# The Value of Employee Retention: Evidence from a Natural Experiment

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## Abstract

We estimate the firm-level returns to retaining employees using difference-in-differences analysis and a natural experiment where the enforcement of employee non-compete agreements was inadvertently reversed in Michigan. We find that non-compete enforcement boosted the short-term value of publicly-traded companies by approximately 9%. The effect is increasing in local competition and growth opportunities, and offset by patenting.

## 1. Introduction

Organization scholars have long explored the ways that firms seek an advantage over markets in coordinating production (Alchian and Demsetz, 1972; Williamson, 1979). Much research has focused on the efficiency of matching workers to jobs (Jovanovic, 1979), the coordination of complementarities (Milgrom and Roberts, 1990), and the development of firm-specific human capital (Becker, 1962). Although employees can be a source of great value for firms, employee departure can impose costs and destroy value.

This study sheds light on a related question: what is the value of *preventing* employee departure? We build on theories of human and organization capital to show how employee departure affects firm value. Empirically, we focus on the value of employee non-compete agreements as a mechanism for restricting employee departure. When signing a non-compete, a worker promises not to work at a rival firm for a specified period of time after leaving. Garmaise (2011) has shown that firms are able to pay lower wages given non-competes, and Starr (2014) argues that non-competes enable firms to invest more in worker training, but to date no link between non-competes and firm value has been found. The implications for firm value of non-compete agreements are not immediately clear because non-competes at once prevent the loss of human capital *to* a competitor and block the firm's ability to poach *from* a competitor.

We exploit an inadvertent reversal in the enforcement policy of non-compete agreements in Michigan during the 1980s as a natural experiment to estimate the firm value of an important friction to employee mobility. We find that firms in Michigan enjoyed a boost in Tobin's $q$ of approximately 9% ($5.56 million for the median, public firm in Michigan), relative to firms in states that continued to not enforce non-competes. This result is comparable in magnitude to Daines (2001), who finds that firms incorporated in Delaware (and thus subject to superior corporate law) have valuations 5% higher than comparable firms not incorporated in Delaware. We also find the boost to be greater when there is more local competition, when employee mobility is a greater concern, and when firms have more growth opportunities. Finally, we find the valuation increase to be offset by patenting, likely due to protections afforded by intellectual property. We interpret the results, along with extensive robustness checks and placebo tests, as evidence that an ability to retain employees can boost the value of public firms in the short run. Nevertheless, we cannot rule out the possibility that another change may account for some of the gain. We also examine the *near-term* effect on *public* firms, which tend to be larger and more mature than private firms, and draw no conclusions regarding the long-term effect of non-competes, or the effect of non-competes on private, smaller, or startup firms.

## 2. The Impact of Employee Departure

The efficient coordination of employees, as opposed to contracting, is a source of advantage for firms over markets (Williamson, 1975, 1979). Research highlights the value of managerial expertise (Castanias and Helfat, 1991) and practices (Bloom and Van Reenen, 2007), suggesting that it is important to identify and promote the most productive managers (Lazear et al., 2012). In many ways, the process of developing and deploying human resources is similar to other physical, technological, or financial resources: inputs are identified, acquired, and improved. In particular, firms invest considerable resources and time into learning about the particular abilities and complementarities of their employees. Firms configure individuals into teams and attach them to tasks that match their abilities. The first attempt is often sub-optimal, and configurations often evolve as firms adjust to what is revealed (experimentally) to work. Why one arrangement works, and another does not, may be ambiguous and/or so complex as to be tacit—held only in the minds of effective managers. As such, causal ambiguity (or at least ex post uncertainty reduction) plays a key role in the job matching process. All of these investments lead to the development of organizational capital. Both human and organizational capital, however, depend on the continued presence of people, who – unlike property, plant, and equipment – are not owned by the firm, but merely *employed.*

Many scholars view private knowledge as a cornerstone of competitive advantage (Peteraf, 1993; Grant, 1996), and managers rate "know-how" as one of the most important resources of the firm (Hall, 1993). Strategy scholars place particular importance on the role of private knowledge, embodied in firm-specific human capital (Castanias and Helfat, 1991), as a source of quasi-rent generation and appropriation (Klein et al., 1978). While non-disclosure agreements may deter ex-employees from sharing confidential information, it is difficult to know whether the agreement has been violated, let alone gather evidence that such has occurred and win damages in court. Moreover, knowledge is often tacit (Polanyi, 1966), giving individuals an "own-use advantage" (Spulber, 2012) such that individuals have incentives to "spin-out" and form new firms (Agarwal et al., 2004). These concerns lead many firms to codify and patent inventions as a means of protecting themselves from their own employees (Kim and Marschke, 2005). In extreme cases, employees can threaten to hold-up the firm when their departure would cause significant delays and/or lead the employer to miss out on opportunities.

In addition to individuals' knowledge, firms seek to protect collective expertise. Although the literature has generally characterized expertise as a firm-specific asset, it may also be that such value is tied to colleagues (Hayes et al., 2006). In fact, precisely because individuals are embedded into larger teams (Chillemi and Gui, 1997), their departure can disrupt social systems and configurations they leave behind. Inversely, research suggests that "individual" talent is most portable when it is transferred as part of a team, because doing so keeps a complex system of social interactions intact (Groysberg and Lee, 2009). To the extent that employee mobility undermines co-specialization, constraints on mobility may be as important for protecting and sustaining a competitive advantage as firm-specificity itself (Campbell et al., 2012).

*2.4 Employee Non-compete Agreements*

Firms may use incentives to entice employees to stay, but they may also create *dis-*

incentives to keep them from leaving. One such tool is a post-employment covenant not to compete, an employment contract that restricts the behavior of workers after they leave the firm, usually for 1-2 years, but often longer (Marx, 2011). A non-compete either lists specific firms, or describes an entire industry, where the employee is not allowed to compete by either joining or starting another company. Several empirical papers have established the effectiveness of non-competes at binding employees to their employers (Fallick et al., 2006; Marx et al., 2009; Garmaise, 2011). Others have shown the deleterious impact of non-competes on regional entrepreneurship, spillovers, and labor supply (Samila and Sorenson, 2011; Belenzon and Schankerman, 2012; Marx et al., 2014), also consistent with the notion that non-competes block workers from appropriating general knowledge and skills outside the firm.

However, scholars have only begun to examine the firm-level impact of non-compete agreements. Garmaise (2011) uses non-compete policy adjustments to show that firms pay executives less when they can restrict extraorganizational opportunities via non-competes. Younge, Tong, and Fleming (2014) find that public firms are more likely to be acquired when non-competes are enforceable, presumably because more value of the workforce can be retained post-acquisition. Starr (2014) suggests that firm-sponsored training is more common in litigious occupations when enforcement is stronger. Although prior work suggests that there may be a link between non-competes and firm value, no direct connection has been demonstrated to date.

One reason a link might not have been found is that the use of non-competes by rivals offsets the benefits of use by a focal firm. While a firm can retain talent with non-competes, so can rivals.[1] Firms therefore may have an easier time in retaining employees, but also a more difficult time in hiring new ones, or accessing spillovers from rivals (Belenzon and Schankerman, 2012). If all capital were general, the overall outcome might simply depend on a given firm's need for

[1] Franco & Mitchell (2008) also develop a model in which the impact of non-competes depends on the stage of the industry lifecycle; early entry is favored by non-compete agreements, a trend that reverses as an industry matures.

retention, versus their need to recruit employees or access spillovers. Nevertheless, firm-specific skills must be learned over time. When skills are lost through employee departure, new hires have to be trained and integrated into the firm, often into very specific teams. Multiple studies have shown that work groups become more effective over time as they learn each other's strengths and weaknesses (Reagans et al., 2005; Huckman et al., 2009). We therefore expect the costs of replacing firm-specific and organization capital to dominate in the short term. In the long run, however, the causal effect of non-competes is very much an open research question.

## 3. Hypotheses

Firm valuation is inherently a 'forward-looking' concept, presumed to equal the expected present value of all future payouts to claimholders (Tobin, 1969). Market valuation therefore reflects expectations about risk, investment opportunities, and future conditions. When employees depart, they disrupt management practices, distract the firm from managing assets, and delay the exercise of growth options – all of which reduce the expected rate of return for human assets in place, as well as the real option value of associated growth opportunities.

Human resources clearly have value. Lev and Radhakrishnan (2005) show a connection between firm value and organization capital, as proxied by SG&A; Atkeson and Kehoe (2005) find that payments from organization capital account for more than 40% of cash flows from intangible assets; and Eisfeldt and Papanikolaou (2013) find that higher levels of organization capital are related to higher average returns. The value of organization capital, however, is at risk of employee departure and can be compromised when workers leave (Chillemi and Gui, 1997). When an individual or team of individuals leave, it is essential not only to replace their general skills, but also to re-develop the firm-specific expertise and efficiencies that are missing in new hires. Employee non-compete agreements are a mechanism for reducing such costs. In sum, we predict that the valuation of human resources, and thus the firm, will increase in the near-term if there is an unanticipated increase in the enforceability of non-competes.

**H1.** *In the short run, increased enforceability of non-competes will increase firm value.*

### *3.1 The Moderating Effect of In-state Competition*

Firms facing greater regional competition should benefit more, in the short-run, from the ability to retain employees. Given that non-competes constrain the movement of employees to rivals, we anticipate that the effect of non-competes will be amplified in highly competitive regions and industries, for four reasons. First, poaching employees will be more attractive and valuable when there are other firms in the area that value similar skills. (Although firms outside the industry may seek employees with similar skills, if they serve different customers and markets the non-compete would not apply.) Second, prior research shows that competitors may find it easier to attract workers who do not need to relocate (Almeida and Kogut, 1999). Third, when a non-compete dispute arises among firms, it is easier to resolve the governing law when both parties are in the same state Garmaise (2011). Fourth, some non-competes are geographically constrained to a given radius of the company, part of the state, or region of the country (Malsberger, 2004). For all of these reasons, we anticipate:

**H2.** *In the short-run, the effect of non-competes on firm value is increasing in instate competition.*

### *3.2 The Moderating Effect of Patenting*

Given the firm's interest in blocking the loss of, or misappropriation of, human skills and knowledge, the impact of non-competes on firm value may depend on patenting activity. Since Griliches (1981), numerous scholars have investigated whether patent protection contributes to the market value of firms. The general consensus is that it does (Cockburn and Griliches, 1988; Schankerman, 1998; Harhoff et al., 1999), and we expect patenting to offset the effect of non-competes in three ways. First, firms with patents can sue ex-employees for infringement, discouraging them from leaving (Agarwal et al., 2009). Kim & Marschke (2005) find that the risk of employee departure leads to a higher propensity for firms to patent. Second, codifying knowledge into a patent may help firms to reduce their dependency on particular employees by converting tacit knowledge into codified knowledge held by the firm. Third, patenting may reduce outbound spillovers and curb the ability of rivals to use proprietary information. If so, competition may be attenuated and the value of the firm boosted. (Alternatively, Franco and Mitchell (2006) consider a possibly countervailing effect of non-competes on product market competition, which we explore in the robustness section.) While it is difficult to distinguish between these mechanisms, all three lead us to expect:

> **H3.** *In the short-run, the effect of non-competes on firm value is decreasing in patent activity.*

*3.3 The Moderating Effect of Growth Options*

Further building on our baseline hypothesis (H1), we argue that firms with greater growth opportunities incur higher costs when employees depart. Pursuing growth opportunities requires considerable cooperation and flexibility by workers. Given that firms are able to directly observe their own workers, we argue that employers are better able to staff growth-oriented positions when they staff those positions internally. As Kogut and Zander (1992:383) point out, "Because new ways of cooperating cannot be easily acquired, growth occurs by building on the social relationships that currently exist in a firm." Firms may prefer to transfer or promote internal employees with stronger social ties, even if doing so requires more training or on-the-job learning after they take the position. To the extent that non-competes reduce churn and increase tenure, they will be more valuable for firms with growth options. Of course, growth opportunities are intangible and difficult to identify. Following the real options literature (Myers, 1977; Trigeorgis, 1996), we test separate proxies where growth opportunities are expected to be lower or higher (see Smith and Watts, 1992, for example). We follow Grullon et al. (2012) and test four separate proxies for growth options: revenue growth, R&D intensity, firm size, and firm maturity.

**Revenue Growth.** It may seem straightforward that faster-growing firms will continue to have greater opportunities in the near future. However, there are other, reinforcing reasons why faster growing firms may benefit from a sudden enforcement in non-competes. First, growing firms may be targets for labor poaching. Faster-growing firms attract media attention, making them more salient to competitors and recruiters. Second, faster-growing firms should have newer organizational structures, lower comprehensibility, and less taken-for-grantedness in organizational objectives – resulting in greater internal conflict over legitimacy (Suchman, 1995) and greater employee departure. Third, it is more difficult to match workers to jobs as firms grow faster: "the more rapid is the rate of growth in the firm, the faster must employees be promoted to positions either where their performance is less easily measured or where a bad job match is very costly" (Prescott and Visscher, 1980: 459). For all of these reasons, we predict:

**H4a.** *In the short-run, the effect of non-competes on firm value is increasing in revenue growth.*

**R&D Intensity.** Considering that the very purpose of research and development is to generate investment opportunities, it is reasonable to expect that the larger a firm's R&D expenditures, the more growth options the firm will have. Moreover, given that non-competes in theory serve to block the diffusion of trade secrets, we anticipate that the impact of enforceable non-competes on valuation should be increasing in the level of R&D. We predict:

**H4b.** *In the short-run, the effect of non-competes on firm value is greater for firms with higher R&D intensity.*

**Firm Size and Maturity.** Prior research demonstrates that larger firms tend to have a larger proportion of value tied to assets in place, whereas smaller firms rely more heavily on opportunities for growth (Brown and Kapadia, 2007). Regarding age, older established firms have a larger proportion of value tied to assets in place (Lemmon and Zender, 2010) and generate innovations of lower quality (Balasubramanian and Lee, 2008) and lower market value.

**H4c.** *In the short-run, the effect of non-competes on firm value is less for larger firms.*

**H4d.** *In the short-run, the effect of non-competes on firm value is less for mature firms.*

## 4. Empirical Strategy

Non-compete agreements are governed on a state-by-state basis instead of by a federal statute. Although there is variation in the enforceability of non-competes between states, unobserved heterogeneity in the quality of firms can confound a cross-sectional analysis. We therefore exploit an apparently-inadvertent policy reversal in Michigan as a natural experiment to establish the causal impact of non-competes on firm value, an event that has been employed by multiple scholars (Marx et al., 2009; Belenzon and Schankerman, 2012; Younge et al., 2014).

Non-compete enforcement in Michigan had long been governed by Public Act No. 329 of 1905, Section 1: *"All agreements and contracts by which any person, copartnership, or corporation agrees not to engage in any avocation, employment, pursuit, trade, profession or business, whether reasonable or unreasonable, partial or general, limited or unlimited, are hereby declared to be against public policy and illegal and void."* This Act prohibited the use of non-competes until 1985, when the Michigan Antitrust Reform Act (MARA) was passed. The stated purpose of MARA was to centralize and standardize existing doctrine regarding antitrust policy, including collusion and price-fixing (Bullard, 1985). MARA repealed laws regarding antitrust issues. Among those repealed was Public Act No. 329, a law that largely addressed antitrust issues such as "maintain[ing] a monopoly of any trade" (Sections 2-4) and "combinations in restraint of trade" (Sections 5 and 6).

The available evidence suggests that the Michigan legislature repealed Public Act No. 329 due to its antitrust implications, and not to change the enforceability of employee non-compete agreements. More than twenty pages of legislative analysis by both House and Senate subcommittees in Michigan (Bullard, 1985) discuss antitrust concerns as the motivation behind MARA but fail to mention "non-competes" or "covenants not to compete" in deliberations regarding the new statute. It therefore appears that the legislature did not realize that Public Act No. 329 also prohibited the enforcement of non-competes. We could not locate any discussion of Michigan's non-compete policy in law journals just prior to the passage of MARA, whereas we have found multiple articles in the months following the reversal (Alterman, 1985; Levine, 1985; Sikkel and Rabaut, 1985). These findings suggest that the legal community was not aware of the potential for MARA to reverse the long-standing policy of prohibiting non-competes. Interviews with Michigan lawyers active at the time of MARA also support an interpretation that the change in non-compete policy was inadvertent.[2]

In 1987, the Michigan legislature addressed the non-compete issue. Importantly, it did *not* reinstate the pre-MARA ban. Rather, it instituted the "reasonableness" doctrine regarding non-competes, namely, that such post-employment agreements could not be unlimited in duration and scope: Decker (1993) notes that most states allowing non-competes have some version of the reasonableness doctrine in place. Moreover, the reasonableness doctrine was enacted retroactive to MARA.

The evidence therefore suggests that the reversal in non-compete enforcement was unanticipated, yet persistent. Moreover, the newfound enforceability of non-competes following MARA would have applied not only to new employment contracts but also to those previously in place. It might seem counterintuitive for firms to ask employees to sign non-compete agreements where such are unenforceable, but in fact firms frequently do so. California has never allowed non-compete agreements to be enforced, yet Garmaise (2011) finds that 58% of public Execucomp firms headquartered in California report using non-competes with their executives during the period 1992-2004. Further, Kaplan and Stromberg (Kaplan and Stromberg, 2003) find that 70.4% of venture capitalists require that non-compete agreements be used by their portfolio companies, a

[2] 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." His account was 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."

figure which rises only to 73.5% when California is excluded.[3]

It would appear that firms and markets came to fully appreciate the policy change only gradually, preventing us from performing a sharp event study such as abnormal returns. Instead, the Michigan experiment lends itself to a difference-in-differences (DD) analysis (Meyer, 1995). In our analysis, we assign Michigan firms to the 'treatment group,' and firms from states that continued to proscribe employee non-compete agreements to the 'control group.' There were ten states that did not enforce non-competes before or after MARA (Stuart and Sorenson, 2003); we use firms from those states as our control group. By assuming that firms in control states represent what would have happened to firms in Michigan in the absence of MARA, our strategy is to identify the before-to-after difference around the time of MARA, while removing trends from the comparison group. In doing so, our strategy assumes equal trends between treatment and control (Blundell and Costa-Dias, 2000), an assumption we examine in Figure 1.

**----- Insert Figure 1 about here -----**

Figure 1 plots the valuation of publicly traded firms in Michigan on the vertical-axis, across the twenty-year period around MARA on the horizontal-axis (1974-1993). Valuation is calculated as Tobin's $q$ (defined in the next section) and observations are averaged by industry and weighted by total assets to be comparable with our analytics. It appears that Tobin's $q$ was fairly similar between the two groups, but diverged in the late 1980s and converged again in the early 1990s. Figure 1, of course, does not control for factors that may have differed between treatment and control due to the lack of random assignment. In the next section we analyze a tighter window

[3] Firms may require employees to sign non-competes even when non-competes are not enforceable. First, employees may be poorly informed regarding the nature of enforceability and, even if informed, may nonetheless be risk averse with respect to the outcome of legal action and disinclined to engage in the expense and inconvenience of a lawsuit. Moreover, non-compete agreements from one jurisdiction may be enforceable in other jurisdictions, such as when an employee moves to a state that does allow enforcement. While we cannot find firm-by-firm data regarding the use of non-compete agreements in Michigan prior to MARA, the foregoing findings give strong reason to believe that many firms had previously asked employees to sign non-competes which were then activated by the MARA reform.

around MARA, and a sample of firms reporting R&D expense and other covariates, to better control for differences between the treatment and control groups.

## 5. Data and Methods

The sample for our formal analysis includes annual, firm-level data from Compustat for 1984 through 1989, the 3-year window before and 3-year window after MARA. We selected all U.S. manufacturing firms publically listed prior to MARA that were physically headquartered in Michigan or a control state (Alaska, California, Connecticut, Minnesota, Montana, North Dakota, Nevada, Oklahoma, Washington, and West Virginia) that did not enforce non-competes either before or after MARA (Stuart and Sorenson, 2003). Given that ratios can take on extreme values, we dropped the top and bottom 2.5% of observations based on Tobin's $q$ (a robustness test with outliers obtained consistent results), and winsorized all other ratios at the +/– 2.5% level. We merged in data from the NBER patent file (Hall et al., 2001) and assumed *Patents* to be zero when a firm did not have a patenting record.

### *5.1. Dependent variable*

Consistent with prior research on the valuation of intangible assets (e.g., Hirschey, 1982; Villalonga, 2004; Hall et al., 2005), the dependent variable in our valuation models is Tobin's $q$, which we observe for each firm at the end of each year. This seems appropriate for our study given that human capital and organization capital do not appear on the balance sheet. Research suggests that detailed calculations of $q$ (e.g., Lindenberg and Ross, 1981; Lewellen and Badrinath, 1997) may induce sample-selection bias due to data unavailability (DaDalt et al., 2003). We therefore calculated a simple approximation of $q$, defined as the market value of common stock + book value of total assets – book value of common equity, all divided by the book value total assets. As a robustness check, we also tested the Chung & Pruitt (1994) approximation of $q$ and found similar results. Intangible assets may have a multiplicative rather than additive effect on firm value due to fixed costs in developing intangible assets; a semi-log functional form is therefore strongly preferred over a linear functional form for our regression equation (Hirsch and Seaks, 1993). We therefore take the log of *q* as our dependent variable.

*5.2. Explanatory variables*

The variable *Michigan* is an indicator for firms located in Michigan based on the state of each firm's headquarters (not the state of incorporation); *After* is an indicator that equals one for all years following treatment; and the interaction *Michigan * After* identifies the difference-in-differences treatment effect of MARA. Considering that it was not until late 1985 that legal scholars first discovered the change in the enforcement of non-competes and published the discovery in the Michigan Bar Journal (Levine, 1985; Sikkel and Rabaut, 1985), and considering that it then would have taken time for information about the policy change to diffuse out to firms, we take 1987 to be the first full year of treatment in which the policy change would be reflected in Tobin's *q*.

*5.3. Moderating variables*

In addition to the main DD effect of MARA on *q*, we also explore three moderating factors. In each case, we use the pre-MARA value of the moderating variable held constant through the post-MARA period. To estimate how the basic DD effect differs by level of each moderator, we interacted the basic *Michigan * After* effect with each moderating variable and include all lower-level interactions. We calculated *Instate Competition* (H2) as the proportion of total U.S. sales generated by other firms located in the same state and same SIC-3 digit industry as a focal firm based on data from the Compustat Segments file, and mean-center the variable. Our variable for H3, *Patents*, is simply the (logged) number of patent applications submitted in a given year that are eventually granted. Our primary measure for growth opportunities (H4) is *Revenue Growth,* the percent change in gross revenue from the prior year (H4a). However, we follow Grullon et al. (2012) and also test three additional proxies (or inverse proxies) for growth options: an indicator for *High R&D* (H4b), equal to 1 for firms over the median split of R&D intensity in a given year; *Firm Size* (H4c), equal to total revenues divided by 1,000; and an indicator for *Mature Firms* (H4d), equal to zero for firms publically-listed for less than 10 years as of 1987 (the *After* year), and one for public firms listed 10 years are more as of 1987.

*5.4. Control variables*

We added covariate controls to many models to adjust for differences in trends between the treated state (Michigan) and comparison states. While trends over time in comparison states serve as the basic control in a differences-in-differences specification, additional covariates help to control for differences between regions. At the industry level, we use fixed effects by SIC-3. At the firm level, we controlled for each firm's financial condition and other characteristics that might affect a firm's market valuation. Table 2 provides a description and summary statistics for all control variables used in the multivariate analyses.

*5.5. Model specification*

We used ordinary least squares (OLS) with robust standard errors clustered by firm to estimate the equation: $\ln(q_i) = \beta_0 + \beta_1 Michigan_i + \beta_2(Michigan_i * After_i) + \boldsymbol{T_t} + \boldsymbol{I_j} + \varepsilon_i$ .

$\text{Ln}(q_i)$ is the log of *Tobin's q* for each firm-year observation, $\boldsymbol{I_j}$ is a vector of SIC-3 indicators, $\boldsymbol{T_t}$ is a vector of year indicators, and $\varepsilon_i$ is the error term. We included annual indicators to control for changes in *q* over time due to business cycles, market swings, etc.; however, the complete set of annual indicators is collinear with *After*, so the main effect of *After* is not included in specifications that have year fixed effects. In Equation 1, the main DD effect is estimated by $\beta_2$. We included industry indicators at the SIC-3 level to control for between-industry differences. For moderated diff-in-diff analyses, we added a set of covariate control variables ($\boldsymbol{X_j}$) and triple-diff interactions for *Instate Competition (C)*, *Patents (P)*, and *Growth Opportunities (G)*, as estimated by the equation:

$\ln(q_i) = \beta_0 + \beta_1 C_i + \beta_2 P_i + \beta_3 G_i + \beta_4 Michigan_i + \beta_5(Michigan_i * After_i) + \beta_6(Michigan_i * C_i) + \beta_7(After_i * C_i)$
$+ \beta_8(Michigan_i * After_i * C_i) + \beta_9 (Michigan_i * P_i) + \beta_{10}(After_i * P_i) + \beta_{11}(Michigan_i * After_i * P_i) +$

$\beta_{12}(Michigan_i * G_i) + \beta_{13}(After_i * G_i) + \beta_{14}(Michigan_i * After_i * G_i) + \delta \boldsymbol{X}_i + \boldsymbol{T}_t + \boldsymbol{I}_j + \varepsilon_i$.

## 6. Results

The MARA policy change in Michigan created an environment in which firms could begin to enforce non-compete agreements against ex-employees; we therefore predict that firms were better able to stem the loss of firm-specific human capital and protect organization capital after the policy change, at least in the short-run. Consistent with our baseline hypothesis, Figure 1 suggests that trends in Tobin's *q* were roughly similar for publically listed firms in Michigan and control states up to 1987 (the first year in the *After* period). Following MARA, however, trends for Michigan and control states diverge: *q* rises for Michigan firms, both in absolute terms and with respect to the control group. A similar picture emerges from the regression analysis in Table 1. We examine the +3/–3 year window immediately around MARA using univariate OLS models with an indicator for Michigan, an indicator for post-MARA period, and an indicator for the interaction between the two; we exclude all factors that may have been endogenous to MARA. Corresponding to Figure 1, the sample includes all publically listed firms in Michigan and control states; the dependent variable is the natural logarithm of *Tobin's q.* Given a semi-log specification, we interpret coefficients in Table 1 (and thereafter) as approximately equal to a percentage change in *Tobin's q*.

**----- Insert Table 1 about here -----**

Column 1 reports results from a basic model with no industry fixed effects, no year fixed effects, and no matching of observations; here we find that enforceable non-competes were associated with an average increase the market value of firms in Michigan by 7.68%, with statistical significance at the 5% level. In Column 2 we add industry fixed effects (at the SIC-3 level), and in Column 3 we add year fixed effects. Fixed effects increase the magnitude of the basic effect to 9.28%, and increases statistical significance to the 1% level.[4] Finally, in Column 4 we apply

[4] The *After* indicator becomes collinear with a complete set of annual indicators and is dropped from such models.

propensity score matching (PSM) and match two comparison observations to each treated observation based on the multivariate probability of selection into *Michigan* (Rosenbaum and Rubin, 1983).[5] PSM drops the sample size from 4,559 to 1,660, reduces the magnitude of the effect slightly to 8.18%, and lowers statistical significance back to 5%; matched results, however, suggest that the basic effect is robust to the composition of the sample, and that the magnitude of the basic effect is approximately 9%.

Both Figure 1 and Table 1 lend support to our baseline hypothesis (H1). While a comparison group serves as the basic control mechanism in a DD analysis, the inclusion of covariates may help to adjust for differences in trends between the treatment and control groups (Blundell and Costa-Dias, 2000). Therefore, in the next sections we move to a multivariate regression framework with the inclusion of control variables. Many firms in the full sample, however, did not report a complete set of control variables; we therefore reduced the sample down to those firms reporting R&D expense and other control variables, and use that sample for further analysis. Table 2 provides a description and summary statistics for the reduced sample ($n$=1,874). As a robustness check, we replicated Table 1 on the reduced sample and found results to be similar in magnitude and statistical significance (available from the authors).

**----- Insert Table 2 about here -----**

In Column H1 of Table 3, we add covariate controls, year fixed effects, and industry fixed effects; we again find support for our base hypothesis, as the coefficient on *Michigan * After* reflects a 12.74% increase in $q$, significant at the 1% level. In Columns H2 through H4a, we test interaction terms for our moderating hypotheses. As predicted, we find that *Instate Competition* and *Growth Opportunities* appear to increase the strength of the basic effect of MARA whereas *Patents* seems to attenuate it: *Michigan * After * Instate Competition* has a positive coefficient that is significant at the

[5] We matched on *Assets*, *ROA*, *SG&A*, and *SIC-2*. We continued to include year and industry fixed effects, and matched on pre-MARA values to ensure that MARA did not influence the matching criteria.

10% level, *Michigan * After * Patents* has a negative coefficient that is significant at the 5% level, and *Michigan * After * Revenue Growth* has a positive coefficient that is significant at the 5% level. Following prior work (Grullon et al., 2012), we also test three alternative proxies for growth opportunities: an indicator for *High R&D*, *Firm Size* (inverse proxy), and an indicator for *Firm Maturity* (inverse proxy). The effect of MARA appears stronger for firms with a higher-than-median level of R&D (significant at the 10% level), lower for larger firms (significant at the 1% level), and lower for mature firms (i.e., firms past the first 10 year period after their IPO), significant at the 5% level. Results for all three alternative measures of growth opportunities are consistent with our prediction that firms with greater growth opportunities have more to lose from employee departure.

**----- Insert Table 3 about here -----**

To illustrate the interaction and economic significance of each moderator, Figure 2 plots the predicted DD percentage increase in *Tobin's q* (y-axis) across a percentile range of the moderating variable (x-axis). Predictions are based on the models reported in Table 3. Michigan firms at the 30$^{th}$ percentile of *Instate Competition* had a 9.25% predicted DD increase in *Tobin's q*, whereas Michigan firms at the 70$^{th}$ percentile had a 16.50% increase in *q*. Now working against the effects on non-compete enforcement, Michigan firms that did not patent in the year before MARA had an 18.58% predicted DD increase in *Tobin's q*, whereas Michigan firms at the 70$^{th}$ percentile of patenting in the year before MARA had only a 10.44% increase in *q*. Finally, Michigan firms at the 30$^{th}$ percentile of *Revenue Growth* had only a 1.98% predicted DD increase in *Tobin's q*, whereas Michigan firms at the 70$^{th}$ percentile had a 22.80% increase in *q*.

**----- Insert Figure 2 about here -----**

*6.1. Robustness*

While the natural experiment in Michigan addresses many concerns regarding unobserved heterogeneity, one might nonetheless be concerned that other factors in Michigan and/or the control states could have shifted so as to produce our results. In this section, we provide a battery of robustness checks, placebo tests, matched samples, alternative comparisons, and supplemental analyses to check the plausibility of our results.

**Matched Samples.** Matching can be used to improve common support between treated and control observations and to reduce model dependence (Heckman et al., 1998); we therefore checked the robustness of our moderated DD effects with matched samples. In Column 1 of Table 4, we followed our approach from Column 4 of Table 2 and used propensity score matching, with 2 controls for each treated observation, matched on the basis of *Assets*, *Return on Assets*, *Selling General & Administrative Expense*, and industry at the SIC-2 level. We retained all covariate controls, year fixed effects, and industry fixed effects at the SIC-3 level, and again matched on a pre-MARA basis to ensure that MARA did not influence the matching criteria. With PSM we continue to find results in the 10 % range (10.19%), and significant at the 5% level. Next, to check the robustness of our matching approach, we turn to Coarsened Exact Matching (Iacus et al., 2011), a non-parametric algorithm that segments the joint distribution of covariates into strata and weighted observations from each stratum to match observations between the treatment and control groups (see Azoulay et al., 2010 for a recent application of CEM). We matched on *Assets*, *Return on Assets*, *Selling General & Administrative Expense*, and industry at the SIC-2 level, while continuing to include year and industry fixed effects. The matched observations were then analyzed in Column 2; with a CEM sample, we find that enforceable non-competes are associated with increased market value of firms in Michigan by 13.47%, with statistical significance at 5%.

**----- Insert Table 4 about here -----**

**Alternative Comparisons.** Although our results are robust to matched samples, it is possible that misleading cases were included in the comparison set. To check the sensitivity of our results to composition of the comparison set, we changed the definition of the comparison group in Column 3 from firms in states identified by Stuart and Sorenson (2003) as not enforcing non-competes, to firms in states rated by Garmaise (2011) as having a low enforcement-level.[6] We find a magnitude for *Michigan * After* of 9.45%, significant at the 5% level, similar to our results from propensity score matching and other models. Another worry could be that the auto industry in Michigan was special, and that matching fails to control for the unique characteristics of that industry. Therefore, in Column 4 we drop the Auto industry *entirely*, and again find a magnitude for *Michigan * After* of 9.77%, a result that again is similar to results from propensity score matching and other models, significant at the 5% level.

Next, it is possible that a spurious "Midwest effect" could explain a rise in *Tobin's q* in Michigan. To rule out the possibility that regional trends in the Midwest (unrelated to MARA) explain the observed boost in firm value, we pretend in Column 5 that the MARA reform took place in states near Michigan (OH, IN, IL, WI, PA) and mark firms from those Midwest states (but not Michigan) as 'treated.' We then performed a placebo DD test, comparing the (non-Michigan) Midwest firms to the same control cases and find an insignificant effect for the *State * After* coefficient. Results from the placebo test are consistent with the view that our results relate to the passage of MARA, and do not relate to regional developments.

**Antitrust Reform**. In addition to changing the enforcement of non-competes, MARA also instituted antitrust reforms intended to reduce price-fixing and collusion. Although the evidence on the link between antitrust enforcement and price-fixing is mixed (Feinberg, 1980; Choi and Philippatos, 1983; Sproul, 1993; McCutcheon, 1997), event studies show that markets respond negatively to antitrust actions (Bosch and Eckard Jr, 1991; Gunster and Van Dijk, 2011). We therefore expect that the antitrust provisions in MARA should *depress* firm value, especially for the public firms that are the subject of this study. To check this possibility, we examined another prominent antitrust reform that occurred around the same time as MARA: the Texas Free

[6] Garmaise (2011) rates the enforceability of non-competes from 0 to 9. Column 3 tests a comparison set from control states ranked below 5 (AK AZ CA CO CT HI MT NH NM NY ND OK RI VA WV WI LA ME MS NE NJ NC WY).

Enterprise and Antitrust Act of 1983 (Folsom, 1991: page 955, footnote 54). Using the same set of comparison firms used in our main analysis, Column 6 tests the DD effect on *q* for firms in Texas and we find a statistically insignificant coefficient on the *State * After* variable. This suggests that antitrust aspects of MARA are unlikely to explain a *boost* in *q*.

**Employee mobility.** Our theory builds on the notion that employee non-compete agreements promote employee retention. While this is well-established in the non-compete literature (Fallick et al., 2006; Marx et al., 2009; Garmaise, 2011), we check that similar patterns of mobility hold in our sample of publicly-traded firms. Ideally, we would examine arrivals and departures of all employees, but such data is not available for our sample. We therefore follow Almeida and Kogut (1999), Tratjenberg, Shiff, and Melamed (2006); Agrawal, Cockburn, and McHale (2006); Breschi and Lissoni (2009), and many others, and use patent data to track employee mobility. While the patent record does not include all employees, it does include knowledge workers, whom firms often attempt to retain with non-competes.

Table 5 reports the results of our robustness check for employee mobility. We limit the analysis to employees of the publicly-traded firms in our sample, link the firms in our sample to the NBER patent file, identify inventors from the Dataverse repository (Lai et al., 2009) for all patents in our sample, then retrieve *all* patents from co-inventors (so as to check for employee mobility, regardless of whether that was within, from, or into our sample), checked firm names by hand for duplicates, and eliminated moves to non-profit entities such as universities or governmental jobs where non-competes would not apply. Inventors with only a single patent are discarded from the analysis for a move can be calculated only for inventors with more than one patent. The dependent variable is 0 or 1, depending on whether a given patent represents a move as determined by a change in assignee. We included covariates for the total number of patents per firm, the total number of patents per inventor, and days elapsed since an inventor's prior patent. Table 5 reports the results of a difference-in-difference analysis, showing that mobility is substantially lower for Michigan inventors in our sample, following MARA.

**----- Insert Table 5 about here -----**

**Product Market Competition.** Prior theoretical work suggests that non-compete agreements may affect product market competition (Franco and Mitchell, 2005). If non-compete agreements reduce the generation of 'spin-outs' from established firms, then non-competes may also reduce competition in product markets. If this is the case, then an increase in the enforceability of non-competes may increase Tobin's *q*, but not for the reasons we suggest – a valuation increase might be due to monopolistic protection from competition instead of protection of organizational capital. To check for this possibility, we performed a difference-in-differences analysis of product market competition before and after MARA, using the same model specifications and sample reported earlier. We found no evidence that product market competition shifted in Michigan after MARA (the coefficient on *Michigan * After* was near zero and statistically insignificant). We report complete details of the analysis in an Online Appendix.

**Other Robustness Tests**. Continuing with Table 4, Column 7 tests the sensitivity of choosing 1987 as the first *After* year – we change *After* to 1986 and find the magnitude of *Michigan * After* to be somewhat higher (14.65%) and significant at the 1% level. Column 8 explores whether our results might be due to sample attrition. We include in the sample only those firms that were active before MARA, but one might be concerned that the exit of Michigan firms with low *q* (or of control-state firms with high *q*) after MARA could bias our results. In Column 8 we restrict the sample to firms listed both before MARA and as of 1989 (the end of our window). We find consistent results in Column 8, with the magnitude of *Michigan * After* continuing to be in the 10% range (9.91%) and significant at the 5% level. In Column 9, we check whether repeated observations affect the significance of our results. While we cluster standard errors in all models at the firm-level, research suggests that difference-in-differences models can suffer from serial auto-correlation and within-group dependence, because the indicator variable for 'treatment' (i.e., *Michigan*) is highly correlated between periods within states. Bertrand et al. (2004) therefore recommend clustered block-bootstrapping methods as a way to correct standard errors for such non-independence when the number of observations is high. Another approach to dealing with non-independence is to collapse observations in the before and after periods, or to take one observation as a pretest, and one observation as a post-test for each firm. Given the delayed onset of treatment in our context, we took one observation from the first year of the sample, and one observation from the last year of the sample, for each firm and block-bootstrapped standard errors. In Column 9 we find a stronger effect size (at 16.6%) and weaker statistical significance (10%); unreported results with one-before and one-after observation, but no block-bootstrapping, are essentially the same. In Column 10 we test for a contemporaneous change in the capital-to-labor ratio (calculated as the natural logarithm of the ratio of total property, plant and equipment, to the total number of employees). We expect that treated firms in Michigan should have economized on the use of capital per employee after MARA, such that the capital-to-labor ratio would fall. Indeed, in Column 10 we find that the capital-to-labor ration fell 9.1%, significant at the 5% level.

Finally, we conducted a range of robustness tests that we do not tabulate. We checked the sensitivity of our calculation of *Tobin's q* by testing an alternative proposed by (Chung and Pruitt, 1994), and found broadly consistent results. We tested the sensitivity of our sampling approach by retaining outliers – results are similar. Although we included a broad range of covariates in Tables 3 and 4, many control variables were insignificant. We therefore removed insignificant controls and re-tested all models, and found consistent results. We also explored whether the gains in *q* might vary by measures of a firm's intangibles, including PP&E divided by SG&A expenditures, SG&A divided by Assets, service vs. manufacturing industries, and the ratio of skills vs. unskilled labor (using industry classifications from the BLS Occupational Employment Statistics survey). In none of these cases did we find measures of intangibles to explain much variance in the impact of enforceable non-compete agreements on $q$.

## 7. Conclusion

Exploiting an inadvertent policy reversal in Michigan, which rendered employee non-compete agreements enforceable for the first time in many years, we show that the ability to block employee mobility to competitors was associated with a 9% rise in Tobin's $q$ for treated firms, as compared to a control group of firms from states where non-competes continued to be proscribed. The connection between enforceable non-compete agreements and firm value is increasing in the level of local competition, somewhat attenuated by patenting, and greater for firms with growth options. We believe our paper is the first to provide evidence that employee retention can impact firm value. Nevertheless, we interpret our results cautiously. We perform a range of robustness and placebo tests, but cannot categorically rule out the possibility that other shifts in the treatment or control states affect our results. We only examine public firms, and it is likely that different dynamics hold for private firms, as well as smaller and more entrepreneurial firms. Moreover, our study explores a short time window, and there may be deleterious effects of non-competes in the long-run due to the limited circulation of talent and ideas (Belenzon and Schankerman, 2012). Disentangling such contingencies is an important next step in assessing the overall effect of non-competes on employee mobility and firm performance.

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**Table 1.** Basic Difference-in-Differences Effect of MARA on *Tobin's q* (*n*=4,559)

Table 1 analyzes the basic "difference-in-differences" (DD) effect of the MARA policy reversal on *Tobin's q* for a +/– 3 year window before and after MARA. The sample includes all public firms in Michigan or a comparison state (AK CA CT MN MT NV ND OK WA WV). The dependent variable is the natural logarithm of *Tobin's q*, a transformation that better controls for heteroskedasticity and better estimates the non-linear effects of intangible assets on market value. Column 1 is the basic model with no fixed effects, including an indicator for *Michigan* (*Mean*=0.1022), an indicator *After* for the post-MARA period (*Mean*=0.4542), and an indicator for the interaction *Michigan * After*. Column 2 includes industry fixed effects at the SIC-3 level. Column 3 includes annual indicators for year fixed effects; the variable *After* is co-linear with year fixed effects and drops out of Columns 3 and 4. Column 4 reduces the sample through Propensity Score Matching by selecting two control observations for each treated observation, and reducing the sample from 4,559 to 1,660 observations. All models are estimated by OLS and cluster robust standard errors by firm (S.E. in parentheses). *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Michigan | -0.1257*** | -0.0443 | -0.0443 | -0.0704 |
| | (0.038) | (0.043) | (0.043) | (0.046) |
| After | -0.0525*** | -0.0740*** | | |
| | (0.012) | (0.012) | | |
| Michigan * After | 0.0768** | 0.0935*** | 0.0928*** | 0.0818** |
| | (0.032) | (0.032) | (0.032) | (0.037) |
| Constant | 0.3458*** | 0.3464*** | 0.3839*** | 0.3689*** |
| | (0.013) | (0.012) | (0.014) | (0.023) |
| Observations | 4,559 | 4,559 | 4,559 | 1,660 |
| R-squared | 0.008 | 0.228 | 0.232 | 0.374 |
| Industry Fixed Effects | -- | Yes | Yes | Yes |
| Year Fixed Effects | -- | -- | Yes | Yes |
| Propensity Score Matching | -- | -- | -- | PSM 2:1 |

## **Table 2.** Summary Statistics for the Multivariate Sample (*n*=1,874)

The multivariate sample includes firm-level data for public firms in Compustat from 1984 through 1989, the 3-year window before and 3-year window after the Michigan Antitrust Reform Act (MARA). The sample includes firms that were listed prior to MARA, that reported R&D Expense and other covariates, and that were headquartered in either Michigan or a comparison state (AK CA CT MN MT NV ND OK WA WV). Comparison states did not enforce non-competes, either before or after MARA; state affiliation was based on the location of corporate headquarters. The top and bottom 2.5% of observations for *Tobin's q* were dropped as outliers; all other ratios were winsorized at the +/– 2.5% level. Firm-level data for *Patents* were merged into the sample from the NBER Patent Citations Data File. Moderating variables were mean-centered to zero to facilitate interpretation (with the exception of *High R&D Firms*, which was centered at the median of the indicator variable).

### **Panel A:** Descriptive Statistics

| | Variable | Mean | SD | Min | Max | Description | |
|---|---|---|---|---|---|---|---|
| 1 | Tobin's q | 1.51 | 0.63 | 0.80 | 4.31 | Tobin's q defined as ((PRCC_F*CSHO)+AT–CEQ )/AT | Compustat |
| 2 | Tobin's q (log) | 0.35 | 0.35 | -0.23 | 1.46 | Log of Tobin's q | Compustat |
| 3 | Assets (log) | 4.68 | 1.59 | 1.36 | 10.71 | Log of total assets | Compustat |
| 4 | Patent Stock (log) | 1.38 | 1.74 | 0.00 | 7.33 | Log of depreciated count of patents over prior 5 years | Compustat |
| 5 | R&D Stock (log) | 2.45 | 1.51 | 0.01 | 8.75 | Log of depreciated total R&D expenses over prior 5 years | Compustat |
| 6 | Employees (log) | 1.03 | 1.04 | 0.03 | 5.95 | Log of total employees | Compustat |
| 7 | SG&A Expense | 140.80 | 436.54 | 0.83 | 4,596.00 | Selling, general and administrative expense | Compustat |
| 8 | Net Income | 36.42 | 209.03 | -434.90 | 4,625.20 | Net income | Compustat |
| 9 | Return on Assets | 0.02 | 0.13 | -0.50 | 0.19 | Return on assets | Compustat |
| 10 | Capital Exp. Intensity | 0.07 | 0.05 | 0.01 | 0.24 | Ratio of capital expenditures to total assets | Compustat |
| 11 | Leverage | 0.41 | 0.21 | 0.07 | 0.97 | Ratio of book value of debt to market value of equity | Compustat |
| 12 | Liquidity | 0.37 | 0.20 | -0.10 | 0.79 | (Current assets - current liabilities) / by total assets | Compustat |
| 13 | Instate Competition | 0.00 | 0.13 | -0.10 | 0.90 | Proportion of competitor sales (by SIC-3) in same state | Compustat |
| 14 | Patents | 0.00 | 1.31 | -0.95 | 5.16 | Number of new patents granted (by application date) | NBER |
| 15 | Revenue Growth | 0.00 | 0.44 | -1.01 | 6.02 | Percent change in revenue from prior year (median-centered) | Compustat |
| 16 | High R&D Firms | -0.01 | 0.50 | -0.51 | 0.49 | Indicator for firms over median split of R&D intensity | Compustat |
| 17 | Firm Size | 0.00 | 3.72 | -0.81 | 61.91 | Total revenues divided by 1,000 (mean centered) | Compustat |
| 18 | Firm Maturity | 0.00 | 0.50 | -0.47 | 0.53 | Indicator for firms listed for 10 or more years as of 1987 | Compustat |
| 19 | Michigan | 0.09 | 0.28 | 0.00 | 1.00 | Indicator for the state of Michigan | Compustat |
| 20 | After | 0.44 | 0.50 | 0.00 | 1.00 | Indicator for the "after" period (1987 through 1989) | Compustat |

### **Panel B:** Correlations

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) | (17) | (18) | (19) | (20) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 1.00 | | | | | | | | | | | | | | | | | | | |
| 2 | 0.97 | 1.00 | | | | | | | | | | | | | | | | | | |
| 3 | -0.14 | -0.14 | 1.00 | | | | | | | | | | | | | | | | | |
| 4 | -0.10 | -0.09 | 0.63 | 1.00 | | | | | | | | | | | | | | | | |
| 5 | -0.09 | -0.08 | 0.82 | 0.62 | 1.00 | | | | | | | | | | | | | | | |
| 6 | -0.16 | -0.15 | 0.93 | 0.63 | 0.79 | 1.00 | | | | | | | | | | | | | | |
| 7 | -0.03 | -0.03 | 0.65 | 0.51 | 0.65 | 0.73 | 1.00 | | | | | | | | | | | | | |
| 8 | -0.02 | -0.02 | 0.46 | 0.37 | 0.44 | 0.50 | 0.67 | 1.00 | | | | | | | | | | | | |
| 9 | 0.16 | 0.17 | 0.21 | 0.14 | 0.08 | 0.18 | 0.09 | 0.13 | 1.00 | | | | | | | | | | | |
| 10 | 0.13 | 0.14 | 0.09 | 0.10 | 0.04 | 0.09 | 0.07 | 0.07 | 0.09 | 1.00 | | | | | | | | | | |
| 11 | -0.18 | -0.17 | 0.12 | 0.02 | 0.05 | 0.16 | 0.05 | -0.01 | -0.37 | -0.05 | 1.00 | | | | | | | | | |
| 12 | 0.20 | 0.20 | -0.35 | -0.17 | -0.23 | -0.39 | -0.26 | -0.17 | 0.28 | -0.17 | -0.70 | 1.00 | | | | | | | | |
| 13 | -0.01 | -0.01 | -0.08 | -0.10 | 0.00 | -0.13 | -0.08 | -0.02 | -0.10 | 0.02 | -0.05 | 0.10 | 1.00 | | | | | | | |
| 14 | -0.06 | -0.05 | 0.62 | 0.92 | 0.63 | 0.63 | 0.56 | 0.41 | 0.13 | 0.10 | 0.00 | -0.15 | -0.09 | 1.00 | | | | | | |
| 15 | 0.29 | 0.29 | -0.08 | -0.06 | -0.13 | -0.13 | -0.08 | -0.04 | 0.11 | 0.18 | -0.09 | 0.16 | 0.03 | -0.04 | 1.00 | | | | | |
| 16 | 0.20 | 0.21 | -0.19 | -0.06 | 0.22 | -0.19 | 0.01 | 0.00 | -0.09 | 0.04 | -0.19 | 0.26 | 0.20 | -0.04 | 0.06 | 1.00 | | | | |
| 17 | -0.09 | -0.10 | 0.55 | 0.42 | 0.50 | 0.58 | 0.71 | 0.88 | 0.06 | 0.06 | 0.05 | -0.25 | 0.00 | 0.46 | -0.07 | -0.04 | 1.00 | | | |
| 18 | -0.20 | -0.19 | 0.45 | 0.42 | 0.31 | 0.46 | 0.25 | 0.16 | 0.16 | -0.04 | 0.16 | -0.25 | -0.19 | 0.38 | -0.19 | -0.27 | 0.18 | 1.00 | | |
| 19 | -0.04 | -0.05 | 0.10 | 0.05 | 0.01 | 0.14 | 0.03 | 0.11 | 0.10 | 0.06 | 0.03 | -0.10 | -0.04 | 0.03 | -0.05 | -0.18 | 0.10 | 0.14 | 1.00 | |
| 20 | -0.11 | -0.11 | 0.09 | 0.09 | 0.16 | 0.02 | 0.04 | 0.03 | -0.02 | -0.13 | 0.12 | -0.11 | 0.02 | 0.04 | -0.03 | 0.04 | 0.00 | 0.05 | -0.01 | 1.00 |

## **Table 3.** Multivariate Difference-in-Differences Effect of MARA on *Tobin's q* ($n$=1,874)

Table 3 analyzes the multivariate difference-in-differences (DD) effect of MARA on *Tobin's q* for –/+ 3 years before/after MARA. The sample and measures are reported in Table 2. The dependent variable is the natural log of *Tobin's q*. Column H1 adds covariate controls to the basic difference-in-differences model of Table 1. Columns H2 to H4d then test the moderating conditions hypothesized in the text (*Instate Competition*, *Patents, Revenue Growth, High R&D, Large Firms,* and *Mature Firms)* by adding each variable as a triple-difference-in-differences interaction (each moderator is used in-place of the label "Moderator" in the table below). Each moderator is held constant after MARA. The variable *Growth Opportunities* is equal to *Revenue Growth* in Columns H1–H4a, *High R&D Firms* in Column H4b, *Firm Size* in Column H4c (inverse proxy), and *Mature Firms* in Column H4c (inverse proxy). All models are estimated by OLS, include year and industry fixed effects at the SIC-3 level, and cluster robust standard errors by firm (S.E. in parentheses). ***$p$<0.01, **$p$<0.05, *$p$<0.10.

| | (H1) | (H2) | (H3) | (H4a) | (H4b) | (H4c) | (H4d) |
|---|---|---|---|---|---|---|---|
| **Moderator =** | None | Instate Competition | Patents Granted | Revenue Growth | High R&D | Firm Size | Mature Firms |
| Assets (log) | -0.0320 | -0.0339 | -0.0359 | -0.0294 | 0.0174 | -0.0171 | -0.0156 |
| | (0.030) | (0.030) | (0.030) | (0.030) | (0.032) | (0.031) | (0.030) |
| Patent Stock (log) | -0.0274** | -0.0269** | -0.0285** | -0.0294** | -0.0268** | -0.0295** | -0.0236* |
| | (0.013) | (0.013) | (0.013) | (0.013) | (0.013) | (0.014) | (0.014) |
| R&D Stock (log) | 0.0361* | 0.0395* | 0.0354 | 0.0400* | -0.0260 | 0.0211 | 0.0247 |
| | (0.022) | (0.022) | (0.022) | (0.021) | (0.026) | (0.021) | (0.022) |
| Employees (log) | -0.0690 | -0.0698* | -0.0566 | -0.0743* | -0.0741* | -0.0765* | -0.0819* |
| | (0.042) | (0.042) | (0.042) | (0.042) | (0.043) | (0.043) | (0.043) |
| SG&A Expense | 0.0001* | 0.0001* | 0.0001 | 0.0001* | 0.0001* | 0.0001 | 0.0001* |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Net Income | -0.0000 | -0.0000 | 0.0001 | -0.0000 | -0.0000 | 0.0002* | 0.0000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| ROA | 0.5157*** | 0.5234*** | 0.5146*** | 0.4659*** | 0.5610*** | 0.5211*** | 0.5919*** |
| | (0.100) | (0.100) | (0.100) | (0.099) | (0.096) | (0.099) | (0.098) |
| CAPX Intensity | 0.9635*** | 0.9425*** | 0.8999*** | 0.9421*** | 1.0730*** | 1.2102*** | 1.1709*** |
| | (0.218) | (0.218) | (0.216) | (0.217) | (0.219) | (0.217) | (0.219) |
| Leverage | 0.1498* | 0.1463 | 0.1407 | 0.1518* | 0.1709** | 0.1824** | 0.1998** |
| | (0.090) | (0.089) | (0.091) | (0.087) | (0.087) | (0.083) | (0.084) |
| Liquidity | 0.2062** | 0.1906** | 0.1861** | 0.2264*** | 0.2179** | 0.2652*** | 0.2392*** |
| | (0.087) | (0.087) | (0.087) | (0.086) | (0.085) | (0.085) | (0.085) |
| Instate Competition | -0.0596 | 0.1947 | -0.0324 | -0.0784 | -0.0333 | 0.0019 | -0.0348 |
| | (0.132) | (0.188) | (0.133) | (0.134) | (0.132) | (0.139) | (0.140) |
| Patent Applications | 0.0360* | 0.0351* | 0.0279 | 0.0391** | 0.0408** | 0.0416** | 0.0427** |
| | (0.019) | (0.019) | (0.019) | (0.019) | (0.019) | (0.019) | (0.019) |
| Growth Opportunities | 0.1595*** | 0.1581*** | 0.1572*** | 0.2163*** | 0.1755*** | -0.0198** | -0.1227*** |
| | (0.029) | (0.029) | (0.029) | (0.029) | (0.037) | (0.008) | (0.040) |
| Michigan | -0.0099 | -0.0192 | -0.0028 | -0.0109 | 0.0061 | -0.0088 | -0.0156 |
| | (0.069) | (0.069) | (0.067) | (0.068) | (0.079) | (0.070) | (0.068) |
| Michigan * After | 0.1274*** | 0.1294*** | 0.1231*** | 0.0982** | 0.1400** | 0.1175** | 0.1760*** |
| | (0.046) | (0.044) | (0.046) | (0.046) | (0.055) | (0.046) | (0.067) |
| Michigan * Moderator | | -0.3742 | -0.0638** | 0.0319 | 0.0139 | 0.0022 | -0.0222 |
| | | (0.240) | (0.030) | (0.180) | (0.146) | (0.010) | (0.121) |
| After * Moderator | | -0.4109*** | 0.0385*** | -0.1799*** | -0.0648** | 0.0070* | 0.1208*** |
| | | (0.117) | (0.010) | (0.043) | (0.032) | (0.004) | (0.031) |
| MI*After * Moderator | | 0.4883* | -0.0576** | 0.9120** | 0.1945* | -0.0147*** | -0.2781** |
| | | (0.268) | (0.026) | (0.431) | (0.106) | (0.005) | (0.126) |
| Constant | 0.3215*** | 0.3302*** | 0.3498*** | 0.2997*** | 0.2339** | 0.2411** | 0.2382** |
| | (0.109) | (0.109) | (0.110) | (0.105) | (0.103) | (0.105) | (0.106) |
| Observations | 1,874 | 1,874 | 1,874 | 1,874 | 1,874 | 1,874 | 1,874 |
| R-squared | 0.300 | 0.304 | 0.309 | 0.312 | 0.291 | 0.275 | 0.285 |

## **Table 4.** Robustness Checks

Table 4 tests the robustness of our main results. The DV for Columns 1-9 is logged *q*; the DV for Column 10 is the log of the capital-to-labor ratio (total property, plant and equipment, divided by number of employees). Columns 1 and 2 match treated to control observations on the basis of *Assets*, *ROA*, *SG&A*, and industry: Column 1 uses Propensity Score Matching to match 2 control to 1 treated observation; Column 2 uses Coarsened Exact Matching to match observations by strata. Column 3 uses firms in states with a non-compete enforcement index of less than 5 (Garmaise 2011). Column 4 drops the auto industry. Column 5 tests a 'Midwest Placebo' (OH, IN, IL, WI, PA) as if they had been treated. Column 6 tests the Texas Free Enterprise and Antitrust Act of 1983, an antitrust reform that did *not* change non-compete enforcement. Column 7 changes the *After* year to 1986. Column 8 drops firms with missing years. Column 9 estimates a model with just the first year (1984) and the last year (1989) of the sample, and block-bootstraps standard errors. All models are estimated by OLS, have year and industry fixed effects at the SIC-3 level, and cluster robust standard errors by firm (S.E. in parentheses). *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

| | (1) PSM 2:1 | (2) CEM Strata | (3) Alt. Controls | (4) Drop Auto | (5) Midwest Placebo | (6) Texas Antitrust | (7) After in 1986 | (8) Full Data | (9) First and Last Years | (10) Capital to Labor Ratio |
|---|---|---|---|---|---|---|---|---|---|---|
| Assets (log) | 0.0032 | -0.0195 | -0.0007 | -0.0337 | 0.0290 | -0.0807** | -0.0318 | -0.0271 | -0.0211 | 0.8264*** |
| | (0.056) | (0.052) | (0.015) | (0.031) | (0.029) | (0.032) | (0.030) | (0.036) | (0.031) | (0.040) |
| PatentStock (log) | 0.0001 | -0.0201 | -0.0193** | -0.0313** | -0.0284** | -0.0308** | -0.0264* | -0.0218 | -0.0234 | 0.0455*** |
| | (0.020) | (0.021) | (0.008) | (0.014) | (0.013) | (0.014) | (0.014) | (0.015) | (0.020) | (0.017) |
| R&DStock (log) | -0.0125 | 0.0658** | 0.0234** | 0.0439* | 0.0435** | -0.0160 | 0.0367* | 0.0434* | 0.0171 | 0.1091*** |
| | (0.033) | (0.030) | (0.010) | (0.022) | (0.018) | (0.023) | (0.022) | (0.025) | (0.021) | (0.027) |
| Employees (log) | -0.0189 | -0.0671 | -0.0541** | -0.0819* | -0.1277*** | 0.0482 | -0.0703* | -0.0958* | -0.0535 | -0.8735*** |
| | (0.068) | (0.064) | (0.025) | (0.043) | (0.040) | (0.046) | (0.042) | (0.051) | (0.040) | (0.055) |
| SG&A Expense | 0.0001* | 0.0000 | 0.0000 | 0.0001 | 0.0001*** | 0.0000 | 0.0001* | 0.0001* | 0.0001 | -0.0002*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Net Income | -0.0001*** | -0.0001 | -0.0000 | 0.0003** | -0.0001*** | 0.0000 | -0.0000 | -0.0000 | 0.0001 | -0.0000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| ROA | 1.0619*** | 1.8070*** | 0.9100*** | 0.5048*** | 1.6733*** | 0.5367*** | 0.5143*** | 0.6943*** | 0.5143*** | 0.0794 |
| | (0.282) | (0.312) | (0.086) | (0.101) | (0.184) | (0.142) | (0.100) | (0.120) | (0.152) | (0.119) |
| CapX Intensity | 1.2967*** | 1.0656*** | 1.1773*** | 0.9611*** | 0.9921*** | 1.1156*** | 0.9459*** | 0.8853*** | 0.7058** | 3.0692*** |
| | (0.368) | (0.352) | (0.144) | (0.223) | (0.283) | (0.223) | (0.218) | (0.264) | (0.287) | (0.291) |
| Leverage | 0.0433 | -0.0600 | -0.0285 | 0.1907** | 0.0218 | 0.0315 | 0.1529* | 0.1598 | 0.0132 | -0.4978*** |
| | (0.135) | (0.105) | (0.048) | (0.091) | (0.091) | (0.096) | (0.090) | (0.113) | (0.113) | (0.134) |
| Liquidity | 0.3568** | -0.0183 | -0.0260 | 0.2258** | -0.0717 | 0.2924** | 0.2095** | 0.2286** | 0.1905 | -1.4497*** |
| | (0.171) | (0.157) | (0.055) | (0.088) | (0.105) | (0.121) | (0.087) | (0.100) | (0.124) | (0.147) |
| Instate Comp. | -0.0099 | -0.2775 | -0.0390 | 0.0225 | -0.1012 | -0.0824 | -0.0640 | -0.0473 | -0.1219 | -0.2567 |
| | (0.143) | (0.175) | (0.048) | (0.168) | (0.076) | (0.113) | (0.133) | (0.209) | (0.154) | (0.186) |
| Patent Apps | 0.0172 | 0.0060 | 0.0281** | 0.0397** | 0.0308 | 0.0508*** | 0.0344* | 0.0312 | 0.0284 | 0.0049 |
| | (0.029) | (0.031) | (0.012) | (0.020) | (0.020) | (0.019) | (0.019) | (0.022) | (0.025) | (0.025) |
| Growth Opp. | 0.1607*** | 0.1841*** | 0.0036*** | 0.1654*** | 0.0035 | 0.0557*** | 0.1593*** | 0.1395*** | 0.1848*** | -0.1482*** |
| | (0.049) | (0.058) | (0.001) | (0.030) | (0.004) | (0.015) | (0.029) | (0.031) | (0.034) | (0.042) |
| Michigan | -0.0259 | -0.0417 | -0.0824* | 0.0238 | -0.0736 | 0.0717 | -0.0442 | 0.0412 | -0.0682 | 0.1438* |
| | (0.063) | (0.058) | (0.047) | (0.082) | (0.052) | (0.051) | (0.070) | (0.093) | (0.050) | (0.075) |
| Michigan*After | 0.1019** | 0.1347** | 0.0945** | 0.0977** | 0.0613 | -0.0708 | 0.1465*** | 0.0991** | 0.1663* | -0.0911** |
| | (0.047) | (0.052) | (0.044) | (0.049) | (0.045) | (0.052) | (0.046) | (0.044) | (0.090) | (0.042) |
| Constant | 0.0751 | 0.2969 | 0.3096*** | 0.3071*** | 0.1768 | 0.5536*** | 0.3087*** | 0.3082** | 0.3648 | -0.0198 |
| | (0.225) | (0.212) | (0.061) | (0.110) | (0.125) | (0.125) | (0.109) | (0.132) | (0.296) | (0.159) |
| Observations | 557 | 824 | 4,898 | 1,812 | 1,297 | 1,890 | 1,874 | 1,398 | 592 | 1,865 |
| R-squared | 0.505 | 0.516 | 0.292 | 0.308 | 0.479 | 0.354 | 0.301 | 0.311 | 0.285 | 0.879 |

## Table 5: Robustness Check of Employee Mobility

Table 5 examines the difference-in-difference effect of enforceable non-compete agreements among patent-holding employees of the public firms in our sample. The DV is whether a given patent represents mobility as signified via a change in assignee vs. the inventor's prior patent. Standard errors are clustered at the firm level. *p*<0.01, ** *p*<0.05, * *p*<0.10.

| | (1) | (2) |
|---|---|---|
| Worker had moved previously | | 0.2609 |
| | | (0.531) |
| Days elapsed since prior patent | | 0.0008*** |
| | | (0.000) |
| Worker's number of patents | | -0.2529*** |
| | | (0.033) |
| Firm's number of patents | | 0.0001*** |
| | | (0.000) |
| Michigan | 0.9026*** | 0.9468*** |
| | (0.326) | (0.319) |
| After | 0.6578*** | 0.2316 |
| | (0.163) | (0.169) |
| Michigan * After | -1.7064*** | -1.6918*** |
| | (0.405) | (0.399) |
| Constant | -3.5297*** | -2.6210*** |
| | (0.153) | (0.198) |
| Observations | 9,176 | 9,176 |
| Log-Likelihood | -1,729.05 | -1,613.18 |

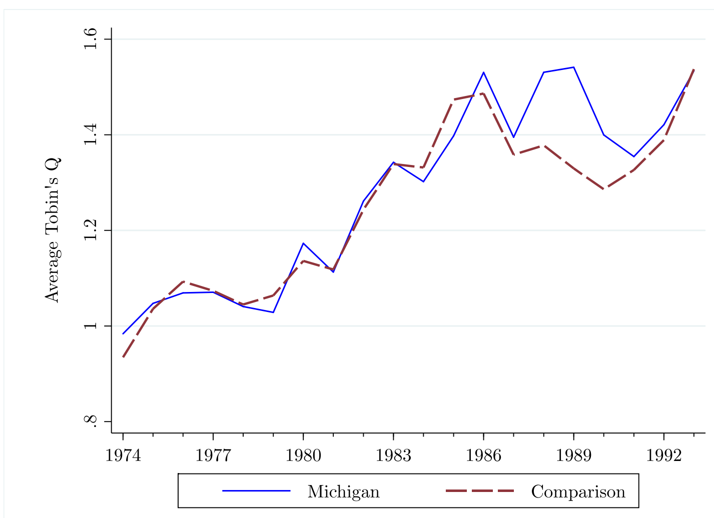
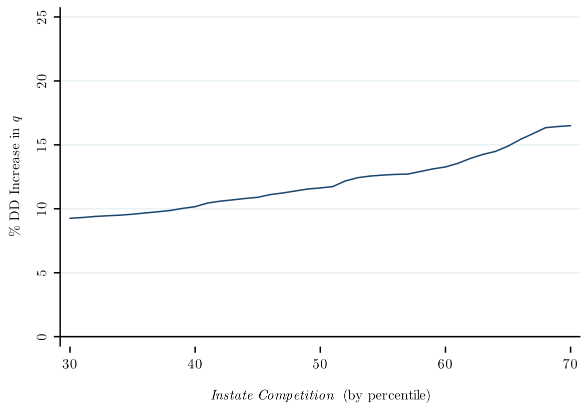
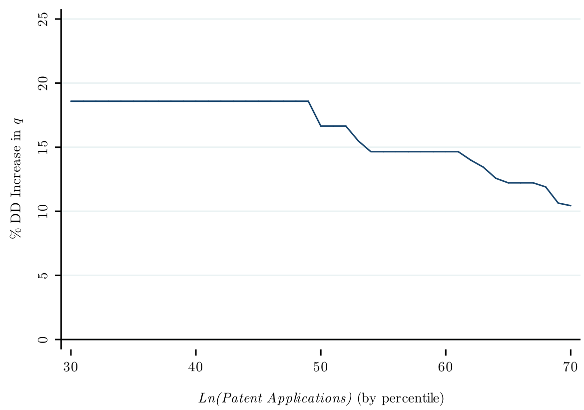



**Figure 1.** Trend in *Tobin's q* for public firms in Michigan and comparison states, 1974 – 1993.

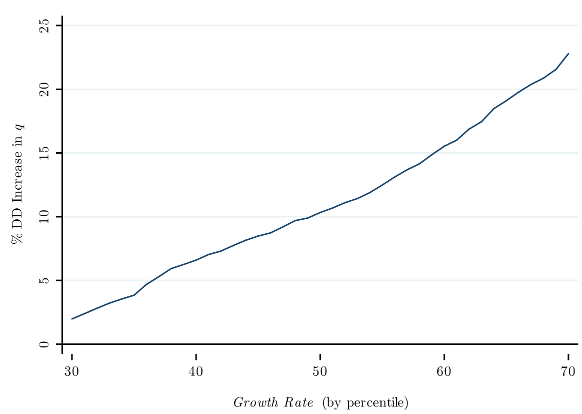
Figure 1 plots the descriptive trend in *Tobin's q* for all publically traded firms in Michigan (the treated state) and publically traded firms in control states (AK CA CT MN MT NV ND OK WA WV) for the 20-year period around MARA. End-of-year values are averaged by industry, weighted by assets, and collapsed to year. 1987 is the first year of the *After* period in the analysis.




**Panel A:** *Instate Competition*




**Panel B:** *ln(Patent Applications)*




**Panel C:** *Revenue Growth*

**Figure 2.** Predicted diff-in-diff percentage increase in *Tobin's q* by percentile of moderator.

The panels of Figure 2 graph the predicted difference-in-differences increase in the level of *Tobin's q* (as a percentage change, plotted on the Y axis) by level of each moderating variable (plotted along the X axis). Predictions are based on coefficients from the estimated Full Model (Column 4 of Table 6).

## Online Appendix:
## Employee Non-Compete Agreements and Product-Market Competition

Prior theoretical work examines competition in the context of how non-compete agreements affect the generation of *de novo* 'spin-outs' as well as the entry of *de alio* rivals, and thus product market competition (Franco and Mitchell, 2005). Franco and Mitchell find two countervailing mechanisms. On the one hand, non-compete agreements block the departure of employees and therein attenuate the emergence of intra-industry spin-outs. On the other hand, the promise of greater protection of intellectual property via non-compete enforcement might entice new entrants, especially early in the industry lifecycle (see also Franco & Mitchell, 2008). Franco & Mitchell point out that product market competition is bound up in the two-way-street problem, for production by parent firms and children firms (i.e., spin-outs) may be particularly rivalrous and accompanied by tough product market competition. As such, if non-competes reduce spinouts, then non-competes could also be reducing product market competition; in such a case, the sudden enforceability of non-competes may be related to the increase in Tobin's q that we identify, but not for the reasons we suggest – the valuation increase might simply come from monopolistic protection from competition.

Given that the connections between non-competes, industry life-cycle, and product market competition are still being developed in theoretical work, we investigate the matter empirically. To not lengthen the paper, we summarize our analysis of non-competes and product market competition in the manuscript, and provide complete details here in the Online Appendix. We performed a supplemental difference-in-differences analysis of product market competition before and after MARA. Using the same model specifications and sample reported in the paper, we constructed a new dependent variable for product market competition. We measure product market competition as the median, industry-adjusted, Lerner Index (Lerner, A.P., 1934. The Concept of Monopoly and the Measurement of Monopoly Power. The Review of Economic Studies 1, 157-175) at the 3-digit SIC level. Using the Lerner Index as our product market competition measure is consistent with prior work on product market competition and firm performance (Nickell, S.J., 1996. Competition and corporate performance. Journal of Political Economy 104, 724-746) as well as more recent work on competition and innovation (Aghion, P., Bloom, N., Blundell, R., Griffith, R., Howitt, P., 2005. Competition and innovation: An inverted-U relationship. The Quarterly Journal of Economics 120, 701-728; Aghion, P., Van Reenen, J., Zingales, L., 2013. Innovation and Institutional Ownership. The American Economic Review 103, 277-304). Following this research, we calculate the price cost margin (*PCM*) for each firm using data provided by Compustat as:

$$PCM_i = (SALE_i - COGS_i - XSGA_i) / SALE_i$$

We substitute $PCM_i = OIBDP_i / SALE_i$ where there was missing data for COGS or XSGA. Compustat variables are defined as: SALE = sales/turnover (net); COGS = cost of goods sold; XSGA = sales, general and administrative expenses; OIBDP = operating income before depreciation. We winsorize *PCM* to +1 and −1 around the mean of *PCM* to mitigate the influence of outliers. Next, we compute a sales-weighted, industry-adjusted, firm-specific measure of market power for each firm, as:

$$MarketPower_i = PCM_i - \sum_j^N \omega j \quad PCM_j$$

$PCM_i$ is the price cost margin for firm *i* calculated above; $\omega_j$ is the proportion of total revenue contributed by each firm *j* in the same SIC-3 industry as firm *i*; $PCM_j$ is the price cost margin for each firm *j* in the same SIC-3 industry as firm *i*; the sales-weighted price cost margins are summed over the N firms in the same SIC-3 industry as firm *i*. We take the median value of *MarketPower* by SIC-3 to derive an industry-level measure of product market competition. In computing *MarketPower* we use all Compustat firms in each industry, not only those in our sample.

We re-analyzed our sample with *MarketPower* as the dependent variable and report these results in the table below. We find no evidence that product market competition shifted for Michigan firms after MARA. The coefficient on *Michigan * After* is near zero in magnitude as well as statistically insignificant in all models of Table A1.

**Table A1:** Difference-in-differences estimates of firm market power

| | (1)<br>Basic DD<br>Result | (2)<br>Year<br>FE | (3)<br>Year &<br>Industry FE | (4)<br>Covariate<br>Controls | (5)<br>PSM<br>2:1 |
|---|---|---|---|---|---|
| Assets (log) | | | | 0.0009 | 0.0002 |
| | | | | (0.001) | (0.002) |
| Patent Stock (log) | | | | 0.0009 | -0.0010 |
| | | | | (0.001) | (0.001) |
| R&D Stock (log) | | | | -0.0013 | -0.0011 |
| | | | | (0.001) | (0.002) |
| Employees (log) | | | | 0.0007 | 0.0017 |
| | | | | (0.002) | (0.004) |
| SG&A Expense | | | | 0.0000 | 0.0000 |
| | | | | (0.000) | (0.000) |
| Net Income (Loss) | | | | 0.0000 | -0.0000 |
| | | | | (0.000) | (0.000) |
| ROA | | | | 0.0141*** | 0.0261* |
| | | | | (0.004) | (0.014) |
| CapX Intensity | | | | -0.0001 | -0.0244 |
| | | | | (0.012) | (0.034) |
| Leverage | | | | 0.0009 | -0.0079 |
| | | | | (0.004) | (0.008) |
| Liquidity | | | | -0.0024 | -0.0239 |
| | | | | (0.007) | (0.022) |
| Instate Competition | | | | 0.0044 | 0.0038 |
| | | | | (0.004) | (0.007) |
| Patent Applications | | | | -0.0018 | 0.0009 |
| | | | | (0.002) | (0.001) |
| Revenue Growth Rate | | | | 0.0019* | 0.0037 |
| | | | | (0.001) | (0.002) |
| Michigan | 0.0009 | 0.0033 | 0.0027 | 0.0024 | 0.0027 |
| | (0.003) | (0.003) | (0.002) | (0.002) | (0.003) |
| Michigan * After | 0.0008 | -0.0041 | -0.0030 | -0.0036 | -0.0025 |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) |
| Constant | 0.0968*** | 0.0916*** | 0.0917*** | 0.0895*** | 0.1064*** |
| | (0.002) | (0.002) | (0.001) | (0.007) | (0.019) |
| Observations | 2,047 | 2,047 | 2,047 | 1,860 | 584 |
| R-squared | 0.000 | 0.017 | 0.674 | 0.682 | 0.560 |
| Year FE | | Yes | Yes | Yes | Yes |
| Industry FE | | | Yes | Yes | Yes |
| Controls | | | | Yes | Yes |
| Matching | | | | | Yes |

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.10