REGIONAL DISADVANTAGE?
NON-COMPETE AGREEMENTS AND BRAIN DRAIN

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Abstract

We construct inventor career histories using the U.S. patent record from 1975 to 2005 and demonstrate a brain drain among patenting inventors from states that enforce employee non-compete agreements to those that do not. Non-compete enforcement drives away inventors with greater human and social capital although retaining those who are less productive and less collaborative. We address causality-related concerns with a difference-in-differences study design based on an inadvertent reversal of Michigan's non-compete enforcement policy.

Keywords: non-compete agreements, labor mobility, technology policy, regional economics

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1. INTRODUCTION

Why has Silicon Valley become the most entrepreneurial and technologically successful region in the world today? More generally, what are the dynamic sources of "regional advantage" (Saxenian 1994)? Understanding the microfoundations of regional clustering is of interest both to scholars of agglomeration and to policymakers who wish to encourage entrepreneurship. Although natural advantages have been shown to contribute to agglomeration (Ellison and Glaeser 1999), recent evidence suggests that Marshallian mechanisms such as labor pooling have an even greater effect (Rosenthal and Strange 2001; Roos 2005; Ellison, Glaeser, and Kerr 2010).

The benefits of labor pooling are often attributed to the interorganizational mobility of workers, which not only facilitates better job matching (Helsley and Strange 1990) but also encourages individual investment in human capital (Diamond and Simon 1990; Rotemberg and Saloner 2000) given the expanded market for one's expertise and reduced risk of holdup by one's employer. To the extent that these positive externalities are increasing in the size of the labor pool, it is important to understand factors that contribute to the local supply of talent. Yet most theoretical and empirical work on labor pooling focuses on the circulation of workers among firms within a region rather than how those workers came to be in that region in the first place. In their study of cluster formation, Bresnahan, Gambardella, and Saxenian (2001) suggest that in addition to traditional sources of new workers such as local universities, workers can also be acquired from outside the region. Indeed, the literature on interstate migration has identified multiple factors that influence regional relocation, including higher wages (Borjas, Bronars, and Trejo 1992) and distance (Lansing and Mueller 1967). But as Greenwood points out, these antecedents are largely tied to geography or other natural advantages and are thus “almost completely devoid of direct policy implications” (1975:421).

This article explores the implications for labor pooling of a widely-used yet rarely studied aspect of employment contracts: covenants not to compete (hereafter, “non-competes”). Non-competes place restrictions on the sorts of job offers an ex-employee may accept after leaving the firm, usually for a term of 1-2 years. Gilson (1999) has argued that Silicon Valley’s high rates of interorganizational labor mobility are better explained by California’s long-standing refusal to enforce non-competes than by appealing to “culture.” Subsequent empirical work confirms that non-compete agreements constrain the interorganizational mobility of workers within a region (Fallick, Fleischman, and Rebitzer 2007; Marx, Strumsky, and Fleming 2009; Garmaise 2009); however, no work to date has considered whether non-competes might also influence the very supply of talent in a focal region.

In this article, we show that non-competes affect labor market pooling not only by restricting mobility within a region but by redistributing talent *across* regions as well. Although we do not measure agglomeration directly and do not claim to have performed a full welfare analysis, we demonstrate that state sanction of non-competes contributes to a “brain drain” of knowledge workers—at least those that manifest themselves as inventors in patent data—from regions that enforce non-competes to those that do not. Whereas prior brain-drain studies have generally explored the loss of talent from less developed countries to more industrialized nations (Bhagwati and Rodriguez 1975), here we observe a domestic redistribution of talent as in studies of internal migration (Greenwood 1975; Borjas et al. 1992; Boustan et al 2008). Moreover, we find that the brain drain is increasing in human and social capital: non-competes drive away those workers who are most productive and collaborative whereas retaining arguably less attractive workers.

Firms use non-competes in order to protect both trade secrets as well as "goodwill" embedded in customer lists and other intangible assets. Although companies frequently ask employees to sign non-disclosure agreements barring them from sharing trade secrets, violations can be difficult to detect whereas it is more straightforward to determine whether an ex-employee is working at a competitive firm. In addition to protecting proprietary information, non-competes offer firms a number of advantages including employee retention (Fallick et al. 2006; Marx et al. 2009), the ability to pay lower wages (Garmaise 2009), and a reduced threat of competition from new entrants (Stuart and Sorenson 2003). These advantages for firms however are obtained at the expense of individuals' career flexibility, leaving the state sanction of non-competes a controversial policy issue. In 2008 alone, four states reformed their non-compete laws, some restricting the enforceability of non-competes although others expanded the rights of firms to use such contracts.[1] That policymakers continue to come to such varying conclusions regarding non-competes suggests a lack of consensus regarding the regional implications of these employment contracts.

We argue that highly-skilled technical professionals (such as inventors) within the U.S. who live in states that enforce non-competes have incentives to relocate to states where such agreements are not enforced, given the regional fragmentation of enforcement policy in the absence of a federal statute. Employment lawyers often counsel clients subject to non-competes to take jobs in states that do not sanction non-competes; moreover, hiring managers and

[1] 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. China recently added a requirement (PRC Labor Contract Law of 1 January 2008, Article 23) that firms enforcing non-competes against ex-employees compensate them during the term of the agreement.

headhunters alike advertise the benefits to prospective employees of working in a region where they are not subject to non-competes (Marx 2010). Moving to a non-enforcing state in order to avoid a non-compete is facilitated by the "public policy exception" whereby contracts written in other states when they would be contrary to the laws of the focal state. The governing case is *Application Group Inc. v. Hunter Group Inc*., 61 Cal 4th App 881, 72 Cal. Rptr. 2d 73 (1st Distr. 1998), in which an employee of a Maryland firm emigrated to California in order to take a new job. Although the employee had been subject to a non-compete, the California judge refused to enforce the agreement because it violated California law. [2]

For both of these reasons, regional variance in the enforcement of non-competes should give rise to a brain drain at the state level. Further, we argue that this effect should be amplified for inventors who are more productive and more collaborative. Limited job mobility within a state that enforces non-competes is likely to entail higher opportunity costs for more productive inventors, with their past track record also making them more visible to and hence likely to be recruited by out-of-state firms. Likewise, those with significant past collaborative ties might have greater awareness of out-of-state opportunities through their professional networks, and hence be more likely to emigrate to the best available option even outside their original state. Interestingly, both of these kinds of elite knowledge workers are probably exactly the kind of talent a state would normally be particularly interested in retaining.

[2] 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. Similarly, in *Advanced Bionics Corp. v. Medtronic Inc.*, 59 P. 3d 231 (Calif. 2002) (Stulz), although the plaintiffs obtained a favorable ruling from a Minnesota court regarding the enforceability of an ex-employee's non-compete agreement, the California Supreme Court stated that it did not see why "any Minnesota judgment [would be] binding on the parties" then resident in California.

We present analysis in support of our arguments based upon the U.S. patent record from 1975-2005. One empirical approach for such analysis would be to demonstrate a cross-sectional pattern that inventors in states that allow enforcement of non-competes are more likely to emigrate, and that this emigration is disproportionately weighted towards moving to non-enforcing states vs. other enforcing states. Although such cross-sectional patterns do indeed hold in our data (as reported in the Appendix), attaching a causal interpretation to these patterns is difficult. In order to more directly get at the issue of determining causal effects, we employ a difference-in-differences model that exploits a natural experiment arising from an inadvertent reversal of Michigan's non-compete enforcement policy. In particular, we examine changes in emigration patterns from Michigan around the time of this policy reversal, comparing them against a baseline of emigration trends for ten other states that continued to proscribe non-competes throughout the period of our study.

2. EXPLOITING MICHIGAN'S POLICY REVERSAL AS A NATURAL EXPERIMENT

Non-compete enforcement in Michigan had long been prohibited by Public Act No. 329 of 1905: "*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 Act prohibited the use of non-competes until 1985, when Michigan Antitrust Reform Act (MARA) was passed. MARA led to the repeal of numerous laws and acts, including Public Act No 329 which also addressed antitrust provisions. But lawmakers were apparently unaware that by so doing, they had also lifted the longstanding ban on non-competes. More than twenty pages of legislative analysis by both House and Senate subcommittees in Michigan fail to mention non-competes as a motivation for MARA (Bullard 1983).

Further evidence for the inadvertent nature of Michigan's non-compete reversal is found following the enactment of MARA. Although we could not locate any discussion of Michigan's non-compete policy in law journals just prior to 1985, in the months following the reversal multiple articles appeared in publications for legal practitioners (Alterman 1985; Levine 1985; Sikkel and Rabaut 1985). These articles highlighted the new enforceability of non-competes in the state, news which may have spread among law firms, which would have then informed their clients in hopes of generating contractual or prosecutorial work (Bagley 2006). These developments suggest that the legal community was not aware of the potential for the law to be reversed but learned of the reversal quickly thereafter.

Moreover, less than two years after the passage of MARA, the Michigan legislature amended MARA section 4(a), effective retroactively to the enactment of MARA. Importantly, the reasonableness doctrine *did not reinstate* the pre-MARA ban on non-compete agreements but merely provided some limitations regarding the scope and duration of non-competes, as is common in most states that permit their enforcement. This post-hoc, retroactive amendment suggests that the legislature later realized it had repealed the non-compete ban without fully considering its implications. Indeed, both House and Senate legislative analyses of the section 4(a) amendment to MARA state that a key motivation for the amendment was "to fill the statutory void" (Trim 1987a, b).

Interviews with Michigan labor lawyers active at the time of MARA are supportive of an interpretation of the repeal as inadvertent. Robert Sikkel (2006) reported, *"There wasn't an effort* *to repeal [the ban on] 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."* His account was independently corroborated by Louis Rabaut (2006): *"There was no buildup, discussion, or debate of which I was aware—it was really out of the blue. As I talked to others, this appeared to be a rather uniform reaction. I have never been able to identify any awareness—and I examined this at the time—that this was a conscious or intentional act. It was part of the antitrust reform and it may have been overlooked. I am unaware of anyone that lobbied for the change."*

Taken together, these pieces of evidence indicate that the MARA policy reversal was an unanticipated and exogenous event that provides the opportunity for a natural experiment as far as the change in non-compete enforcement policy is concerned (also see Marx et al. 2009 for a similar argument). Even if there had been secret lobbying contributing to the non-compete reversal, the change would still have been exogenous to the patent holders who are the subjects of this study. The impact of introducing non-compete enforcement on interstate emigration can therefore be identified using a difference-in-differences approach. If non-compete enforcement drives increased emigration, there should have been an increase in emigration from Michigan (before versus after the MARA policy change) over and above the prevailing temporal pattern of emigration rates in states that continued not to enforce non-competes throughout the period (the control group). To avoid confounding effects of the MARA reform upon the career patterns of inventors, only those inventors active before MARA are included in the analysis.

3. DATA AND METHODS

We analyze patterns of interstate mobility by U.S. knowledge workers using the U.S. patent database, heuristically identifying patents that belong to the same person in order to construct career histories for 540,780 patenting inventors from 1975 through 2005 (Lai, D'amour, and Fleming 2009). In other words, we use patent data not to measure innovation but rather—as several others have—to establish employment histories (Almeida and Kogut 1999, Tratjenberg, Shiff, and Melamed 2006; Agrawal, Cockburn, and McHale 2006; Breschi and Lissoni 2009). Clearly, not all innovative activity is captured by patent records; nonetheless, patenting inventors represent an important category of skilled workers involving the sorts of trade secrets firms seek to protect using non-competes.[3] And although the patent database does not provide exhaustive coverage of all spans of employment, it nonetheless offers an unusual opportunity to track hundreds of thousands of individuals over long periods of time.

Each patent typically contains each inventor's hometown and employer, making it possible to reconstruct work histories and co-authorship networks for numerous individuals. Because the U.S. Patent and Trademark Office does not require applicants to supply a unique identifier, we build upon heuristic methods (Trajtenberg, Schiff and Melamed 2006) to identify which patents belong to a particular inventor (for the full algorithm, see Lai et al. 2009). Tracking geographic location across successive patents for each inventor allows us to identify instances of inter-state mobility.[4] Because the exact timing of a move cannot be determined, we use the midpoint of the time window between the last patent in the former state and the first

[3] If patents and non-competes were substitutes, it would be inappropriate to use patent data in this study. Yet Marx et al. (2009) found little difference in patenting rates once Michigan began to enforce non-competes.

[4] Given that we can detect mobility only in instances where an inventor successfully files for a patent both before and after a move, we do not observe all moves involving patenting inventors. Moves involving patenting inventors are, in any case, only a subset of all moves involving skilled workers (whether patenting or not). This caveat is important to bear in mind in interpreting our findings, a point we return to in the discussion section.

patent observed after the move to a new state to estimate the year of the move. By construction, an inventor's first patent cannot indicate a move. Analysis is therefore restricted only to the inventor's subsequent patents; inventors with only one patent are thus excluded.

We identify *emigration*—i.e. workers leaving the state when they change jobs— by a pair of patents belonging to the same inventor where neither the assignees (the owner of the patent, typically the firm for which the inventor works) nor the states match. Movement from employment to non-employment (as measured by a subsequent patent that lacks an assignee) is considered, as firms can enforce non-competes against ex-employees who strike out on their own. Changing from self-employment to employment, however, is not considered, as individuals will not ask themselves to sign a non-compete.

The explanatory variable is the interaction of a pair of indicators for Michigan residence and the post-MARA time period. Time-varying control variables include annual indicators, the number of patents the inventor had been granted in the pre-MARA period (logged), the number of days between the two patents (logged) and whether the inventor had previously emigrated. We also account for characteristics of the prior patent using indicators for six top-level technical classifications to which the prior patent was assigned (Hall, Jaffe, and Trajtenberg 2001) and the logged number of patents belonging to the firm to which the prior patent was assigned (as a proxy for firm size). Given the over-representation of the automotive industry in Michigan (Singleton 1992), we include an indicator for automotive patents as well.

Matching

We implement Coarsened Exact Matching (Iacus, King, and Porro 2009; see Azoulay, Graff Zivin, and Wang 2010 for a recent application) to improve covariate balance between the treatment group of Michigan inventors and the control group of inventors in states that continued not to enforce non-compete agreements. This technique non-parametrically segments the joint distribution of the covariates into a finite number of strata; controls are then selected only from strata that include observations in the treatment group. Following matching, coarsened variables are discarded; the original treatment and control observations from matched strata are retained and analyzed as described below. Our covariate set for matching included the inventor's patenting rate, the logged number of patents belonging to the firm to which the prior patent was assigned, the interval between the inventor's patents, the inventor's first patenting year, and the percentage of an inventor’s patents that were automotive. All of these are measured on a pre-MARA basis in order to ensure that the matching criteria were not influenced by the policy reversal.

Rather than assign arbitrary cut-points, we relied on the Coarsened Exact Matching implementation in Stata to automatically determine the number and boundaries of the matching “bins” in order to optimize an objective function based on Scott’s rule (Scott 1992). One concern might be that an automated matching algorithm would pick too coarse a set of cut-points that do not reflect sufficiently stringent matching. However, this is not the case given the high number of bins generated: for patenting rate, 122; for firm size, 45; for patenting interval, 217; for automotive, 170. (Discrete variables were matched exactly.) Table I gives descriptive statistics and correlations for the matched observations.[5]

[5] Stringent matching naturally comes at the cost of fewer treated observation being matched to control-group observations (50.1%). To ensure that our key findings are not sensitive to this, we also carried out analysis with less stringent matching that forced only 20 bins per continuous variable and matched 95.7% of treated observations; the subsequent analysis revealed similar results. As detailed in the robustness models later, the results remain similar even in an analysis that skips matching altogether and employs all observations.

Models

We estimate a logistic model of the likelihood that a given patent *i* indicates that its inventor *j* emigrated. Letting E_{ij} indicate emigration from one state to another, $\boldsymbol{X}_{ij}$ a vector of covariates of the patent, $\boldsymbol{Z}_i$ a vector of time-independent covariates of the inventor, and $\boldsymbol{W}_{it}$ a vector of time-varying covariates of the inventor, the estimation equation is therefore $Pr(E_{ij} = 1) = e^{(\beta \boldsymbol{X}ij + \gamma \boldsymbol{Z}i + \lambda \boldsymbol{W}it)} / (1 + e^{(\beta \boldsymbol{X}ij + \gamma \boldsymbol{Z}i + \lambda \boldsymbol{W}it)})$. Each patent is taken as an observation, with the regression analysis reporting robust standard errors clustered by inventor to account for non-independence of observations from the same inventor. Observations are weighted appropriately based on the number of matched control observations found for each focal observation. All models are estimated using Stata 10.

4. RESULTS

Descriptive data in Panel A of Table II illustrates a brain drain out of Michigan following the 1985 MARA policy reversal: During a symmetric window from 1975-1996 surrounding the 1985 policy reversal, the rate of emigration grew faster in Michigan (0.96% to 1.66%) than in states that did not enforce non-competes (0.73% to 1.06%). The relative risk of post-MARA emigration was 1.72 in Michigan, 18.8% higher than in states that continued not to enforce non-competes (where the relative risk of post-MARA emigration was 1.45).

This effect is also obtained via multivariate logistic analysis. Table III assesses the impact of the policy reversal in a series of progressively longer intervals surrounding MARA, initially just 1985 and 1986 but eventually the largest possible symmetric window given our data source: 1975-1996. To be sure, the impact of the policy reversal is not immediately obvious; given the inadvertent nature of the policy reversal and the time likely taken for news to diffuse, this is not entirely surprising. But once the effect sets in for 1983-1988, it stays stable as the window widens. We further analyze the widest window, in column (6), to examine the extent to which the coefficient on the interaction of Michigan and post-MARA suggests that the policy reversal substantially increased the likelihood of emigration not just as a statistical finding but also in terms of the economic magnitude of the effect.

Following Greene (2009), we assess the magnitude of the interaction effect by calculating the predicted probability of emigration for various values of the explanatory variables, holding other covariates at their means. As both of the variables in our interaction term are dichotomous, instead of constructing a graph we compute the change in relative risk of emigration for Michigan inventors using predicted probabilities from the regression table, essentially reconstructing the components of Table II, Panel A from the regression model. From column (6) of Table III, the predicted probability of emigration for non-Michigan inventors is 0.04% before MARA and 0.07% thereafter. Similarly, the predicted probability of emigration for Michigan inventors is 0.04% before MARA and 0.31% afterward. Thus the relative risk of post-MARA emigration versus pre-MARA emigration is 7.24 for Michigan inventors and 1.58 for non-Michigan inventors. Comparing these two numbers (which themselves represent changes in emigration for each of the two groups over the time periods), the change in relative risk of emigration for Michigan inventors is 256% larger than for non-Michigan inventors.

Robustness tests

In Table IV, we subject the brain drain result to a number of additional tests. (Column (1) repeats column (6) from Table III to facilitate comparison.) We begin by examining whether the effect is driven primarily by migration to California. The inclusion of California in the control group might bias the findings towards our expected results for at least three reasons. First, given the state's extensive landmass, California might offer disproportionately many relocation opportunities. Second, given that California's Business and Professions code Section 16600 is the longest-standing prohibition against non-compete enforcement (arguably as strict as Michigan's Public Act 305 of 1905 yet dating back to 1872 - see Gilson 1999), Michigan inventors seeking jobs elsewhere might have particularly targeted California rather than the emigration patterns being more general. Third, the entrepreneurial dynamics of Silicon Valley may have been attractive to many of the inventors in this study irrespective of the non-compete issue. But the model reported in column (2) shows that the brain drain finding for Michigan is not driven primarily by California. In this model, we exclude all emigration to California, which dramatically reduces the number of observations. Nonetheless, statistical significance on the key interaction term is maintained and with a slight increase in magnitude. In column (3), we relax the constraint of Coarsened Exact Matching and employ all patent data from 1975 through 1996, with qualitatively similar results.

Columns (4-8) establish the uniqueness of the brain drain result using a series of placebo regressions. Column (4) addresses a key alternative explanation: that what appears to be a brain drain driven by non-competes could be an artifact of larger patterns of migration. We rule this out by changing our dependent variable from interstate moves coincident with job changes to interstate moves within the same firm—in other words, while being transferred by one's employer to an office in another state. Aside from binding employees to their employers, the chief impact of post-employment non-compete agreements is on those who change jobs; accordingly, we would not expect non-competes to affect those who remain with their current employer. In column (4), we indeed find no evidence that Michigan inventors were more likely to be transferred across state lines by their employers following the policy reversal.

A related concern might be that migration to the states where non-competes were unenforceable was a more general phenomenon not driven by anything that happened in Michigan. Although we have partially addressed this concern in column (2) by excluding California from the analysis, it is nonetheless possible that other states in the control group acted as “magnets” for emigrating inventors. We rule out this explanation in column (5) by replacing the control group with its complement: instead of measuring emigration out of Michigan to states that continued not to enforce non-competes, we estimate the likelihood of emigration to the 39 states that consistently enforced such agreements during the window from 1975 through 1996. We find no evidence of increased emigration out of Michigan to enforcing states; in fact, weak evidence in column (5) suggests that Michigan inventors became less likely to emigrate to other enforcing states once Michigan began to enforce non-competes.

Next we address the concern that the brain drain, although not epiphenomenal with overall patterns of interstate migration, may be due to a general exodus from the state of Michigan and not to non-compete policy in particular. In column (6), instead of measuring emigration out of Michigan we treat inventors in Ohio as the experimental group. Like Michigan, it is a medium-sized Midwestern state that experienced a declining economy in the later 1980s and early 1990s. If the brain drain were merely a result of general migration patterns, we would expect to see Ohio inventors likewise moving to non-enforcing states. But again, no statistically-significant evidence of a MARA-coincident brain drain is obtained. We similarly do not find statistically-significant effects for other states including Illinois, Pennsylvania, and New York.

Columns (7) and (8) assess the importance of the timing of the MARA policy reversal in 1985 in order to address the potential concern that the labor flows observed in this regression are coincident with longer-term transfers of talent from Michigan to the control states and have little to do with the MARA policy reform of 1985. We perform two placebo regressions, one where the policy reversal takes place in 1984 and one where it takes place in 1986. Although column (7) shows a positive coefficient on the interaction term, it is considerably smaller in magnitude than in column (1) and lacks statistical significance at conventional levels. Moving the date of MARA in column (8) ahead one year, to 1986, produces weak evidence of a brain drain, not inconsistent with our general result given that news of the inadvertent policy reversal may have taken time to diffuse. Unreported placebo regressions placing the date of the MARA reform further from 1985 returned even weaker results.

Finally, we address the question of whether emigration out of Michigan might have been offset by immigration into the state. In column (9), we instead model moves *to* a focal state in order to test whether Michigan's adoption of non-compete enforcement might simultaneously have lured out-of-state inventors and thus cancelled out the brain-drain effect. Consistent with the dyad models, we find no statistically-significant offsetting immigration trend into Michigan following MARA, indicating that the inadvertent imposition of non-compete enforcement in Michigan was responsible for a net brain drain of patenting inventors.

Moderating effects: productivity and connectedness

In order to examine whether the effect of non-competes is greater for inventors with greater human and social capital, we modeled additional inventor-level interactions. Descriptive inventor-career data in Panel B of Table II show that the relative risk of post-MARA emigration by more productive inventors—i.e., those with more than the median number of patents prior to the policy reversal—was 107.8% higher in Michigan than elsewhere. By contrast, the relative risk of post-MARA emigration by Michigan inventors at or below the median number of patents was 20.3% *lower* than their peers in states that continued not to enforce non-competes. (An alternate measure of citations returned similar though weaker results). More productive workers might perceive greater constraint from non-competes; given their higher opportunity costs, they would therefore be more motivated to seek employment in less restrictive regions. Moreover, such inventors should be more attractive to out-of-state employers and are thus more likely to be recruited. Consequently, workers with higher levels of human capital may be at once more eager and more able to emigrate whereas lower-value workers are kept at their jobs - and thus in the region - by non-compete agreements.

Table II, Panel B also illustrates increased (decreased) emigration by inventors with more (less) social capital. It shows that the relative risk of post-MARA emigration for those within extended co-authorship networks was 153.8% higher in Michigan than in the control states. (As with the above measures of patenting, extended co-authorship networks were measured strictly on a pre-MARA basis.) By contrast, the relative risk of post-MARA emigration was 29.0% lower for those without extended networks. (A simple measure of authorship degree returned similar though weaker results). We model extended networks with an indicator of co-authorship linkage into the largest connected component (the “Largest National Component,” hereafter LNC) in the United States. This component includes, by a path of any length, 42.2% of all 1975-1985 inventors across the United States (median path length=16, longest path length=64).

The explanatory power of the LNC is consistent with the "strength of weak ties" argument (Granovetter 1975); in contrast to direct and immediate social relationships, which provide higher fidelity but often redundant information to the focal inventor, extended networks provide non-redundant information regarding employment opportunities, including with firms in other states. Inventors with greater out-of-state networks may also be more aware of differences in non-compete enforcement among states, knowledge that might be particularly valuable considering that 45.5% of the respondents in a 2008 IEEE survey were not aware that some states including California do not enforce non-competes (Marx 2010). That said, we doubt that the influence of the LNC is fully causal; instead, the most productive and collaborative inventors unknowingly self-select into the LNC (as the full extent of the indirect co-authorship networks could only be known upon publication of the patent or patent application in later years), and are surely unaware of the full extent of their extended co-authorship network.

For multivariate analysis, the count of the number of pre-MARA patents is interacted with the Michigan indicator, the post-MARA indicator, and their interaction. The three-way interaction captures whether the effect of the policy reversal was increasing in the number of pre-MARA patents. Another indicator, similarly interacted, indicates whether a given inventor was a member (before the MARA policy reversal) of the LNC. In Table V, column (1) again repeats the baseline results from column (6) of Table III. The covariates for productive and collaborative inventors are entered separately in columns (2) and (3). Weak statistical significance on the three-way interaction term in column (2) does not support the argument that more highly productive inventors were more likely to emigrate from Michigan (as suggested by Panel B of Table II). Column (3), however, does suggest that inventors in the largest national component were more likely to emigrate when subject to non-competes. Moreover, in column (4) this result survives the inclusion of the three-way interaction for productivity, indicating that the relationship between membership in the largest national component and increased emigration is not simply an artifact of having patented more frequently.

5. DISCUSSION

Drawing on a difference-in-differences model of interstate mobility following an inadvertent policy reversal, we have shown that employee non-compete agreements encourage the migration of workers from regions where such contracts are enforceable to regions where they are not. Robustness and placebo regressions suggest that this brain drain is not simply an artifact of westward migration patterns to California or of a more general exodus from Michigan. Moreover, this pattern is amplified for workers with higher levels of human and social capital, stripping enforcing regions of some of their most valuable knowledge workers while retaining those of lesser value.

To the extent that one can draw normative conclusions from the above findings, policymakers who sanction the use of non-competes could be inadvertently creating a potential regional *disadvantage*. From a regional policymaker's perspective, the free flow of particularly high-ability talent to the best opportunities seems beneficial as long as it occurs locally (Saxenian 1994), whereas such talented workers who take out-of-state jobs are a loss to the region. Regions that choose to enforce employee non-compete agreements may therefore be subjecting themselves to a domestic brain drain not unlike that described in the literature on international emigration out of less developed countries (Bhagwati and Rodriguez 1975).

Although some have argued that emigration neither harms nor helps non-emigrants as emigrants take with them merely their own marginal product (Grubel and Scott 1966), others have argued that the loss of skilled workers may generate negative externalities. For example, Rotemberg and Saloner (2000) note that one advantage of clustered economic activity is an increased incentive for individual workers to invest in their human capital. Indeed, Garmaise (2009) shows that workers invest less in their own human capital where non-compete agreements are enforced more aggressively. Loss of some of the most collaborative and productive inventors should be particularly troubling, given the disproportionate importance of "star" scientists and engineers in the creation of knowledge and the founding of entrepreneurial firms (Zucker and Darby 1988). If the relocation dynamics of elite inventors persist over time—cross-sectional analysis of emigration patterns for enforcing vs. non-enforcing states in the appendix indicates this is true—non-enforcing regions might enjoy the accumulation of top talent whereas regions that enforce non-competes experience an exodus of expertise. Figure I suggests this may indeed be the case: not only is the share of patenting inventors growing over time in non-enforcing states, but the likelihood of inventors being located in non-enforcing states is increasing in their count of forward citations.

We do not, however, claim to have directly linked non-compete enforcement to agglomeration; in fact, we have not even measured the latter explicitly. Our aim here has been to show that the non-compete enforcement can diminish the supply of skilled labor—a necessary condition for labor pooling, which has in turn been shown to be a key mechanism underlying agglomeration (Rosenthal and Strange 2001; Ellison et al. 2009). Coupled with earlier studies (Fallick et al. 2007; Marx et al. 2009), this work suggests that enforcement of non-compete agreements might act as a brake on labor pooling in two ways. First, regions that allow firms to enforce non-compete clauses against ex-employees drive some of their most highly valued skilled workers out of the region, decreasing the local supply of talent. Second, the inter-organizational mobility of those workers who remain in the region is lower when non-competes are enforced. Given the role of labor pooling as a microfoundation of agglomeration, we should therefore expect more clustering in regions such as Silicon Valley where non-competes are unenforceable.

Although we see this study as a useful step in the right direction, we advocate caution in developing normative prescriptions. Although patent data allow us to follow the careers of a large number of individuals over time, we probably do not observe a significant fraction of the inter-state moves given that only instances of mobility involving an inventor patenting before as well as after a move can be detected. Moreover, because patent holders are in turn only a subset of the skilled worker population, we have not unambiguously established that the brain drain finding applies to the population as a whole. For example, there is a possibility that the brain drain we observe for patenting individuals gets replenished by other skilled workers who have not (yet) patented and migrate in the opposite direction. However, given that patenting inventors are likely to be of above-average ability relative to the overall population, our results are still indicative of a brain drain in the sub-population of relatively high-ability skilled workers, even if this does not necessarily imply brain drain in the population as a whole. This interpretation is consistent with our more nuanced finding that, even amongst the patenting individuals, the ones with higher levels of human and social capital seem most likely to move.

In the end, however, understanding the overall welfare effects in a way that facilitates unambiguous normative conclusions requires an explicit general equilibrium framework capturing all possible implications of non-compete enforcement. Such an approach would need to explicitly model how non-competes influence the assumed processes through which skilled workers are "born" as well as how they make the emigration choice, while also capturing the processes through which firms decide to create new jobs in alternate locations and match skilled workers to these jobs.

The impact of non-compete agreements on individuals and firms needs to be further explored in order to perform a full welfare assessment of non-competes. For example, from a firm's perspective, non-competes ease the challenge of retention and decrease labor costs, but they may depress R&D investment (Garmaise 2009), and it remains unknown if they also increase the difficulty of hiring new and specialized talent. In general, the strategic advantages and disadvantages of non-competes remain unclear both theoretically and empirically. Individuals faced with non-competes may react in a variety of ways, such as changing their technical focus. This may be a loss for the region if inventors abandon expertise in important areas, but such could also represent a net gain if changing fields engenders greater creativity. However, if the best inventors leave a region after they have identified promising breakthroughs, most local benefits of non-competes may be lost to regions that prohibit enforcement. Given recent and ongoing policy debates, we see answering such questions as an important next step.

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Table I: Descriptive statistics for difference-in-differences analysis of domestic emigration, 1975-1996.

		Mean	Stdev	Min	Max	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1)	patent indicates emigration from previous state	0.002	0.046	0.000	1.000	1.000									
(2)	days since last patent (ln)	5.251	2.239	0.000	8.995	0.046	1.000								
(3)	inventor had emigrated previously	0.007	0.081	0.000	1.000	0.057	-0.027	1.000							
(4)	prior employer's number of patents (ln)	3.297	2.172	0.000	8.673	-0.013	-0.182	0.018	1.000						
(5)	inventor's pre-MARA patenting rate	0.512	0.284	0.134	1.684	-0.016	-0.175	-0.010	0.195	1.000					
(6)	auto industry	0.013	0.111	0.000	1.000	0.000	0.014	-0.003	-0.010	-0.027	1.000				
(7)	Michigan	0.333	0.471	0.000	1.000	0.005	0.019	-0.056	0.098	-0.016	0.076	1.000			
(8)	post-MARA	0.403	0.490	0.000	1.000	-0.007	-0.041	0.076	0.063	0.169	-0.003	-0.071	1.000		
(9)	number of pre-MARA patents	1.294	0.635	0.693	4.043	0.000	-0.108	-0.001	0.156	0.274	-0.003	0.106	-0.211	1.000	
(10)	in largest national component	0.180	0.384	0.000	1.000	0.000	-0.115	0.002	0.482	0.248	-0.026	0.068	0.009	0.271	1.000

Notes: Observations are restricted to those inventors in Michigan as well as states that continued not to enforce non-competes. Observations are matched using Coarsened Exact Matching. *n*=38,681.

Table II: Domestic emigration from Michigan vs. baseline states that do not enforce non-competes.

Panel A: Comparison for all inventors.

	pre-MARA	post-MARA	relative risk
Michigan	0.96%	1.66%	1.719
non-Michigan	0.73%	1.06%	1.447
Michigan % increase over non-Michigan			*18.8%*

Panel B: Comparison for highly productive and more collaborative inventors.

PATENTS (median=3)

	median and below				above median		
	pre-MARA	post-MARA	relative risk		pre-MARA	post-MARA	relative risk
Michigan	0.97%	1.18%	1.217	Michigan	0.95%	2.36%	2.488
non-Michigan	0.67%	1.02%	1.527	non-Michigan	0.94%	1.12%	1.198
Michigan % increase over non-Michigan			*-20.3%*	*Michigan % increase over non-Michigan*			*107.8%*

LARGEST NATIONAL COMPONENT (LNC)

	not included in LNC				member of LNC		
	pre-MARA	post-MARA	odds ratio		pre-MARA	post-MARA	odds ratio
Michigan	0.89%	1.04%	1.174	Michigan	1.21%	3.31%	2.731
non-Michigan	0.62%	1.03%	1.653	non-Michigan	1.06%	1.14%	1.076
Michigan % increase over non-Michigan			*-29.0%*	*Michigan % increase over non-Michigan*			*153.8%*

Notes: First, the comparison is done for inventors with an above-median number of patents vs. those at or below the median. Second, the comparison is done for those in the largest connected "national component" vs. those not in the component. N= 210,151 (includes data not matched by coarsened exact matching; matched data analysis returns stronger results).

Table III: Varying time windows for difference-in-differences logistic regressions on domestic emigration.

	(1)	(2)	(3)	(4)	(5)	(6)
window surrounding MARA	*1985-1986*	*1983-1988*	*1981-1990*	*1979-1992*	*1977-1994*	*1975-1996*
Michigan * post-MARA	17.7095	3.5486**	2.7578***	1.5546*	1.4307**	1.5206**
	(0.0000)	(1.3146)	(0.8026)	(0.6053)	(0.5092)	(0.4752)
Michigan	-1.0035	-0.4905	-0.2022	0.0864	0.1162	0.0256
	(0.8182)	(0.5749)	(0.4065)	(0.3703)	(0.3344)	(0.3223)
post-MARA	-13.9673***	-3.1410*	-1.8947	-1.2416	0.5439	0.4629
	(2.3934)	(1.2627)	(1.2975)	(1.4686)	(0.8290)	(1.1680)
days since last patent (ln)	1.1034*	0.7152*	0.8658***	0.8196***	0.8616***	0.8773***
	(0.5059)	(0.2876)	(0.2384)	(0.2061)	(0.1710)	(0.1622)
inventor had emigrated previously		1.5412	2.8531***	2.9092***	3.3181***	3.2764***
		(1.3707)	(0.8636)	(0.6541)	(0.4476)	(0.4532)
prior employer's number of patents (ln)	0.0348	0.0002	0.0185	-0.0254	-0.0490	-0.0889
	(0.1418)	(0.1061)	(0.0786)	(0.0737)	(0.0589)	(0.0570)
inventor's pre-MARA patenting rate	0.3413	-0.1322	0.3681	0.6034	0.3145	0.3875
	(2.2019)	(1.5481)	(1.1217)	(1.0202)	(0.8243)	(0.7471)
auto industry			0.7679	0.4435	-0.3377	-0.4151
			(1.0214)	(1.0596)	(1.2728)	(1.1969)
Constant	-13.6354**	-9.4744**	-11.9281***	-13.0512***	-11.8171***	-12.2804***
	(4.7101)	(3.6139)	(3.0518)	(2.7139)	(2.1357)	(2.0568)
Observations	2265	6285	10038	15499	20714	23348

Notes: The dependent variable is the likelihood that a given patent indicates domestic emigration, for U.S. inventors in Michigan or other non-enforcing states, 1975-1996. All models include year, industry, and first-patent-year cohort indicators; data are matched by Coarsened Exact Matching. Robust standard errors are in parentheses, clustered by inventor.

+ Significant at the 10% level; * significant at the 5% level; ** significant at the 1% level.

Table IV: Robustness checks for difference-in-differences logistic regressions of domestic emigration.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	repeats Col. 6 of previous table	*Robustness test: excludes California*	*Robustness test: without matching*	*Placebo: dependent variable is within-firm transfers*	*Placebo: control group is enforcing states*	*Placebo: treatment state is Ohio*	*Placebo: MARA date is 1984*	*Placebo: MARA date is 1986*	*Immigration into the state*
Michigan * post-MARA	1.5206**	1.8777**	0.3763+	-0.6289	-0.3093+	0.5102	0.7129	0.8192+	0.0096
	(0.4752)	(0.6503)	(0.2081)	(0.4750)	(0.1583)	(0.4229)	(0.5164)	(0.4938)	(0.2366)
Michigan	0.0256	-1.4908**	0.3131*	0.4237	0.1571	1.1641***	0.1363	0.2821	-1.0101***
	(0.3223)	(0.4697)	(0.1406)	(0.3088)	(0.1007)	(0.2451)	(0.3623)	(0.3089)	(0.1392)
post-MARA	0.4629	0.1064	0.0688	0.0437	-1.0642*	-0.8558	-0.0840	-0.6709	-2.0047***
	(1.1680)	(1.2263)	(0.4689)	(0.8645)	(0.5260)	(0.7526)	(1.2330)	(0.8168)	(0.5453)
days since last patent (ln)	0.8773***	0.7899***	0.9211**	0.2728***	0.9510***	0.8672***	0.8252***	0.8705***	0.8792***
	(0.1622)	(0.2092)	(0.0692)	(0.0684)	(0.0745)	(0.1686)	(0.1587)	(0.1520)	(0.0980)
inventor had emigrated previously	3.2764***	3.0940**	3.2559**	3.2363***	-0.2656	0.5574	3.2492***	3.4775***	-0.2549
	(0.4532)	(1.0065)	(0.1709)	(0.4307)	(0.4097)	(0.6518)	(0.4727)	(0.3946)	(0.5033)
prior employer's number of patents (ln)	-0.0889	-0.2648***	-0.0738**	0.1980**	2.1492***	2.1633***	-0.0569	-0.0675	2.5072***
	(0.0570)	(0.0793)	(0.0227)	(0.0622)	(0.1614)	(0.2881)	(0.0573)	(0.0582)	(0.1892)
inventor's pre-MARA patenting rate	0.3875	1.0686	0.3175	-0.7394	-0.1498***	-0.0984*	0.2501	0.2875	-0.0108
	(0.7471)	(1.1426)	(0.2028)	(0.6356)	(0.0208)	(0.0482)	(0.7794)	(0.7235)	(0.0336)
auto industry	-0.4151		-0.9171	1.0404	0.3993***	0.2436	-0.2469	-0.3672	0.3416+
	(1.1969)		(0.7103)	(0.9540)	(0.1208)	(0.2520)	(1.1496)	(1.1752)	(0.1767)
Constant	-12.2804***	-10.5657***	-25.6651	-6.4489***	-9.9012***	-10.2019***	-12.4515***	-11.5036***	-9.0346***
	(2.0568)	(2.0581)	(0.0000)	(1.4483)	(0.6557)	(1.4806)	(2.1842)	(1.7987)	(0.9315)
excludes California	no	yes	no	no	no	no	no	no	no
block-bootstrap	no	no	no	no	no	no	no	no	no
Coarsened Exact Matching	yes	yes	no	yes	yes	yes	yes	yes	yes
control group = enforcing	no	no	no	no	yes	no	no	no	no
DV =	emigration	emigration	emigration	internal xfer	emigration	emigration	emigration	emigration	immigration
treatment state	Michigan	Michigan	Michigan	Michigan	Michigan	Ohio	Michigan	Michigan	Michigan
MARA year	1985	1985	1985	1985	1985	1985	1984	1986	1986
Observations	23348	12208	174086	26609	50710	24494	23080	24732	29663

Notes: Observations are for patenting U.S. inventors in Michigan or other non-enforcing states, 1975-1996. For most models, the dependent variable is whether a given patent indicates domestic emigration; the experimental group is Michigan; the control group is states that did not enforce non-competes throughout the period of the dataset; the MARA reform is assumed to have taken place in 1985; and Coarsened Exact Matching is used. All models include year, industry, and first-patent-year cohort indicators. Robust standard errors are in parentheses, clustered by inventor. The auto-industry indicator is dropped in Column 2 due to perfect prediction as those in the auto industry who emigrated from Michigan went exclusively to California.
+ Significant at the 10% level; * significant at the 5% level; ** significant at the 1% level.

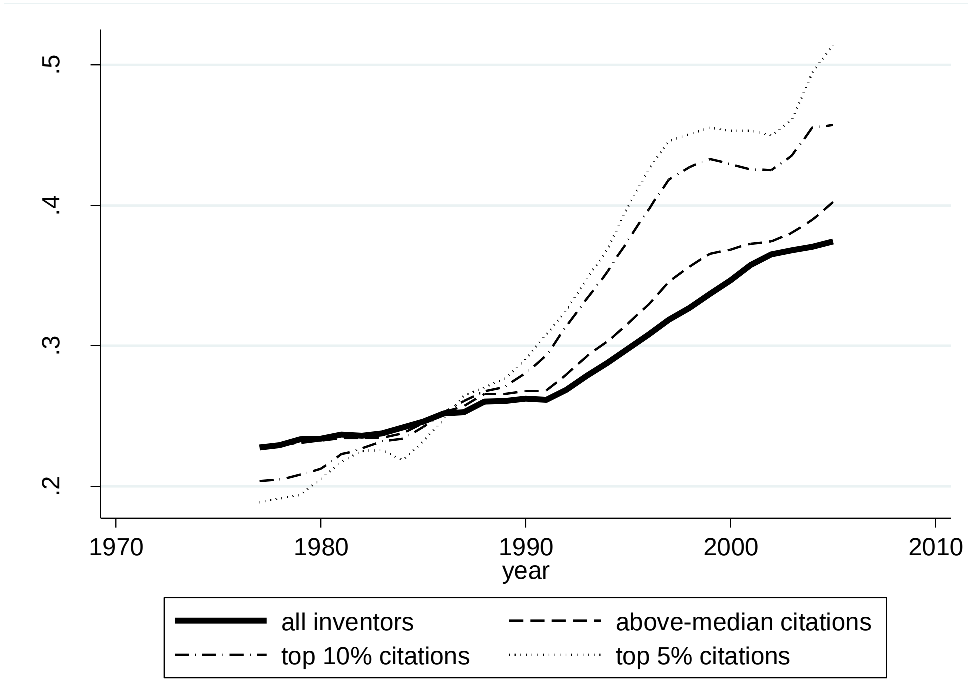
Table V: Difference-in-differences logistic models of factors moderating the likelihood of domestic emigration.

	(1)	(2)	(3)	(4)
days since last patent (ln)	0.8214**	0.8783***	0.8836***	0.8863***
	(0.1624)	(0.1644)	(0.1614)	(0.1625)
inventor had emigrated previously	3.2184**	3.2688***	3.2980***	3.3097***
	(0.4164)	(0.4497)	(0.4376)	(0.4429)
prior employer's number of patents (ln)	-0.0651	-0.0875	-0.1202+	-0.1175+
	(0.0540)	(0.0557)	(0.0654)	(0.0648)
inventor's pre-MARA patenting rate	0.6227	0.1151	0.1534	-0.0337
	(0.7135)	(2.3854)	(0.7517)	(2.4005)
auto industry	-0.2317	-0.4204	-0.4470	-0.4523
	(1.0865)	(1.1937)	(1.2250)	(1.2278)
Michigan	-0.0242	-0.0742	0.0943	-0.1302
	(0.3064)	(0.8264)	(0.3808)	(0.8206)
post-MARA	-3.1959*	-1.5900	-2.9365**	-1.6888
	(1.5572)	(0.0000)	(1.0513)	(0.0000)
Michigan * post-MARA	1.1021*	1.7048	1.2339*	1.8205
	(0.4592)	(1.2742)	(0.5350)	(1.2728)
# of pre-MARA patents		0.0340		-0.0714
		(0.8945)		(0.9123)
post-MARA * number of pre-MARA patents		0.2264		0.5501
		(0.7536)		(0.7288)
Michigan * number of pre-MARA patents		0.0735		0.1804
		(0.5183)		(0.5160)
Michigan * post-MARA * number of pre-MARA patents		-0.1460		-0.4758
		(0.8401)		(0.8137)
in largest national component			0.6650	0.7244
			(0.4548)	(0.4459)
Michigan * largest national component			-0.3445	-0.4310
			(0.7099)	(0.7026)
post-MARA * largest national component			-1.8101	-2.0300+
			(1.1028)	(1.0713)
Michigan * post-MARA * largest national component			1.7370	1.9262+
			(1.1445)	(1.1250)
Constant	-25.7043**	-26.7060	-25.9666***	-27.4949
	(1.6436)	(0.0000)	(1.9322)	(0.0000)
Observations	23242	24898	24898	24898

Notes: Observations are patenting U.S. inventors in Michigan or other non-enforcing states, 1975-1996. All models include year, industry, and first-patent-year cohort indicators. Data in most models are matched using Coarsened Exact Matching. Robust standard errors are in parentheses, clustered by inventor.

+ Significant at the 10% level; * significant at the 5% level; ** significant at the 1% level.

Figure I: Share of patent holders in enforcing vs. non-enforcing states by forward citations, 1975-2005.



Notes: The solid black line represents the three-year rolling average of the share of all inventors who reside in non-enforcing states in a given year. The dashed line indicates the share for inventors who are above the median in forward citations (5-year window); the dot-dashed line is for those in the top 10% of patent productivity; the dotted line is for the top 5%. Michigan is omitted from the graph as its non-compete enforcement policy reversed in 1985.

APPENDIX

Cross-sectional evidence of emigration from enforcing states

Even in cross-sectional analysis, we find that inventors in states that allow enforcement of non-competes are more likely to emigrate—to leave the state when changing jobs—compared with those in non-enforcing states. Our key explanatory variable indicates whether non-competes were enforced in the state where the inventor lived when the prior patent was filed. States that prohibited enforcement over the entire time period of this study include Alaska, California, Connecticut, Minnesota, Montana, Nevada, North Dakota, Oklahoma, Washington, and West Virginia (Stuart and Sorenson 2003).[1]

Table A.1 shows that, over the course of their careers, individual inventors are more likely to emigrate from states that enforce non-competes (0.088 versus 0.069, Pearson's chi-squared statistic = 564.4) than from states where non-competes are not enforceable. This univariate analysis does not control for important characteristics of individuals or states that might condition migration, but in multivariate analysis we account for characteristics of inventors, including time varying characteristics. Accordingly, we use the inventor-patent as the observation (as opposed to the inventor-career in Table A.1). Time-varying control variables also include annual indicators, the number of patents the inventor had been granted in the previous five years (logged), the number of days between the two patents (logged), whether the inventor had previously emigrated, as well as characteristics of the prior patent. These include indicators for six top-level technical classifications to which the prior patent was assigned (Hall, Jaffe, and Trajtenberg 2001), and as a proxy for firm size the logged number of patents belonging to the firm to which the prior patent was assigned. Table A.2 gives descriptive statistics and correlations.

We estimate a logistic model of the likelihood that a given patent *i* indicates that its inventor *j* emigrated. Letting E_{ij} indicate emigration from one state to another, $\boldsymbol{X}_{ij}$ a vector of covariates of the patent, $\boldsymbol{Z}_i$ a vector of time-independent covariates of the inventor, and $\boldsymbol{W}_{it}$ a vector of time varying covariates of the inventor, we estimate $Pr(E_{ij} = 1) = e^{(\beta \boldsymbol{X}ij + \gamma \boldsymbol{Z}i + \lambda \boldsymbol{W}it)} / (1 + e^{(\beta \boldsymbol{X}ij + \gamma \boldsymbol{Z}i + \lambda \boldsymbol{W}it)})$. Each patent is taken as an observation, with the regression analysis reporting robust standard errors clustered by inventor to account for non-independence of observations from the same inventor. All models are estimated using Stata 10.

The models in Table A.3 proceed as follows. Column 1 shows the effect of control variables alone. As expected, inventors who had emigrated previously are more likely to do so again. Those who worked at large firms were less likely to emigrate, although those with a larger number of patents were more likely to do so.[2] Column 2 adds the explanatory variable for states that enforce non-competes, which enables us to compare the predicted likelihood of emigration for enforcing states (0.0136) vs. non-enforcing states (0.0101) when holding all other variables at their means.[3] Inventors were thus 34.7% more likely to emigrate from enforcing states, echoing the results obtained in Table A.1. In Column 3, we

[1] As described earlier, the state of Michigan reversed its non-compete enforcement policy in the mid-1980s and is thus excluded from the cross-sectional analyses. Results are robust to the inclusion of Michigan data.

[2] This result may also be indicative of an increased ability to observe behavior by inventors who patent more often. We are thus reluctant to infer a causal relationship between patent productivity and emigration.

[3] The predicted probabilities from the regression analysis in Table 3 are smaller than those in the univariate Table A1 because the latter measures mobility over the career of the inventor while the former is at the patent level.

show that the results are not solely due to a "California effect."[4] We exclude all patents in California, which reduces the number of observations and the magnitude of the explanatory variable only somewhat (to 25.6%, 0.0138/0.0110) yet maintains its level of statistical significance.

Descriptive evidence of emigration to non-enforcing states

The individual-level model, although it offers a window into whether inventors moved *from* enforcing versus non-enforcing states, is inadequate for assessing whether non-compete enforcement also influences whether they move *to* enforcing versus non-enforcing states because it cannot take into account either the supply or characteristics of possible states to which the inventor might move. Hence, we analyze emigration between state dyads, calculating the proportion of inventors emigrating from a given state to any other state. In addition to accounting for the supply of enforcing versus non-enforcing states, this approach enables us to control for regional characteristics of both the originating and destination states. The proportion is given by the count of moves for a given (directional) dyad-year divided by the count of inventors for the source state in the dyad for that year.[5] For example, the proportion of California-to-Massachusetts emigration during 1996 would be a distinct observation from the proportion of Massachusetts-to-California emigration in 1996.

Figure A.1 provides descriptive evidence that inventors were more likely to relocate to states that prohibited enforcement.[6] It shows that the proportion of inventors emigrating from a given enforcing state to any non-enforcing state was higher than to any other enforcing state, and that this was true throughout the time period of our dataset. Coupled with the prior analysis, this suggests a net brain drain from enforcing states to non-enforcing states. Figure 1 also suggests that overall rates of emigration are increasing throughout the 1975-2005 period.

In multivariate analysis, the explanatory variables are indicators for three out of the four possible pairings of source and destination non-compete regimes: from an enforcing state to an enforcing state (ES2ES), enforcing state to non-enforcing (ES2NES), and non-enforcing to enforcing (NES2ES). The omitted category is moving from a non-enforcing state to a non-enforcing state (NES2NES); coefficients in the model should be interpreted relative to a baseline effect of NES2NES. Control variables account for a number of regional factors. First, we account for the logged distance between the midpoints of the two states, as proximity may influence emigration. The size of the labor force and the number of business establishments are both included. To control for general economic conditions and changes in wealth, the models include both GDP and personal income per capita PCPI in thousands of dollars, deflated by

[4] California is distinguished in at least two ways that might exaggerate patterns of inventors not emigrating from non-enforcing states and thus bias toward finding a brain drain. First, California's Business and Professions Code Section 16600 is the longest-standing and arguably the strictest law banning the use of non-compete agreements (Garmaise 2009), dating back to its incorporation as state in the 1880s (Gilson 1999) and having been reaffirmed by the state Supreme Court as recently as August of 2008 (*Edwards v. Arthur Andersen* 44 Cal. 4th 937). Second, given the state's extensive landmass it offers many local relocation opportunities to its large population. For both of these reasons, one might be concerned that California alone is responsible for the brain drain effect.

[5] As we are not modeling time-varying characteristics of inventors, it is not necessary to use the patent as the unit of analysis here although doing so produces results that are both statistically and economically similar.

[6] Note that dividing the number of moves from a focal state to one another state by the full number of inventors in the focal state yields rather small proportions.

Bureau of Labor Statistics' experimental Consumer Price Index for 2002. Controls are logged to reduce skewness and kurtosis. Table A.4 gives descriptive statistics and correlations.

In Table A.5, we estimate a generalized linear model using a logistic link function for the fractional dependent variable (Papke and Wooldridge 1996). Each dyad-year observation is weighted by the number of inventors in the source state in order to prevent those with very small inventor populations (e.g., Alaska) from exerting undue influence in the model. Column 1 examines the control variables, and Column 2 adds the explanatory variables. Again, the baseline (omitted) explanatory variable is an indicator of movement from a non-enforcing state to another non-enforcing state. Interpretation is facilitated by dividing the predicted emigration proportion when ES2NES=1 (0.0017) by the predicted emigration proportion from Column 2 when ES2ES=1 (0.0010), holding other covariates at their means. Doing so reveals that the proportion of inventors emigrating from a given enforcing state to any non-enforcing state is 64.5% higher than emigrating from a given enforcing state to any other enforcing state.

Column 3 repeats the analysis of Column 2 but excluding moves to California for reasons described above, again reducing the number of observations. Here, the coefficients are closer though still distinct (even in unreported formal statistical tests), suggesting that the effect is not fully driven by California. Even when excluding California in Col. 3, the predicted proportion of inventors emigrating from an enforcing state to any non-enforcing state is 15.0% less than from an enforcing state to any other enforcing state.

These results establish that inventors who emigrate from states that enforce non-competes are disproportionately likely to move to non-enforcing states. Moreover, those who emigrate from states that do not enforce non-competes are also more likely to move to states that do not enforce non-competes; this indicates that the "brain drain" of inventors from enforcing states is not offset by a replenishing flow of inventors from non-enforcing states, but indeed represents a net loss of talent.

Table A.1: Domestic emigration of U.S. inventors, by state-level non-compete enforcement.

		State enforces non-competes		
		no	yes	Total
inventor	no	158,332	368,199	526,531
took job		*93.08%*	*91.19%*	*91.75%*
in another	yes	11,768	35,560	47,328
state		*6.92%*	*8.81%*	*8.25%*
	Total	170,100	403,759	573,859
		100%	*100%*	*100%*

Notes: "Emigration" refers to inventors leaving a state to join a new firm, as indicated by a change in both the assignee and the inventor's home state between consecutive patents. Emigration is measured over the entire career of the inventor from 1975 through 2005, and is segmented by the non-compete enforcement regime of their original state. The unit of analysis is an inventor's career in a particular state; the number of observations exceeds the number of inventors in order to account for those who move from enforcing to non-enforcing states or vice versa.

Table A.2: Descriptive statistics for analysis of domestic emigration by U.S. inventors, 1975-2005.

		Mean	Std. Dev.	Min	Max	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1)	patent indicates emigration from previous state	0.023	0.151	0.000	1.00	1.00									
(2)	days since last patent (ln)	5.291	2.043	0.000	9.84	0.10	1.00								
(3)	inventor had emigrated previously	0.019	0.138	0.000	1.00	0.21	0.07	1.00							
(4)	prior employer's number of patents (ln)	4.238	2.530	0.000	9.43	-0.07	-0.22	-0.06	1.00						
(5)	inventor's number of patents in last 5 years (ln)	1.969	0.896	0.693	6.28	-0.07	-0.56	-0.05	0.29	1.00					
(6)	prior state number of employed persons (ln)	15.341	0.864	12.006	16.61	-0.03	-0.03	-0.02	0.02	0.04	1.00				
(7)	prior state number of establishments (ln)	12.399	0.855	9.067	13.64	-0.03	-0.05	-0.02	0.05	0.07	0.98	1.00			
(8)	prior state GDP (ln)	12.805	0.948	8.993	14.23	-0.03	-0.05	-0.02	0.05	0.07	0.98	0.99	1.00		
(9)	prior state personal income per capital (ln)	10.353	0.183	9.378	10.83	0.00	-0.03	0.01	0.09	0.07	0.23	0.30	0.38	1.00	
(10)	prior state enforces non-competes	0.698	0.459	0.000	1.00	0.02	0.03	0.01	0.03	-0.05	-0.35	-0.36	-0.36	-0.16	1.00

Notes: "Emigration" refers to someone taking a new job in a different state, measured as a change in both the assignee and the inventor's home state on the inventor's subsequent patent. *n*=2,623,851.

Table A.3: Logistic regressions of emigration by U.S. inventors from enforcing vs. non-enforcing states.

	(1)	(2)	(3)
days since last patent (ln)	0.6780**	0.6790**	0.7063**
	-0.0086	(0.0086)	(0.0088)
inventor had emigrated previously	2.4761**	2.4724**	2.4148**
	(0.0190)	(0.0191)	(0.0194)
prior employer's number of patents (ln)	-0.1110**	-0.1144**	-0.1190**
	(0.0031)	(0.0031)	(0.0028)
inventor's number of patents in last 5 years (ln)	0.4095**	0.4145**	0.4185**
	(0.0170)	(0.0171)	(0.0156)
prior state number of employed persons (ln)	-0.5311**	-0.6137**	-0.6818**
	(0.0793)	(0.0784)	(0.0794)
prior state number of establishments (ln)	-0.2133**	-0.1807*	-0.1320+
	(0.0735)	(0.0735)	(0.0755)
prior state GDP (ln)	0.5288**	0.6144**	0.6603**
	(0.0607)	(0.0593)	(0.0609)
prior state personal income per capital (ln)	0.5133**	0.6092**	0.5300**
	(0.0593)	(0.0593)	(0.0613)
prior state enforces non-competes		0.3012**	0.2309**
		(0.0127)	(0.0169)
Constant	-10.6837**	-12.1728**	-11.6455**
	(0.7406)	(0.7419)	(0.7603)
log-likelihood	-257457.74	-257019.84	-220422.4
Excludes California?	No	No	Yes
Observations	2623851	2623851	2109049

Notes: The dependent variable is the likelihood that an individual patent indicates that a given inventor emigrated domestically when changing jobs, 1975-2005. All models include year, industry, and first-patent-year cohort indicators. Robust standard errors are in parentheses, clustered by inventor.

+ Significant at the 10% level; * significant at the 5% level; ** significant at the 1% level.

Table A.4: Descriptive statistics for domestic emigration by U.S. inventors between state dyads, 1975-2005.

		Mean	Stdev	Min	Max	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
(1)	proportion of inventors in "from" state emigrating to "to" state	0.003	0.127	0.000	1.000	1.00												
(2)	ES2ES	0.641	0.479	0.000	1.000	-0.02	1.00											
(3)	ES2NES	0.162	0.368	0.000	1.000	0.30	-0.59	1.00										
(4)	NES2ES	0.162	0.368	0.000	1.000	-0.01	-0.59	-0.19	1.00									
(5)	distance between states (ln)	6.727	0.710	3.045	7.890	-0.18	-0.18	0.09	0.10	1.00								
(6)	prior state number of employed persons (ln)	14.256	0.981	12.070	16.615	-0.03	0.03	0.02	-0.05	-0.04	1.00							
(7)	subsequent state number of employed persons (ln)	14.256	0.981	12.070	16.615	0.22	0.03	-0.05	-0.02	-0.04	-0.01	1.00						
(8)	prior state number of establishments (ln)	11.302	0.936	9.364	13.686	-0.03	0.03	0.02	-0.07	-0.03	0.99	0.00	1.00					
(9)	subsequent state number of establishments (ln)	11.302	0.936	9.364	13.686	0.23	0.03	0.01	0.01	-0.03	0.00	0.99	0.01	1.00				
(10)	prior state GDP (ln)	11.575	1.041	8.993	14.362	-0.01	0.03	-0.04	-0.04	-0.05	0.97	0.01	0.98	0.02	1.00			
(11)	subsequent state GDP (ln)	11.575	1.041	8.993	14.362	0.23	0.03	0.02	0.02	-0.05	0.01	0.97	0.02	0.98	0.04	1.00		
(12)	prior state personal income per capita (ln)	10.197	0.252	9.378	10.982	0.08	-0.06	-0.01	0.06	-0.02	0.25	0.09	0.31	0.13	0.43	0.20	1.00	
(13)	subsequent state personal income per capita (ln)	10.197	0.252	9.378	10.982	0.14	-0.06	0.01	-0.01	-0.02	0.09	0.25	0.13	0.31	0.20	0.43	0.67	1.00

Notes: Each dyad is directional; e.g., California-to-Massachusetts in 1996 is a separate observation from Massachusetts-to-California in 1996. n=67,680. ES2ES is an indicator for movement from an enforcing state to another enforcing state; ES2NES indicates movement from enforcing to non-enforcing; and NES2ES is non-enforcing to enforcing.

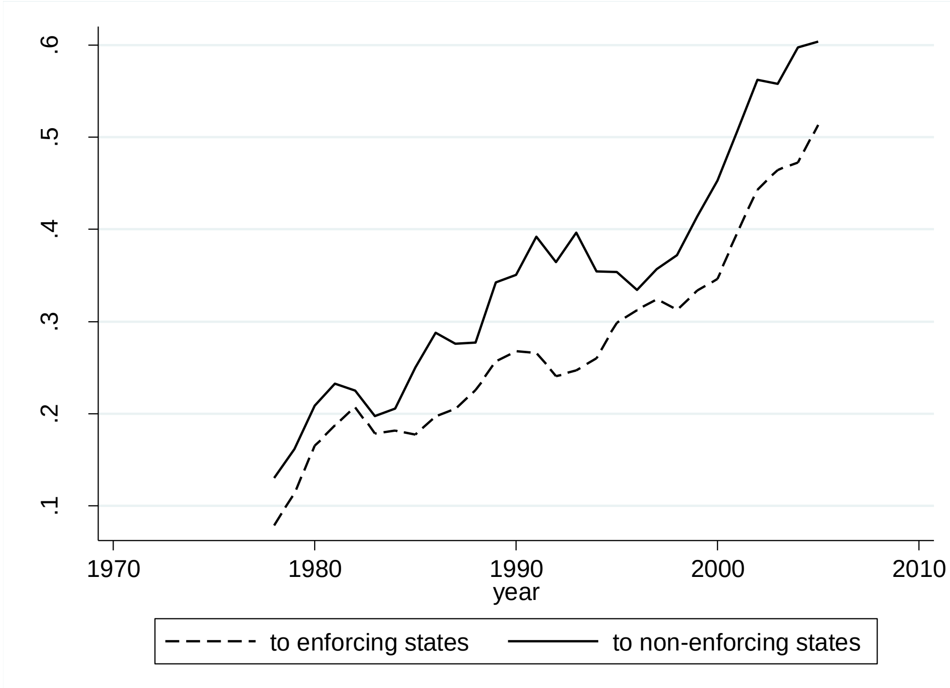
Table A.5: Generalized linear models of emigration between state dyads by U.S. inventors, 1975-2005.

	(1)	(2)	(3)
distance between states (ln)	-0.5968**	-0.6507**	-0.7928**
	(0.0130)	(0.0120)	(0.0114)
prior state number of employed persons (ln)	-1.4285**	-1.3783**	-1.4080**
	(0.1076)	(0.1027)	(0.0973)
subsequent state number of employed persons (ln)	0.0919	0.0526	0.2856**
	(0.1062)	(0.1030)	(0.0986)
prior state number of establishments (ln)	0.8854**	0.8365**	0.9530**
	(0.0970)	(0.0917)	(0.0947)
subsequent state number of establishments (ln)	1.1892**	1.2764**	1.3746**
	(0.0879)	(0.0849)	(0.0846)
prior state GDP (ln)	0.5256**	0.5376**	0.3696**
	(0.0860)	(0.0824)	(0.0799)
subsequent state GDP (ln)	-0.1816*	-0.2293**	-0.7103**
	(0.0741)	(0.0752)	(0.0748)
prior state personal income per capita (ln)	0.2841**	0.3033**	0.0613
	(0.1047)	(0.1009)	(0.0891)
subsequent state personal income per capita (ln)	1.9702**	1.7138**	1.9466**
	(0.0871)	(0.0919)	(0.0846)
ES2ES		-0.2823**	0.2512**
		(0.0298)	(0.0453)
ES2NES		0.2160**	0.3909**
		(0.0345)	(0.0474)
NES2ES		-0.2346**	0.1471**
		(0.0329)	(0.0479)
Constant	-34.2600**	-31.4602**	-28.5307**
	(1.4722)	(1.4282)	(1.3300)
Excludes California?	no	no	yes
Observations	67680	67680	64860

Notes: The dependent variable is the number of inventors emigrating that year from the originating state in the dyad to the destination state, divided by the number of inventors in the originating state that year. The omitted category is an indicator for one non-enforcing state to another; included categories are from one enforcing state to another (ES2ES), enforcing to non-enforcing (ES2NES), and non-enforcing to enforcing (NES2ES). Robust standard errors are in parentheses, clustered by inventor.

+ Significant at the 10% level; * significant at the 5% level; ** significant at the 1% level.

Figure A.1: Domestic emigration by U.S. inventors to enforcing vs. non-enforcing states, 1975-2005.



Notes: We plot the three-year rolling average of the proportion of inventors emigrating each year from states that enforce non-competes, to any enforcing state (dashed line) vs. any non-enforcing state (solid line). The y-axis is the number of inventors emigrating from the origin state of a dyad to the destination state in a given year, divided by the number of patenting inventors in the origin state for that year. Michigan is omitted from the graph as its non-compete enforcement policy reversed in 1985.