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Noncompetes and Inventor Mobility: Specialists, Stars, and the Michigan Experiment

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Abstract: Several scholars have documented the positive consequences of job-hopping by inventors, including knowledge spillovers and agglomeration and the concentration of spinoffs. This work investigates a possible *antecedent* of inventor mobility: regional variation in the enforcement of post-employment noncompete covenants. While previous research on non-competes has been largely focused on California and Silicon Valley, we exploit Michigan's inadvertent reversal of its noncompete enforcement legislation as a natural experiment to investigate the impact of noncompetes on mobility. Using the U.S. patent database and a differences-in-differences approach between inventors in states that did not enforce and did not change enforcement of non-compete laws, we find that relative mobility decreased by 34% in Michigan after the state reversed its policies. Moreover, this effect was amplified 14% for “star” inventors and 17% for “specialist” inventors.

Introduction

The loss of key employees to a competitor can be devastating, especially for high-technology companies whose "most valuable assets walk out the door every night." In 2005, for example, Microsoft engaged in a public legal battle for several months to prevent Google from hiring away Kai-Fu Lee, its vice president responsible for search technology, who had signed an agreement not to compete against Microsoft for one year after leaving the company. Given the specialized investments required to hire and train technical personnel, it is perhaps no surprise that managers have a love/hate relationship with the movement of workers from one company to the other—depending on whether they are gaining or losing talent.

Since Arrow's (1962) observation that "mobility of personnel among firms provides a way of spreading information", researchers have sought to understand the implications of interorganizational worker mobility. Scholars have demonstrated the connection between mobility and spillovers (Stolpe 2002; Agrawal, Cockburn et al. 2003; Breschi and Lissoni 2003; Song, Almeida et al. 2003), noting that such employer-to-employer moves can facilitate knowledge transfer both locally (Almeida and Kogut 1999) and over great distances (Rosenkopf and Almeida 2003; Singh 2006a). In addition to infusing the hiring firm with knowledge, employee mobility has been shown to be associated with changes in strategic direction (Boeker 1997), organizational structure (Klette, Moen et al. 2000) the compensation structure of R&D staff (Moen 2005), and innovation (Singh 2006b). The growth of industries (Franco and Filson 2000; Klepper 2002; Klepper and Sleeper 2002) and even regions (Rosengrant and Lampe 1992; Saxenian 1994) has been attributed in part to the movement of technical personnel between firms.

By comparison, the *antecedents* of firm-to-firm worker mobility have received less attention. Economists have modeled the contracts and conditions under which technical personnel can be convinced not to expropriate their knowledge in service of competitors (Pakes and Nitzan 1982; Anton and Yao 1995), yet empirical studies are few and tend to focus on the characteristics of firms more likely to hire away competitors' employees while trying to grow (Almeida, Dokko et al. 2003), firms that lose workers due to strategic disagreements (Klepper and Thompson 2006), or comparisons of mobility across different regions (Fallick, Fleischman, and Rebitzer forthcoming).

The purpose of this paper is to investigate one antecedent of employee mobility: the enforcement of post-employment covenants not to compete (hereafter, “noncompetes”). We begin by reviewing the legal history of noncompetes as well as relevant empirical research. Next, we outline the design of a natural differences-in-differences experiment based on Michigan’s *inadvertent* mid-1980s reversal of its enforcement policy. By observing changes in patent assignee for inventors, we demonstrate a relative drop in the mobility of Michigan inventors following the change of non-compete laws. This effect is amplified for highly-cited inventors and for inventors with specialized skills.

THEORY AND PREDICTIONS

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 1414, only a few decades after the Bubonic plague had decimated the European labor supply, subsequent to the passing of the Ordinance of Labourers that essentially outlawed unemployment in post-medieval England. Thus a plaintiff’s request to enjoin one of his former clothes dyers from working in the same town for six months was met with disdain from the judge, who threatened the plaintiff himself with jail time for having sought to restrict a citizen from practicing his trade. The principle of keeping skilled labor in the public domain was further established 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 “particular restraints” entered into voluntarily by employees. The courts typically stipulated a “reasonableness test,” including the geographic scope and duration of the agreement (Decker 1993). Modern non-competes often specify a list of competitors the employee may not join, rather than a trade the employee may not practice. Both the duration and the geographic reach of the agreement continue to be essential components.

Firms use non-competes to protect their interests: to prevent the disclosure of trade secrets, to honor customer confidentiality, and to prevent competitors from appropriating the specialized skills of its employees (Valiulis 1985). Whereas in the medical profession client lists and territory rights are paramount, industries tend to place higher value on trade secrets or employee skills the former employer has paid to develop. One might argue that trade secrets are already protected by the non-disclosure agreement (NDA) employees are generally required to sign, but violations of an NDA can be difficult to detect or prove (Hyde 2003). Preventing an ex-employee from joining a competitor via a noncompete decreases 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), the enforcement of noncompetes varies significantly from state to state. For example, California's Business and Professions Code section 16600 (California 1865) is reminiscent of early English law: *"Except as provided in this chapter, every contract by which anyone is restrained from engaging in a lawful profession, trade, or business of any kind is to that extent void."* Note that although contracts typically stipulate a "choice of law"—a state under whose laws the agreement is to be governed—in *Frame v. Merrill Lynch* (1971) the California courts forbade corporations from specifying out-of-state jurisdiction as a means of cherry-picking one's noncompete enforcement regime.

Gilson (1999) traces the lineage of California's statute back to its inception in 1865 as a "historical accident," an artifact of rapid law-making as California sought statehood. Yet section 16600 has been upheld by the courts and not overturned by the legislature. Citing the attenuating impact of noncompetes on employee mobility, Gilson concludes that this practice is in fact "the causal antecedent" of the high-velocity labor market as well as the unique culture Saxenian attributes to Silicon Valley. Gilson's hypothesis went untested until 2003, when Stuart and Sorenson (2003) examined the effect of initial public offerings (IPOs) and acquisitions on founding rates of biotech firms in regions that enforce noncompetes versus those that did not. That proportionally more biotech firms were founded in states that proscribe enforcement of noncompetes is consistent with Gilson's hypothesis. However, as the Stuart and

Sorenson analysis measures firm foundings, it does not directly track individual mobility. Thus it is unknown how many of the founders in their sample would have been affected by noncompetes (i.e., deserting another biotech firm) or not (leaving an unrelated firm, or having been previously unemployed).

More direct evidence of mobility was established in Fallick, Fleischman, and Rebitzer's (forthcoming) 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 industry-specific increase in intraregional employee mobility for the California computer industry vs. other states. The authors caution, however, against interpreting their results as unequivocal support for Gilson's hypothesis: "[W]hile there appears to be a 'California' effect on mobility in information technology clusters, we have no direct evidence that this is due to the absence of enforceable noncompete agreements. As a result we cannot rule out the role that other factors (such as local culture) may play in sustaining high rates of employee turnover." Ideally, differences in mobility would be established not through cross-sectional analysis but through an experiment: by randomly reversing the noncompete enforcement policy in one state, and comparing changes in intraregional mobility rates between that state and those that did not change their noncompete laws. In the next section, we describe why Michigan may afford such an experiment.

Noncompete enforcement: the Michigan experiment

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 (which bears striking resemblance to California section 16600): *"All agreements and contracts by which any person…agrees not to engage in any avocation or employment…are hereby declared to be against public policy and illegal and void."* This law governed all noncompetes prosecuted in the state until 27 March 1985, when the Michigan Antitrust Reform Act (MARA) repealed MCL 445 and with it the prohibition against noncompetes.

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

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

Interviews with Michigan labor lawyers (authors of a Michigan Bar Journal article on noncompetes that appeared in October of 1985) support this interpretation (Rabaut 2006; Sikkel 2006). Responding to our neutral questions in Appendix A, Robert Sikkel reported that, "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." Louis Rabaut indicated that, "There wasn't an effort to repeal noncompetes. We backed our way into it. The original prohibition was contained in an old statute that was revised for other issues…we were not even thinking about noncompete language…All of a sudden the lawyers saw no proscription of noncompetes. We got active and the legislature had to go back and clarify the law."

Like any law, noncompetes are subject to interpretation by the courts, thereby varying the level of enforcement. Texas courts, for example, have at times been lenient given its very strict noncompete enforcement statute (Wood 2000). It is also possible that noncompetes have no influence upon mobility – their effect has never been established directly. Nonetheless, Michigan is the only state we know of to have clearly – and inadvertently - changed its enforcement policy in the past century. [1] Given that Michigan's shift in noncompete enforcement appears to have been exogenous, we propose that Michigan affords a "natural experiment" in which to directly test the impact of noncompetes on worker mobility.

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

Building upon this baseline hypothesis, we should observe these effects most strongly for highly-valued or "star" inventors. The inventor described in the opening of this paper, Kai-Fu Lee, provides a highly publicized example. Such inventors would be influential, well-known, and probably capable of inventing newer and more valuable technologies. Stars will be more valuable to current employers and potential competitors alike, as they form the basis for new firms and technological breakthroughs (Zucker,

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

Darby et al. 1998). They will be better known throughout the industry and more likely to understand proprietary secrets of their employers. Thus the optimal contract to retain a scientist (Pakes and Nitzan 1982) will require increased compensation unless their mobility can be constrained. Such individuals will have a greater number of job opportunities, and at the same time, be less able to pursue them within a region that enforces noncompetes. Their original employer will be more adversely affected by their departure and more willing to pursue legal action, due to the loss of an important contributor and the probable loss of proprietary secrets. Thus we predict that:

Hypothesis 2: Relative to other non-enforcing states, Michigan intraregional mobility for "star" inventors should decrease even further subsequent to the passage of MARA legislation.
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Inventors with specialized skills may likewise feel greater impact from the enforcement of noncompetes. Because noncompetes do not proscribe the practice of a trade but instead typically list a set of competitors (Valiulis 1985), those with generally-applicable skills may be able to practice their trade at a new firm not competitive with their current employer. For example, C++ programmers currently employed by a speech recognition firm may find work with a database company, an online order-fulfillment processing operation, or a manufacturer of embedded systems—none of which compete with the current employer. Speech-recognition engineers at the same firm, however, may find that the market for their more specialized skills is limited to competitors, which are proscribed by the noncompete agreement. Thus we predict that inventors with specialized skills are more likely to be immobilized by noncompetes.

Hypothesis 3: Relative to other non-enforcing states, the Michigan intraregional mobility for "specialist" inventors should decrease even further subsequent to the passage of MARA legislation.
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STUDY DESIGN

If the initiation of noncompete enforcement via the passage of MARA had a measurable impact on worker mobility in Michigan, we would expect the effect to show up most convincingly in a difference between Michigan's mobility pre-MARA and post-MARA versus other states that did not enforce noncompetes, both pre and post-MARA. It would not suffice to observe a difference between Michigan's pre-MARA mobility and post-MARA mobility, for many factors may have contributed to changes in mobility of 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 noncompetes. If noncompetes retard inventor mobility, then we should see a difference between the baseline ratio and the ratio of post-MARA mobility in Michigan vs. that of those same states (which continued not to enforce noncompetes).

In a controlled experimental setting, one observes the same subjects both before and after the stimulus. 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

In selecting a dataset with which to test our hypotheses, we evaluated the strengths and weaknesses of those used in previous work. Simply tracking firm foundings does not necessarily capture interorganizational movement of personnel, so we sought a data source focusing on individuals. The Current Population Survey (CPS) 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. No one person in the CPS is surveyed for more than 1½ years (Fallick, Fleischman, and Rebitzer forthcoming), so the longest we would be able to observe mobility on either side of the MARA legislation is nine months. This limited window is especially problematic given that it may have taken some number of months and even years for news of MARA's passage to diffuse and influence inventors' employment choices.

Several of these weaknesses are overcome by the U.S. patent database. First, patent holders tend to be the sort of scientists and engineers our study seeks to track. Second, 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 the summer of 2006 (we also include the more limited NBER data from 1960-1974). Third, 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.

Patent data, however, have a variety of documented weaknesses (Griliches 1991; Desrochers 2001; Alcacer and Gittelman 2004) including the fact that many inventors and entire industries do not patent (Levin, Klevorick et al. 1987). Patents routinely take years to process (Jaffe and Lerner 2004), and the optical-character scanning of paper applications by the patent office creates some errors in computer-readable patent files (Miller 2005). Moreover, attempting to detect inventor movement using patents is necessarily inexact for three reasons. First, we may fail to detect moves that occurred between an inventor's patents (e.g., an inventor patented in city A during 1987 and in city C during 1989 but also lived in city B during 1988). Second, even when we observe a move, we do not know precisely when it occurred within the time interval of the two application dates (Song, Almeida et al. 2003). Third, and most challenging, patents are not indexed by inventor. Thus our longitudinal analysis of inventor mobility between firms required us to determine which patents belong to which inventor. For this we leveraged and refined existing algorithms (Trajtenberg, Shiff et al. 2006; Fleming, King et al. forthcoming; Singh, 2006b). Details of the inventor-matching algorithm are given in Appendix B.

Of course no such matching algorithm will be completely free of either Type I or Type II errors, where Type I error is the possibility that the algorithm will fail to identify all of an inventor's patents and Type II error is the possibility that an inventor will be matched with patents they did not invent. Our approach is to design a robust estimation model and conduct sensitivity analyses of the algorithm at various degrees of conservatism. As will be discussed in the results section, we found very little variation between running the algorithm at a very conservative level (many Type I, few Type II) and at a very loose level (few Type I, many Type II). We believe this to be indicative that our study design—comparing relative mobility rates across regions—remains mostly insensitive to the algorithm itself since we are not drawing conclusions except from the comparison of mobility rates in Michigan and other non-enforcing states. Hence, if mobility rates in Michigan are underrepresented or overrepresented by too conservative an algorithm, they will likewise be underrepresented or overrepresented outside of Michigan.

In this dataset, the inventors "at risk" of moving are those who patented in Michigan or in another non-enforcing state before MARA was passed, including the following: Alaska, California, Connecticut, Minnesota, Montana, North Dakota, Nevada, Oklahoma, Washington, and West Virginia (Malsberger 1996). For example, if an inventor had a patent in the non-enforcing state of Connecticut in 1983, all of that inventor's patents from 1960 through 2006 would be inspected for moves. If an inventor never patented in a non-enforcing state or did not do so until after MARA was passed, that inventor's patents would not be included in the dataset.

Employing a middle-of-the-road sensitivity setting for our inventor-matching algorithm, the resulting dataset contains 98,468 inventors who patented in Michigan or in another non-enforcing state prior to MARA. Following these inventors throughout their careers yields 372,908 patents between 1960 and 2006, for a patent-per-inventor ratio of 3.79[2]. 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 Stople (2002) estimated that 20% of inventors had moved. An inspection of only Michigan patents in the same timeframe reveals a similar

[2] We find a greater number of patents per inventor than Trajtenberg, Shiff, et al. (2006), largely because our sample is restricted to US inventors. Also, this data set includes patents that were applied for prior to 1999, but not granted until after 1999, thus are not contained in the NBER data set. The dramatic rise in the rate of overall patenting during the years 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 individuals with less unusual names.

ratio of patents per inventor (61,615/16,885=3.65) but a significantly lower average number of moves per inventor (3,307/16,885=.196); we will return to the baseline difference in the mobility of Michigan inventors vs. those in non-enforcing states in the Results section. In terms of assignee matching, we assumed that mergers, acquisitions, and corporate rechristening would introduce spurious moves. For example, earlier patents for 3M Corporation were assigned to "Minnesota Mining & Manufacturing." Thus we identified all pairs of assignee moves and manually checked the moves for all pairs that appeared more than once, using electronic sources.

Variables

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

The explanatory variables include a time period indicator, Michigan residence, and measures of the degree to which the inventor was a star or specialist. The time-period indicator *postmara* indicates a patent application date of 1986 or later. The indicator variable *Michigan* indicates whether the inventor resided in Michigan at the time of patent application. The variable *lntotalcites* is used to identify "star" inventors by citations in the year prior to their patents, as highly cited patents are associated with innovations of larger technical and market value (Trajtenberg 1990; Harhoff, Narin et al. 1999). In order to assess the degree to which an inventor is a specialist vs. a generalist, we calculate the diversity of an inventor's inventions *(inv_entropy)* with a Shannon Entropy measure based on patent technology class information. The Shannon Entropy of a discrete random variable X, with possible outcomes $x_1 ... x_n$ is defined as:

$$H(X)=\sum_{i=1}^{n} p(x_i)\log_{436}\left(\frac{1}{p(x_i)}\right)=-\sum_{i=1}^{n} p(x_i)\log_{436} p(x_i) \qquad (1)$$

Where $p(x_i)=\Pr(X=x_i)$ is the probability of the ith outcome of X. The log base 436 refers to the number of USPTO technology classes. In this case, the outcome x_i is the relative frequency of each technology class across all of inventor's patents. Given the technology classes across all of an inventor's patents, we calculate the relative frequency of each technology class. For example, an inventor with patents in a single technology class will have an entropy value of 0. A second inventor with two technology classes will have a high entropy value (close to 0.5) if the classes occur in equal ratios.

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 college student, in the closing year of the study window. The six non-exclusive NBER patent categories are used to control for industrial differences, including Chemical (74.6% of patents), Computers & Communication (51.0%), Drugs & Medical (9.3%), Electric & Electronic (22.4%), and Other (14.1%) (Hall, Jaffe, and Trajtenberg 2001). To control for firm size we calculated the number of patents attributable to an assignee each year based on each patent's application year (*firmpats*). An indicator variable was created for patents whose assignees were colleges and universities (*university)* as employees of such institutions are not bound by noncompetes. We entered an indicator for residence in a state that does enforce noncompetes (*enforce*) as inventors who left a non-enforcing state and subsequently patented in an enforcing state remained in the risk set. Finally, *priorchurn* becomes and stays 1 in the time periods after an inventor has first moved, controlling for prior propensity to move. Tables 1 and 2 provide summary statistics and correlation tables.

Table 1: Summary statistics for intrastate employer mobility (change in patent assignee) of U.S. patented inventors in states that do not enforce non-competes, 1960-2006 (n=372,908).

	Mean	Std. Dev.	Min	Max
churning	0.07	0.26	0.00	1.00
priorchurn	0.28	0.45	0.00	1.00
enforce	0.07	0.25	0.00	1.00
Michigan	0.17	0.37	0.00	1.00
postmara	0.31	0.46	0.00	1.00
university	0.02	0.14	0.00	1.00
lnfirmpats	2.37	2.33	0.00	8.37
lntotalcites	0.25	2.18	-2.30	7.76
inv_entropy	0.25	0.12	0.00	0.71

Table 2: Correlation statistics for intrastate employer mobility (change in patent assignee) of U.S. patented inventors in states that do not enforce non-competes, 1960-2006 (n=372,908).

		1)	2)	3)	4)	5)	6)	7)	8)	9)
1)	churning	1.00								
2)	priorchurn	0.09	1.00							
3)	enforce	0.00	0.06	1.00						
4)	Michigan	-0.03	-0.06	-0.12	1.00					
5)	postmara	0.06	0.49	0.09	-0.04	1.00				
6)	university	0.02	0.02	0.01	-0.02	0.05	1.00			
7)	lnfirmpats	-0.07	-0.18	0.04	0.07	-0.03	0.06	1.00		
8)	lntotalcites	0.07	0.47	0.12	-0.04	0.59	0.03	0.06	1.00	
9)	Inv_entropy	0.08	0.29	0.09	0.02	0.32	0.03	0.13	0.44	1.00

Methods

Since we wish to understand the influence of noncompete laws on mobility *rates*, we estimate a hazard model with individual patents as the observations. Upon entering the risk set with their first patent, inventors remain at risk of interorganizational mobility through the end of the study. As such, all inventors are "uninformatively" right-censored (Singer & Willett 2003), and repeated "failures" or moves are possible. Moves are noted when the inventor's next patent indicates a new assignee within the same state; thus subsequent spells are calculated from the filing date of the inventor's first patent at a new employer, which renders the data Conditional-B format (Hosmer & Lemeshow, 1999). We employ robust standard errors for within-subject clustering (White, 1980) and break ties using the Breslow approximation (an Efron approximation delivers identical results since all subjects are right-censored). Support for the proportionality assumption of the Cox model is found in the plot of –ln(-ln(*S(t)*) against the log of event time for post-MARA Michigan inventors vs. others, where *S(t)* is the "recovered" survival function. That these two lines are approximately parallel on the graph (available from the authors) supports the assumption that the hazard rates of the two groups are proportional to each other.

We employ a variety of interactions in order to explore the effect of MARA on inventor mobility. The interaction of *Michigan* and *postmara* tells us whether overall inventor mobility was different in Michigan following the passage of MARA. That interaction variable is then interacted with *lntotalcites* and *inv_entropy* in order to explore the effect of MARA on "star" and "specialist" Michigan inventors. Requisite two-way interactions are included wherever three-way interactions are used.

Results

Figure 1 illustrates the raw mobility of inventors in Michigan and other non-enforcing states from 1975 to 2000, as measured by the percentage of patents that indicate a change in assignee (Data after 2000 become increasingly thin, as files from the US patent office reflect only granted patents where as our analysis uses the application date.) Non-Michigan states demonstrate a fairly monotonic increase in mobility over the whole time period. Michigan mobility increases similarly during the early years, levels off in the 1980s, and jumps radically in the late 1990s. It appears that MARA did not cause an absolute decrease in Michigan mobility, though it may have contributed to a decrease relative to other states that continued to proscribe noncompetes. Rabaut (2006) ascribed the late 1990s upturn as resulting from a judicial pendulum swing. On a scale of 1 to 10, with 1 being complete inability to enforce noncompetes and 10 being the opposite, he indicated that Michigan went from a 1 before MARA to an 8 immediately after passage and then back to, "…somewhere between 4 and 6. Judges got sick of noncompetes. At first they felt they had to enforce them but then they looked harder at being 'reasonable.'"

Figure 1: Annual mobility rates of inventors in states that do not enforce non-competes, 1975-2000. Annual mobility rates are calculated by dividing the number of moves that year by the number of patents during the same period.

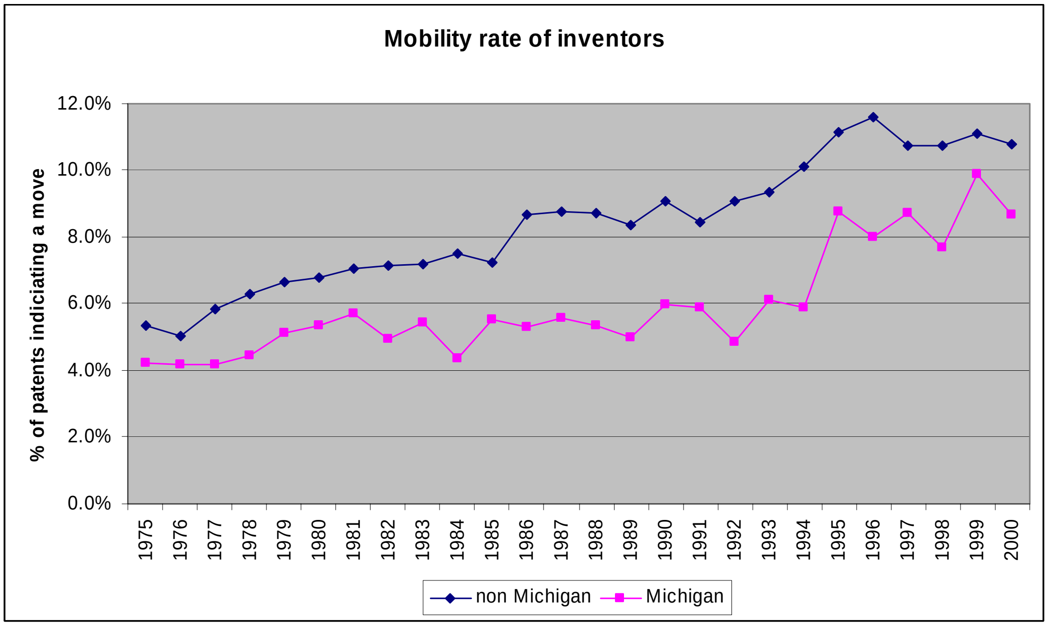

Table 3 presents univariate analysis of the difference in mobility ratios between Michigan and other non-enforcing states. This difference corresponds to the gap between the two lines in Figure 1, each of which is calculated by dividing the number of patents indicating a move in a given year by the total number of patents. Three different tests confirm the relative drop of Michigan mobility in Figure 1, including a five-year window centered about MARA, a ten-year window, and when including all data from 1975 through 2000.

Table 3: Univariate t-test statistics for relative mobility ratios between Michigan and other non-enforcing states (i.e. the difference between Michigan's ratio and the others' ratio). Mobility ratios are computed by dividing the number of patents indicating a move divided by the total number of patents; then, Michigan's ratio is subtracted from the ratio of other non-enforcing states to deliver the statistic below.

1975	1976	1977	1978	1979	1980	1981	1982	1983	1984	1985
1.1%	0.9%	1.7%	1.9%	1.5%	1.4%	1.4%	2.2%	1.7%	3.2%	1.7%
1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996
3.4%	3.2%	3.4%	3.4%	3.1%	2.6%	4.2%	3.2%	4.2%	2.4%	3.6%
1997	1998	1999	2000							
2.0%	3.0%	1.2%	2.1%							

0.01671*	5-year window surrounding MARA
0.00001***	10-year window surrounding MARA
0.00017***	pre-MARA since 1975 vs. post-MARA through 2000

Table 4 illustrates a set of models with the *postmara* indicator for the time period after 1985. Considering the control variables first, prior mobility has a strong and unsurprisingly positive effect on future movement, indicating heterogeneity in inventor preferences for changing employers. University inventors are more likely to change assignees, which probably occurs most often with the graduation of students into the private or academic sector. Inventors with more diverse technical backgrounds are more likely to move, indicating they probably have a broader range of external opportunities. Large firms are more likely to retain their employees, perhaps due to their increased legal and financial resources and the resulting credibility of the threat of lawsuit. Cohort indicators (not shown) demonstrate increased mobility over time. The industrial controls indicate that Drugs and Medical inventors moved approximately 20% more than the NBER baseline "other category." Chemical inventors moved approximately 18% more, Computers and Communication inventors 16% more, Electric and Electronic inventors 6% more, and Mechanical inventors did not move differently from the baseline category.

Table 4: Cox event-history models for intrastate employer mobility (change in patent assignee) of U.S. patented inventors in states that do not enforce non-competes, 1960-2006 (n=372,908 spells, 98,468 inventors, and 27,478 job changes). All models include cohort indicator control variables.

	Model 1	Model 2	Model 3	Model 4	Model 5
NBERcat_chemical	0.1868***	0.1867***	0.1836***	0.1848***	0.1774***
	(0.0192)	(0.0192)	(0.0190)	(0.0192)	(0.0190)
NBERcat_compcomm	0.1669***	0.1668***	0.1621***	0.1664***	0.1603***
	(0.0154)	(0.0154)	(0.0152)	(0.0154)	(0.0152)
NBERcat_drugsmed	0.2415***	0.2408***	0.2161***	0.2409***	0.2152***
	(0.0285)	(0.0285)	(0.0269)	(0.0285)	(0.0269)
NBERcat_electronic	0.0635***	0.0635***	0.0580**	0.0639***	0.0603**
	(0.0189)	(0.0189)	(0.0185)	(0.0188)	(0.0185)
NBERcat_mechanical	0.0045	0.0052	0.0050	0.0064	0.0072
	(0.0178)	(0.0178)	(0.0175)	(0.0178)	(0.0175)
priorchurn	0.4117***	0.4109***	0.3959***	0.4103***	0.3939***
	(0.0165)	(0.0165)	(0.0169)	(0.0165)	(0.0168)
enforce	0.0845*	0.0820*	0.0825*	0.0798*	0.0776*
	(0.0403)	(0.0402)	(0.0391)	(0.0404)	(0.0391)
university	0.3847***	0.3860***	0.3935***	0.3855***	0.3945***
	(0.0366)	(0.0366)	(0.0367)	(0.0366)	(0.0367)
lnfirmpats	-0.0621***	-0.0621***	-0.0626***	-0.0621***	-0.0629***
	(0.0029)	(0.0029)	(0.0029)	(0.0029)	(0.0029)
inv_entropy	1.4315***	1.4347***	1.4194***	1.6446***	1.9072***
	(0.0939)	(0.0939)	(0.0923)	(0.1194)	(0.1174)
Michigan	-0.2228***	-0.1741***	-0.1695***	0.0055	0.0485
	(0.0223)	(0.0257)	(0.0282)	(0.0699)	(0.0723)
postmara	-0.2582***	-0.2454***	-0.3385***	-0.1398**	-0.0888+
	(0.0190)	(0.0197)	(0.0242)	(0.0485)	(0.0487)
lntotalcites	0.0594***	0.0594***	-0.0308***	0.0595***	-0.0355***
	(0.0065)	(0.0065)	(0.0062)	(0.0065)	(0.0062)
postMARA*Michigan		-0.1036*	-0.0526	-0.4432**	-0.4140**
		(0.0422)	(0.0610)	(0.1580)	(0.1558)
postMARA*lntotalcites			0.1813***		0.1913***
			(0.0134)		(0.0134)
Michigan*lntotalcites			0.0288+		0.0388*
			(0.0169)		(0.0168)
postMARA*Michigan*lntotalcites			-0.0530		-0.0703*
			(0.0353)		(0.0341)
postMARA*inv_entropy				-0.3794*	-0.9191***
				(0.1593)	(0.1587)
Michigan*inv_entropy				-0.7004**	-0.8310**
				(0.2672)	(0.2702)
postMARA*Michigan*inv_entropy				1.2044*	1.3317**
				(0.5235)	(0.4790)
log likelihood	-263374.37	-263370.5	-263123.84	-263363.44	-263100.13

Model 2 enables us to disentangle the effect of MARA on the mobility of Michigan inventors in general. The interaction of *Michigan* and *postmara* indicates that inventors in Michigan became less mobile following the passage of MARA, controlling for post-MARA period effects on all inventors as well as mobility patterns of Michigan inventors vs. other non-enforcing states. This establishes the unsurprising baseline hypothesis (H1) that the mobility of Michigan inventors should have decreased following the passage of MARA. Models 3-5 explore whether “star” and “specialist” inventors in Michigan were affected differently than others by MARA. Models 3 and 4 test each type of inventor independently. In model 5, the three-way interaction of *Michigan*, *postmara,* and *totalcites* shows an increased negative and significant effect of MARA on the mobility of stars as predicted by H2. The interaction of *Michigan, postmara,* and *inv_entropy* shows a negative and significant effect of MARA on the mobility of specialists. Thus H3 also receives support. Using model 5 to interpret the size of the hypothesized effects, it appears that Michigan inventors became 33.9% less mobile following MARA compared to inventors in non-enforcing states. A one-standard-deviation increase in the degree to which an inventor was highly-cited (i.e., a “star”) implied 14.2% lower mobility[3]. Similarly, a one-standard-deviation increase in inventor specialization implied 17.3% lower mobility, indicating an immobilizing effect of noncompetes on inventors with specialized skills.

We tested robustness in a variety of ways, displayed in Table 5. MARA was passed in March of 1985, but some time was likely required for news of the change to diffuse, as represented by the aforementioned law review articles that appeared later in the year. We experimented with having *postmara* start in 1985 (Model 6) and 1987 (Model 7) yet found no material differences. Concerned that a limited number of industry categories might understate the industry effects, we decomposed the six NBER categories into 17 categories with no major effects (Model 8, decomposition available from the authors). We also estimated parametric models; the log survival function plot indicated the Gompertz model in which the rate of decline in the hazard increases with time (that is, inventors become less likely

[3] Considering the effect of a one standard deviation change of the independent variable:
14.2% =100(1- $e^{(1-0.0703*2.18)}$.

to move over time). Results from the Gompertz model (Model 9) were likewise similar to those of the Cox model, except for the difference in likelihood scores, which we would expect from the difference between the partial likelihood estimation method used for Cox models and maximum likelihood used for parametric models. Lastly, given that California patents represent 46.3% of those in our sample, we wanted to rule out the possibility that Michigan's apparent decrease in post-MARA mobility was actually driven by an increase in mobility among California inventors, as might be plausible given the growth of the microcomputer industry in the 1980s. We thus repeated the analysis on the subset of inventors who never patented in California (Model 10). This yielded largely similar results, with the exception that the significance of the star inventor interaction dropped to a p value of 0.10.

As described above, we believe that the differences-in-differences study design may ameliorate the effects of Type I and Type II matching errors. Nevertheless, we ran our inventor-matching algorithm at six levels of conservatism and found no meaningful differences in the results (unreported, but available from the authors).

Table 5: Robustness checks for models of intrastate employer mobility of U.S. patented inventors in states that do not enforce non-competes, 1960-2006. All models include cohort indicator control variables.

	Model 6	Model 7	Model 8	Model 9	Model 10
NBERcat_chemical	0.1799***	0.1751***		0.1859***	0.1910***
	(0.0190)	(0.0189)		(0.0190)	(0.0295)
NBERcat_compcomm	0.1613***	0.1600***		0.1645***	0.1930***
	(0.0152)	(0.0152)		(0.0152)	(0.0220)
NBERcat_drugsmed	0.2203***	0.2119***		0.2181***	0.2987***
	(0.0272)	(0.0267)		(0.0269)	(0.0425)
NBERcat_electronic	0.0600**	0.0602**		0.0596**	0.0939**
	(0.0185)	(0.0184)		(0.0185)	(0.0309)
NBERcat_mechanical	0.0068	0.0068		0.0101	0.0857**
	(0.0176)	(0.0175)		(0.0175)	(0.0268)
priorchurn	0.3952***	0.3925***	0.3762***	0.3905***	0.3294***
	(0.0168)	(0.0168)	(0.0169)	(0.0171)	(0.0262)
enforce	0.0760+	0.0795*	0.0878*	0.0744+	0.3983***
	(0.0391)	(0.0390)	(0.0374)	(0.0393)	(0.0389)
university	0.3899***	0.3969***	0.2823***	0.3903***	0.3851***
	(0.0366)	(0.0367)	(0.0383)	(0.0367)	(0.0617)
lnfirmpats	-0.0625***	-0.0627***	-0.0663***	-0.0617***	-0.1150***
	(0.0029)	(0.0029)	(0.0030)	(0.0029)	(0.0049)
inv_entropy	1.9201***	1.9070***	1.6920***	2.1567***	1.6312***
	(0.1222)	(0.1143)	(0.1436)	(0.1141)	(0.1867)
Michigan	0.0664	0.0529	0.0293	0.0447	0.1874*
	(0.0758)	(0.0709)	(0.0710)	(0.0710)	(0.0800)
postmara	-0.1001*	-0.1019*	-0.0050	-0.0104	0.0196
	(0.0473)	(0.0499)	(0.0462)	(0.0482)	(0.0829)
lntotalcites	-0.0327***	-0.0384***	-0.0398***	-0.0205***	-0.0569***
	(0.0066)	(0.0060)	(0.0062)	(0.0062)	(0.0105)
postMARA*Michigan	-0.4166**	-0.4502**	-0.3814**	-0.4197**	-0.4602**
	(0.1486)	(0.1626)	(0.1409)	(0.1588)	(0.1607)
postMARA* lntotalcites	0.1686***	0.2097***	0.1831***	0.1703***	0.1842***
	(0.0127)	(0.0138)	(0.0129)	(0.0133)	(0.0277)
Michigan* lntotalcites	0.0338+	0.0364*	0.0407*	0.0391*	0.0681***
	(0.0176)	(0.0162)	(0.0167)	(0.0168)	(0.0187)
postMARA*Michigan* lntotalcites	-0.0644+	-0.0755*	-0.0657*	-0.0696*	-0.0666
	(0.0330)	(0.0352)	(0.0321)	(0.0351)	(0.0406)
postMARA*inv_entropy	-0.8576***	-0.9888***	-1.1842***	-1.2203***	-0.8971***
	(0.1581)	(0.1593)	(0.1572)	(0.1550)	(0.2573)
Michigan*inv_entropy	-0.9322***	-0.8860***	-0.6166*	-0.8129**	-0.5746+
	(0.2813)	(0.2668)	(0.2648)	(0.2634)	(0.2945)
postMARA*Michigan*inv_entropy	1.4063**	1.5509**	1.2288**	1.3376**	1.2681*
	(0.4734)	(0.4943)	(0.4491)	(0.4809)	(0.5041)
MARA year	1985	1987	1986	1986	1986
# industry controls	6 (NBER)	6	17	6	6
Event-history model	Cox	Cox	Cox	Gompertz	Cox
Exclude California patents	No	No	No	No	Yes
log likelihood	-263127.43	-263057.75	-262948.16	-39970.407	-106251.03

Discussion

The results add to the evidence that the enforcement of noncompetes depress inventor and professional mobility (Gilson, 1999: Stuart and Sorenson, 2003). This paper is the first to our knowledge to apply longitudinal analysis to the question of noncompete enforcement, and the design lessens causality concerns. Further, the analysis distinguishes the greater effect of noncompetes on high-value or "star" contributors from the greater population as well as on "specialist" inventors whose skills are not widely marketable beyond direct competitors.

This evidence has important implications, in light of the large literature that indicates the importance of mobility for knowledge spillovers and entrepreneurship (Stolpe 2002; Agrawal, Cockburn et al. 2003; Breschi and Lissoni 2003). Constraining the flow of people and thus knowledge (Almeida and Kogut 1999), enforcing regions may fail to develop entrepreneurial and technologically dynamic economies. Consistent with Gilson's (1999) arguments, industry growth may be attenuated as startups fail to condense in enforcing regions. The networks of small companies so crucial to Silicon Valley's growth (Saxenian 1994) would be less likely to develop in regions that enforce noncompetes.

These results open up a variety of further research opportunities. Perhaps most promisingly, if noncompetes inhibit mobility within a region, do they also increase emigration from that region? That specialists and stars are immobilized by noncompetes within a region more than other inventors suggests that they may seek new 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 the agglomeration of talent in non-enforcing areas such as Silicon Valley. One obstacle to this line of inquiry with patent data is the difficulty of identifying inventors across state lines, since an identical home address cannot be used to match.

Through our study of this topic we also became aware of anecdotal evidence of what we call "involuntary sabbaticals" as a response to noncompetes. For example, JetBlue founder & CEO David Neeleman was unable to found the now-prominent airline for five years after being dismissed from

Southwest Airlines, who refused to reduce the term of the five-year noncompete agreement he had signed (Wells 2002).[4] Following last year's legal wrangling over Kai-Fu Lee's defection from Microsoft to Google, industry evangelist Vic Gundotra chose not to contest his noncompete when leaving Microsoft for Google. Instead, he decided to take a year off as described in Google's official statement:

"Mr. Gundotra has resigned from Microsoft and entered into an agreement with Google. Though the financial arrangements are confidential, he will not be a Google employee for one year and intends to spend that time on philanthropic pursuits. We are uncertain what precise role he will play when he begins working for Google, but he has a broad range of skills and experience which we believe will be valuable to Google." (Romano 2006)

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

These results also open the question of whether noncompetes influence the day-to-day 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 yet still being subject to a noncompete? If individuals cannot extract the full value of their contributions to the company since they are prevented from exploring their market value through external opportunities, will they in turn be less creative? Critics of these arguments might counter that just as the temporary monopoly conferred by a patent is necessary to induce investment in R&D, the control afforded by noncompetes is necessary for firms to invest in human capital. In the case of patents, however, Lerner and Jaffe (2004) argue that information asymmetries and poor administration of the patent process have combined to hurt, not help, innovation. Rabaut (2006) also reported that employers in Michigan became less enamored with noncompetes over time, because while they appreciated the use of noncompetes as a hiring shield, they began to realize that it also deprived them of a hiring sword. Many firms, he reported, just wanted to compete.

Further research is required to understand the organizational and strategic implications of noncompetes and inventor mobility. For example, will unsanctioned spinoffs place more strategic distance between themselves and their jilted parent firms where noncompetes are enforced? Will this

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

result in less focused clusters in regions that enforce non-competes? The spatial clustering of innovation has been well established (Audretsch and Feldman, 1996) but the influence of noncompetes upon clustering remains unexplored. Might large companies in enforcing regions be less aggressive in pursuing new or disruptive markets if their current employees, who best know the "chinks in the armor" of their current strategy, are prevented from competing after leaving, even after being fired? Or will firms in enforcing regions become more aggressive, because they know that their advantage was fleeting? These questions are central to the strategy, technology, and regional policy literatures.

Conclusion

This work exploited an inadvertent change in Michigan noncompete law in 1985 as a natural experiment, comparing the change in mobility of Michigan inventors to the change in mobility of similar inventors in other states that did not change their enforcement. We found a relative decrease in Michigan mobility of 33.9% once noncompetes began to be enforced, with an additional 14.2% effect for highly-cited inventors and a 17.3% attenuation of mobility for specialist inventors.

Ultimately, and as is often the case surrounding issues of sanctioned monopolies, policy planners must decide when the interests of incumbent firms outweigh those of individual careers and possibly regional development. While much work remains in establishing higher-level connections between, say, noncompete enforcement and economic productivity, we hope to contribute to that debate.

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Appendix A: Interview questions for Michigan labor lawyers

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

Appendix B: Inventor Identification and Matching Algorithms

The data set is based on patent data made available by the National Bureau of Economic Research, Inc. (NBER) for patents granted from January 1, 1963 through December 31, 1997. Patents granted since January 1, 1998 are drawn from the United States Patent and Trademark Office (USPTO) data for patents granted. New data is released and downloaded each week.

Our algorithm builds on work by Fleming, King, and Juda (2006), Singh (2006b), and at the NBER (Trajtenberg, Shiff, and Melamed 2006), with two exceptions. The different matches in the algorithms are partially attributable to the fact that we are only matching inventors who reside in the United States. This frees us to utilize population data and data set of names that are specific to the United States. The second major difference is the absence of the Soundex transforms of inventor names. The Soundex algorithm is useful when errors introduced are errors based on a person's hearing. Soundex is ideal for capturing errors such as the name Geoffrey and Jeffrey. However, all the data is entered into the patent system is through optical recognition software, which introduces errors where characters appear to be very similar visually. Optical recognition software introduces errors such as an 'e' is entered as a 'c', and R is transformed to a K. For this reason, we decided to concentrate our efforts on as much data cleaning as possible to maximize matches, and have chosen not to utilize the Soundex approach.

Prior to running the matching algorithm a pre-matching data set is created. A substantial amount of data cleaning and normalization of the data takes place to maximize the probability that inventors' patents are matched. Every city's spelling checked, each city-state pair is verified, and all state abbreviations are confirmed. Each zip code is verified, and if missing a correct zip code is assigned. Inventors occasionally use a county designation, rather than a city when listing their residence. Each US inventor that used a county name was researched individually to attempt to identify whether the inventor has other patents that provide a city designation. If other patents exist for the inventor within a relevant time frame and the city resides within the listed county, then the pre-matching data is updated.

Inventors often used nicknames, Dan instead of Daniel for example. 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 pulled, and have been manually researched to see if the same inventor appears under his or her full name. If patents are identified containing the full name, then the pre-matching data is updated. Punctuation and foreign characters introduce additional errors, such as ø is replaced by O SLASHED' within a name transforming JøRGENSEN into J.OSLASHED.RGENSEN. We have attempted to identify all possible instances of transformed characters and corrected them. To further normalize the data prior to running the matching algorithm all spaces, accents and punctuation is removed from inventor names.

Assignees frequently appear under a number of variations (for example, AT&T INC, AT&T CORPORATION and AT AND T CORPORATION). The assignee names have been researched manually and names have been uniform where appropriate. Companies that have undergone a merger or acquisition appear under the name of the acquiring firm beginning the date of the official merger transaction. Merger and acquisition data is made available by Worldwide Mergers, Acquisitions, and Alliances Databases in SDC Platinum - Securities Data Company (SDC). In addition to the SDC data, we have extracted all inventor moves from one assignee to a different assignee and research each firm pair to explicitly determine if any merger, acquisition or name changes activity occurred that may indicate a move or exit where a change did not actually occur.

Technology class is another variable used in the matching process; however technology classes are subject to revisions monthly by the USPTO. To ensure that all patents are represented under uniform coding scheme, technology classes are updated monthly based on the US Patent and Trademark Office's most current release of the Current Classification of US Patents CD.

There are a variety of choices in determining what makes a name common and what makes a name rare. It is tempting to use the frequency of the name in the data base as applied in (Trajtenberg, Shiff et al. 2006), however this will introduce a bias toward under-matching US inventors. A name that is very common in Japan, China or India will still be very uncommon in any given US city. 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. Not all names from the 1990 are included in the data set. For Surnames, the most common 88,799 names are included, for female first names the most common 4,275 and for the males the most common 1,219 names. Frequencies are calculated for each name based on a sample of 6,290,251 names over all, and the Cumulative Frequency in percent form is utilized in out calculations. If a name is present in our data set, yet not in the US Census Bureau name lists, then it is assumed to be as uncommon as the least frequently occurring name in the Census Bureau's data set, hence that name is assigned the same cumulative frequency in percent form, that is a value of 100. Middle name frequencies are based on the frequency of middle names in the data set itself, and the cumulative frequency in percent form is the value employed. The most common names contribute the least toward the matching score, and the least common names contribute the most to the name matching score.

The size of the city an inventor resides in greatly influences whether an inventor should match, even when two records share a fairly unusual name. The population associated with each zip code within the US is assigned to the pre-matching data. The population data for each population zip code is from the ZIPCodeWorld's data set. The population distribution is broken into deciles and a score is assigned to each decile category.

Matching Process

The contents of the cleaned, assembled pre-matching data set are as follows:
Inventor Firstname
Inventor Last name
Inventor Middle name
Inventor City
Inventor State (or province)
Inventor Country
Inventor Zip code
Patent Primary Technology Class
Population Decile Score
Assignee Identification Number (if available)
Inventor Last Name Cumulative Frequency
Inventor First Name Male Cumulative Frequency
Inventor First Name Female Cumulative Frequency
Co-Inventors

Each patent is initially assigned its observation number as its inventor identification number (InventorID). Observations that are identical across all the fields in each record, the zip code population is in the bottom 2 deciles, contain a complete middle name and whose last names are not ranked in the top

2000 last names are updated to share the same InventorID. These are records that would receive the maximum (or very near maximum) score in the matching algorithm, and would be matched with certainty. This pre-assignment of the InventorIDs greatly reduces the time required for the matching algorithm to complete.

Two identical data sets are created and each observation in the first data set is joined to all the observations in the second data set sharing the same last name. If the first name in the first dataset is not a subset of the first name in the second data set, or vice versa then the record is dropped and precluded from being a possible match. For example, Tim is a subset of Timothy and would be kept, but Mary is not a subset of Timothy (or vice verse) and would be dropped. Since cumulative percentages are bounded between 1 and 100, all the variables that use the cumulative percentages have scores that are bounded between 1 and 10. For all the observations that remain in the joined file the following scoring is created for each observation pair.

$$\text{First Name Score} = \frac{\text{Min(Male First Name Cumulative Percent, Female First Name Cumulative Percent)}}{10}$$

$$\text{Last Name Score} = \frac{\text{Last Name Cumulative Percent}}{10}$$

$$\text{Middle Name Score} = \frac{\text{Middle Name Cumulative Percent}}{10}$$

Note: If the middle name is missing no points are received toward the Middle Name Score.

Population/Size decile score contribution:
10^{th} Decile = 1 (largest Cities)
9^{th} Decile = 2
8^{th} Decile = 3
7^{th} Decile = 4
6^{th} Decile = 5
5^{th} Decile = 6
4^{th} Decile = 7
3^{th} Decile = 8
2^{th} Decile = 9
1^{th} Decile = 10 (smallest Cities)

If the cities are not the same then,

$$\text{Population Decile Score} = \text{Max(First City Decile Score, Second City Decile Score)}$$

If assignees are the same then the Assignee Score = 9
If the technology classes are the same then Technology Class = 9
If cities are the same or zipcodes[5] are the same then the City Score = 9
If the states or provinces are the same then the State Score = 3

$$\text{CoInventor Score} = \frac{\text{\% of CoInventor Last Names Jointly Occuring in Both Records}}{10}$$

The final matching score is the sum of all the individual scores. If the final score is greater that the predetermined threshold then the inventors are matched, and the two records will share a common InventorID.

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