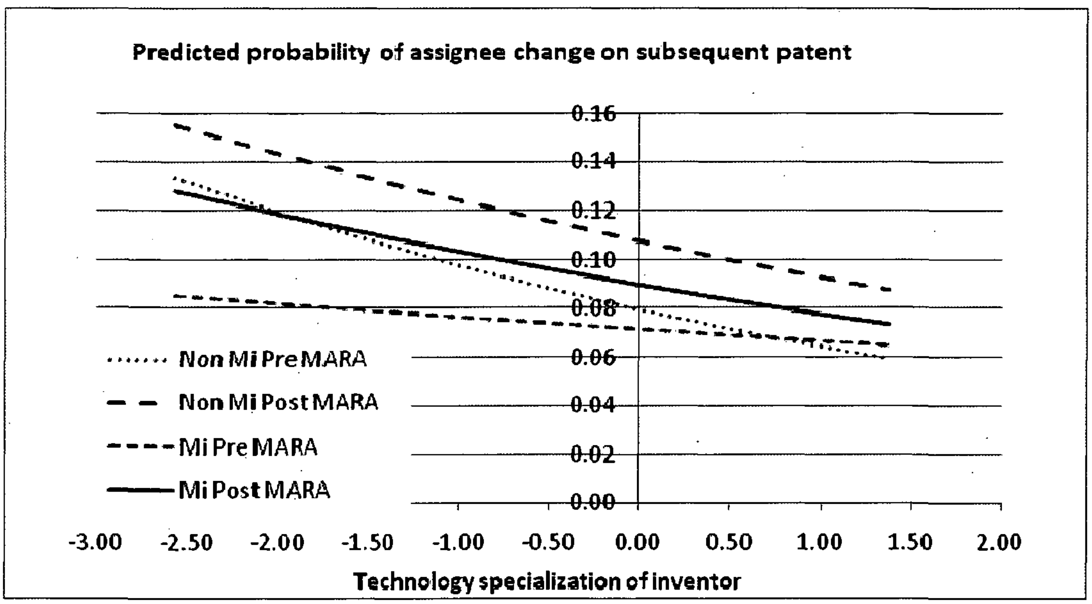


Figure 4 graphs the effect of technology-specialized skills upon mobility. As before, we consider the mobility rate of an inventor one standard deviation above the mean. The predicted probability of movement for a technically-specialized inventor outside of Michigan was 6.87% prior to MARA and 9.72% 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- vs. 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.

Figure 4: Interaction effects for technology specialization on employer mobility, for all inventors who patented in a non-enforcing state prior to 1986. 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.



Robustness

We performed a variety of robustness checks. While the differences-in-differences design should help to ameliorate sensitivities of the matching algorithm, we ran six other tradeoff 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 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 (200, as recommended by Bertrand et al.). As shown in Model 17, significance levels resemble the non-bootstrapped Model 15, suggesting that inference based on this differences-in-differences model is sound.

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 non-monotonic 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 datapoint). Rate models demonstrated similar results.

DISCUSSION

We interpret these results cautiously, for several reasons. As noted above, the analysis depends on patent data which, 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.[8]

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.

[8] 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 under represent the impact of the change in the law. Further, 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." The expectations alone of legal action can 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 (Agarwal, Ganco, and Ziedonis 2008).

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 (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 inter-firm 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 upon 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 non-enforcing 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 since 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 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 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, 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 noncompetes on mobility (Fallick, Fleischman, and Rebitzer 2006). Further, 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 in order 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. While 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.

3: GOOD WORK IF YOU CAN GET IT...AGAIN: A FIELD STUDY OF NON-COMPETES AND EX-EMPLOYEES

ABSTRACT

In this qualitative study, I examine how individuals respond when they are restrained from practicing their chosen profession by non-compete agreements. Although prior literature has examined whether non-competes bind employees to their employers, left unexplored is whether such contracts also influence occupational trajectories for those who nonetheless leave. Moreover, prior studies have not observed non-compete contracting directly, a limitation I address with interviews and a survey. Analysis of field data reveals that ex-employees subject to non-competes employ four approaches: (1) *occupational detours*, in that they change their line of work to avoid infringing on the non-compete; (2) *unpaid sabbaticals*, avoiding employment entirely; (3) *seeking shelter* by taking employment with a large firm that can indemnify them against a possible lawsuit; or (4) *lying low* to avoid detection by their prior employer as they continue to work in the same field. The implications of these approaches include reduced compensation, the atrophy of specialized skills, and the loss of professional ties. Factors influencing the approach chosen include seniority, wealth, and the degree to which one's skills would be difficult to transfer to another industry. The results have implications for theories of occupational change and add to our understanding of how the enforcement of firm-level regulations by the state "trickle down" to individuals.

INTRODUCTION

How does the state influence economic outcomes, not only for firms but also for those whom they employ? Although economists have long concerned themselves with measuring how policy shapes market interactions, organizational scholars have been slower to address such issues. Of course, neoinstitutionalists have identified coercion as one of three isomorphic forces that divert firms from economically optimal outcomes (Scott 1995), but when such studies have investigated the impact of intervention by the state, they have generally focused on implications for organizational forms and practices as opposed to the individuals who compose the firm (Roy 1997; Dobbin and Dowd 1997). To the extent that individual outcomes are realized in large part by individuals' participation within firms (Baron and Bielby 1980), firm-level regulation by the state may have critical implications for workers themselves.

One area in which the oversight of firms by the state might affect individuals is the administration of employment contracts. This paper explores a component of employment contracts, post-employment covenants not to compete (hereafter, "non-competes"), which are ostensibly designed to protect against the disclosure of trade secrets. Although a few studies have begun to explore the implications of non-compete agreements (Stuart & Sorenson 2003; Fallick, Fleischman, and Rebitzer 2006; Garmaise 2007), very little direct evidence exists regarding the usage, terms, and impact of such contracts. Moreover, while most work to date has focused on the question of whether non-competes bind employees to their employers, comparatively unexplored is the issue of whether such contracts also shape occupational trajectories for those who nonetheless leave their employers.

I address these limitations using a field-based approach. First, I conduct open-ended interviews with a small, theoretically-sampled group of individuals followed by several dozen semi-structured interviews with a random sample of knowledge workers from a single industry. I then supplement this data with a cross-industry survey. I find that non-competes indeed influence the occupational trajectories of those who sign them and then change jobs. Four responses became evident from the field data. First, individuals took *occupational detours* in that they switched organizational fields for at least the duration of the non-compete agreement, experiencing lost wages and the atrophy of their specialized skills. Second, those with sufficient financial resources chose an *unpaid sabbatical* during which they did not seek paid employment. Third, several of those who chose not to honor the non-compete decided to *seek shelter* with a large firm that could reduce the likelihood of career interruption due to a lawsuit aimed at enforcing the non-compete. Fourth, those who continued working at a small firm in their field chose to *lie low* in order to avoid detection by their former employer. The decision to either honor or violate the non-compete was shaped both by personal wealth as well as the degree to which an individual has difficult-to-transfer expertise in a particular field.

The remainder of the paper is structured as follows. First, I briefly review the literature on how institutions impact economic outcomes for individuals as well as extant work on non-competes. Second, I motivate the qualitative approach and describe my methodology. Third, I review what the field data indicate regarding how non-competes shape occupational choices for those who change jobs. Fourth, I discuss how these results add insight to theories regarding occupational change and institutional context more generally.

INSTITUTIONS, NON-COMPETES, AND OCCUPATIONAL CHANGE

Susskind and Zybkow (1978:31) define institutions as "social organizations that enact socially desirable outcomes" that would not be attained by the free market alone. They delineate between stimulative interventions such as the shaping of research and development trajectories via government funding (Dosi 1982) and regulative interventions, including tax policy, antitrust enforcement, and intellectual property protection. Each of these has been studied extensively by economists, who have produced myriad studies of the impact of institutional "rules of the game" (North 1990), particularly in the area of property rights. Both the lack of strong institutional protection for investors (LaPorta, Lopez-de-Silanes, Schleifer, and Vishny 1996) and the pressure of "extractive" institutions (Acemoglu, Johnson, and Robinson 2002) have been shown to concentrate ownership at the expense of investment by a broader set of entrepreneurs.

The state also plays a role in neoinstitutional theory. Although much work in this area tends to focus on normative or mimetic mechanisms, Scott (2000) also identifies "formal and informal pressures" from external resource providers as a third, coercive, type of isomorphism. Indeed, sociologists have sought to understand how the state shapes economic outcomes (Evans, Rueschemeyer, and Skocpol 1985), even though some have disdained the study of regulation as having "little theoretical import" (Hannan and Carroll 1995:540). One stream of research in this tradition explores the consequences of industry-specific interventions by the state (Haveman, Russo, and Meyer 2001; Scott 2004), while another has examined the implications of cross-sector policies including antitrust law (Stearns and Allan 1996; Dobbin and Dowd 1997). Taken together, these studies deliver a sociological account of how actions by the state affect organizations. But with the exception of a few studies that examine the impact of civil rights legislation (Edelman 1990; Dobbin and Sutton 1998), prior work has virtually ignored the impact of firm-focused regulation on the individuals who compose some of the most differentiating resources of those organizations. Baron and Bielby (1980) argue that the organization of work, in terms of both within-firm structure and institutional cross-firm arrangements, can profoundly affect individual attainment. If so, then our understanding of institutional theory, as well as the processes of stratification, may be enhanced by mapping whether and how firm-level regulations by the state hold implications for individuals.

One aspect of firm-level regulation that may affect individuals is the enforcement of employment contracts by the state. Although such agreements generally govern individuals while employed by a particular firm, they may also restrict behavior thereafter. One such example is a non-disclosure agreement, in which an employee promises never to disclose proprietary information. While non-disclosure agreements are believed to be widespread, employers can find it difficult to establish whether ex-employees are honoring their commitment (Hyde 2003). Thus many employment contracts contain non-competition clauses. Those who sign non-compete agreements covenant not to work for competitors for a period of time after leaving the firm. Yet the post-employment impact of non-compete agreements has received little attention, perhaps partially due to the difficulty of observing the contracts themselves. Indeed, one limitation of empirical studies to date is that they have analyzed large-sample datasets under different non-compete enforcement regimes but without knowing whether individuals in the sample had actually signed such agreements.[9] This study addresses this limitation using fieldwork.

A second limitation of extant studies is that they focus on how non-competes discourage employees from changing jobs (Fallick et al. 2007) but rarely consider whether such agreements impact the occupational trajectories of those who nonetheless left their employers. Stuart and Sorenson (2003) found a connection between non-compete enforcement and the founding of new biotech firms following IPOs and acquisitions, presumably because non-competes blocked executives and key technical personnel from leaving to start new companies in the same industry. But their firm-level data did not allow them to examine the individual mechanisms at play: whether individuals simply decided not to leave their firms, or whether they left but were not able to continue to work in the same industry once they had left. As such, the question of whether non-competes trigger occupational changes for ex-employees remains unexplored.

Herbert Parnes (1954) may have been the first to address the phenomenon of occupational change, defining it as "when workers not only change employers but also change tasks." Early work on occupational change was performed largely by

[9] Garmaise (2009) notes the percentage of companies in the Execucomp survey that report requiring non-compete agreements of their executives but does not use this information in his analysis.

psychologists, who emphasized dispositional determinants. For example, Holland (1985) posited that those who decide to change their occupations often do so because they seek better congruence or fit with their personality structure. Dissatisfaction with compensation or breadth of responsibility (Mihal, Sorce, and Comte 1984) have also been cited as drivers of occupational change. Demographics have been found to be key explanatory variables of occupational change, as have prior occupational choices. Younger workers have been found to be more likely to change occupations (Markey and Parks 1989, Harper and Haq 1997). Education raises the likelihood of an occupational change due to increased human capital (Sicherman and Galor 1990). Workers who specialize in a given field by accumulating specific human capital appear less likely to change occupations (Rosen 1972; Topel 1991; Baker and Aldrich 1996).

A tacit assumption in this literature is that occupational change is a voluntary act, evident both in the focus on individual attributes and choices as determinants of occupational change as well as in the generally-positive view scholars seem to have taken regarding the outcome of occupational change. For example, Ibarra (2002) defines occupational change as "a move into a position of greater managerial responsibility and organizational status." Sicherman and Galor (1990), while admitting it is possible for someone to accept a lower level occupation, likewise characterize occupational change as moving to higher-level occupations (see also Markey and Parks 1989). Yet, to the extent that non-compete agreements are enforced by the state—or to the extent that individuals believe such contracts are enforceable—occupational change may be triggered involuntarily and with less favorable consequences than the existing literature has suggested. The remainder of the paper marshals qualitative data to identify how non-compete agreements might shape occupational trajectories for ex-employees.

METHODS

Both the lack of grounded research on non-competes as well as the difficulty of collecting relevant data indicate that achieving methodological fit—"internal consistency among elements of a research project" (Edmondson and McManus 2007:1156)—is of particular importance. Prior work on non-competes contains some provisional models and tests hypotheses using large-sample data yet leaves key mechanisms underspecified, especially regarding how non-competes affect ex-employees. Thus I conduct an exploratory, inductive study relying on field data. First, I attempt to bootstrap the grounded-theory process (Glaser and Strauss 1967) with a pilot study of individuals whose occupational trajectories were impacted by non-competes. Second, I refine the resulting constructs in a second step using several dozen interviews drawn from a random sample of patent holders in a single industry. Third, I employ a survey instrument for a variety of industries and a larger sample size.

Theoretical-sample interviews

In an initial stage, I interviewed thirteen individuals whose occupational trajectories had been affected by non-competes. Fearing that such might be a rare event, I began by theoretically sampling selecting for such cases (Glaser and Strauss 1967).

Some referrals came via the author of a December 2007 *Boston Globe* column about non-competes, who received e-mails from seven readers interested in sharing their experiences. With their permission and his introduction, I interviewed them as well as six personal contacts (including contacts of theirs whom I did not previously know) who had likewise been affected by non-compete agreements. Ten of the interviews were conducted in person (in Massachusetts); interviewed by phone were two in Washington state and one in New Jersey.

The interviews in this sample were open-ended, lasting more than an hour on average and focusing on the experiences of interviewees with non-competes. I transcribed the interviews and coded them using Atlas.ti software. I began by performing "open" coding, categorizing portions of the interview transcript without a predetermined rubric. I then moved to categorization via "axial" coding, grouping related categories in order to arrive at the constructs described later in the paper (Strauss and Corbin 1999).

Random-sample interviews

The initial set of interviews yielded avenues for exploration, yet the decision to sample on the dependent variable in order to obtain relevant cases renders the results only minimally generalizable. Thus I constructed a random sample from a full population of knowledge workers for a single industry, not knowing in advance whether any of them had even signed a non-compete agreement. Eisenhardt (1989) advocates building a sample from a single industry in order to control for extraneous variation. I drew my sample from the automatic speech recognition (ASR) industry, for three reasons. First, ASR is a high technology industry, with intellectual property protection playing a critical role in establishing competitive advantage. Second, many ASR inventors obtained a Ph.D. in speech recognition or a related field and as such developed skills that are specific to ASR and not easily transferable to other fields. Third, I hoped that my own professional experience in the industry would enable me to understand the dynamics of the industry (for a description of the ASR industry, see Marx 2009.)

However, I did not want to take advantage of my professional contacts in order to build a convenience sample (Weiss 1994). Instead, I identified the population of inventors in the ASR industry using data from the U.S. patent and trademark office.[10] Although not every ASR scientist or engineer has filed a patent, those who have been awarded patents are by definition involved with the creation of intellectual property. Indeed, these workers may be among those whose departure firms may be particularly eager to prevent by means of a non-compete. To assemble the population of these inventors, I first created a list of firms active in the ASR industry from 1952 through 2006 by reviewing seven ASR trade journals as well as historical documents chronicling the inception of the industry. This yielded a list of 595 firms, 454 of which were US-based. For these firms, I extracted all of their patents in the ASR-related technical classes: 704, 379/88, and 371/42-44. This resulted in a list of 3,108 patents, from which I extracted a list of unique inventors. As patents do not contain a social security number or other unique identifier for each inventor, it was necessary to match inventor names across

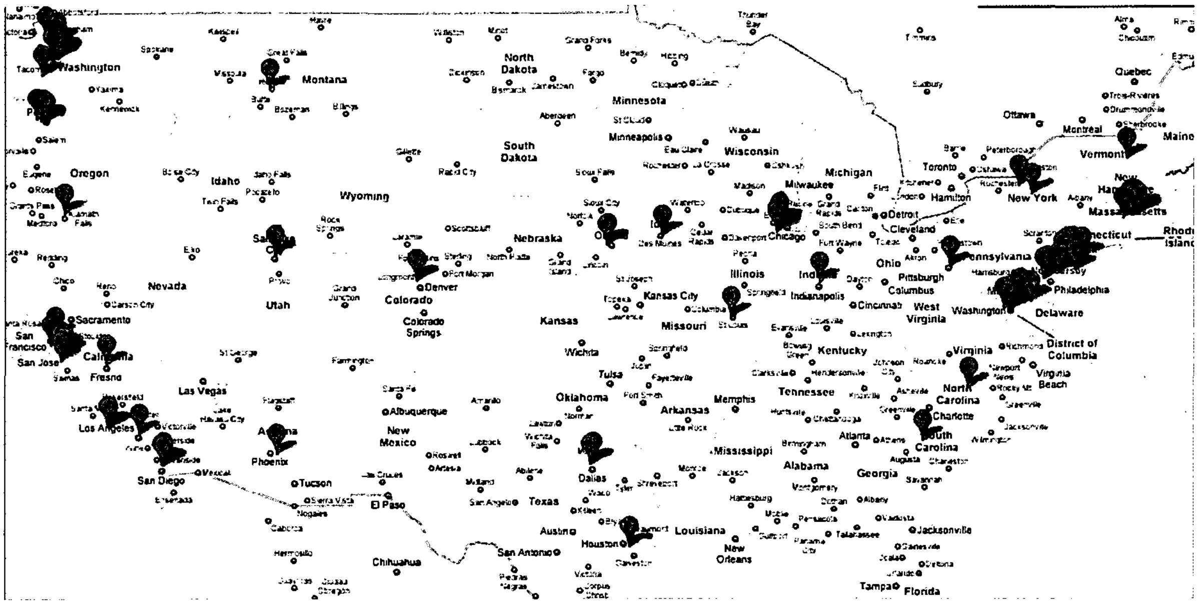
[10] ASR is a worldwide industry, but I restricted my sample to the U.S.-based inventors both for convenience and cost reasons in data-gathering as well as to control for variation in federal legal regimes.

patents. Simple name matching yielded 1,459 inventors.[11] Desiring to focus on inventors for whom ASR represented a primary focus, I retained only inventors who had at least two patents in one or more of the ASR-related technical classes. The final list contained 550 inventors.

To build a sample from this population, I assigned each inventor a random number between zero and one and then sorted the list by the assigned number. One advantage of constructing the list after this fashion is that the first *n* inventors in the randomly-ordered list constitutes a random sample. I then worked my way from the top of the list down, attempting to contact each inventor and request an interview. As some inventors' most recent patent was in the mid-1970s, contacting them proved nontrivial. I utilized a number databases of public information in order to locate the inventors, including Yahoo! People (white pages lookup), Google Search, Intelius, and LinkedIn. In most cases I contacted inventors by phone, though I introduced myself by email when available. In order to reach 60 ASR inventors (Weiss 1994), I found it necessary to attempt to contact the first 107 in the randomly-ordered list, a success rate of 56.1%. A comparison of those I was able to reach and those I did not reach along characteristics available from the patent records revealed few differences in geography or the date of their most recent patent; however, those I reached tended to have more patents than otherwise. Figure 5 depicts the geographical distribution of the ASR inventors.

[11] I checked only for a match of last names and first names between two patents, and for the absence of any difference in the middle name (middle names are often abbreviated with an initial or missing entirely). Much more sophisticated name matching algorithms are available (Trajtenberg 2006; Fleming, King, and Juda 2007; Marx et al. forthcoming), but these are designed to match inventors in the entire patent database. By comparison, I am matching within the small set of technical classes, so the exercise is considerably simplified.

Figure 5: Geographic distribution of randomly-sampled inventors from the automatic speech recognition since 1970 that were invited to participate in study.



Eight of the 60 declined to be interviewed, leaving a total of 52. Whenever practical, I conducted interviews in person, but several inventors preferred to speak by phone and two requested an e-mail interview. Those inventors in areas with low concentrations of ASR inventors were not invited to speak in person given the cost and time associated with travel. Still, I conducted 21 of the interviews face to face, including trips to Seattle WA, San Jose CA, Santa Barbara CA, Phoenix AZ, Philadelphia PA, Pittsburgh PA, and Basking Ridge NJ. The semi-structured interview protocol was approved by the Harvard Business School Internal Review Board, as was the use of oral consent. Interviews lasted approximately 40 minutes on average. I asked permission to take written notes as well as to record the conversation for later transcription. All but three interviewees allowed me to record our conversation, generating 508 single-spaced pages of transcripts.

In conducting interviews, I followed a structured protocol (see Appendix D) to the extent practical. I typically began by asking how the interviewee got started in the ASR industry and then reviewed their sequence of employment from firm to firm. I asked what precipitated the departure from each employer and whether their subsequent job was still in the ASR industry. If the subsequent employer was not in the ASR industry, I asked why they chose to leave the industry. If the subsequent job was in the ASR industry, I asked whether their prior employer had any objection to them joining a competitor. If they indicated not, I asked whether they had signed a non-compete at the prior employer.

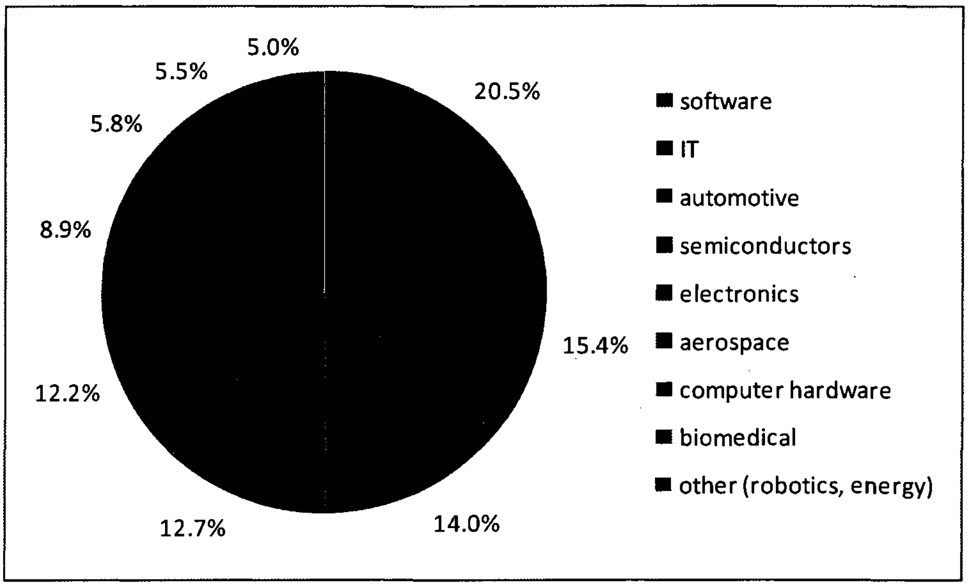
Following Strauss and Corbin (1999), I selectively coded the interview transcripts for the constructs identified during the open and axial coding of the theoretically-sampled interviews while remaining open to new insights from the random-sample interviews. As the circumstances surrounding a non-compete may vary by job, I chose not the person but the person-job dyad as the unit of analysis. For six of the person-job dyads, the interviewee could not remember whether or not the employer had included a non-compete in the employment contract. These were discarded, leaving 110 dyads for analysis. I assessed the reliability of my coding by having a second coder, a graduate student previously unacquainted with me, code a 20% randomly-selected subsample of the person-job dyads. Cohen's kappa (Cohen 1968) ranged from 0.75 to 1.0 for various indicators, with a mean of 0.877; each disagreement was subsequently resolved via discussion.

Cross-industry survey

While the selection of a single industry for the field interviews may have helped to control for extraneous variation, doing so leaves in question is how broadly the findings can be generalized. Thus I approached the Institute of Electrical and Electronics Engineers (IEEE), a nonprofit professional association founded in 1884 and with more than 375,000 members in 160 countries. Approximately 215,000 of those numbers are in the United States. The survey instrument (Appendix E) was developed in collaboration with IEEE staff and was also approved by the Harvard Business School Internal Review Board. As the survey was limited to 20 questions, it was not possible to gather complete career histories as was done in the interviews. Instead, respondents were asked to describe the circumstances surrounding the most recent non-compete they were asked to sign (if any, during the past 10 years).

Invitations to participate in the survey were sent by e-mail to 5,000 randomly selected IEEE members in the U.S., excluding government and military employees as well as students. Filling out the survey was strictly voluntary, with no incentive (penalty) for (non)participation. The invitation e-mail included a unique URL for each IEEE member in order to prevent individuals from filling out the survey more than once. Identifiers were removed by IEEE before the survey results were delivered to me, and the age and gender (if known) for each respondent were added from the IEEE membership database. The response rate was 20.6%, yielding 1029 usable survey responses. Figure 6 shows the distribution of respondents by industry.

Figure 6: Distribution of IEEE survey respondents by industry, *n*=1029.



The relative consistency between the ASR interview data and survey results, shown in Table 5, suggests that ASR is at least in some respects representative of other technical industries. The percentages of interviewees vs. respondents who were female, specialists, and founder/CEO were not statistically distinguishable. The same was true for the percentage who signed the non-compete when asked, and who left their employer after signing the agreement. (Age, length of the contract, when the non-compete was presented, and whether a lawyer was consulted were not collected reliably during the interviews.) The one notable difference was the percentage asked to sign a non-compete. The 37.2% for the ASR interviews is lower than the 46.8% in the survey but may partially be explained by two factors. First, many ASR companies are concentrated in Silicon Valley—approximately one-third of the person-job dyads were located in California—which might lower the percentage as California companies often eschew such contracts given that they are non-enforceable in the state.[12] Second, eight of the person-job dyads occurred at AT&T Bell Laboratories, one of the two ASR pioneers (along with IBM T.J. Watson Laboratories; see Marx 2009 for a full overview of the industry). Multiple interviewees noted that Bell Labs did not require non-competes, even though located in New Jersey, an enforcing state, due to its unique charter. Said one Bell Labs veteran, *"There was a 1956 consent decree: Bell Labs could not make money from its research, except to the extent it helped build the Bell System. So there was absolutely no economic incentive for them to prevent people from doing that stuff outside Bell Labs."*

Table 5: Comparison of descriptive statistics from the ASR interviews and IEEE survey. Statistical significance at the 5% level is indicated by '*'.

	ASR interviews	IEEE survey	Difference
n	110 usable dyads	1029 responses	
% female	9.6%	6.8%	-ns-
% specialist	51.7%	59.1%	-ns-
% founder/CEO	5.0%	7.7%	-ns-
% asked to sign	37.2%	46.8%	*
% of those, who signed	97.5%	92.6%	-ns-
% of those, who later left	60.1%	61.9%	-ns-

[12] In the survey, location was collected only for those who had left their employer after signing a non-compete and was thus not included in the table. If included, it would suggest that California was overrepresented among ASR firms in the interviews, 37.2% vs. 20.6% in the survey, also helping to explain why a lower percentage of those working at ASR firms were asked to sign.

RESULTS

The field data reveal four responses used by individuals who were prevented from practicing their profession by non-competes. Those who complied with the agreement did so by not seeking employment in the same industry, either by taking *occupational detours* by switching to a different field, or via *unpaid sabbaticals* in that they accepted unemployment for the period of the agreement. However, several of those for whom not working in their chosen field would be particularly costly chose not to honor the agreement—either by *seeking shelter* with a large firm that could reliably promise to indemnify them against a potential lawsuit, or if continuing with a smaller firm, *lying low* to avoid detection by their former employer.

Occupational Detours

Those who complied with the non-compete after resigning did so by not immediately working at a firm that competed with their former employer. For most, this involved taking an *occupational detour:* finding a job at a firm in a different industry for at least the duration of the agreement. Such detours were embarked upon reluctantly—particularly by those whose skills difficult to transfer to other industries—and held negative implications for their compensation, continued development of their expertise, and the maintenance of their professional networks. In some cases, the decision to follow this path was driven directly by the threat of a lawsuit. For example, a recent college graduate with a degree in private aviation management found it necessary to take an occupational detour after he was sued for violating a non-compete he had signed. He had become dissatisfied with his compensation and left the company in hopes of obtaining a more attractive offer elsewhere, but was ultimately left with a less-well paying job outside of his chosen field.

> *"It was week away from college. I would sign anything—I would sign my life away. You don't think of those things when you're interviewing for the position. All you can think of is becoming the CEO of the company in ten years and staying with that company forever. And then reality sets in and you're underpaid and there are other companies out there. [But w]hen we got a cease-and-desist we moved to find a job outside of aviation. We didn't want to go to court; we didn't want to pay the court fees. [My new job] is like an entry level sales position, probably 30 to 40% pay cut."*

Similarly, a speech-recognition scientist recounted being reminded of his non-compete obligations by his former employer. Upon leaving, he joined a firm in a related industry but eventually became frustrated at his inability to utilize the specialized ASR skills he had developed during his Ph.D. studies. *"I decided to go back and work on the core algorithms,"* he said, *"[but] when I interviewed with <ASR company> my prior employer said you-can't-do-that. It had only been one and a half years since I had left, and my agreement was two years. So, I ended up rejoining my former employer."*

In other cases, ex-employees proactively decided to take occupational detours in order to comply with the non-compete, not in response to a direct threat from their former employer but rather based on the expectation that the agreement would be enforceable. Another speech recognition scientist with a Ph.D. in the field from a top university had joined an ASR startup as an early employee, but was laid off following a disagreement with the founder. Rather than attempt to work in the same field, he left the ASR industry entirely in order to avoid infringing on the agreement he had signed:

"I had a very strong anti-competition agreement with <former employer>...so for two years I couldn't have gotten involved in another speech recognition company in any case. I suppose I could have fought it, but it was at least a concern. The employees were very much aware of these non-competition agreements. And many of them, certainly the more sophisticated ones, on a regular basis would sort of do a gut check and say, 'Well, if I'm ever gonna leave and there's gonna be two years when I'm not doing speech recognition work, what would I do for two years if I couldn't do speech recognition?'"

A similar approach was taken by an engineering manager at another ASR company, who also explicitly ruled out working at companies within the same industry: *"I purposely looked for non-speech companies because of the non-compete. In fact I was recruited by some speech companies that I didn't even consider."* In fact, one-quarter of those in the random sample who left their employers after having signed a non-compete (six of 24) left the ASR field for at least the duration of the non-compete, taking an occupational detour.

Taking a detour negatively affected occupational trajectories in at least two ways. First, although those who changed fields looked for jobs in which they could utilize some of their skills, they inevitably lost the ability to develop and enhance expertise specific to the industry they had left in order to comply with the non-compete. Of survey respondents who complied with the non-compete by leaving their industry, 28.4% claimed that their skills atrophied due to the non-compete as compared with 16.8% of those who did not comply ($p<0.023$). Moreover, the non-compete restricted the use not just of training provided by the employer but also expertise developed prior to joining that firm, including educational experience. Observed one ASR industry veteran,

"I've been in this industry for 20 years. I have a PhD in the field. I walked in the door with an enormous amount of experience, and while I worked there for a year in a half

they added maybe, what, 2% to that? And now they want to prevent me from working in speech and using any of what I know? That's not right."

Second, the inability to exercise their existing skills led to them to take jobs with compensation lower than they could earn if able to continue to work in their chosen field. *"I intentionally looked for general-purpose programming, and I took a substantial pay cut to go there. And I ended up staying for a couple years, you know, once you get into something"* recalled a principal scientist who, although not formally threatened by her former employer, avoided legal entanglement by taking an occupational detour. In the survey, 13.7% of those who honored the non-compete reported that their compensation was negatively affected as compared with 4.7% of those who violated the non-compete ($p<0.007$). Both of these consequences were amplified for those with deep expertise that would be difficult to transfer to other industries. An ASR engineer who was careful only to take jobs for which he would not be required to sign a non-compete, underscored the costs of taking an occupational detour: *"the only thing I'm not flexible on is that I want to stay in speech and I intend to die in it. That's what I'm good at. If I switched I'd be starting over. I'd take a pay cut and I'd be starting as a nobody."*

Unpaid sabbaticals

In a few cases, instead of taking an occupational detour an individual did not seek paid employment for the duration of the non-compete. The option to embark on an *unpaid sabbatical* was facilitated by personal financial resources. For example, an independently wealthy executive who became dissatisfied following an acquisition resigned from the company and *"went to [university] for a year and worked there [unpaid] for a* *year because of the non-compete. I certainly chose not to try to fight the non-compete. Just for clarity's sake I didn't want to have any question around it."* Similarly, an engineer who left his employer while subject to a non-compete described how he was able to avoid an occupational detour: *"well, we had stock options, so that gave us cash to sit on, so we were in a reasonably good position. We didn't have to have a job lined up the next day."* While some simply relied on reserves to "wait out" the period of the non-compete, others took advantage of their resources in order to reduce their exposure to such restrictions in the future. An independent contractor found that the non-competes he was asked to sign routinely outlasted the consulting engagements themselves. The frequent occupational detours or spans of unemployment led him to abandon his consulting practice, which was only possible because he could self-fund the transition:

> *"I've taken a bit hit in cash flow. I have a lot of open space on my calendar. [I'm] converting my business from sale of service to sale of product to try to avoid the contract altogether. But I'm single; I don't have kids; I don't have a mortgage, so I have a large slush fund I can live off of."*

Given the compensation and skills-utilization concerns associated with honoring the non-compete, it is perhaps little surprise that many ex-employees attempted to avoid the post-employment restrictions imposed by the agreement. Moreover, many interviewees felt that they had signed the non-compete under duress and without prior notice. As shown in Table 6, the non-competes of survey respondents were included with the employment offer less than one third of the time, and nearly one-quarter of the time on the first day of work. Said one such worker, *"No, I never received any information ahead of time before showing up to my first day. And then it was the first day when I had all the paperwork in front of me: health insurance, 401(k), and the non-compete. It was either 'sign it and work here or don't sign it and don't work here.'"*

Another worker described a similar experience: *"in the 11th hour they just tried to bully me into signing it."* Further, some non-competes restricted the occupational trajectories of ex-employees for an extended period of time. As shown in Table 7, more than one-third of non-competes restrict the ability of an ex-employee to work at a competitor for more than one year after leaving the firm, and nearly 15% are greater than two years in length.

Table 6: Timing of non-compete requests, for those who signed in the IEEE survey. *n*=453.

When non-compete was requested	**%**	**Cum %**
At the time of the offer	30.5%	30.54%
After the offer was accepted, but before the first day at the company	22.27%	52.71%
On the first day at the company	24.43%	77.14%
After the first day at the company	22.85%	100.00%

Table 7: Lengths of non-competes individuals were asked to sign in IEEE survey. *n*=1029.

Length of non-compete	**%**	**Cum. %**
Only while working at the company	17.9%	17.9%
Up to one year after leaving the company	47.1%	65.0%
More than one year but no more than two years after leaving the company	20.7%	85.6%
More than two years but no more than five years after leaving the company	11.8%	97.4%
More than five years after leaving the company	2.6%	100.0%

Seeking Shelter

Some who continued to work in the same field attempted to avoid litigation by selecting a subsequent employer with sufficient resources to deter legal action by the former employer—i.e. by eschewing small firms in favor of larger ones. They expressed concern both regarding the possibility of litigation from their former employer as well as the non-compete they might be asked to sign at their subsequent employer. Several interviewees expressed concern that a lawsuit could be more damaging if they subsequently joined a small firm. Said one ex-employee: *"I consciously excluded small companies because I felt I couldn't burden them with the risk of being sued. [They] wouldn't necessarily be able to survive the lawsuit whereas a larger company would."* Others saw larger firms as more able to indemnify them against a potential lawsuit from their former employer. A sales professional who was forced to quit a 50-person startup after being sued for violating his non-compete agreement was persuaded to join a 30,000-person company also competitive with the original employer with the following assurance,

> *"'Let's just go, and if they sue you we'll take care of it.' Their legal opinion was they did not see <former employer> as a competitor and they were prepared to move forward with the offer and they would defend that position. I'm wondering if <former employer> felt they could intimidate <small company> in a way that they felt they couldn't intimidate <large firm >."*

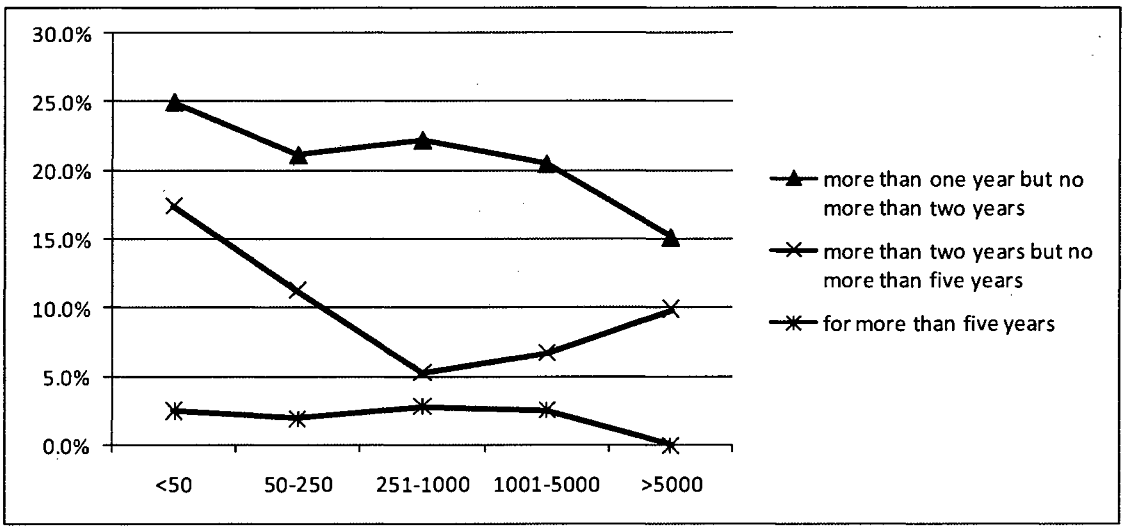
Interviewees indicated that joining a large firm would lessen the likelihood of a lawsuit not only thanks to its ability to handle litigation, as described above, but also because it could provide an occupational detour within the firm. *"At <large firm >it would be easy for them to find another job for me outside speech for a year,"* reasoned an ex-employee who explored how he might continue working in the ASR industry despite his non-compete. By comparison, a small firm would lack either the organizational slack or the diversity of operations necessary to enable someone to work in a different area for the duration of the non-compete and then return to their field of expertise. Another engineer, who along with co-workers joined a 10,000-employee software company, described how his new employer provided an occupational detour within the firm: *"Many of us were protected by giving us assignments that have absolutely nothing to do with speech recognition."*

In addition to the protection a large firm could afford against a lawsuit against existing non-competes, some interviewees saw advantages in how large companies might handle future non-competes. Even though one could imagine that larger firms with more market power in greater financial or legal resources might be more likely to engage in litigation, non-executives in particular felt that they would be less of a target if they worked at a large company. Said one engineer, *"I think for the majority of people I'd expect a non-compete is not worth it for <large firm> to try and spend their time trying to sue you over it."* Others felt that the standardized HR practices in large companies might leave them less vulnerable to arbitrary treatment than could be the case in a smaller firm:

> *"In the case of [large firm] I wouldn't say that an individual hiring manager would have any say in how legal issues are dealt with or enforced. But in a small company, a senior person in a small company could have a lot of say over the legal counsel at least with his intent. That's why I think I felt comfortable with the non-compete from [large firm]. They don't need the bad press."*

Ex-employees may also have avoided small companies in order to avoid being required to sign unusually long non-competes. Although one might suspect that larger firms could be inclined to require more punitive non-compete agreements, the survey data suggests the opposite. For those who signed a non-compete and subsequently left their employer, Figure 7 displays the extent to which firms of varying sizes utilize non-competes longer than one year (approximately one-third of all such agreements). Smaller firms—and, in particular, those with fewer than 250 employees— appeared more likely to use non-competes that extended two years or longer (significance at p<0.0371).

Figure 7: Length of non-competes signed at firms of various sizes in the IEEE survey. *n*=281.



Lying Low

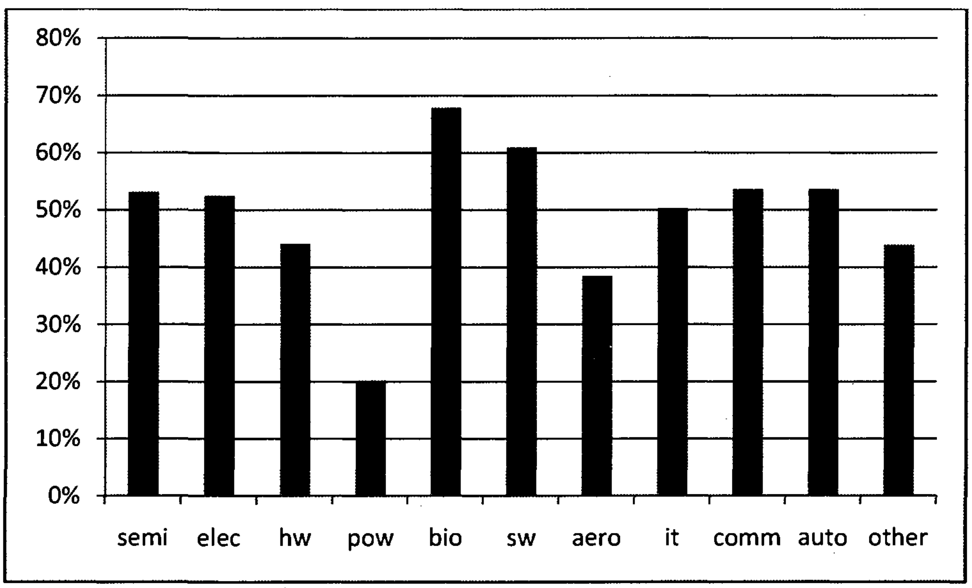
Not all ex-employees who decided not to comply with the non-compete joined large firms. Those who took jobs with small companies also took steps to prevent their prior employer from knowing that they were competing with them, with negative implications both for the strength of their professional ties and the growth of their firms. One engineer, whose former boss called her almost weekly after she resigned to ensure that she was not working for a competitor, avoided detection as an independent consultant by lowering her profile. *"These were really piddly jobs, I mean, I was taking on several* *really small insurance companies – places where nobody would run into me – and the kinds of things I tended to do were very short term."* Limiting the size of clients she would undertake in order to avoid a potential lawsuit regarding the non-compete slowed the growth of her consultancy. Similarly, a would-be entrepreneur delayed founding his new firm until the day after his non-compete expired. Professional ties were weakened for those who did not continue to work in their industry openly (e.g., by joining a large firm). *"People would quit and not say where they were going, so I lost touch with a lot of colleagues in my field,"* recounted an ASR engineer. Another scientist, who had joined a small startup that directly competed with his former firm, avoided attending professional conferences or having conversations with colleagues outside the company:

> *"We were hiding very low. [Current employer] had an automated – something where you could dial people's names, and we were not in that system because they didn't want [former employer] to find out who was actually working at [current employer]. I think you could dial XXX and then our names, and you could get to us. And if we ran into people we knew who were still at [former employer], we'd like hem and haw and say, 'well, I don't really want to tell you where I'm working right now.'"*

DISCUSSION

While the field data offer deep insights into the mechanisms underlying how non-competes shape occupational trajectories, the findings should be interpreted cautiously for several reasons. First, the bulk of the interviews are drawn from a single industry, and while a comparison of the interview and survey data suggests that the automatic speech recognition industry resembles other technical fields in several respects, ASR may differ from other settings in consequential aspects. In particular, the degree of specialization required by many engineers in the ASR industry may exaggerate the effects of non-competes, as specialization was shown in the prior chapter to exacerbate the effects of non-competes. Moreover, as shown in Figure 8, software firms in the survey asked employees to sign non-competes more aggressively than in most other industries.[13] Thus these results should not be taken as indicators of frequency. The constructs derived from these data should be best viewed as propositions, the frequency or magnitude of which can best be assessed by larger-sample hypothesis testing. Nonetheless, this study establishes mechanisms, grounded in field data, by which non-competes affect the postemployment occupational choices of ex-employees.

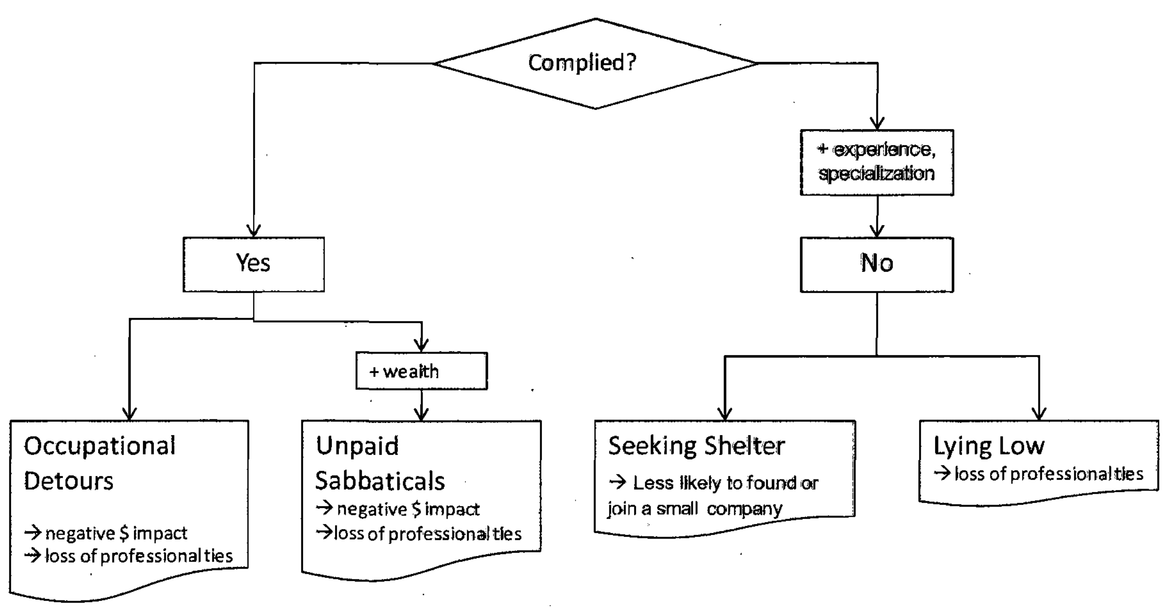
Figure 8: Percentage of individuals in the IEEE survey asked to sign a non-compete, by industry. *n*=1029.



[13] Software firms may be particularly eager to implement non-competes in order to protect tacit knowledge that may be difficult to codify formally as with patents. That said, non-competes do not appear to be substitutes for patenting. Biotech, in which non-competes are used even more aggressively than software, has repeatedly been cited as an industry for whom knowledge is more easily codifiable and thus for whom patent protection is more effective than for industries such as software where knowledge is more tacit (Cohen, Nelson, and Walsh 2000; Ziedonis 2000; Cockburn and Henderson 2003). This notion is also supported by the finding in the first chapter that patenting rates in Michigan changed little following its 1985 reversal of non-compete enforcement policy, when compared with other states that continued not to enforce non-competes.

The flowchart in Figure 9 summarizes the findings of regarding the implications of non-competes for ex-employees. Those who complied with the non-compete did so by ceasing to work in their existing field, with negative implications for their professional advancement, ties to colleagues, and compensation. Of those, the few with substantial wealth took *unpaid sabbaticals* in that they did not work for pay, but most workers found it necessary to take *occupational detours* by working in a different field for at least the duration of the non-compete. Workers who were more experienced and more highly specialized were less likely to comply with non-competes, *seeking shelter* with a large company that could indemnify them against potential litigation or *lying low* as they continued to work at a smaller firm. Joining a large company appeared to be the only method of keeping ties with professional colleagues, regardless of whether one honored or violated the non-compete.

Figure 9: Depiction of findings regarding the impact of non-competes on ex-employees.

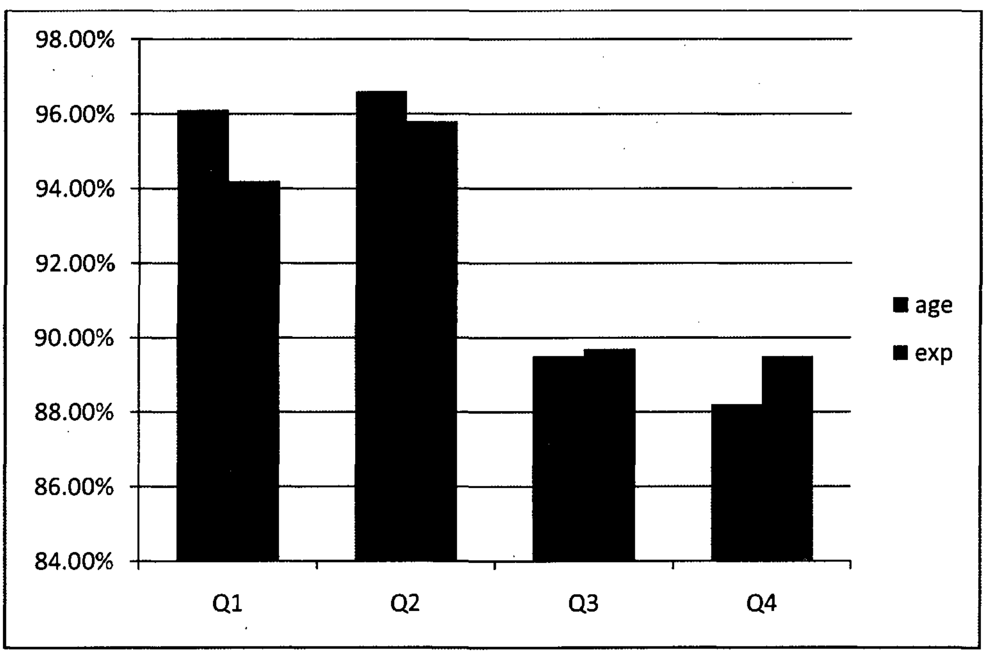


Theoretical implications

These findings also contribute to our understanding more generally of how firm-level regulations "trickle down" to individuals. The enforcement of non-competes by the state—or even the perception that such contracts are enforceable—leads individuals to undertake occupational changes at considerable professional cost. Those who comply with the non-compete either change industries or cease working entirely, with negative financial and professional implications and in stark contrast to the picture generally painted by scholars of occupational change. The consequences of occupational change for those subject to non-competes resemble more closely those of displaced workers, whom Fallick (1996) defines as those with a limited ability to return to a comparable job following an involuntary, structurally-driven separation. Disproportionately concentrated in low-skilled industries experiencing declining demand, such as manufacturing, mining, and construction, displaced workers tend to be strongly attached to the sector in which they were employed and experience gaps in employment as well as reduced wages due to the inability to utilize their skills. While the outcomes of occupational changes effected by non-competes resemble those of displaced workers, the characteristics of those subject to non-competes often do not. Rather, in many cases Ph.Ds and senior executives in growth industries exit their profession for a period of months or years in order to satisfy a non-compete agreement.

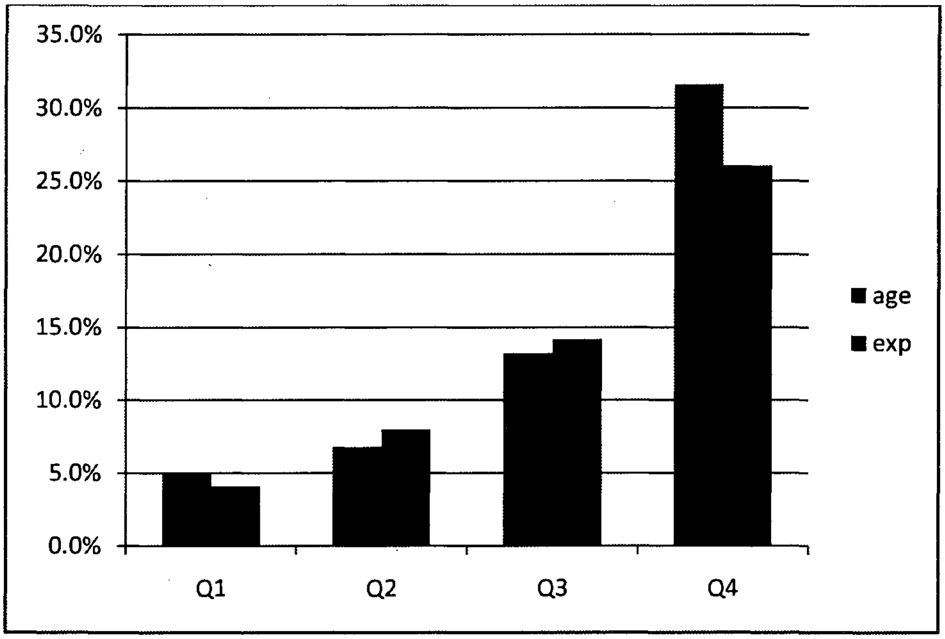
These results also suggest boundary conditions for "typecasting" theory (Zuckerman et al. 2003), which recommends that individuals specialize in early career. In their study of the feature-film labor market, Zuckerman et al. (2003) found that allowing oneself to become typecast by playing a narrow set of roles predicted success in early career, as individuals gain credibility by developing specialized expertise. Yet these results would suggest a possible risk of becoming typecast or specialized for those employed in labor markets—unlike filmmaking or academia—in which non-competes constrain extraorganizational opportunities. Ironically, this risk may be particularly acute in early career, when the benefit of typecasting is purported to be greatest. Indeed, the survey revealed that young, inexperienced workers are less likely to refuse to sign non-competes, as shown in Figure 10.[14] Younger, less experienced workers who signed a non-compete also consulted a lawyer less often before doing so; Figure 11illustrates this difference for quartiles of both age and experience.

Figure 10: Percentage of individuals in the IEEE survey who signed non-competes when asked, by quartiles of age and experience. ***n*=1029.**



[14] The survey measures of age and experience are correlated at r=0.77.

Figure 11: Whether a respondent to the IEEE survey who signed a non-compete had sought legal advice prior to doing so, by age and experience quartiles. ***n*=453.**



This work also adds to our understanding of how institutions influence entrepreneurship. Stuart and Sorenson (2003) demonstrated that non-competes discourage the founding of new firms; this work suggests that non-competes also discourage the *growth* of small firms as ex-employees with relevant skills are redirected to large companies. Moreover, these data illuminated four mechanisms by which non-competes discourage participation in entrepreneurial ventures, both for those who honor the agreement as well as those who choose to violate it. First, non-competes prevent the deployment of relevant expertise by forcing ex-employees to take "occupational detours." Several scholars have noted the importance of within-industry expertise in facilitating the growth of entrepreneurial ventures (Carroll, Bigelow, Seidel, and Tsai 1996; Klepper and Sleeper, 2005; Haveman and Cohen, 1994). But if workers—in particular, workers with deep expertise in a particular field—change fields when changing jobs due to a non-compete, they will be unable either to found or join a new venture in that same industry. Second, even if workers avoid switching to new fields and can afford to finance an "unpaid sabbatical," their plans to found a new venture will be delayed for the duration of the non-compete. Third, those who "seek shelter" from potential litigation by joining a large companies by definition shun small firms. Fourth, even if workers "lie low" in order to continue working in a smaller company, they may find it more difficult to grow the new venture while keeping a low profile.

Practical implications

Aside from the implications for individual occupational flexibility described above, this study is also of value to managers and policymakers. Managers choosing where to locate a new plant, or entrepreneurs looking to found a new venture, should consider the enforceability of non-competes in various regions lest they find it more difficult to attract talent with relevant skills. While these advantages must of course be offset by the potential advantage of more easily retaining workers, the recruiting advantages may be of particular benefit to young organizations.

Those whose locations are already fixed in enforcing regions may nonetheless benefit from reconsidering the use of non-competes. To the extent that workers are (or become) aware of the ways in which non-competes can constrain their future occupational choices, they may become less eager to sign such agreements. If so, hiring managers may be able to gain recruiting advantage by eschewing non-competes. Observed one founder, *"if you look at the tradeoff between the times you could enforce these vs. the* *benefit of being able to say we don't have non-competes then we'd be better off not to have them."* Even if firms continue to use non-competes, they may nonetheless manage to maintain a recruiting advantage by enforcing them only loosely.

These data may also be of use to policymakers attempting to weigh the costs and benefits of enforcing non-competes. During 2008 alone, New York, Oregon, Idaho, and Louisiana reformed their laws regarding non-competes, with the two former states restricting the ability of firms to implement such agreements while the latter two states extended such powers. That legislators have come to such different conclusions regarding the appropriate treatment of non-competes suggests a dearth of guidance regarding the implications of non-competes. Thus legislators considering the degree to which non-competes should be enforced in their jurisdictions will find these results instructive, as they suggests that non-competes affect not only individual careers but also additional outcomes of interest including participation in entrepreneurial ventures and the utilization of technical expertise. Moreover, if individuals forsee difficulties in exercising specialized skills due to non-competes, will they be as willing to invest to develop such expertise? Will regions that tightly enforce such agreements thus experience a "brain drain" of talent to non-enforcing regions as workers become aware of the limitations on their occupational flexibility?

4: NON-COMPETES, TECHNICAL EXPERTISE, AND STAFFING SMALL FIRMS

ABSTRACT

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

INTRODUCTION

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

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

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

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

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

Inventors may choose to protect their work in a variety of ways, including patents, copyrights, trademarks, or trade secrets. Of these, patents are by far the most thoroughly researched, with hundreds of articles by economists and other social scientists. Given that a primary motivation for the patent system is to provide incentives for innovation (Schumpeter 1942; Teece 1986), many researchers have examined the optimal design of the patent system, including breadth of scope (Klemperer 1990; Gallini 1992; Lerner 1994) and duration of protection (Nordhaus 1969; Kamien and Schwartz 1974; Gilbert and Shapiro 1990). Others have assessed the connection between patenting and R&D (Hall, Griliches and Hausman 1986; Mansfield 1986; Sakakibara and Branstetter 1999), particularly in the area of fostering cumulative innovation (Kitch 1977; Merges and Nelson 1990; Heller and Eisenberg 1999; Walsh, Arora, and Cohen 2003; Murray and Stern 2007). In addition to the promise of protecting innovation, scholars have established the threat of litigation as a primary motivation for patenting (Hall and Ziedonis 2001; Schankerman and Scotchmer 2001; Lanjouw and Schankerman 2001; Somaya 2003).

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

The paucity of research on trade secrets may be due in part to the difficulty of assembling data, as no national registry is available as with patents and trademarks. The challenge of tracking trade secrets extends to firms, as well. Although employees signing non-disclosure agreements promise not to divulge trade secrets to competitors, violations can be difficult to detect. Given the difficulty of policing trade secret disclosure, many firms adopt employee non-compete agreements, which place restraints on the ability of ex-employees to join a competitor or work in the same field for a specified period of time after resigning (Hyde 2003). Non-compete enforcement policy is regionally fragmented in the U.S., with several reforms in 2008 alone.[15] That various states continue to come to different conclusions regarding non-compete enforcement suggests a lack of agreement regarding the effect of non-competes on innovation and entrepreneurship.

Legal scholars have long considered the implications of employee non-compete agreements (Blake 1960; Valiulis 1985; Decker 1993). But it was not until Gilson's

[15] The states of Idaho (Id. SB1393) and Louisiana (La. R.S. 23:921) extended the ability of firms to enforce non-competes, while Oregon (Or. SB248) and New York (Ny. S02393) restricted their ability to do so.

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

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

HYPOTHESES

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

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

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

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

While most workers require some form of income, those with sufficient financial means may be able to "wait out" a non-compete by remaining unemployed for the duration of the agreement.[16] Alternatively, those who need to work but do not wish to change fields may take steps to avoid detection by former employers. In either scenario, one might expect a greater (apparent) interval between jobs among those for whom non-competes are enforced.

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

[16] For example, Microsoft executive Vic Gundotra chose not to contest his non-compete when leaving for Google. Instead, he decided to remain unemployed for one year, as described in Google's official statement: *"Mr. 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)

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

Several studies have established that the legal system involves asymmetric costs for small versus large firms. Lanjouw and Schankerman (2004) noted that small firms are disadvantaged when settling legal disputes regarding intellectual property because they lack large portfolios of protected intellectual property as well as in-house counsel. As evidence of this advantage, Lanjouw and Lerner (1996) found that firms with greater financial and legal resources often obtain preliminary injunctions against weaker firms in order to drive quick and favorable settlements. Even when explicit legal action is not taken, the perception of greater facility or resources may suffice: merely a "reputation for toughness" in patent litigation is more effective in stopping knowledge spillovers to firms that are small or young (Agarwal, Ganco, and Ziedonis 2008). Given these asymmetries, smaller companies may avoid legal confrontations. Koen's (1992) survey revealed that small companies were much more likely than large firms to consider the costs of potential litigation before deciding to initiate a particular type of R&D activity. Likewise, Lerner (1995) demonstrated that biotech firms with higher litigation costs (i.e., small firms) tend to avoid patenting in fields occupied by their competitors.

Given the costs associated with non-compete litigation,[17] similar asymmetries could lead a smaller firm to avoid hiring personnel in the same field who are subject to non-competes, lest it be drawn into a lengthy legal battle. Individuals perceiving such obstacles may likewise steer clear of small companies. Indeed, interviewees and survey respondents in the prior chapter felt that larger firms would be better able to indemnify them against potential non-compete lawsuits. They noted that large employers, owing to the diversity of their businesses and organizational slack, might more easily able to offer them non-infringing tasks to work on during the term of the non-compete but with the promise of returning to their field immediately after the restriction expires. Thus large companies also appeared more able to prevent such lawsuits in the first place. Taken together with the lower likelihood of smaller firms trying to hire individuals subject to non-competes, this reasoning suggests the following:

Hypothesis 2: *Ex-employees subject to non-competes will be less likely to join small firms when changing jobs.*

[17] For example, in 2005 Nortel Networks paid $11.5 million for the right to hire Motorola COO Mike Zafirovski as its CEO, who was subject to a non-compete agreement (McMillan 2005).

METHODS

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

Two additional pieces of evidence suggest that the non-compete enforcement policy reversal may indeed have been unintended. First, two years following MARA the Michigan legislature established the "reasonableness" doctrine—limiting the scope and duration of non-competes as is common in most states that enforce non-competes—possibly indicative that the repeal of the earlier ban had not been fully deliberated. Indeed, legislative analysis of the reasonableness doctrine states that its role was "to fill the statutory void" (Trim 1987). Second, Michigan labor lawyers active at the time of MARA and authors of relevant Michigan Bar Journal articles indicated that the reversal was unexpected. Louis Rabaut (2006)reported *"there wasn't an effort to repeal non-competes. We backed our way into it. All of a sudden the lawyers saw no proscription of non-competes...the legislature had to go back and clarify the law."* Robert Sikkel (2006), another Michigan-based lawyer active at the time of MARA, echoed Rabaut's view: *"It was really out of the blue. I have never been able to identify any awareness that this was a conscious or intentional act. I am unaware of anyone that lobbied for the change."*

The above evidence suggests that Michigan's change in enforcement was an exogenous event rather than an example of the legislature simply "catching up" with the courts or responding to lobbying efforts.[18] Michigan is the only state known to have clearly and inadvertently reversed its enforcement policy in the past century. As such, it is a candidate for use as a natural experiment in assessing the implications of non-compete agreements.

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

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

[18] Garmaise (2007) notes that 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 and thus cannot be said to be inadvertent or accidental. Garmaise argues that the policy change is nonetheless exogenous to workers who did not closely follow such deliberations. However, while changes in those states either tightened or loosened constraints on enforcement, none fully reversed the previous enforcement policy as was the case in Michigan.

prosecute such cases: "the bill would greatly enhance the effectiveness of antitrust law in Michigan by increasing the penalties for violation [and] would give Michigan courts access to the case law developed by the federal courts."

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

Data

Data are U.S. utility patents granted from 1963 through 2006, assembled from the NBER patent data file (Hall, Jaffe, and Trajtenberg 2001) and augmented via updates from the U.S. Patent and Trademark Office website. Although patent data have many weaknesses as economic indicators (Griliches 1991), I utilize them not to measure innovation but rather to establish occupational patterns over time. These data are attractive for this study as patent holders are sorts of workers whose knowledge of trade secrets firms try to protect using techniques including non-competes.[19]

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

Although attractive in several ways for this study, patent database also has limitations. Because inventors are not required to submit patents at regular intervals, work histories reconstructed from a trail of patents may contain gaps, perhaps even omitting an entire span of employment with a particular firm. I am more likely to detect employer changes for those who patent more frequently, which is controlled for in the models. Moreover, the exact timing of the move is not known; I use the midpoint

[19] If patents and non-competes were substitutes, it would seem inappropriate to use patent data to evaluate the impact of non-competes. Yet Marx et al. (forthcoming) found no drop (or increase) in the rate of patenting when Michigan began to enforce non-competes, suggesting that patents and non-competes are complements, not substitutes.

between the last patent at the former firm and the first patent at the subsequent firm. (Assuming the move occurred at the time of the last patent at the former firm strengthened the results, and assuming it occurred at the first patent at the new firm returned similar though weaker results.) A second limitation, common to most other studies of non-competes (Stuart and Sorenson 2003; Fallick et al. 2006) is that the patent database does not indicate which inventors signed a non-compete and which did not. As a result, findings of this study may understate the impact of non-compete agreements for those who sign them.

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

The population for this study is restricted to the 98,468 inventors who were granted at least one patent in Michigan or another state that did not enforce non-competes prior to MARA.[20] Michigan inventors represent the experimental group, with inventors in states that continued to proscribe non-compete enforcement as the control group. A total of 27,478 employer changes were found for these inventors, 3,307 of which were in Michigan. An employer change is defined as a pair of sequential patents for a single inventor where the assignee for the first patent is different than the assignee for the second patent. Because at least two patents are necessary to detect a move, inventors with only one patent are necessarily excluded from the analysis. Likewise, inventors with multiple patents all assigned to the same firm (i.e. who never changed jobs) are excluded. I take unassigned patents—those where the assignee is missing and thus assigned to the inventor—as indicators of self-employment, I excluding moves from self-employment to employment as individuals will likely not require themselves to sign a non-compete. Each employer change is an observation in this analysis, as both hypotheses are contingent on employees leaving their employer.

Variables

I operationalize the dependent variable for Hypothesis 1a by exploiting the classification of patents into technical fields by the U.S. Patent and Trademark Office. While there are hundreds of technical classes, the National Bureau of Economic Research has grouped these into two levels of categories (Hall, Jaffe, and Trajetenberg 2001).[21] Six top-level NBER categories correspond to broad sectors of the economy, whereas the 36

[20] States with specific legislation restricting enforcement of non-competes include Alaska, California, Connecticut, Minnesota, Montana, North Dakota, Nevada, Oklahoma, Washington, and West Virginia (Malsberger 1996).

[21] Appendix 1 of Hall, et al. (2001) lists the aggregation of individual patent classes into both subcategories and top-level categories.

NBER subcategories correspond more closely to individual industries. Each patent receives a primary technical classification (and may receive secondary classifications as well), which I use as an indicator of the field a given inventor was working in at the time. Of course an inventor may have multiple patents at a given employer, and these patents may have primary technical classifications in different subcategories, so I compare the mode (most frequent) subcategory for each of the two employers. If the two mode subcategories differ, the dependent variable is set to 1. If more than one mode is found, ties are broken by the highest-numbered subcategory (and, as a robustness check, by the lowest-numbered subcategory).

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

I proxy for the size of the firm subsequently joined—the dependent variable for Hypothesis 2—using a measure of its patenting frequency. One concern with this approach is that firms may not patent at the same rate each year, particularly small firms. Thus I employ a rolling three-year window of the number of patents granted; too long a window would lead younger or short-lived firms to appear smaller relative to those that were in business for many years. (In order to test the robustness of the three-year assumption, I also produce estimates using one- and five-year windows.) A second concern involves the distribution of firm sizes, particularly in Michigan, as spurious support for Hypothesis 2 could be found if it were the case that large firms dominated the

Michigan economy. To address this, I normalize the three-year patent count, dividing by the mean for all firms patenting that year in the same state.[22] For example, a firm that had seven patents in Michigan from 1980-1982 would have its count normalized by the average number of patents filed per firm in Michigan during the same period. Thus the dependent variable indicates the relative size of the firm joined by ex-employees when compared to other firms in the same state that they might have joined. Finally, the variable is logged in order to reduce skewness.

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

[22] Dividing by the median instead of the meeting yielded similar results, as did calculating the denominator for Michigan versus non-Michigan rather than for each individual state, or calculating the denominator for pre-MARA versus post-MARA instead of each individual year.

employee base and r=0.65 with logged revenue, both statistically significant at the 0.1% level.[23] The measures of employee base and revenue were themselves imperfectly correlated (r=0.91).

Explanatory Variable

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

Control Variables

Control variables include characteristics both of the employers (assignees) and the employees (inventors). As employer changes are detectable conditional on patenting, I control for an individual's propensity to patent using the (logged) number of days between patents as well as the (logged) number of patents in the pre-MARA period divided by the number of active patenting years. As some inventors moved to states that enforced non-competes, such as Massachusetts, a dummy variable accounts for these

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

observations. Another indicator captures whether the prior employer was a university, as universities are unlikely to ask non-competes of their students, faculty, or staff. Period effects are captured by annual indicators, and cohort effects are controlled for with a dummy variable for each inventor's first patenting year.

Given the role of the automobile industry in Michigan's economy, it is important to account for those inventors who leave jobs in the auto industry.[24] As noted by Singleton (1992), the 1980s were a decade of sharp fluctuations in the auto industry due to foreign competition as well as the oil shocks of the previous decade. Hence, those in the auto industry who changed jobs might be more likely than others to change fields. Moreover, if leaving large auto manufacturers they might have been likely to move to smaller firms. Either of these effects could confound the evaluation of the hypotheses. Thus I control not only for a patent being in the auto industry, but also for the interaction of that indicator with Michigan, post-MARA, and their interaction in order to account for temporal effects. (Omitting auto-related observations altogether did not substantially affect the results.)

For Hypothesis 1a, some individuals may simply be more prone to shift fields; thus a dummy variable captures whether a given inventor had previously changed fields when changing jobs. For both hypotheses, I include a measure for the size of the former employer, given Sorensen's (2007) finding that those who work for large firms are more likely to again join a large firm when changing jobs. Descriptive statistics are given in Table 8.

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[24] Auto patents are identified by assignee name according to Plunkett Research. Their list of auto-industry firms is at http://www.plunkettresearch.com/Industries/AutomobilesTrucks/AutomobilesandTrucksIndustryIndex/tabid/91/Default.aspx

Table 8: Descriptive statistics for inventors who were granted at least one patent in a non-enforcing state in 1985 or earlier and who changed employers. Employer change is determined by two subsequent patents of the same inventor having different assignees; *n*=27,478.

	Variable	Mean	Stdev	Min	Max	1)	2)	3)	4)	5)	6)	7)	8)	9)	10)	11)
1)	change in NBER subcategory	0.5336	0.4989	0.0000	1.0000	1.0000										
2)	post-MARA	0.6416	0.4795	0.0000	1.0000	-0.0688	1.0000									
3)	Michigan	0.1204	0.3254	0.0000	1.0000	0.0160	-0.0417	1.0000								
4)	inventor previously changed subcategory	0.3984	0.4896	0.0000	1.0000	-0.0618	0.3820	-0.0538	1.0000							
5)	inventor's pre-MARA patent frequency (L)	-0.5644	0.8614	-2.8230	3.0910	-0.0724	-0.0132	0.2138	0.2138	1.0000						
6)	former firm's normalized patents, last 3 years (L)	-1.2604	1.2643	-2.2970	4.0565	0.0185	-0.0697	-0.0153	-0.2115	0.0114	1.0000					
7)	subsequent firm's normalized patents, last 3 years (L)	-1.7074	1.0280	-2.3000	4.2250	-0.0156	-0.0746	-0.0390	-0.0523	0.0155	0.1832	1.0000				
8)	prior patent was in auto industry	0.0193	0.1377	0.0000	1.0000	0.0147	-0.0059	0.2438	-0.0413	-0.0218	0.1451	0.0028	1.0000			
9)	state enforces non-competes	0.0679	0.2517	0.0000	1.0000	-0.0328	0.0781	-0.0999	0.0612	0.0857	0.0259	0.0405	-0.0127	1.0000		
10)	prior patent was at university	0.0327	0.1779	0.0000	1.0000	-0.0134	0.0299	-0.0234	0.0012	0.0096	0.0763	0.0512	-0.0258	0.0162	1.0000	
11)	duration between patents (L)	6.2634	1.7913	0.0100	9.3325	0.1435	-0.1143	0.0247	-0.3149	-0.3277	0.0771	0.0231	0.0272	-0.1118	-0.0284	1.0000

Models

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

$$\Pr(CF_{ij} = 1) = e^{(\beta Xij + \gamma Zi + \lambda Wit)} / (1 + e^{(\beta Xij + \gamma Zi + \lambda Wit)})$$

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

For Hypothesis 1b, I estimate an OLS regression on the (logged) duration between the last patent at the inventor's former firm and the first patent at the inventor's subsequent firm. The regression equation is similar to that of the logit aside from the inclusion of the error term. I also estimate an OLS model for Hypothesis 2, on the size of the subsequent firm joined by the ex-employee. In all models, standard errors are clustered by inventor to account for the non-independence of observations (White, 1980).[25] All estimates are calculated using the STATA 10 software package.

RESULTS

I first examine support for Hypothesis 1a. Univariate examination of subcategory changes, conditional on employer changes, indeed suggests that Michigan inventors became more likely to shift fields once non-competes were enforced, as compared with other states that continued not to enforce non-competes. As shown in Table 9, although the proportion of inventors changing fields when changing jobs was not statistically distinguishable before Michigan's policy reversal, it become so afterward, with Michigan experiencing less of a drop than non-enforcing states. Unreported factorial ANOVA analysis shows that the interaction of Michigan and post-MARA is positive and statistically significant at the 5% level ($p<0.026$).

Figure 12 shows the trend over time regarding field-changing contingent on job changing in Michigan compared with other non-enforcing states. In the years just following the policy reversal, however, non-enforcing states experience a steep decline in the rate of inventors changing fields when changing jobs. As in the case of job mobility patterns, Michigan fails to follow the trend of the other non-enforcing states. In fact,

[25] Given that both models use the same data, seemingly-unrelated regression (SUR) might appear appropriate. However, the dependent variables do not share the same form. In unreported results, I estimated SUR both by converting the dependent variable for H1 into a continuous measure and also by converting the dependent variable for H2 into a dichotomous outcome. Both yielded consistent results. Stata 10, however, does not cluster standard errors for the continuous SUR model and failed to converge for the bivariate probit SUR model unless clustering was omitted; hence, SUR results are not shown.

aside from 1994 (in which the rates match), the rate of inventors changing fields when changing jobs is consistently higher in Michigan during the post-MARA period, contrary to the pre-MARA period of fluctuation.

Table 9: Univariate comparison of the proportion of employer changes involving a change in field, as represented by a difference in the most frequent NBER subcategory at the prior versus subsequent employer (for those with a patent in a non-enforcing state prior to 1985); ***n*=27,478. The period prior to the policy reversal is referred to as "pre-MARA."**

	pre-MARA	post-MARA	*pre- vs. post-MARA t-tests*	*n*
Non-Michigan	0.580	0.503	*-0.076****	24,171
Michigan	0.572	0.540	*-0.034**	3,307
MI vs. non-MI t-tests	*0.047*	*-0.037***		
n	8,838	18,640		

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

Figure 12: Plot of the proportion of employer changes involving a change in field per year for those with a patent in a non-enforcing state prior to 1985.

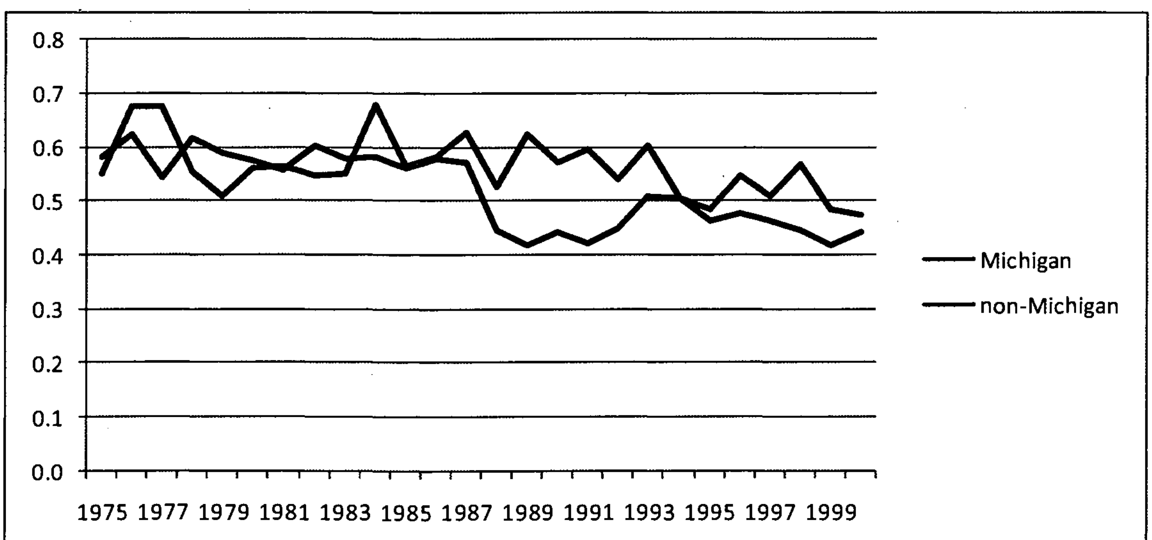


Table 10 shows the results of logit regressions on the dichotomous outcome of changing fields, contingent on changing employers. Model 1, which excludes the interaction term for the explanatory variable, confirms that inventors who have already changed fields are more likely to do so again. Inventors who patent more frequently, or who joined larger companies, are less likely to change fields. The duration between patents increases the chance of changing fields. Other variables are not significant. Adding the explanatory interaction variable in Model 2 significantly improves model fit ($p<0.028$ in an unreported likelihood-ratio test) and suggests that the policy reversal indeed raised the likelihood that an inventor would change fields when changing jobs. A marginal-effects calculation of the interaction effect, holding all other variables at their means, yields an interaction effect of 4.4%.

Table 10: Logit models of the likelihood that an inventor will change fields. The population is restricted to those inventors who were granted at least one patent in a non-enforcing state in 1985 or earlier and who changed employers. Employer change is determined by two subsequent patents of the same inventor having different assignees; n=27,478.

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
post-MARA	-1.2218	-1.2626	-1.2626	-0.7265	-1.7661
	(1.1307)	(1.1240)	(2.8705)	(0.9305)	(1.1817)
Michigan	0.0455	-0.0572	-0.0572	-0.0336	-0.0584
	(0.0476)	(0.0670)	(0.0674)	(0.0644)	(0.0672)
post-MARA * Michigan		0.1732*	0.1732*	0.1461+	0.0790
		(0.0873)	(0.0854)	(0.0850)	(0.0878)
inventor previously changed subcategory	0.0741*	0.0752*	0.0752*		0.1426***
	(0.0332)	(0.0332)	(0.0330)		(0.0347)
inventor's pre-MARA patent frequency (L)	-0.0501*	-0.0505*	-0.0505*	-0.0436*	-0.1487***
	(0.0216)	(0.0215)	(0.0227)	(0.0214)	(0.0232)
former firm's normalized patents, past 3 years (L)	0.0154	0.0150	0.0150		-0.0412***
	(0.0106)	(0.0106)	(0.0117)		(0.0112)
subsequent firm's normalized patents, last 3 years (L)	-0.0483***	-0.0492***	-0.0492***		-0.0774***
	(0.0125)	(0.0125)	(0.0123)		(0.0133)
last patent was in auto industry	0.2418	0.2353	0.2353		0.3018
	(0.2192)	(0.2191)	(0.2078)		(0.2116)
last patent was in auto industry * post-MARA	-0.0514	-0.0381	-0.0381		0.1968
	(0.3278)	(0.3279)	(0.3203)		(0.3318)
last patent was in auto industry * Michigan	-0.1162	-0.0675	-0.0675		0.2324
	(0.2684)	(0.2698)	(0.2619)		(0.2601)
last patent was in auto industry * post-MARA * Michigan	-0.0476	-0.1652	-0.1652		-0.1965
	(0.3928)	(0.3971)	(0.3740)		(0.4011)
state enforces non-competes	-0.0797	-0.0766	-0.0766	-0.0884	-0.1801*
	(0.0651)	(0.0652)	(0.0627)	(0.0649)	(0.0791)
last patent was at university	-0.0650	-0.0653	-0.0653		-0.5369***
	(0.0748)	(0.0748)	(0.0838)		(0.0851)
duration between patents (L)	0.1375***	0.1374***	0.1374***	0.1349***	0.1380***
	(0.0089)	(0.0089)	(0.0092)	(0.0089)	(0.0100)
Constant	17.2326***	17.2493***	16.8443***	17.4621***	18.1339***
	(1.4473)	(1.1169)	(0.1217)	(1.0974)	(1.1245)
log-likelihood	-18573.898	-18571.512	-18571.512	-18484.545	-17526.671
DV represents change in top-level category or sub-category	sub	sub	sub	sub	top-level
Cohort dummies	yes	yes	yes	no	yes
Block-bootstrapped standard errors?	no	no	yes	no	no

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

However, as noted by Ai and Norton (2003) as well as Hoetker (2007) neither the sign, the magnitude, nor the statistical significance of the interaction effect in a non-linear model is necessarily that of the coefficient on the interaction term. Given that the effect of a change in any covariate in such models depends on the initial probability of the outcome variable, interpreting interaction effects requires taking into account not only the coefficient on the interaction variable (and those on the interacted variables) but the values of all other variables in the model. Norton, Wong, and Ai (2004) supply the Stata *inteff* routine for computing the cross-partial derivatives necessary to evaluate a single two-way interaction effect for each observation. When applied to Model 2, *inteff* yields an average interaction effect of 4.2%, indicating that the simple marginal-effects calculation overstated the magnitude of the interaction effect. Figure 13 shows that the interaction effect is positive for all observations, ranging between approximately 2.7% and 4.5%. Figure 14 shows that the effect is significant at the 5% level for almost all observations, with a mean z-statistic of 2.23.

Figure 13: Per-observation interaction effects for logit model of changing fields conditional on changing employers, n=27,478. Graph constructed using inteff.

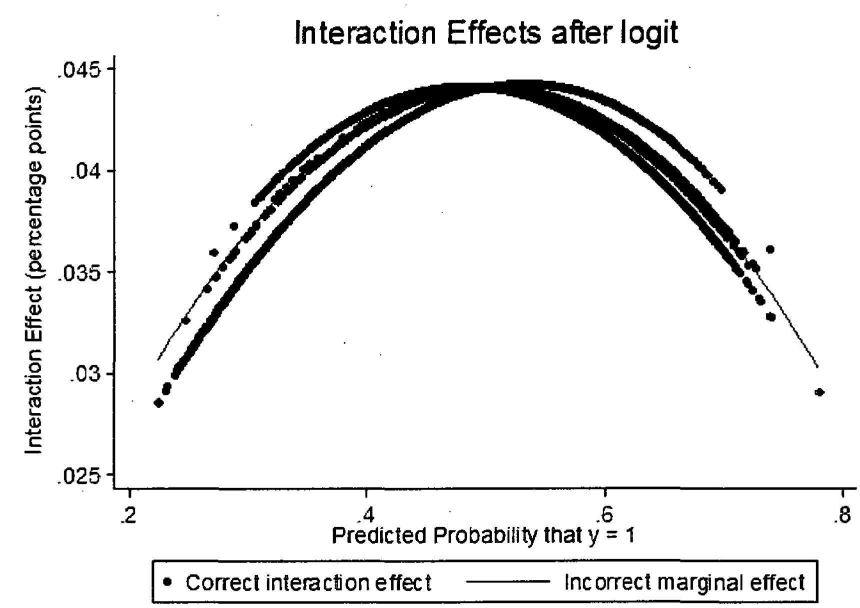
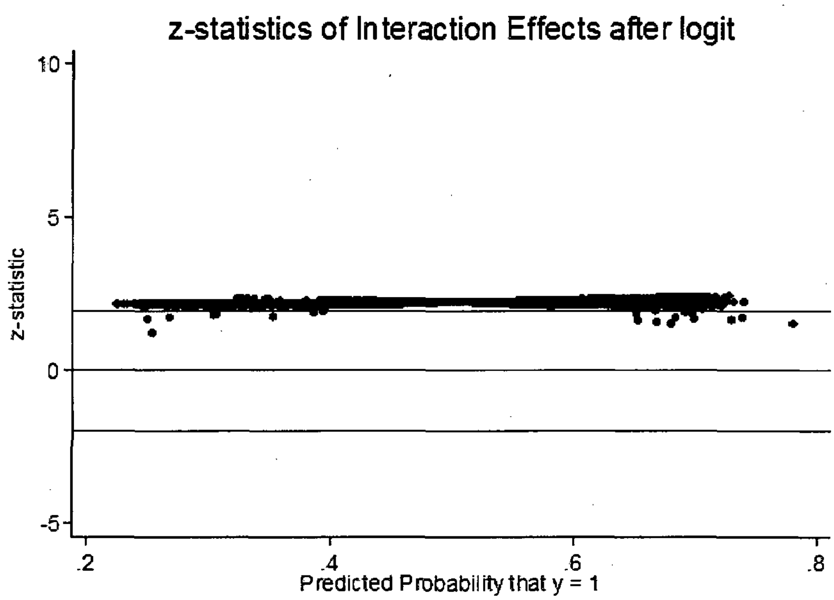


Figure 14: Per-observation z-statistics for interaction effects for logit model of changing fields conditional on changing employers, n=27,478. Graph constructed using inteff.



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

Hypothesis 1b receives weaker support, perhaps due to difficulties in identifying the precise intervals between jobs. Although the univariate analysis in Table 11 suggests that the average interval between jobs grew more for Michigan inventors following the policy reversal, no clear trend is evident in Figure 15. Moreover, both factorial ANOVA analysis and multivariate regression are inconclusive: although the coefficient on the interaction of the Michigan dummy and the post-MARA indicator in Model 6 of Table 12 is positive as expected, it fails to reach significance even at the 10% level. Said coefficient is positive and significant only when the variable is not logged, as in Model 7, which seems inappropriate given the skewness of the unlogged variable.

Table 11: Univariate comparison of the (logged) number of days between the last patent at the former employer of an inventor and the first patent at the inventor's subsequent employer. The population consists of inventors with at least one patent in a non-enforcing state before 1986 who changing jobs; *n*=27,478

	pre-MARA	post-MARA	*pre- vs. post-MARA t-tests*	*n*
Non-Michigan	6.12	6.29	*0.17****	24,171
Michigan	6.14	6.48	*0.34****	3,307
MI vs. non-MI t-tests	*0.02*	*0.19****		
n	8,838	18,640		

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

Figure 15: Plot of the (logged) intervals between the last patent at an inventor's former firm and the first patent at the subsequent firm. Population is restricted to those with a patent in a non-enforcing state prior to the MARA policy reversal; *n*=27,478.

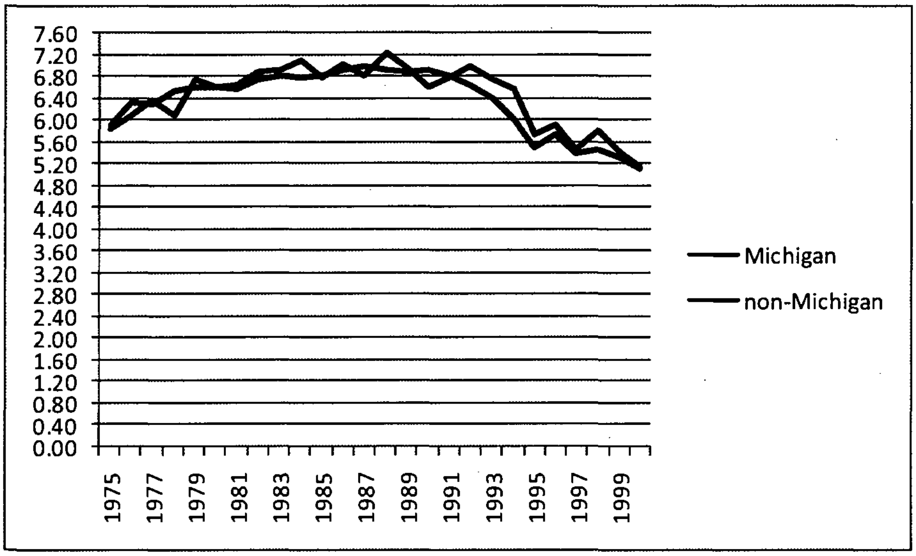


Table 12: OLS models for the (logged) number of days between the last patent at the former employer of an inventor and the first patent at the inventor's subsequent employer; n=27,478.

	Model 6	**Model 7**
post-MARA	0.8968	-824.1422*
	(0.5509)	(382.4921)
Michigan	-0.0075	-25.1277
	(0.0351)	(35.0037)
post-MARA * Michigan	0.0909	212.2111**
	(0.0634)	(64.9368)
# of pre-MARA patents for inventor (L)	-0.7342***	-838.0329***
	(0.0185)	(16.4225)
former employer's normalized # patents (L)	0.0585***	97.4115***
	(0.0087)	(8.0296)
last patent was in auto industry	0.2330+	111.2335
	(0.1197)	(143.8957)
last patent was in auto industry * post-MARA	-0.5304*	-581.2289**
	(0.2361)	(200.0939)
last patent was in auto industry * Michigan	0.0877	558.5638**
	(0.1442)	(197.5766)
last patent was in auto industry * post-MARA * Michigan	0.0532	-648.0711*
	(0.2902)	(271.3213)
State enforces non-competes	-0.3113**	-325.1171***
	(0.1022)	(32.8962)
last patent was at university	-0.2551***	-321.3578***
	(0.0595)	(48.7526)
Constant	4.2063***	1,629.8901***
	(0.5118)	(373.8017)
R-squared	0.254	0.288
Dependent variable logged?	Yes	No

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

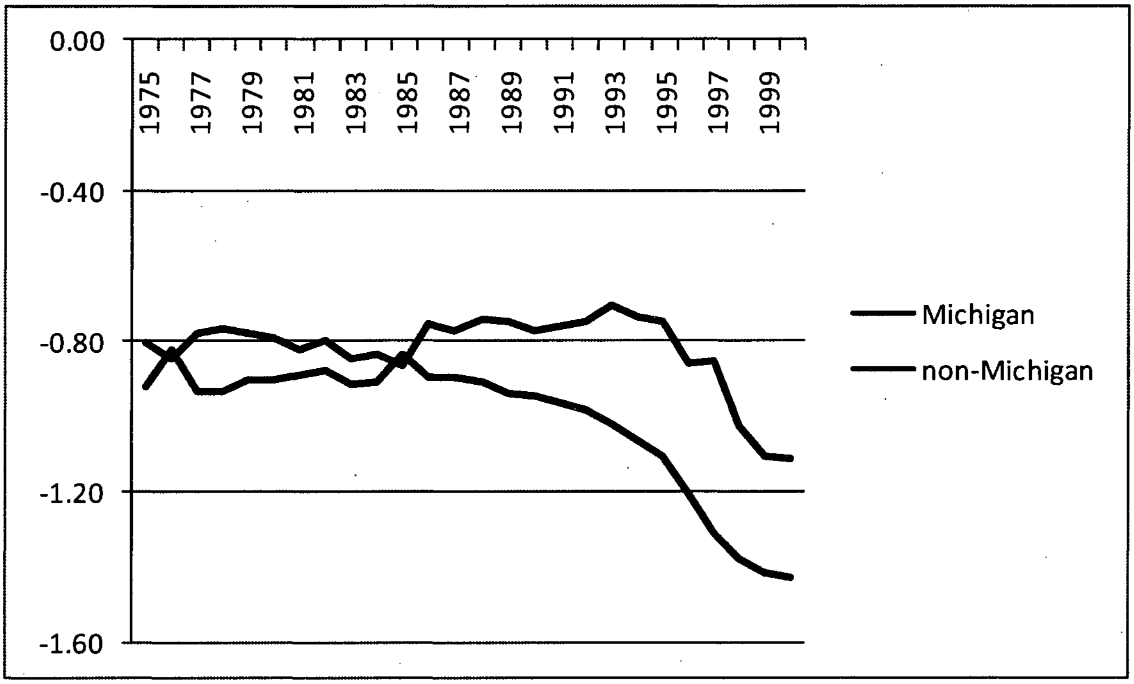
I begin assessing Hypothesis 2 with univariate analysis in Table 13 of the (logged) normalized size of firms subsequently joined by inventors who left their employers. Michigan ex-employees joined larger firms on average following the policy change, while the opposite was true for those outside of Michigan. Although those in Michigan joined discernibly smaller firms than those outside the state prior to the reform, following the policy change the difference was smaller by nearly an order of magnitude and only somewhat statistically significant. Factorial ANOVA analysis shows that the interaction of post-MARA and Michigan is positive as well as significant at the 1% level, lending preliminary support to Hypothesis 2. The trend over time is visible in Figure 16, which shows that the normalized size of firms joined by inventors was consistently larger in Michigan than in other non-enforcing states following the policy reversal.

Table 13: Univariate comparison of the (logged) normalized size of firms joined by inventors when changing jobs; n=27,478. The population is restricted to those inventors who were granted at least one patent in a non-enforcing state in 1985 or earlier and who changed employers. Employer change is determined by two subsequent patents of the same inventor having different assignees.

	pre-MARA	post-MARA	*pre- vs. post-MARA t-tests*	*n*
Non-Michigan	-1.56	-1.76	*0.20****	24,171
Michigan	-1.83	-1.79	*0.04*	3,307
MI vs. non-MI t-tests	*0.27****	*0.03+*		
n	8,838	18,640		

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

Figure 16: Plot of the (logged) normalized size of firms joined by inventors (with at one patent in a non-enforcing state before 1986) when changing jobs. The population is restricted to those inventors who were granted at least one patent in a non-enforcing state.



In multivariate analysis, I employ the size of the subsequent firm as the dependent variable in an OLS regression. Model 8 of Table 14 shows that Michigan inventors tended to join smaller firms than their peers in other states, although inventors outside of Michigan tended to move to smaller firms following the policy reversal. The larger the prior employer was, the larger the subsequent employer was as well. Inventors who patented more frequently were more likely to join larger firms. Those leaving universities were much more likely to join larger firms. Those inventors who had moved to a non-enforcing state and then changed jobs were also more likely to join a larger firm. Other variables are not significant. Adding the explanatory variable in Model 9 shows that once the state of Michigan began enforcing non-compete agreements, Michigan inventors changing jobs joined larger firms as compared with inventors in other states, with strong statistical significance.

Table 14: Multivariate regressions for the size of ex-employees' subsequent firm. The population is inventors granted one or more patents in a non-enforcing state before 1986 and who changed employers. All models have year and cohort indicators.

	Model 8	Model 9	Model 10	Model 11	Model 12
post-MARA	-1.9159***	-1.9387***	-1.7283***	-1.7066***	1.6478
	(0.2843)	(0.2821)	(0.2904)	(0.4766)	(1.3264)
Michigan	-0.0961***	-0.2164***	-0.2148***	-0.2698***	-0.4402***
	(0.0187)	(0.0257)	(0.0256)	(0.0336)	(0.0805)
post-MARA * Michigan		0.2037***	0.1956***	0.2560***	0.3329**
		(0.0359)	(0.0358)	(0.0450)	(0.1048)
# of pre-MARA patents for inventor (L)	0.0216*	0.0213*	0.0213*	0.0273**	0.0776**
	(0.0090)	(0.0090)	(0.0090)	(0.0106)	(0.0237)
former employer's normalized # patents (L)	0.1389***	0.1382***	0.1348***	0.1708***	0.2909***
	(0.0059)	(0.0059)	(0.0058)	(0.0071)	(0.0116)
last patent was in auto industry	-0.1438	-0.1510	-0.1406	-0.1760	-0.5298*
	(0.1016)	(0.1015)	(0.1029)	(0.1307)	(0.2576)
last patent was in auto industry * post-MARA	0.1495	0.1650	0.1564	0.1888	0.4460
	(0.1827)	(0.1827)	(0.1825)	(0.2162)	(0.3994)
last patent was in auto industry * Michigan	-0.0034	0.0523	0.0491	0.1421	0.5527+
	(0.1256)	(0.1258)	(0.1265)	(0.1626)	(0.3064)
last patent was in auto industry * post-MARA * Michigan	-0.0601	-0.1974	-0.1914	-0.2883	-0.6854
	(0.2146)	(0.2153)	(0.2146)	(0.2583)	(0.4775)
State enforces non-competes	0.1656***	0.1691***	0.1510***	0.1365***	0.4422***
	(0.0274)	(0.0274)	(0.0275)	(0.0304)	(0.0586)
last patent was at university	0.2175***	0.2169***	0.2251***	0.2640***	0.3957***
	(0.0440)	(0.0439)	(0.0438)	(0.0518)	(0.0774)
duration between patents (L)	0.0032	0.0030	0.0016	-0.0089+	0.0124
	(0.0039)	(0.0039)	(0.0039)	(0.0047)	(0.0102)
Constant	-0.1885	-0.1939	-0.5512*	-0.4834	-18.8582***
	(0.2615)	(0.2590)	(0.2691)	(0.4599)	(1.3555)
Observations	27478	27478	27478	21831	27478
R-squared	0.049	0.050	0.048	0.070	
Model	OLS	OLS	OLS	OLS	logit
Window for patent counts	3	3	5	3	3
Include moves to self-employment	yes	yes	yes	no	yes

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

Models 10-12 provide robustness checks. I test the sensitivity of the model to the choice of a 3-year rolling window in Model 10 using a 5-year window, with similar results. (An unreported model with a one-year window also yields similar coefficients.) In Model 11, I exclude moves to self-employment (i.e. where the subsequent patent is not assigned to a firm), reducing the number of observations yet returning similar results.

Model 12 estimates a logit on the likelihood of the ex-employee's subsequent firm being of above-median size, indicating that Michigan inventors were approximately 6% more likely to join a firm of above-median size following the policy reversal as compared with inventors in other states. Not shown but available from the author are models demonstrating the robustness of this result to block-bootstrapping the standard errors (as in Model 3) and excluding possibly-endogenous variables (as in Model 4).

DISCUSSION

I interpret support for the hypotheses cautiously, given the measurement difficulties involved in individual-level longitudinal studies using patent data. I may fail to capture employer changes and do not know whether they are voluntary.[26] Heterogeneity in the firms' propensity to prosecute non-compete contracts is unobserved as are the contracts themselves.[27] Moreover, it may be difficult to generalize these results beyond the technology-focused companies that employ patent-filing inventors; indeed, non-compete agreements may have their strongest effect in technology-based industries where intellectual property is critical. Nevertheless, support for two of the hypotheses offers contributions both to both theory and practice.

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

[27] Another approach to the analysis would assess the number of non-compete lawsuits before and after the policy reversal. I did not pursue this path for two reasons. Westlaw and other databases include only court decisions, eliminating out-of-court settlements and thus not giving the full picture of legal activity. While another database, the Courthouse News Service, maintains a comprehensive list of all cases filed, its records for Michigan commenced after the policy reversal and as such would not be of use in determining any change in trend.

The first result establishes that non-compete enforcement leads inventors to shift to different fields when leaving their employers. It refines a tenet of human capital analysis: that one "cannot separate a person from his or her knowledge [or] skills…the way it is possible to move physical and financial assets while the owner stays put" (Becker 1962:16). Support for Hypothesis 1a shows that ex-employers are indeed able to separate individuals from the *use* of their skills, by modifying the property rights associated with expertise. Instead of being "automatically vested" in individuals (Becker 1962:17), property rights under non-compete agreements afford firms some measure of ownership over workers' skills, questioning the practicality of the so-called "boundaryless career" (Arthur and Rousseau 1996). This result also blurs Becker's distinction between "general" and "specific" training, where the former is expertise usable by many firms. As non-competes limit the ability of ex-employees to utilize general skills, firms may be more willing to invest in general training than Becker's analysis indicates. However, because non-competes restrict not only the use of skills gained from training by the firm but also prior expertise (including education), individuals who expect to sign non-competes may become less willing to invest in their own human capital lest they find themselves constrained from utilizing it.

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

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

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

CONCLUSION

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

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

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APPENDICES

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, we asked:

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 (2008), 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[28] (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.

[28] This alternative match is to control for cities such as New York City, NY, is one city with many zip codes. 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.

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

Appendix D: Questions for interviews of randomly-sampled patent holders in the automatic speech recognition industry since 1970.

- Demographics (if not evident or disclosed)
- Male/female, age, state of residence, highest education level
- Origins
- How did you get started in the speech recognition industry?
- What was your first job in ASR?
- For each job
- Name
- Size when joined
- Full-time employee, part-time, or contractor
- Role
    - Executive owner, manager, individual contributor...
- Size when left
- Location
- # years spent
- Were you asked to sign an agreement that limited your ability to work for other companies after you left? It's sometimes called a "noncompete."
    - If not, move to next job in the industry
    - When were you asked to sign the noncompete?
        - If after they started at the job, ask why they were not asked to sign a noncompete when they started.
        - Did you consult a lawyer?
            - If not, ask why. Answers may include cost, pressure from employer to sign quickly, or not knowing where to find a lawyer.
    - What were the terms of the noncompete? Length, scope, etc.
- Did you sign it?
    - If no, ask what happened after that, and move to next job in industry
    - Did the noncompete affect your day-to-day work at all, including your motivation level?
    - Did you stay with the company longer than you might have if you had not signed the noncompete?
    - Did you eventually leave the company?
        - (If not, then that's their current job. Wrap up the interview)
    - What did you choose to do after leaving your employer?
    - If did not take another job
        - How did you support yourself during that time?
    - If took a job in a different industry
        - Did the noncompete influence your decision to leave the ASR industry? If so, how?
    - If took another job in the same industry
        - How were you able to keep working in the same industry despite the noncompete?

Appendix E: Instrument for the survey conducted in conjunction with the IEEE engineering society.

1. In what sector do you currently or most recently work?
   - ❍ Semi-conductor
   - ❍ Electronics
   - ❍ Computer Hardware
   - ❍ Power
   - ❍ Biomedical
   - ❍ Software
   - ❍ Aerospace
   - ❍ Information Technology (IT)
   - ❍ Communications
   - ❍ Automotive
   - ❍ Other (please specify) ___________

2. Which one of the following categories best describes your current employment status?
   - ❍ Private industry
   - ❍ Public/government
   - ❍ Educational institution (public or private)
   - ❍ Nonprofit institution (non-educational)
   - ❍ Self-employed
   - ❍ Retired
   - ❍ Unemployed
   - ❍ Full-time student
   - ❍ Other, please specify: ____________________________

3. What is your job title?
   - ❍ Chairman of the Board/President/CEO
   - ❍ Owner/Partner
   - ❍ General Manager
   - ❍ V.P. Operations
   - ❍ V.P. Engineering/Dir.Engineering
   - ❍ Chief Engineer/Chief Scientist
   - ❍ Engineering Manager
   - ❍ Scientific Manager
   - ❍ Member of Technical Staff
   - ❍ Design Engineering Manager
   - ❍ Design Engineer
   - ❍ Hardware Engineer
   - ❍ Software Engineer
   - ❍ Computer Scientist

- Dean/Professor/Instructor
- Consultant
- Retired
- Other, please specify: ______________________________

4. How many years have you worked in this profession?_______
5. How transferable is your expertise outside of your industry?
    - Very transferable - it wouldn't be difficult for me to find a job in another industry.
    - Somewhat transferable – I could change, but it would be inconvenient.
    - Not transferable – it would be difficult for me to find a job in another industry.
    - I don't know

6. In the last 10 years, have you been asked to sign a non-compete agreement? (If "no", skip to Demographics)
    - Yes
    - No

7. Thinking only about the most recent non-compete agreement you were asked to sign, for how long did the agreement limit your right to work?
    - Until I left the company.
    - Up to one year after leaving the company.
    - More than one year but no more than two years after leaving the company.
    - More than two years but no more than five years after leaving the company.
    - For more than five years after leaving the company.

8. Did you sign the non-compete agreement? (if "no", skip to Demographics)
    - Yes
    - No

9. When were you asked to sign the non-compete agreement?
    - When you were offered the job or with the offer letter
    - After you accepted but before you started the job
    - The day you started the job
    - Sometime after your first day on the job

10. Did you consult a lawyer before signing the non-compete agreement?
    - Yes, I had a lawyer review the non-compete agreement.
    - No, I couldn't find a lawyer
    - No, I couldn't afford a lawyer.

- ❍ No, I felt under time pressure to sign and thus didn't try to get a lawyer.
- ❍ No, I personally didn't feel it was necessary.
- ❍ No, the company said it was non-negotiable so I decided not to.
- ❍ Other, please specify.

11. In the last 10 years, have you left a company where you had signed a non-compete agreement? (if "no", skip to Demographics)
    - ❍ Yes
    - ❍ No

12. Did the most recent non-compete agreement you signed deter you from working at (or as an independent contractor for) a competitor?
    - ❍ No
    - ❍ Yes, I did not interview with competitive firms due to the non-compete agreement I had signed.
    - ❍ Yes, even though I obtained a job offer with a competitor I declined it due to the non-compete agreement I had signed.
    - ❍ Yes, I accepted a job with a competitor but then resigned due to (the threat of) legal action by my former employer.

13. Thinking about your most recent experience with a non-compete agreement, what did you choose to do after leaving your employer? (Please check all that apply.)
    - ❑ Took a job in the same industry – the non-compete agreement wasn't an issue
    - ❑ Took a job with an employer I felt could defend me against a potential lawsuit
    - ❑ Took steps to keep my prior employer from knowing I was working in the industry
    - ❑ Took a job in a different industry
    - ❑ Remained unemployed
    - ❑ Moved to another state or country where the noncompete was not enforced
    - ❑ Other (please explain)______________________________

14. In which state were you living when you left the most recent employer with whom you had a non-compete agreement?
15. What was your role in the company when you left that employer?
    - ❍ Owner/executive
    - ❍ Manager
    - ❍ Individual contributor (employee)
    - ❍ Independent contractor
    - ❍ Other (please explain)________________________

16. Roughly how large was the company with which you had the non-compete agreement when you left it (# of employees)?
    - ❍ Fewer than 50 employees
    - ❍ 50-250 employees
    - ❍ 251-1000 employees
    - ❍ 1001-5000 employees
    - ❍ More than 5000 employees
    - ❍ No

17. Did the non-compete agreement have an impact on your personal income the year after you left your employer?
    - ❍ Positive impact
    - ❍ No impact
    - ❍ Negative impact
    - ❍ Don't know

18. Did your decision to sign a non-compete agreement increase, decrease, or have no impact on your ability to utilize your professional skills?
    - ❍ Increased my ability to utilize my skills
    - ❍ No impact
    - ❍ Decreased ability to utilize my skills – had to retrain or learn new skills before I could resume working

19. Are you aware that non-compete agreements are generally not enforceable in several states, including California?
    - ❍ Yes
    - ❍ No

20. If you have any additional comments on non-compete agreements, please indicate them here.

    ____