REGIONAL DISADVANTAGE?

NON-COMPETE AGREEMENTS AND BRAIN DRAIN

Matt Marx
MIT Sloan School of Management
100 Main St., E62-478
Cambridge, MA 02142

Jasjit Singh
INSEAD
1 Ayer Rajah Avenue
Singapore 138676

Lee Fleming
Harvard Business School
Morgan 485
Boston, MA 02163

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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 appears to drive away inventors with greater human and social capital. We address causality-related concerns with a difference-in-differences study design based on an inadvertent reversal of Michigan's non-compete enforcement policy.

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INTRODUCTION

Why has Silicon Valley become the most entrepreneurial and technologically successful region not only in the U.S. but arguably in the world? More generally, how can policymakers achieve "regional advantage" (Saxenian 1994)? In high-tech industries, factor endowments are less critical than assembled resources including skilled labor, which regions must both attract and retain. Skilled workers are essential not only to staff existing firms but also to launch and grow entrepreneurial startups (Zucker, Darby, and Brewer 1998). Hence, avoiding a "brain drain" of human capital is critical to regional planners at many levels. Although the term is most commonly associated with out-migration from less developed countries to the U.S. or other nations (Kwok and Leland 1982; Gould 1994; Grubel and Scott 1996), sub-national regions such as states also compete to attract and retain skilled labor. As Borjas, Bronars, and Trejo show using CPS data, states such as Massachusetts are shown to "export[] its most able workers" (1992:148).[1] Indeed, the fiercest competition for talent may come from not from abroad but from within the same country as domestic relocation is not inhibited by immigration policy.

At least since Ravenstein's (1885) exploration of why workers moved from one region to another within the United Kingdom, scholars have sought to understand the individual and contextual determinants of within-nation or "internal" migration. (See Greenwood (1997) for a review of the literature.) Numerous factors explaining relocation within national boundaries have been identified, including characteristics of individuals such as age (Plane 1993) and social connections (particularly among immigrants, see Reher and Silvestre 2009) as well as regional characteristics such as geographic distance (Lansing and Mueller 1967) and climate (Graves

[1] Policymakers' concern about retaining talent is underscored by the report "Preventing a Brain Drain: Talent Retention in Greater Boston" (Greater Boston Chamber of Commerce 2003).

1979). A particularly frequent finding within this literature is the role of economic constraints in spurring out-migration. Sjaastad (1962) was the first to formally model the decision to emigrate as an investment in one's human capital, an intuition subsequently borne out in studies using microdata as regions with more attractive job prospects enjoy greater in-migration (Treyz et al. 1993; Blackburn 2010). Moreover, out-migration is not limited to the un(der)employed but rather appears to be increasing in opportunity cost. Better educated and more highly skilled workers are more likely to relocate in response to economic constraints in their current region (Borjas, Bronars, and Trejo 1992; Bartel and Koch 2011).

Given the responsiveness of top talent to strong relocation incentives, identifying actionable measures that policymakers can implement to attract and retain key workers would seem a key potential contribution of this literature. But as Greenwood acknowledges, despite several decades of scholarship "few direct links have ever been drawn between policy tools, such as migrant subsidies and regional employment policies, and internal migration" (1997:648). In this article, we identify an actionable regional employment policy that might influence inter-regional migration of skilled workers: post-employment agreements not to compete (hereafter, "non-competes"). We find that non-competes indeed contribute to a "brain drain" from regions that enforce such contracts to those that do not. Our evidence is based on a difference-in-differences analysis of an inadvertent reversal of non-compete enforcement policy in Michigan. The sudden imposition of enforceable non-compete agreements in 1985 contributed to emigration out of Michigan and into states that continued not to enforce such contracts. Moreover, this "brain drain" appears to be amplified for workers with higher levels of human capital and social capital—exactly the sort of talent a region would most want to retain.

EMPLOYEE NON-COMPETE AGREEMENTS AND REGIONAL DYNAMICS

Non-competes place restrictions on the sorts of jobs an ex-employee may take after leaving the firm, usually for a term of 1-2 years. Although companies frequently ask employees to sign non-disclosure agreements that bar 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 competing firm. These advantages for firms are however obtained at the expense of individuals' career flexibility (Fallick, Fleischman, and Rebitzer 2007; Garmaise 2011), leaving the state sanction of non-competes a controversial policy issue.[2] That policymakers continue to come to such varying conclusions regarding non-competes indicates a lack of consensus regarding the regional implications of these employment contracts in the absence of a federal statute. Samila and Sorenson (2011) show that a marginal dollar of venture capital is more efficient at producing startups, jobs, and patents where non-competes are not enforceable, but beyond their study there exists little evidence regarding regional implications of non-competes.

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. Employment lawyers often counsel clients subject to non-competes to take jobs in states that do not sanction non-competes; moreover, hiring managers and headhunters alike advertise the benefits to prospective employees of working in a region where

[2] In 2008 alone, four states reformed their non-compete laws, some restricting the enforceability of non-competes while others expanded the rights of firms to use such contracts. 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. In 2010, Georgia amended its constitution to strengthen non-competes (Ga. HR 178).

they are not subject to non-competes (Marx 2011). Moving to a non-enforcing state in order to avoid a non-compete is facilitated by the "public policy exception" whereby judges are not obligated to uphold out-of-state contracts which 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. [3]

For both of these reasons, we expect that 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 whose patents are highly cited and who have greater collaborative ties as they wish to preserve the ability to leverage their task-specific human capital (Gibbons and Waldman 2004). Limited job mobility within a state that enforces non-competes is likely to entail higher opportunity costs for these important inventors, with their past track record also making them more visible to and hence more 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.

Still, we consider the existence of a brain drain an empirical question, given the null hypothesis that if non-competes serve to bind employees to their employers then those same

[3] 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 (20 Cal. App. 3d 669) the California courts forbade corporations from specifying out-of-state jurisdiction as a means of cherry-picking one's non-compete enforcement regime.

workers ought to remain in their current region. The argument above presumes that (unobservable to us) workers in enforcing states will know that the public policy exception enables them to take jobs in states that restrict the enforcement of non-competes. The next section describes our analytical approach.

EMPIRICAL STRATEGY

We present analysis in support of our arguments based upon the U.S. patent record from 1975-2005. One approach 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. While 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.[4]

Non-compete enforcement in Michigan had long been prohibited by Public Act No. 329 of 1905. But in 1985, the Michigan legislature passed the Michigan Antitrust Reform Act

[4] States with statutes that limited non-compete enforcement during the entire period of this study include Alaska, California, Connecticut, Minnesota, Montana, Nevada, North Dakota, Oklahoma, Washington, and West Virginia (Stuart and Sorenson 2003).

(MARA). Although the primary purpose of MARA was centralize antitrust law, in doing so it repealed numerous statutes including Public Act No. 329, which not only addressed antitrust but also had contained the code proscribing the enforceability of non-competes. Marx, Strumsky, and Fleming (2009)[5] provide evidence from the legislative record as well as interviews with practicing lawyers active at the time that the change in non-compete policy was inadvertent. Although the legislature acted two years later to institute a "reasonableness standard" governing the appropriate length and scope of a non-compete—as in the case in virtually all states that enforce non-competes—it importantly *did not reinstate the previous ban*. As such, the Michigan policy reversal may be seen as an exogenous shock to the extent that firms 1) became aware of the change and 2) had or put non-competes in place which could subsequently be enforced.

It might seem unlikely that firms would have implemented non-competes among their employees in Michigan prior to 1985, yet evidence exists that firms frequently have employees sign non-competes even when they are unenforceable under state law. Among firms covered by Execucomp from 1992-2004, Garmaise (2011) finds that 58% of those located in California use non-competes even though the state's Business and Professions code Section 16600 has strictly banned the enforceability of non-competes since the 1870s (Gilson 1999). Kaplan and Stromberg (2003) find similar levels of non-compete use among California entrepreneurs, indicating that not only large, publicly traded firms use non-competes despite legal sanction to the contrary.

[5] While Marx et al. (2009) also exploit the Michigan policy reversal as a natural experiment, they use it to establish the stated effect of non-competes: to bind employees to their employers. Here, we instead examine an unintended effect of such contracts—that employees leave not only the firm but the region. Firms using non-competes surely do not intend for such contracts to drive some of their most talent employees out of state.

Thus, although we lack data on the use of non-competes among all firms in Michigan prior to the reform—and we doubt that such data are obtainable—there is strong reason to believe that many Michigan firms had signed non-competes on the books.[6] Given that the repeal of Public Act No. 305 merely removed the ban and did not stipulate any governing timeframe, all such contracts would have become immediately enforceable.

Moreover, the appearance of multiple articles in the *Michigan Bar Journal* (Alterman1985, Levine 1985, Sikkel and Rabaut 1985) regarding the newfound enforceability of non-competes promoted awareness of the issue, certainly within the community of practicing lawyers and also likely among the leadership of local firms. Lawyers would have transmitted the news to their clients in hopes of generating new contractual and prosecutorial work (Bagley 2006). Louis Rabaut, a Michigan attorney during the time of MARA, recounted that following the reversal "all of a sudden the lawyers saw no proscription of non-competes."[7]

Importantly, employees' reaction to enforceable non-competes need not be spurred by legal action. In a related field study of "career detours" in which ex-employees left their industry

[6] Having employees sign non-competes might appear costly to the firm if workers bargained for higher wages in consideration of future employment restrictions. But data from a 2009 survey of IEEE engineers indicated that 70% of the time firms do not ask for the non-compete until after the applicant has accepted the job, restricting the ability of workers to bargain (Marx 2011). This practice remains legal except in Oregon as of 2008.

[7] One might ask whether the change in the law produced a flurry of non-compete lawsuits. Legal databases such as Westlaw record only court *decisions*; they do not track the actual number of cases *filed*. We have identified another source, the Courthouse News Service, which tracks all filed cases, but they began collecting data in Michigan long after 1985.

when leaving their jobs, Marx (2011) found only one instance in which the move was prompted by a legal threat (which itself never materialized into a court case). Workers routinely took actions to avoid the potential consequences of non-compete infringement, as was illustrated by an engineer in the internet-search industry we happened to come in contact with. Previously based in New York, he had worked at another internet-search firm when an attractive offer arrived from a competitor with a nearby office. When his former employer verbally threatened him with legal action—though no suit was formally brought—the new employer changed his job offer from its New York office to its California office. "That non-compete," said the engineer in a thick Brooklyn accent, "is the only reason I'm working in California today."

The following section describes the data and empirical approach we use to exploit the inadvertent Michigan policy change described above as a natural experiment to study the impact of introducing non-compete enforcement on interstate emigration. If non-compete enforcement indeed drives emigration, there should have been an increase in emigration from Michigan (after the MARA policy change over and above the baseline temporal pattern of emigration rates in states that continued not to enforce non-competes throughout the period (the control group).

Sample Construction

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. 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. And while 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.

Because the USPTO does not require applicants to supply a unique identifier, it is a non-trivial exercise to reconstruct work histories and co-authorship networks for all individuals. Fortunately, the patent record contains each inventor's name, hometown, employer, and technology classifications, enabling disambiguation of authorship (for the full algorithm and details, see Lai et al. 2009; for earlier approaches, see Trajtenberg et al. 2006). Tracking geographic location across successive patents for these inventors allows us to identify instances of inter-state mobility.[8] Because the exact timing of a move cannot be precisely determined, we use the midpoint of the time window between the last patent in the former state and the first patent observed after the move to a new state to estimate only the year of the move.

Our sample consists of all patents "at risk" of being associated with an inter-state mobility event since the last patent by the same inventor. By construction, an inventor's first patent cannot indicate a move; analysis is therefore restricted only to the inventor's subsequent patents. For the same reason, inventors with only one patent are excluded. We identify *emigration*—workers leaving the state when they change jobs— by a pair of patents belonging to the same inventor where neither the assignees nor the states match. Movement from employment

[8] Given that we can detect mobility only in instances where an inventor files for a patent both before and after a move, we do not observe all moves of patenting inventors. Moves in our study are, in any case, only a subset of all moves involving skilled workers (patenting or not).

to self-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 obviously do not ask themselves to sign a non-compete. 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.

Although the exogenity of the Michigan policy reversal is attractive for purposes of identification, it is important to establish that the treatment and control groups are comparable along the pre-treatment control variables. We use Coarsened Exact Matching (Iacus, King, and Porro 2009) 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. Our matching criteria 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 algorithmically determine the appropriate number and boundaries of the matching "bins" in order to optimize an objective function based on Scott's rule. One concern might be that an automated matching algorithm could 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 patent percentage, 170. (Discrete variables were matched exactly.) Table I gives descriptive statistics and correlations for the matched observations.[9]

Econometric Model

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.[10] Observations are weighted appropriately based on the number of matched control observations found for each focal observation. All models are estimated using Stata 10.

The key variable of interest in our difference-in-differences analysis is naturally the interaction of the indicators for Michigan residence and the post-MARA time period, after these two indicators have also been entered directly in the model to capture the baseline effects. 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 since the preceding patent by this

[9] Stringent matching naturally comes at the cost of fewer (50.1%) treated observations being matched to control-group observations. To ensure that our findings are not overly sensitive to this, we carried out analysis with less stringent matching of 20 bins per continuous variable, matching a much higher fraction (95.7%) of treated observations but yielding similar results.

[10] Clustering standard errors at the state level yields similar results.

inventor (logged) and whether the inventor had previously emigrated. We also account for characteristics of the last patent prior to the inventor's move 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.

RESULTS

Descriptive data in 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 in Michigan (0.24% to 0.32%) while dropping in states that did not enforce non-competes (0.20% to 0.13%). The relative risk of post-MARA emigration was 1.35 in Michigan, twice as high an in states that continued not to enforce non-competes (where the relative risk of post-MARA emigration was 0.68).

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. We start with the 1983-1988 time period given that the reform was passed in 1985 and may have taken some time to diffuse; we then examine models in two-year increments thereafter until the widest possible symmetric window surrounding MARA given the data (1975-1996). For the remainder of our analysis, we use the widest window in Column (5). In this model, the coefficient on the interaction of the Michigan and post-MARA indicators is statistically significant at the 1% level, suggesting that the policy reversal increased the likelihood of emigration.

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 table, essentially reconstructing the components of Table II A from the regression. 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.

In Table IV, we subject the brain drain result to a number of additional tests. First, we establish that the brain drain was not just a manifestation of a general exodus from Michigan which might be unrelated to the non-compete enforcement policy per se. We do so by showing that the brain drain was channeled into states that continued not to enforce non-competes and thus became more attractive labor markets for workers following Michigan's inadvertent adoption of non-compete enforcement. Specifically, the placebo test in Column (1) replaces the control group used in prior analysis 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. Consistent with the mechanism behind the brain result being non-compete enforcement policy, we find no evidence of increased out-migration from Michigan to enforcing

states; in fact, weak evidence in Column (1) suggests that Michigan inventors became somewhat less likely to emigrate to other enforcing states once Michigan began to enforce non-competes.[11]

Next, we examine whether the effect is driven primarily by migration to California. While such a finding would not necessarily rule out the importance of non-compete enforcement policy, one might be concerned that our results constitute a "California effect" for at least three reasons. First, 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. Second, given the state's extensive landmass (and, more broadly, other natural factors including attractive weather), California might offer disproportionately many relocation opportunities. 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. The analysis reported in Column (2) shows that the brain drain finding is not driven primarily by an exodus of Michigan inventors to California in the post-MARA period. In this model, we exclude all emigration to California, which reduces the number of observations but retains statistical significance on the key interaction term and with a slight increase in magnitude.

In the next three models, we address the possibility that the brain drain is explained by industry mix as a difference-in-differences specification can be vulnerable to non-random sources of variation (Bertrand, Duflo, and Mullainathan (2004). Although our previous models controlled for automotive patents, in Column (3) we explore whether the decline of other

[11] In unreported models, we find that emigration out of Michigan was not offset by immigration into the state, so the effects reported here are indicative of a net loss of inventors.

industries in Michigan could have been responsible for the brain drain. For each of the 36 industry subcategories defined by Hall, et al. (2001), we analyze the growth or decline in Michigan patenting during our sample window, using a three-year running average given that patenting can fluctuate from year to year. Starting with a sample drawn from the 22 subcategories with non-trivial share (greater than 1%) in Michigan patenting, we label the bottom quartile as "declining" Michigan industries[12] and exclude them from the model in Column (3). If these declining industries were exclusively responsible for the brain drain, we would not expect to see a positive and significant coefficient on the interaction of Michigan and post-MARA anymore. But the previous finding still holds, suggesting that inventors even in vibrant and growing industries departed Michigan following the non-compete policy reversal.

Even if inventors were not "pushed" to emigrate from industries that were declining in Michigan, one might have the complementary concern that they were "pulled" by attractive opportunities in industries that were growing quickly outside of Michigan. In Column (4), we repeat the exercise of the previous model except that we instead identify the top quartile of industries according to their patenting growth in the control states.[13] Excluding these from the model restricts our analysis to industries in the control states that were growing less slowly, where we would not expect to see a positive and significant coefficient if inventors were merely leaving Michigan given attractive industries elsewhere. But Column (4) shows that emigration from Michigan is not limited to industries that were growing rapidly in the control states. Finally,

[12] These are Mechanical (misc), Agriculture, Organic Compounds, and Other (misc). The full breakdown of subcategory patenting growth/decline is available from the authors.

[13] The top quartile of growth industries in the control states are Semiconductor Devices, Communications, Information Storage, Measuring & Testing, and Drugs & Medical (misc).

Column (5) includes state-industry fixed effects in order to control for further variation, with consistent results. Taken together, these models indicate that the brain drain was not solely due to the effect of growing or declining industries.

The next two models address the concern that the observed brain drain might not be unique to Michigan but possibly an artifact of more general patterns of migration. The placebo test in Column (6) treats inventors in Ohio as the experimental group. Like Michigan, Ohio 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 no statistically-significant evidence of a MARA-coincident brain drain out of Ohio is obtained. Neither do we find post-MARA emigration from other states including Illinois, Pennsylvania, and New York.

In Column (7), we dig deeper into the possibility that the brain drain is an artifact of general patterns of relocation and migration not related to non-competes. We attempt to 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. We would not expect non-competes to affect those who remain with their current employer and indeed find no evidence that Michigan inventors were more likely to be transferred across state lines by their employers following the policy reversal.

Columns (8) and (9) 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. Moving the reform

date back one year in Column (8) shows no evidence of a brain drain while moving the date of MARA in Column (9) ahead one year produces only weak evidence. Unreported regressions placing the date of the MARA reform further from 1985 returned even weaker results.

Finally, the nationwide cross-sectional analysis in the Appendix provides additional evidence that the pattern of emigration from enforcing states to non-enforcing states is not unique to Michigan. While cross-sectional analysis is obviously subject to concerns about unobserved heterogeneity, consistency of this cross-sectional finding with the results above derived from the natural experiment from the Michigan context is reassuring.

Moderating effects

The policy implication of a non-compete brain drain may depend on the nature of the workers most affected. For example, if the emigrating workers came from the pool of just the least valuable in the region, policymakers might in fact favor non-compete enforcement. As discussed above, however, it is arguably more likely that the labor markets where non-competes are not enforced would siphon away more (rather than less) valuable workers for whom the career concerns imposed by non-competes would seem to impose the greatest costs.

In order to examine whether the effect of non-competes on emigration is indeed greater for more valuable workers, we examine how the effect varies across inventors with different levels of human and social capital measures we can derive from patent data. We use average citations to pre-MARA patents as a measure of the quality and impact of an inventor's prior work. Since highly cited patents have been shown to be more valuable, technically, economically, and socially (Trajtenberg 1990), inventors coming up with these patents are likely to be more valuable for a state. Likewise, we use the number of pre-MARA co-authors as a

measure of propensity to collaborate. Collaborative linkages have been shown to increase knowledge diffusion, both within and across firms (Singh 2005), so inventors with more such linkages are likely to be more valuable in generating knowledge spillover benefits for a state.

Descriptive inventor-career data in Panel A of Table V show that the relative risk of post-MARA emigration by inventors with highly cited work —i.e., those with more than the median number of citations per patent prior to the policy reversal—was 186.8% higher in Michigan than elsewhere. By contrast, the relative risk of post-MARA emigration by Michigan inventors at or below the median number of citations per patent was only 46.1% higher than their peers in states that continued not to enforce non-competes. This is consistent with a view that elite inventors—those that produce high-impact inventions—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 while lower-value workers are kept at their jobs—and thus in the region—by non-compete agreements.

Panel A of Table V also shows that the relative risk of post-MARA emigration by more connected inventors—i.e., those with more than the median "degree" or number of patent co-inventors prior to the policy reversal—was 236.3% higher in Michigan than elsewhere. By contrast, the relative risk of post-MARA emigration by Michigan inventors at or below the median number of co-authors was only 37.0% higher than their peers in states that continued not to enforce non-competes. (As with the above measures, the number of collaborative linkages for an inventor was measured strictly on a pre-MARA basis.) Inventors with greater social capital should be more likely to emigrate for at least three reasons. First, they are more likely to hear

about job opportunities through their collaborative ties. Second, they are more likely to be known outside their firm and to receive outside offers of employment. Third, given trends in collaborative invention and the apparent greater productivity of teams (Wuchty, Jones, and Uzzi 2007), collaborative inventors are more likely to be valued by outside employers. [14]

For multivariate analysis corresponding to the above, we present split-sample analyses in Table VI of the likelihood of emigration by inventors of varying levels of human and social capital. Columns (1-4) explore the dimension of human capital, and Columns (5-8) explore social capital. Those with above-median citations per patent exhibit economically and statistically stronger emigration in Column (1) than those below the median in Column (2). A similar pattern emerges for degree (i.e., number of past collaborative ties): both the magnitude and statistical significance of the interaction term are stronger for those with above-median

[14] We note that in this analysis, emigration of highly-cited and highly collaborative inventors in the control states was not constant but dropped significantly following MARA. This raises a question regarding whether our estimated effects are driven by the policy change in the treatment state or by something we do not capture regarding the control states. In calculations available from the authors, we observed that the drop in emigration rates is driven most prominently by California and Connecticut, two control states jointly responsible for more than half of patenting and each of which saw an emigration drop of approximately 20%. When we exclude California and Connecticut in Panel B of Table V, emigration levels in the control states are similar pre- and post-MARA and our results continue to hold. A similar exercise in columns (3-4) and (7-8) of Table VI preserves the regression results.

degree in Column (5) than for those with below-median degree in Column (6). Thus the brain drain appears to be most pronounced among those with higher levels of human and social capital.

CONCLUSION

Drawing on a difference-in-differences model of interstate mobility following an inadvertent policy reversal in Michigan as a natural experiment, 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. The result is robust to a number of placebo tests and alternative specifications. 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. 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 regional *disadvantage* as far as retention of knowledge workers is concerned.

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Table I: Descriptive statistics for patent sample used for 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=23,351 patents.

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

	pre-MARA	post-MARA	relative risk
Michigan	0.24%	0.32%	1.353
non-Michigan	0.20%	0.13%	0.677
Michigan % increase over non-Michigan			*99.9%*

Table III: Difference-in-differences logistic regressions with varying time windows.

	(1)	(2)	(3)	(4)	(5)
window surrounding MARA	*1983-1988*	*1981-1990*	*1979-1992*	*1977-1994*	*1975-1996*
Michigan * post-MARA	3.5486**	2.7578***	1.5546*	1.4307**	1.5194**
	(1.3146)	(0.8026)	(0.6053)	(0.5092)	(0.4749)
Michigan	-0.4905	-0.2022	0.0864	0.1162	0.0259
	(0.5749)	(0.4065)	(0.3703)	(0.3344)	(0.3223)
post-MARA	-2.7983**	-1.7065	-2.0315	-1.0651	-0.8453
	(1.0617)	(1.1019)	(1.3440)	(0.7659)	(0.7726)
days since last patent (ln)	0.7152*	0.8658***	0.8196***	0.8616***	0.8780***
	(0.2876)	(0.2384)	(0.2061)	(0.1710)	(0.1623)
inventor had emigrated previously	1.5412	2.8531***	2.9092***	3.3181***	3.2777***
	(1.3707)	(0.8636)	(0.6541)	(0.4476)	(0.4531)
prior employer's number of patents (ln)	0.0002	0.0185	-0.0254	-0.0490	-0.0887
	(0.1061)	(0.0786)	(0.0737)	(0.0589)	(0.0570)
inventor's pre-MARA patenting rate	-0.1322	0.3681	0.6034	0.3145	0.3921
	(1.5481)	(1.1217)	(1.0202)	(0.8243)	(0.7462)
auto industry		0.7679	0.4435	-0.3377	-0.4147
		(1.0214)	(1.0596)	(1.2728)	(1.1972)
Constant	-9.8171***	-12.1163***	-12.2614***	-11.8302***	-11.5732***
	(2.8805)	(2.8719)	(2.6316)	(2.0885)	(1.8583)
Observations	6285	10038	15499	20714	23351

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. 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. The auto-industry indicator is dropped in the narrowest window as a perfect predictor. + Significant at the 10% level; * significant at the 5% level; ** significant at the 1% level; *** significant at the 0.1% level.

Table IV: Supplementary analysis using difference-in-differences logistic regressions of domestic emigration.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Control group is enforcing states	*Excludes migration to California*	*Only non-declining Michigan industries*	*Only non-growth industries outside MI*	*State-industry fixed effects*	*Treatment state is Ohio*	*Dependent variable is within-firm transfers*	*MARA date is 1984*	*MARA date is 1986*
Michigan * post-MARA	-0.3093+	1.8777**	1.7871***	1.3219*	2.4726*	0.5102	-0.6289	0.7117	0.8306+
	(0.1583)	(0.6503)	(0.528)	(0.590)	(1.016)	(0.4229)	(0.4750)	(0.5162)	(0.4932)
Michigan	0.1571	-1.4908**	-0.0356	0.2018		1.1641***	0.4237	0.1367	0.2825
	(0.1007)	(0.4697)	(0.333)	(0.356)		(0.2451)	(0.3088)	(0.3623)	(0.3090)
post-MARA	-1.0642*	-0.3465	-0.9946	-0.8826	-1.2618	-0.8558	1.3276	-0.9136	-0.8618
	(0.5260)	(0.8819)	(0.785)	(0.972)	(1.028)	(0.7526)	(1.0165)	(0.7816)	(0.7679)
days since last patent (ln)	0.9510***	0.7899***	0.9172***	0.8999***	0.8735***	0.8672***	0.2728***	0.8259***	0.8789***
	(0.0745)	(0.2092)	(0.164)	(0.160)	(0.153)	(0.1686)	(0.0684)	(0.1588)	(0.1512)
inventor had emigrated previously	-0.2656	3.0940**	3.1315***	3.2912***	3.3673***	0.5574	3.2364***	3.2504***	3.4852***
	(0.4097)	(1.0065)	(0.482)	(0.536)	(0.562)	(0.6518)	(0.4307)	(0.4726)	(0.3970)
prior employer's number of patents (ln)	2.1492***	-0.2648***	-0.0948	-0.1374*	-0.0847	2.1633***	0.1981**	-0.0567	-0.0678
	(0.1614)	(0.0793)	(0.061)	(0.064)	(0.055)	(0.2881)	(0.0622)	(0.0573)	(0.0582)
inventor's pre-MARA patenting rate	-0.1498***	1.0686	0.3610	0.1528	0.5454	-0.0984*	-0.7391	0.2553	0.3032
	(0.0208)	(1.1426)	(0.777)	(0.822)	(0.814)	(0.0482)	(0.6356)	(0.7782)	(0.7232)
auto industry	0.3993***		-0.2660	-0.3097	-0.5899	0.2436	1.0405	-0.2465	-0.3715
	(0.1208)		(1.181)	(1.304)	(1.264)	(0.2520)	(0.9540)	(1.1498)	(1.1785)
Constant	-9.9012***	-10.1128***	-11.7250***	-11.5754***	-28.4934***	-10.2019***	-7.7331***	-11.6309***	-11.5669***
	(0.6557)	(2.0500)	(1.617)	(1.947)	(2.099)	(1.4806)	(1.5826)	(2.0009)	(1.7762)
excludes California	no	yes	no	no	no	no	no	no	no
control group = enforcing	yes	no	no	no	no	no	no	no	no
DV =	emigration	emigration	emigration	emigration	emigration	emigration	internal xfer	emigration	emigration
treatment state	Michigan	Michigan	Michigan	Michigan	Michigan	Ohio	Michigan	Michigan	Michigan
MARA year	1985	1985	1985	1985	1985	1985	1985	1984	1986
Observations	50710	12208	20601	20578	21135	24494	23351	23351	23351

Notes: Observations are for patenting U.S. inventors in Michigan or other non-enforcing states, 1975-1996. 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; *** significant at the 0.1% level.

Table V: Domestic emigration differences for inventors with different track record and collaborative ties.

Panel A: Full sample including Michigan and all control states.

CITATIONS PER PATENT

	median and below				above median		
	pre-MARA	post-MARA	relative risk		pre-MARA	post-MARA	relative risk
Michigan	0.20%	0.33%	1.625	Michigan	0.27%	0.31%	1.134
non-Michigan	0.13%	0.14%	1.112	non-Michigan	0.26%	0.10%	0.395
Michigan % increase over non-Michigan			*46.1%*	*Michigan % increase over non-Michigan*			*186.8%*

DEGREE

	median and below				above median		
	pre-MARA	post-MARA	odds ratio		pre-MARA	post-MARA	odds ratio
Michigan	0.25%	0.22%	0.870	Michigan	0.21%	0.51%	2.388
non-Michigan	0.17%	0.11%	0.635	non-Michigan	0.29%	0.20%	0.710
Michigan % increase over non-Michigan			*37.0%*	*Michigan % increase over non-Michigan*			*236.3%*

Panel B: Restricted sample excluding California and Connecticut from the control states.

CITATIONS PER PATENT

	median and below				above median		
	pre-MARA	post-MARA	relative risk		pre-MARA	post-MARA	relative risk
Michigan	0.10%	0.10%	1.067	Michigan	0.08%	0.15%	1.768
non-Michigan	0.29%	0.24%	0.852	non-Michigan	0.52%	0.52%	1.004
Michigan % increase over non-Michigan			*25.2%*	*Michigan % increase over non-Michigan*			*76.0%*

DEGREE

	median and below				above median		
	pre-MARA	post-MARA	odds ratio		pre-MARA	post-MARA	odds ratio
Michigan	0.13%	0.05%	0.354	Michigan	0.05%	0.18%	3.680
non-Michigan	0.38%	0.21%	0.558	non-Michigan	0.48%	0.51%	1.059
Michigan % increase over non-Michigan			*-36.5%*	*Michigan % increase over non-Michigan*			*247.6%*

Notes: Emigration rate out of Michigan is affected by their exclusion because many Michigan inventors moved to either CA or CT.

Table VI: Regression analysis of domestic emigration differences for inventors with different track record and collaborative ties.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Citations per patent: above the median	*Citations per patent: below the median*	*Citations per patent: above the median - no CA or CT*	*Citations per patent: below the median- no CA or CT*	*Degree: above the median*	*Degree: below the median*	*Degree: above the median - no CA or CT*	*Degree: below the median - no CA or CT*
Michigan * post-MARA	1.9696*	1.2806+	1.7743*	2.2983	1.6609**	1.2772+	2.7794**	1.1238
	(0.904)	(0.699)	(0.860)	(1.498)	(0.643)	(0.721)	(1.001)	(1.505)
Michigan	0.0528	0.0634	-1.9631**	-1.7160+	-0.2100	0.0842	-2.7234**	-1.5517*
	(0.388)	(0.557)	(0.619)	(1.031)	(0.466)	(0.427)	(0.893)	(0.749)
post-MARA	-17.2553***	-0.5175	-0.0754	-0.5385	-0.8423	-0.4812	2.3482+	-1.1246
	(1.195)	(1.007)	(1.261)	(1.114)	(0.855)	(1.350)	(1.406)	(4.318)
days since last patent (ln)	1.1013***	0.6382***	0.7959**	0.6467*	0.7813***	1.0719***	0.5333*	1.2609**
	(0.227)	(0.166)	(0.286)	(0.277)	(0.224)	(0.209)	(0.223)	(0.404)
inventor had emigrated previously	2.4897**	3.5876***	3.0379*	1.2000	3.0475***	1.9542	2.6669**	
	(0.772)	(0.578)	(1.372)	(1.390)	(0.573)	(1.267)	(0.957)	
prior employer's number of patents (ln)	-0.0191	-0.1541+	-0.1078	-0.4535*	-0.0259	-0.1414	-0.1826	-0.1925
	(0.081)	(0.081)	(0.086)	(0.187)	(0.079)	(0.091)	(0.113)	(0.150)
inventor's pre-MARA patenting rate	0.3321	0.2251	0.8202	3.9135*	0.6444	-0.9706	2.3482+	-1.1246
	(1.077)	(1.090)	(1.322)	(1.756)	(0.888)	(1.491)	(1.406)	(4.318)
auto industry		0.0796		1.7598*		0.4257		
		(1.505)		(0.754)		(1.053)		
Exclude California and Connecticut?	no	no	yes	yes	no	no	yes	yes
Constant	-12.0385***	-9.2847***	-11.8498***	-7.3185**	-12.0285***	-10.8265***	-8.7701***	-28.9845
	(1.994)	(1.652)	(2.526)	(2.434)	(2.094)	(2.142)	(2.240)	(0.000)
Observations	7,991	11,405	3,689	6,088	6,387	12,969	968	2,524

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 models are matched using Coarsened Exact Matching. Robust standard errors are in parentheses, clustered by inventor. The auto-industry indicator is dropped in some models as a perfect predictor.
+ Significant at the 10% level; * significant at the 5% level; ** significant at the 1% level; *** significant at the 0.1% level.