HOW ANTICIPATED EMPLOYEE DEPARTURE AFFECTS ACQUISITION LIKELIHOOD: EVIDENCE FROM A NATURAL EXPERIMENT

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ABSTRACT

This study draws on strategic factor market theory and argues that acquirers' decisions regarding whether to bid for a firm reflect their expectations about employee departure from the firm post-acquisition, suggesting a negative relationship between the anticipated employee departure from a firm and the likelihood of the firm becoming an acquisition target. Using a natural experiment and a difference-in-differences approach, we find causal evidence that constraints on employee mobility raise the likelihood of a firm becoming an acquisition target. The causal effect is stronger when a firm employs more knowledge workers in its workforce and when it faces greater in-state competition; by contrast, the effect is weaker when a firm is protected by a stronger intellectual property regime that mitigates the consequences of employee mobility.

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INTRODUCTION

Strategic management scholars share the view that acquisitions represent an important strategy for sourcing resources to broaden a firm's knowledge base, foster innovation, and improve organizational performance (Capron & Mitchell, 1998; Hall, 1988; Hitt, Hoskisson, Ireland, & Harrison, 1991). Academic research and anecdotal evidence suggests that acquisitions often are driven by firms' desire to acquire the human talents of the target companies (Buono & Bowditch, 1989; Coff, 1999a, 2002; Ranft & Lord, 2000; Ranft & Lord, 2002; Wysocki, 1997a, 1997b). As a result, the management of human capital has been an increasingly important topic for both mergers and acquisitions (M&A) research and practice (see Bruner, 2004; Deloitte, 2010; Ellis, Reus, Lamont, & Ranft, 2011; Haspeslagh & Jemison, 1991; Heimeriks, Schijven, & Gates, 2012).

Prior research suggests that acquisition of human capital from a target company can present challenges to the acquiring firm. Acquiring firms routinely confront problems of information asymmetry before an acquisition (Akerlof, 1970; Ravenscraft & Scherer, 1987) and risks of employees departing the target company after an acquisition (Buono & Bowditch, 1989; Jemison & Sitkin, 1986). These challenges are likely to be heightened in human capital-intensive companies, whose most valuable assets "walk out the door every night" (LaVan, 2000). Prior acquisition research has examined how *ex ante* information problems associated with human capital can affect firms' acquisition strategies (e.g., Coff, 1999a), and how acquirers can work to reduce *ex post* employee departure from the acquired company (e.g., Larsson & Finkelstein, 1999; Ranft & Lord, 2000; Ranft & Lord, 2002). To our knowledge, however, no research has investigated how the anticipated departure of employees from a firm *ex post* may affect acquirers' decision as to whether to bid for the firm *ex ante*.

In this study, we draw on strategic factor market theory to examine how anticipated post-acquisition employee departure from a firm affects the likelihood of the firm becoming an acquisition target. As Barney (1986) suggests, firms acquire resources in the factor market and make acquisition decisions based on their expectations about the future use of those resources. Human capital is a critical resource for generating competitive advantage (Castanias & Helfat, 1991) that can affect the future outcome of an acquisition (Coff, 1999a, 2002; Ranft & Lord, 2002). Because human capital is embedded in individual employees, we argue that employee departure from a potential target firm will reduce the attractiveness of the acquisition to acquirers in several important ways, thus shaping acquirers' *ex ante* decision regarding whether to bid for that firm. Specifically, we suggest that the potential for employee departure from a target firm introduces uncertainty into acquirers' assessment of the value of the acquisition: to the extent that a target firm's employees are less likely to depart, an acquisition is more likely to be attractive to acquirers and we predict that acquirers will be more likely to bid for the firm.

To empirically test our argument, we exploit a natural experiment in Michigan wherein an inadvertent reversal of its prohibition of enforcing non-compete agreements (NCAs) provides an observable, exogenous source of variation in employee mobility (Marx, Strumsky, & Fleming, 2009). Using a difference-in-differences approach, we find causal evidence that constraints on employee mobility, due to an increase in NCA enforcement, significantly raise the likelihood that a Michigan firm becomes an acquisition target, compared to firms in other non-enforcing states that did not change NCA enforcement. We further test a set of conditions under which constraints on employee mobility produce a more or less pronounced effect on the likelihood of acquisition. We find that the causal effect is stronger when a firm is faced with a greater exposure to the negative consequences of employee mobility, such as when a firm employs more knowledge workers in its workforce and when it faces greater in-state competition. By contrast, we find that the effect is weaker when a firm is protected by a stronger intellectual property regime that can mitigate some of the negative consequences of employee mobility. Taken as a whole, our results provide a consistent pattern of evidence suggesting that employee mobility is a major consideration affecting acquirers' decisions to use acquisitions as a strategy to source human capital.

THEORY AND HYPOTHESES

Acquisitions have become an increasingly important means for firms to source external knowledge (Arora & Gambardella, 1990; Cohen & Levinthal, 1990; Grant, 1996). Practitioners have long observed that firms often undertake acquisitions to obtain new knowledge and fresh talents (e.g., Link, 1988;

Roberts, 2006; Wysocki, 1997a, 1997b). Empirical studies have provided ample evidence attesting to many of the benefits that acquisitions can bring to the acquiring firms, including desired knowledge, greater innovation, speedy new product introduction, and enhanced organizational performance (e.g. Ahuja & Katila, 2001; Capron, 1999; Puranam, Singh, & Zollo, 2006).

Despite these potential benefits, significant challenges exist for firms that pursue acquisitions as a knowledge sourcing strategy. The challenge related to the management of human capital, in particular, has been the focus of a growing body of literature on M&As. One important stream of research examines acquirers' strategic choices to deal with human capital-intensive targets before an acquisition deal is concluded. Researchers have suggested that acquiring firms confront increased difficulty in the *ex ante* assessment of human capital-intensive targets due to information asymmetry (e.g. Akerlof, 1970; Ravenscraft & Scherer, 1987). To deal with this challenge, research has shown that acquirers may employ particular contractual clauses such as earnouts (Datar, Frankel, & Wolfson, 2001), use a greater proportion of equity as payment, lengthen the negotiation time (Coff, 1999a), select more geographically proximate targets (Chakrabarti & Mitchell, 2012), rely on information from other sources such as alliances (Ragozzino & Reuer, 2011; Schildt & Laamanen, 2006), or choose not to close the deal (Coff, 2002).

In addition to such *ex ante* difficulty, acquiring firms also face significant challenges to retain the human talents of the acquired company and protect the embedded knowledge and skills *post*-acquisition (Buchholtz, Ribbens, & Houle, 2003; Hambrick & Cannella, 1993; Walsh, 1988). Indeed, as a *Wall Street Journal* article indicates, "it has never been easier for talented people to walk away, leaving the acquired company an empty shell" (Wysocki, 1997b). As a result, a prominent stream of research has developed around 'acquisition integration', which specifically examines how acquirers may retain and motivate the employees of acquired companies during integration and how such human capital-related integration efforts affect acquisition performance (Ashkenas, DeMonaco, & Francis, 1998; Haspeslagh & Jemison, 1991; Pablo, 1994; Schweiger & DeNisi, 1991; Shrivastava, 1986). For instance, research has suggested that while acquirers often use both financial and non-financial incentives to help retain key employees of the acquired companies (O'Reilly & Pfeffer, 2000; Ranft & Lord, 2000), non-financial ones (e.g., autonomy, status, commitment) can be more effective (Ranft & Lord, 2000). Research has further shown that employee retention is a critical part of acquirers' integration plan, which contributes significantly to synergy realization and acquisition performance (Cannella & Hambrick, 1993; Cording, Christmann, & King, 2008; Ellis *et al.*, 2011; Larsson & Finkelstein, 1999; Zollo & Singh, 2004).

This study examines how anticipated employee departures from a firm *ex post* may affect acquirers' decision as to whether to bid for the firm *ex ante*. Studying this question provides one way to link together the two streams of M&A research above: while our primary interest is focused on acquirers' strategic choice *ex ante* (the first stream), the study is related to an important area within the second stream because of our argument about employee departure *post*-acquisition. Specifically, we develop our theoretical argument by drawing from the M&A literature that employee departure from acquired firms has negative consequences and reduces the future value of an acquisition to the acquirer, as well as from strategic factor market theory asserting that firms' decisions to acquire strategic resources in the factor market are based on their expectations about the future value of the resources (Barney, 1986).

To begin with, the M&A literature has long argued that departures of employees from the target firm introduces several uncertainties into the acquirer's assessment of the future value of the target and the acquisition deal: First, the acquirer faces uncertainty about the target's estimated "stand alone" value (Coff, 1999a), if critical knowledge or other assets are lost as employee leave the target firm after the acquisition, or if employee departures negatively affect the performance of others who remain with the firm (O'Reilly & Pfeffer, 2000). Second, given that asset combinations and redeployment are often required in the post-acquisition phase, the acquirer faces uncertainty about the transferability of assets and personnel, and thus the synergy value of the deal in the longer run (Barney, 1988; Coff, 1999a). Third, the acquirer also faces uncertainty about potential sources of competitive advantage of the target firm being eroded; for example, as employees leave the firm, proprietary knowledge may leak out to the rival companies they join or the startups they form (Liebeskind, 1996, 1997).

Strategic factor market theory argues that firms acquire strategic resources in the factor market to implement a strategy and they make acquisition decisions based on their expectations about the future value of the resources (Barney, 1986). According to Barney (1986: 1231), a canonical example of a strategic factor market is the "market for buying and selling companies", because the M&A market for public firms tends to be competitive and thus acquirers' acquisition decisions are most likely to reflect their expectations about the future value of a target firm. This logic lays the ground for subsequent strategy research on value creation and value capture in M&As (e.g., Barney, 1988; Capron & Shen, 2007), and it also provides an important foundation for the resource-based view of the firm more generally. In this study, we build on strategic factor market theory and argue that negative effects of employee departure from a public target firm will be reflected in the acquirers' expectations about the future value of the target and thus shape their acquisition decision: to the extent that a firm is more likely to anticipate employee departure post-acquisition, the firm will be less attractive to acquirers and less likely to be an acquisition target, everything else constant; by contrast, a firm that is less likely to anticipate employee departure will then be more likely to be an acquisition target.

Though post-acquisition employee turnover is an important topic in the M&A literature, little research has investigated how anticipated employee departure from a firm *ex post* may influence acquirers' *ex ante* decisions regarding whether to bid for the firm, thus affecting the likelihood of the firm becoming an acquisition target. We suspect that this important topic has not been studied for at least two reasons: first, acquirers' expectations about post-acquisition employee departure are unobservable to researchers; and second, while acquirers see the risk of losing people after an acquisition, there is a great deal of uncertainty about the magnitude of post-acquisition employee departure from the target firm, making it difficult for them to form accurate expectations about such turnover and about the future value of the potential target. However, certain *observable* institutional factors exist that can reduce acquirers' uncertainty about employee turnover and thus can inform their acquisition decisions, as we explain further in the hypotheses below.

Employee departure and acquisition likelihood

A prevalent finding in the M&A literature is that acquisitions are disruptive to the people involved (Haspeslagh & Jemison, 1991; Jemison & Sitkin, 1986; Larsson & Finkelstein, 1999; Ranft & Lord, 2000). All else equal, "people-related problems" increase the turnover of individuals in the target company (Jemison & Sitkin, 1986: 147). Even when an acquirer may ultimately want to downsize or replace certain employees from the target, it stands to reason that the acquirer would prefer to be in a position to decide who will stay and who will go in order to minimize the short-run loss of employees whom the acquirer would otherwise prefer to retain. In addition, employee turnover can have negative effects for acquirers in the longer run because proprietary knowledge may leak out as employees leave and join existing or future competitors.

Employee departure from the target firm can have a negative impact on acquirers in several ways. First, the departure of employees can immediately reduce the stock of knowledge assets held by the target firm. Firms often store knowledge in the experience of individuals (Walsh & Ungson, 1991), especially when such knowledge is tacit or otherwise hard to articulate (Kogut & Zander, 1992). Thus, a portion of the targeted knowledge assets in an acquisition may be lost when employees leave the target firm, especially if they do so quickly before they are able to transfer their knowledge to others (Anand, Manz, & Glick, 1998). Such knowledge loss can result in a short-run reduction in the target's "stand-alone" value and negatively affect the acquirers' expectation about the value of the acquisition.

Second, employee departure can disrupt the social system in which the employees are situated. Departures have been shown to reduce team coordination with respect to knowing who knows what (Reagans, Argote, & Brooks, 2005) and the subsequent rate of organizational learning (Carley, 1992). Individuals' departures can also have a direct negative impact on the performance of others that are connected to them in the longer run. For example, research has shown that the sudden and unexpected loss of a superstar scientist leads to a lasting 5% to 8% decline in the collaborators' quality-adjusted publication rates in the years that follow (Azoulay, Zivin, & Wang, 2010). In acquisitions, researchers have found that departures of employees from the target company following an acquisition can damage the morale of those who stay, negatively affecting acquisition success (O'Reilly & Pfeffer, 2000). Given that acquisition integration entails combining and redeploying existing assets and personnel, disruption caused by employee departure can reduce the "synergy" potential of an acquisition and negatively affect the acquirers' expectation about the future value of the acquisition.

Third, the departure of employees can give away valuable sources of competitive advantage, i.e., proprietary knowledge or technology, to immediate or future competitors. Firms have routinely sought to import product line strategies (Boeker, 1997), product innovations (Rao & Drazin, 2002), and key technical knowledge (Rosenkopf & Almeida, 2003) by recruiting talent from their rivals. Spin-outs founded by former employees also pose competition to the firm in the future (Agarwal, Echambadi, Franco, & Sarkar, 2004; Stuart & Sorenson, 2003b). Risks of knowledge leakage can be particularly high in the case of acquisitions. For example, to enhance the productivity of the acquired firm during integration, an acquirer would often transfer proprietary knowledge and provide trainings to the acquired employees. However, after making significant investments in the employees, the acquirer can face an enhanced risk of employee departure as they walk away to join a current or future rival with the knowledge learned. Knowledge leakage like this will particularly affect the expected future value of an acquisition from the acquirers' perspective.

Applying strategic factor market theory to the case of M&As (e.g., Barney, 1986, 1988) suggests that the negative consequences of employee departure from a target firm will be reflected in the acquirers' expectations about the future value of the target and shape their acquisition decisions. We therefore expect that acquirers will be less (more) likely to bid for a firm that is more (less) likely to experience employee departure after an acquisition. This line of argument suggests a negative relationship between the anticipated employee mobility from a firm and the likelihood of the firm becoming an acquisition target.

Our study focuses on the role of employee non-compete agreements (NCAs) in constraining employee mobility. Employee non-compete agreements are contractual provisions that expressly prohibit employees from joining a competitor, or forming a new firm as a competitor, within particular industries and geographic locations for a certain time period (Gilson, 1999). Also known as "covenants not to compete," NCAs have become a nearly ubiquitous feature of employment contracts in the U.S.; surveys show that a large majority of knowledge workers and upper-level management have signed non-compete agreements with their employers (Kaplan & Stromberg, 2001, 2003; Leonard, 2001). Theoretical research has long suggested that varying levels of enforcement of non-competes contribute to the differential employee mobility and patterns of knowledge diffusion observed in different states (e.g., Franco & Mitchell, 2008; Gilson, 1999; Saxenian, 1994). Recent empirical studies have confirmed the negative relationship between non-compete enforcement and individual mobility. For example, Fallick, Fleischman, and Rebitzer (2006) find greater intraregional employee mobility in the computer industry in California (which proscribes enforcement of NCAs) compared to other states. Marx *et al*. (2009) show that Michigan's reversal of its policy prohibiting NCA enforcement causes a substantial decrease in the mobility of inventors. Garmaise (2011) further finds a negative relationship between NCA enforcement and the mobility of executives in a large number of industries. Finally, scholars have also argued that because enforceable non-compete agreements constrain employee mobility, they can help firms protect proprietary knowledge and limit knowledge leakage to competitors (Liebeskind, 1996, 1997).

Drawing from strategic factor market theory (Barney, 1986), we argue that varying levels of enforcement of NCAs are an observable, exogenous source of variation in employee mobility that affect acquirers' expectations about the future value of a target firm and that acquirers' acquisition decisions reflect such expectations. Specifically, as the enforcement of non-competes governing a firm's employees increases, the anticipated employee departure from the firm post-acquisition decreases; to the extent that this information is reflected in acquirers' acquisition decisions, acquirers are more likely to bid for the firm, increasing the likelihood that the firm will become an acquisition target. Therefore, we propose:

> ***Hypothesis 1:*** *An increase in the enforcement of non-compete agreements will increase the likelihood that a firm will become an acquisition target.*

Hypothesis 1 is our baseline hypothesis. The strength of H1, however, should depend upon several conditions of the target firm. Specifically, we suggest that the effect of an increase in NCA enforcement will be strengthened when a target firm is exposed to greater chances of employee turnover, and that the effect will be weakened when the firm has other means to mitigate the negative consequences of employee departure. We examine these moderators below both to develop boundary conditions for our theory and to develop a coherent pattern of predictions to test the consistency of our theory.

Exposure to employee departure

An acquisition allows the acquirer to obtain certain assets of the target firm. The degree to which the acquirer can use or deploy the acquired assets, however, may depend upon the type of assets acquired. Acquiring firms, for example, will have more secured rights over physical assets, but only limited control over human assets due to the inalienability of human capital (Becker, 1964). In particular, people can quit, or they can bargain for a higher wage if they remain with the organization (Coff, 1997: 372). We examine two conditions under which acquirers will be exposed to greater negative consequences of post-acquisition employee departure, and accordingly benefit to a greater extent from an increase in the enforcement of non-competes: first, when the target firm employs a greater proportion of knowledge workers in its workforce; and second, when the target firm faces greater in-state competition.

Knowledge workers. Knowledge workers present a higher risk of post-acquisition mobility for several reasons. First, knowledge workers tend to be more professionalized and resistant to managerial control (Raelin, 1991). Prior research has argued that knowledge workers are more likely to depart the target company after an acquisition (O'Reilly & Pfeffer, 2000), and has shown that such departure creates uncertainty for the acquiring firm regarding the transfer and replacement of personnel and other assets. The uncertainty associated with employee turnover in human capital-intensive targets can cause otherwise attractive deals to break down (Coff, 2002). Second, knowledge workers are more likely to have access to confidential information and first-hand knowledge of the key capabilities of their employer. They are, therefore, more likely to take that knowledge with them to a competitor when they depart, or use that knowledge to generate spin-outs to compete with their ex-employer in the future (Bhide, 2000). Third, legal theory and the justification for non-compete agreements is rooted in the concept that workplace knowledge is a form of employer intellectual property (Fisk, 2009; Hyde, 2010). Employers apply non-compete agreements specifically to protect workplace knowledge from appropriation by knowledge workers (Bishara, 2006).

Overall, these arguments suggest that knowledge workers are particularly likely to create mobility-related problems following an acquisition, such as loss of valuable knowledge, disruption of existing routines, and promotion of current or future competitors. At the same time, knowledge workers are also more likely to be covered by a non-compete agreement, compared to other types of employees (Kaplan & Stromberg, 2001, 2003; Leonard, 2001). Thus, an increase in the enforcement of NCAs should reduce the risk of knowledge workers' departure and undesired knowledge leakage, thus increasing the attractiveness of a firm as an acquisition target, everything else constant. We therefore hypothesize that an increase in NCA enforcement will have an even stronger effect on acquisition likelihood when knowledge workers comprise a larger proportion of a firm's workforce.

> ***Hypothesis 2:*** *An increase in the enforcement of non-compete agreements will increase the likelihood of acquisition to a greater extent for firms with more knowledge workers.*

In-state competition. Similar to firms employing more knowledge workers, firms facing greater in-state competition also need to contend with greater chances of employee mobility. In-state competition can raise the likelihood and consequences of post-acquisition employee departure for the following reasons. First, proximate competitors are more likely to raid employees than distant competitors. As professional networks tend to be geographically localized (Saxenian, 1994; Sorenson & Stuart, 2001; Stuart & Sorenson, 2003a), a firm's employees are more likely to be raided by nearby competitors within the state. Second, more in-state competition presents greater opportunities for employment outside of the target firm. Greater in-state competition reduces the direct and indirect costs for employees to change their jobs (Almeida & Kogut, 1999). Thus, even if competitors do not actively seek to recruit away a target firm's employees, greater opportunities for employment nevertheless increase the likelihood of employee departure. Finally, with more external opportunities, employees have more bargaining power against their employer. Increased bargaining power can lead to firms paying higher wages and benefits, even if employees do not leave the firm (Coff, 1999b). By contrast, employees with fewer external opportunities will be less likely to leave and have less leverage against their employers.

An increase in the enforcement of NCAs will, in particular, constrain employees from changing employment to work for an in-state competitor, because NCAs are more easily enforced within the same state (Gilson, 1999; Garmaise, 2011). Thus, for firms that face greater in-state competition, an increase in NCA enforcement is particularly likely to reduce the risk of employee departure and knowledge leaking to the competition, thereby increasing these firms' attractiveness as acquisition targets. We therefore hypothesize that an increase in the enforcement of non-competes will have an even stronger effect on acquisition likelihood when a firm faces greater in-state competition:

> ***Hypothesis 3:*** *An increase in the enforcement of non-compete agreements will increase the likelihood of acquisition to a greater extent for firms with greater in-state competition.*

Mechanisms limiting knowledge loss due to employee departure

While the departure of employees from an acquired company has negative short-term and long-term consequences for acquiring firms in general (Cannella & Hambrick, 1993; Coff, 2002; O'Reilly & Pfeffer, 2000; Ranft & Lord, 2000), such consequences may vary across individual companies based on the knowledge protection mechanisms at their disposal. In this study, we focus on the intellectual property (IP) regime as one mechanism for protecting knowledge and limiting the negative consequences of employee mobility. Patents are the strongest form of intellectual property protection in that they unambiguously exclude competitors from using the underlying knowledge (Teece, 1998). Patents also protect firms' interest by preventing the firm's own employees from appropriating the knowledge by starting up new ventures or working for rivals. Kim and Marschke (2005), for example, find that the risk of scientist departure leads to a higher propensity for a firm to patent innovations. Research, however, demonstrates that patents vary in their effectiveness across different industries (Cohen, Nelson, & Walsh, 2000; Levin, Klevorick, Nelson, Winter, Gilbert, & Griliches, 1987). Patents are not particularly effective when competitors can easily invent around them, when the underlying technology is changing so fast that patents become irrelevant, or when the basis for the patents is easily challenged in court (Levin *et al.*, 1987).

The strength of the IP regime therefore affects the extent to which firms can use patents to retain knowledge for their exclusive use. If the IP regime is weak, firms are less able to protect their knowledge in patents, and employee departure is more likely to result in a direct reduction of firms' knowledge stock, as well as a transfer of proprietary knowledge to a current or future competitor. By contrast, if the IP regime is strong, firms have a stronger claim on their patented knowledge and are more able to secure that knowledge even when certain employees leave the firm. A stronger IP regime therefore helps firms limit the risk of knowledge loss due to employee mobility. Consequently, while an increase in NCA enforcement will reduce employee departures and better protect firms' knowledge assets, that effect should be weaker for firms operating in a stronger IP regime, which provides another mechanism for knowledge protection. As a result, an increase in NCA enforcement will increase the attractiveness of firms protected by a stronger IP regime as acquisition targets to a lesser degree, compared to firms operating in a weaker IP regime:

> ***Hypothesis 4**: An increase in the enforcement of non-compete agreements will increase the likelihood of acquisition to a lesser extent for firms protected by a stronger IP regime.*

RESEARCH DESIGN

Empirical challenges exist in developing causal evidence on the link between the enforcement of NCAs and acquisition likelihood. In particular, the level of NCA enforcement within a state rarely changes, and when it does change, it usually changes by a modest amount (Garmaise, 2011; Gilson, 1999). While there is considerable variation in the level of NCA enforcement between states, a cross-sectional analysis can be confounded by selection effects and unobserved heterogeneity. To overcome the issue of endogeneity in our study, we exploit a natural experiment related to a policy reversal of NCA enforcement that occurred in Michigan.

The Michigan natural experiment

In 1985, the Michigan legislature passed the Michigan Antitrust Reform Act (MARA) to harmonize

Michigan state law with the Uniform State Antitrust Act (Bullard, 1985). In passing MARA, however, research suggests that legislators also inadvertently repealed Michigan statute 445.761, a statute that previously prohibited the enforcement of non-compete agreements in Michigan (Alterman, 1985). As a consequence, Michigan employers suddenly, and unexpectedly, obtained the legal means to prevent employees from leaving their firms to work for a competitor in Michigan or other states that enforced out-of-state NCAs. (Curtner & Green, 1985: 270) suggested that the Michigan antitrust reform was a result of the wide recognition (among businesses, labor, enforcement agencies, and the bar in Michigan) of the need to consolidate the state's "archaic and fragmented" antitrust laws and "conform more closely to federal law and the Uniform State Antitrust Act." Thus, the reform did not appear to be a result of state politics or other idiosyncratic factors, which may affect M&A activity in different ways (Seldeslachts, Clougherty, & Barros, 2009). Because stronger enforcement of anti-trust regulations, especially at the federal level, is unlikely to cause an increase in M&A activity (Brodley, 1995), anti-trust aspects of MARA should work against us finding our hypothesized effects. It would therefore appear that the repeal of Michigan statute 445.761 provides an appropriate natural experiment for assessing the effect of anticipated employee mobility on acquisition likelihood. Indeed, Marx *et al*. (2009) have demonstrated that the policy reversal significantly reduced the mobility of knowledge workers in Michigan. We would also note that the change of NCA enforcement is relevant for our study because both research and industry practice suggest that acquirers pay a great deal of attention to non-competes when conducting due diligence in M&As (Deloitte, 2010; Garmaise, 2011). In addition, being publicly available information, we believe that the policy change would be reflected in acquirers' acquisition decisions in the highly competitive M&A market (Barney, 1986).

A good natural experiment for research is one in which there is an unexpected, exogenous, and transparent assignment of a 'treatment' status (Meyer, 1995). Such assignment can allow researchers to identify exogenous variation in the explanatory variables and rule out the possibility that policy makers adopted the treatment because of conditions in the prior period (Heckman & Smith, 1999). An unexpected treatment also rules out the possibility that firms might have made economic decisions based on expectations of the treatment. It is, therefore, particularly important for the purposes of this study that the reversal of Michigan's NCA enforcement policy was accidental and unplanned. Marx and colleagues (2009: 887) have examined relevant legislative reports (e.g., Bullard, 1985) and legal reviews (e.g., Alterman, 1985) and conducted interviews with lawyers who then wrote about the policy change; these authors have concluded that the reversal of the enforcement of NCAs in Michigan was an unexpected shock and a truly exogenous source of variation in the mobility of knowledge workers.

The Michigan natural experiment lends itself to a difference-in-differences (DD) analysis (Meyer, 1995). The DD is frequently used to study the effect of policy changes in observational data when the researcher is unable to *randomly* assign subjects into a treatment group versus a control group. Card and Krueger (1994) provide a classic example of the use of DD in labor economics, and Chatterji and Toffel (2010) provide a recent example of the use of the technique in strategic management. In our analysis, we assigned firms in Michigan to the 'treated group' in that firms in Michigan experienced the MARA policy change. We followed prior research and assigned firms in the states of Alaska, California, Connecticut, Minnesota, Montana, North Dakota, Nevada, Oklahoma, Washington, and West Virginia to the 'comparison group' in that these states did not enforce NCAs before or after MARA (Malsberger, Brock, & Pedowitz, 2002; Marx *et al.*, 2009; Stuart & Sorenson, 2003b). By assuming that trends in the comparison group represent trends in what would have happened in the treatment group in the absence of treatment, the DD identifies a causal treatment effect as the before-to-after difference in Michigan, netting out trends from the comparison group. A DD analysis removes observed and/or unobserved differences between treatment and control, provided that those differences remain fixed over time (Wooldridge, 2002). To strengthen the 'equal trends' assumption between the groups, we used Coarsened Exact Matching (Blackwell, Iacus, King, & Porro, 2009) to select firms for comparison that were more similar at the time of treatment (described below) and we also included a number of covariate controls to adjust for potential differences in trends over time (described below).

Sample and data

Our sample construction started with all publicly traded firms in the United States between 1979 and 1998 that could potentially become an acquisition target. We first obtained the base sample from Compustat, excluding financial instruments (e.g., ADRs and ETFs) and securities used internally by the firm (i.e., CUSIPs ending in 990-999 or 99A-99Z). Next, we restricted that sample to include only firms headquartered in Michigan or a comparison state defined earlier, and we further limited the sample to only firms that were listed prior to MARA. We excluded new firms listed after MARA from the sample to ensure that MARA itself did not affect the composition of the sample (we included new firms in a robustness check to be reported below), i.e., to exclude the possibility that some firms might decide to be listed after MARA in response to potential changes in acquisition likelihood. After these steps, we arrived at a preliminary sample of 19,020 firm-year observations.

As the final step in our sample construction, we implemented "Coarsened Exact Matching" (CEM) (Blackwell *et al.*, 2009) to improve the covariate balance of the sample. CEM is a multivariate matching technique that is monotonic imbalance bounding (Iacus, King, & Porro, 2011), and, as such, reduces causal estimation error, model-dependence, bias, and inefficiency (Iacus, King, & Porro, 2009a, 2009b). See Azoulay *et al*. (2010) for another application of CEM. Because matching on too many covariates will cause the analysis to lose much of its statistical power (Heckman, Ichimura, & Todd, 1998), we followed prior research and used CEM to match on a subset of firm-level covariates. We matched on *Assets*, *Liquidity*, and *ROA*, in that these covariates have been shown in prior research (e.g., Field & Karpoff, 2002), as well as our own regression results, to affect the likelihood of acquisition. We used CEM's coarsening function to non-parametrically separate the joint distribution of these covariates into coarsened strata, and then matched firms in Michigan to firms in comparison states by strata, weighting each comparison by the number of matched observations. We matched on the pre-MARA average for each measure in order to ensure that MARA itself could not affect the matching process.

The CEM procedure improved the in-sample multivariate imbalance of our data from $L1 = 0.1612$ to $L1 = 0.0772$ (for a definition of the $L1$ statistic, see Iacus *et al.*, 2011), increased the proportion of firms based in Michigan (most observations in Michigan are matched and only similar observations in comparison states are matched), and increased slightly the average size, years public, ROA, and liquidity of the firms in the sample. To test the sensitivity of our results to our matching procedure, we also included observations dropped by CEM back into the sample in a robustness check to be reported below. By dropping dissimilar observations between firms in Michigan and firms in comparison states, CEM reduced the sample to 18,713 firm-year observations, which served as the 'base population' of firms that could potentially become a target for acquisition (Song & Walking, 1993, 2000).

We then obtained information on acquisition events from Thomson Financial's SDC Platinum M&A database, and matched the acquisition events to our base population of firms available for potential acquisition based on their CUSIPs. We obtained firms' historical CUSIPs from Compustat's historical files. We followed Song and Walkling (2000) and excluded acquisition bids where the deal value was less than $500,000. We also followed prior research to exclude deals labeled as buybacks, exchange offers, privatizations, spinoffs, carveouts, self-tenders, and recapitalizations.

Figure 1A shows the temporal trends of the base population of firms and the acquisition bids for those firms from 1982 to 1998. The top two lines in the figure represent the number of firms by group (Michigan vs. comparison states), and they reveal that the numbers for both groups grew up to 1987 and then declined as firms were acquired or delisted due to firm failure. The bottom two lines represent the number of acquisition bids by group, and they show a spike of acquisition bids for firms in Michigan following MARA in 1988. Figure 1B presents the rates of acquisition (number of acquisitions as a percentage of the number of firms that could become an acquisition target) for both groups.

Insert Figure 1 about Here

Measures

Dependent variable. Consistent with prior research on acquisition likelihood (e.g. Song & Walkling, 1993, 2000), our dependent variable, *Acquisition*, is a dichotomous variable that equals one if the focal firm is the target of an acquisition in a given year based on the acquisition announcements reported by

SDC, and zero otherwise. According to Song and Walkling (1993: 441), this approach "avoids *ex post* selection bias" and offers the advantage to sample "all firms" that become the targets of acquisition.

Given our research focus, our right-hand-side variables are limited to those that are available for *all* listed firms that could potentially receive an acquisition bid. This research design follows prior studies of the likelihood of firms becoming acquisition targets (e.g., Ambrose & Megginson, 1992; Field & Karpoff, 2002; Palepu, 1986; Song & Walkling, 1993, 2000). Variables that are defined at the acquirer level, the dyadic level, or the deal level, therefore, cannot be included in our models given the research design.

Explanatory variables. The DD 'treatment group' variable, *Michigan*, is an indicator variable that equals one if a firm was located in Michigan based on the historical location of the firm's corporate headquarters, and zero otherwise. Compustat's historical files provide information on firms' historical locations required for this variable. The DD 'after' variable, *After*, is an indicator variable that equals one for all years after 1987 (i.e., 1988 or after), and zero otherwise (i.e., 1987 or before). We believe that it would take some time for law firms to disseminate news of the policy change to their clients (Marx *et al*., 2009), and additional time for the knowledge to be absorbed by corporate development managers. It would then take more time for potential acquirers to act upon the knowledge, given the significant amount of time that is usually involved in target search and selection, due diligence, and negotiation before announcing an acquisition; for example, prior M&A research suggests that the M&A process often takes eight months to a year from the date when acquirers officially contact targets (this date is publicly reported to the SEC) to the date of public announcement (Boone & Mulherin, 2007), not including the private, unobservable part of the process. We therefore selected the break between 1987 and 1988 as the dividing point for the before and after periods in the DD analysis. Following prior DD research (Meyer, 1995), we then created an interaction variable *Michigan * After* and used this variable to identify the treatment effect of MARA and test H1.

To test the moderating effects proposed in H2, H3, and H4, we first developed three variables: *Knowledge Workers*, *In-state Competition*, and *IP Protection*; then we extended the basic DD model by including the three-way interaction of *Michigan * After* and each of these three variables (Meyer, 1995).

We mean centered the continuous variables at zero to simplify interpretation of the interaction effects.

For *Knowledge Workers*, we followed prior research (e.g., Coff, 1999a, 2002; Farjoun, 1994) and measured the level of knowledge-workers employed in a focal firm's industry as a proportion of the total workforce employed in that industry. We obtained data on employment levels from the Occupational Employment Statistics (OES) survey from the Bureau of Labor Statistics. Using the OES occupational codebook, we defined knowledge workers to be those with an occupational code below 50,000. This definition includes occupations such as managers, sales workers, scientists, engineers, editors, computer programmers, IT professionals, and so forth. The OES provides data on the breakdown of the total number of people employed in each 3-digit SIC industry by OES occupational code. From the OES data, we calculated the proportion of the total workforce being knowledge workers for a given 3-digit SIC, and then assigned that measure to each focal firm in our sample, weighted by the proportion of the firm's sales in its 3-digit SIC industries. Because the Compustat Segments file provides more comprehensive coverage of firms' sales data by NAICS, we extracted the data by the 4-digit NAICS designation and then converted it to the 3-digit SIC designation using the NAICS to SIC concordance provided by the U.S. Census Bureau. *Knowledge Workers* is a continuous measure from 0 to 1.

For *In-state Competition*, we followed prior research by Garmaise (2011) and measured the proportion of total U.S. sales generated by other firms located in the same state and same industry as the focal firm using data from the Compustat Segments file; the focal firm's own sales was excluded. We assigned the measure to each firm in our sample, weighted by the proportion of the firm's sales in its 3-digit SIC industries. The variable *In-state Competition* is a continuous measure from 0 to 1.

For *IP Protection*, we followed Cohen, Nelson, and Walsh (2000) and used their measure of the mean percentage of product innovations for which patents are an effective mechanism for protecting the underlying knowledge and appropriating the returns. This measure has been used widely in prior strategy research (e.g., Dushnitsky & Shaver, 2009; Shane, 2001). Specifically, we obtained the Cohen *et al*. measure by industry from their Table 1, and assigned the measure to the manufacturing firms in our sample in a way similar to the calculation of the two explanatory variables above. We assigned a value of zero to non-manufacturing firms, as the measure is not relevant to those firms. We rescaled this continuous measure to vary from 0 to 1 to be consistent with the other two explanatory variables.

Control variables. Given the importance of equal trends in a difference-in-differences analysis, we included a wide range of industry, state, industry-by-state, and firm-level controls to account for potential differences in acquisition trends between firms in Michigan and those in comparison states. To control for year-by-year variations, we included a full set of year indicators. To control for cross-industry differences, we included a set of industry indicators: *Auto* (3-digit SICs 371, 375, 379), *Drugs* (SIC 283), *Chemicals* (SICs 281-282, 284-297, 289), *Computers & Communication* (SICs 357, 481-484, 489), *Electrical* (SICs 360-369), *Wholesale* (SICs 500-519), and *Retail* (SICs 520-599)*;* with "service industries & others" as the base category (Marx *et al*., 2009); results are robust to the use of 3-digit SIC industry dummies as shown in a robustness test below. To control for state economic and political conditions, we included four state-level variables: *State GDP (log)*, a continuous variable calculated as the natural logarithm of state GDP based on data from the Bureau of Economic Analysis; *State Business Combination Laws*, an indicator variable coded to one for states passing laws that reduced the threat of hostile takeover (data obtained from Giroud & Mueller, 2010); and *State Establishment Entry* and *State Establishment Exit*, continuous variables calculated as the birth-rate and death-rate of establishments in a focal state based on data from the Business Dynamics Statistics series of the U.S. Census.

Next, we controlled for industry-specific characteristics by including a set of variables at the industry-by-state level (i.e., calculated by the 3-digit SIC industry using data for all firms headquartered in the same state as the focal firm): *Industry-State Tobin's q*, a continuous measure calculated using the following Compustat data fields based on the equation *Tobin's q* = ((PRCC_F * CSHO) + AT – CEQ)/AT (Chung & Pruitt, 1994) to control for differences in industry growth opportunities; *Industry-State Herfindahl*, a measure of industry concentration of sales to control for industry consolidation and merger wave effects (Clougherty & Seldeslachts, 2012; McNamara, Haleblian, & Dykes, 2008); *Industry-State Acquisition Rate* and *Industry-State Acquisition Rate Squared*, continuous measures (from 0 to 1) of the rate of acquisitions over the previous three years to control for merger wave effects (e.g., Palepu, 1986;

Seldeslachts *et al.*, 2009); *Industry-State Acquisition Rate Instate* to control for the rate of within-state acquisitions; *Industry-State Delisting Rate*, a continuous measure from 0 to 1 to control for the rate at which public firms were delisted and dropped out of our sample; and *Industry-State Sales Growth*, a continuous measure of sales growth over the previous three years to control for merger wave effects (Clougherty & Seldeslachts, 2012). We also control for labor market conditions by including a measure *Beale Urban Index*, defined as the level of urbanization based on the local population size and proximity to a metropolitan area, using data provided by the Department of Agriculture, Economic Research Service.

Finally, we included several firm-level variables that have been suggested by prior M&A research to affect the likelihood of acquisition. To control for the size of the firm, we measured *Assets* for each firm (log transformed). We also followed prior research (Field & Karpoff, 2002; Song & Walkling, 1993) to control for the firm's *Liquidity*, defined as the ratio of net liquid assets (current assets minus current liabilities) to total assets. To control for the past performance of the firm, we measured the 3-year trailing average return on assets (*ROA*) for each firm that is in excess of the 3-year trailing average return on assets for the focal firm's 3-digit SIC industry. We also included a control for the firm's *Sales Growth* over the previous three years. To control for changes in the propensity of firms to patent intellectual property, we included *Patents* as a measure of the number of granted patent applications in the current year. We also included a control for the firm's *Years Public* and followed Garmaise (2011) to measure this variable by considering the firm's year of public listing. Given that some firms do not report in the Compustat Segments file (approx. 5%), we include an indicator variable *Reports Segments* that equals one for firms reporting in the Segments file; for firms not reporting in the Segments file, we calculated our explanatory variables based on the firm's primary 3-digit SIC industry. Finally, to control for the attractiveness of a focal company to bidders, we included *Prior Bids (log)*, calculated as the natural logarithm of 1 plus the number of prior acquisition bids made for a focal firm before MARA.

Table 1 presents the descriptive statistics and correlations for all variables used in the study based on our final, CEM-matched sample of 18,713 firm-year observations. Given the historical nature of the Michigan experiment (1980s), we faced several limitations in the availability of data. The SEC did not mandate electronic proxy statements before 1993, and we were therefore unable to control for firm-level differences in anti-takeover defenses (we controlled for state-level differences in business combination laws, as noted earlier). A detailed, geographic breakdown of operations by state was unavailable for most firms in our sample, and we therefore followed prior research (e.g., Garmaise, 2011) and assigned firms to the treatment group (Michigan) versus the comparison group by referring to a firm's corporate headquarters (not their state of incorporation). We do not believe, however, that these data limitations bias our analysis. The DD technique removes fixed differences between treatment and comparison groups (observed or unobserved), provided that those differences remain fixed over time. We also believe that the assignment of firms to states based on the corporate headquarters is conservative: Michigan firms with employees in other states should experience less of the hypothesized effects, while firms headquartered in the comparison states with employees in Michigan should experience at least some of the hypothesized effects. Because a DD analysis measures the *relative* effect of the policy change between the treatment and comparison groups, our measure of firm location should work against us finding our hypothesized results.

Insert Table 1 about Here

Models

We estimated a set of logit models in a difference-in-differences configuration to test whether the increase in the enforcement of NCAs in Michigan affects the likelihood of a Michigan firm becoming an acquisition target (H1), and to examine the conditions under which this relationship is strengthened or weakened (H2-H4). Instead of estimating the before-to-after change in outcomes for the treatment group (i.e., Michigan) and then assuming that the difference is the effect of the policy change, the DD approach adjusts for the counterfactual trend of what would have happened to the treatment group in the absence of the treatment. Our DD model does this by estimating the change in the treatment group and the change in the set of ten comparison states, and then taking the difference of these two differences (hence, the 'difference-in-differences'). Our analysis is at the firm-year level, and we clustered the standard errors by the firm to account for the non-independence of repeated observations with a firm.

RESULTS

To begin the analysis, we compare the observed rate of acquisition of firms in Michigan with the rate of acquisition in comparison states in a univariate analysis. Panel 2A in Table 2 indicates that the rate of acquisition rose by 12.92 percentage points in Michigan, from 6.77% in 1987 before MARA to 19.69% in 1988 after MARA. Not all of this change, however, can be attributed to the effect of MARA, because the rate of acquisition also increased in the comparison states during the period. A difference-in-differences analysis subtracts the difference in the comparison states (1.70 percentage points) from the difference in the treated state Michigan (12.92 percentage points), to determine the effect of the policy change without the confounding influence of other trends that were underway in the economy, and the corresponding univariate difference-in-differences statistic is presented in the bottom right cell of Panel 2A: The treatment effect of MARA was a 11.22 percentage point increase in the acquisition rate from 1987 to 1988. For comparison, we also examined the effect of MARA for two other time windows: 1983-1992 and 1982-1998. The former time window adds four years of data on either side of the base window 1987-1988. The latter time window begins in 1982, because data on acquisitions were thin before the early 1980s and because we needed three prior years' data to calculate the control variable for the industry's prior rate of acquisitions; it ends in 1998 to avoid the acquisition wave associated with the Internet bubble in the late 1990s (Moeller, Schlingemann, & Stulz, 2004). As shown in Panels 2B and 2C, the effect of MARA was a 2.86 percentage point increase in the acquisition rate from 1983 to 1992, and a 0.62 percentage point increase from 1982 to 1998, respectively. The pattern of the results in the three panels indicates that the effect of the policy reversal weakened over time and as the time window widened.

Insert Table 2 about Here

Moving to a regression framework, Table 3 reports results of multivariate difference-in-differences analyses for progressively longer time windows surrounding MARA. Each model adjusts for possible deviations in trends between firms in Michigan and firms in comparison states by including the control variables described earlier. We include an indicator for Michigan (*Michigan*), an indicator for post-MARA (*After*), and the interaction of *Michigan* After*. The interaction of *Michigan * After* estimates the difference-in-differences effect in a multivariate framework. Column 1 examines the effect for the1987-1988 window, Columns 2-6 expand the window progressively by an additional year forward and backward, and Column 7 expands to the full range of data available in the sample (1982 through 1998). Our expectation was that the effect of MARA would be stronger in the short-term around MARA and then weaker as the time window expands. In line with our expectation, we find that the DD effect (i.e., *Michigan* After*) was stronger and statistically more significant immediately around MARA, and that the effect then attenuates over time as the sampling timeframe expands and we move from Column 1 to Column 7.

Insert Table 3 about Here

To examine concerns that our results might reflect certain developments in the Midwest, as opposed to the policy change unique to Michigan, we perform a number of 'placebo' tests of major Midwest states, as well as several other states from around the country, to determine if any of those states experienced similar increases in the likelihood of firms becoming an acquisition target during the study period. Specifically, we conducted placebo tests for Ohio, Illinois, Indiana, Pennsylvania, Connecticut, Wisconsin, Florida, New York and Oregon, by substituting "untreated" firms from the given placebo state as if those firms had been "treated" by MARA (although, in fact, they were not); results for these placebo tests are reported in Columns 1-9 of Appendix A. In none of the placebo tests did we find an increase in the likelihood of acquisition, providing additional evidence that the rise in acquisition likelihood in Michigan was unique to Michigan. We also investigate the issue of whether our results might be due to concurrent changes in Michigan's antitrust laws, instead of its inadvertent reversal of NCA enforcement. Folsom reports (1991: 955, footnote 54) that five other states enacted major new antitrust law in the 1980s: Delaware (1980), District of Columbia (1981), Florida (1980), North Dakota (1987), and Texas

(1983). Among these, only Texas and North Dakota are testable with the M&A data in SDC, which started to systematically track acquisitions in the 1980s. However, there were only six publicly-listed firms headquartered in North Dakota in our sample during the study period, and such a small sample makes it difficult to conduct meaningful analysis. We therefore tested whether the Texas Free Enterprise and Antitrust Act (TFEAA) of 1983 would produce effects similar to those we report for Michigan, and, as expected, we did not find significant results (results reported in Column 10 of Appendix A). In additional analysis (results not tabulated due to space constraint, but available upon request), we tested whether the antitrust reform in Michigan caused Michigan firms to become active acquirers by using two dependent variables, the likelihood of a firm being an acquirer and the number of acquisition bids a firm makes. In neither of the two tests did we find a significant increase for Michigan firms after MARA, suggesting that the antitrust reform itself is not directly related to M&A activity.

Hypotheses testing

Table 4 reports results of a moderated difference-in-differences analysis, wherein we interact *Michigan * After* with the conditions hypothesized about in H2-H4. We use the widest time window available in the sample (1982-1998) to provide a conservative test of our theory, and to increase our statistical power to test the multiple, three-way interactions. Model 1 repeats the results of Column 7 in Table 3 for comparison; Models 2-4 successively add the three-way interaction of *Michigan * After* and each of the three explanatory variables: *Knowledge Workers*, *In-state Competition*, and *IP Protection*; Model 4 is the Full Model including all of the variables at the same time. We therefore interpret the results from Model 4 for our hypothesis testing.

Insert Table 4 about Here

Concerning the control variables, first we find that the likelihood of acquisition is higher in the computers and communications industry (p<0.01). As expected, the likelihood of acquisitions is lower in states passing business combination laws (p<0.001). Higher levels of *Industry-State Tobin's q* are associated with an increase in acquisition likelihood (p<0.001), an indication of firms biding for targets in industries with greater growth opportunities. Industry concentration is negative and significant, suggesting that firms in highly concentrated industries are less likely to be acquisition targets. We find that the intensity of past acquisition activity in an industry is a positive and highly significant predictor of the firm's likelihood of being a target (p<0.001), a finding consistent with prior research (e.g. Palepu, 1986), but the negative and significant coefficient for the squared term (p<0.001) indicates that this effect eventually becomes negative. *Industry-State Delisting Rate* is associated with an increased likelihood of acquisition (p<0.001), perhaps because firms that remain listed become more attractive as acquisition targets. Larger firms are more likely to become acquisition targets (p<0.01). Interestingly, firms with greater *Liquidity* are also more likely to become acquisition targets (p<0.05), an indication that acquisition activity in our sample is not limited to acquirers purchasing distressed firms. Older firms are less likely to become acquisition targets (p<0.01), and firms that do not report data for business segments are also less likely to become acquisition targets (p<0.001). Firms that have received prior bids are much more likely to become a target of acquisition (p<0.001).

Concerning the main effects of variables included in the DD analysis (i.e., *Knowledge Workers*, *In-state Competition*, *IP Protection*, *Michigan*, and *After*), we find that firms with higher levels of *Knowledge Workers* or *IP Protection* are less likely to be acquired (p<0.001). Also, firms based in Michigan are overall less likely to be targets for acquisition (p<0.001), and that the likelihood of acquisition generally increases from the before period to the after period for firms in both Michigan and the comparison states (p<0.001); both of these findings are consistent with our univariate analysis and the graphs shown in Figure 2.

Next we turn to the hypotheses testing results. In our baseline hypothesis (H1), we posit that an increase in the enforcement of NCAs, such as MARA, will increase the likelihood of a firm becoming an acquisition target. The positive and highly significant coefficient for the interaction of *Michigan * After* provides strong support for this hypothesis (p<0.001). This result indicates that firms in Michigan are more likely to be acquisition targets following the passage of MARA, after adjusting for the concurrent increase in the likelihood of acquisition of firms in the comparison states. Hypotheses 2-4 further identify several conditions under which constraints on employee mobility due to the increase in the enforcement of NCAs will be more or less important in shaping firms' likelihood of being a target. Consistent with the prediction in H2, we find that the three-way interaction of *Michigan * After * Knowledge Workers* is positive and significant (p<0.001), suggesting that the effect of MARA on acquisition likelihood is stronger when firms employ more knowledge workers in their workforce that present a greater risk of employee mobility. Similarly, the three-way interaction of *Michigan * After * In-state Competition* is positive and highly significant (p<0.01), providing strong support for H3; this result suggests that the effect of MARA is stronger when firms face greater in-state competition, a condition that can increase the risk of employee turnover. Finally, H4 predicts that the effect of MARA will be weaker when firms have other means such as IP protection to protect knowledge from appropriation by competitors. There is evidence supporting this hypothesis: the three-way interaction of *Michigan * After * IP Protection* is negative and significant (p<0.01), suggesting that the effect of MARA is weaker when firms are protected by a stronger IP regime.

Prediction and interpretation of interaction effects

To demonstrate the economic significance of our results, we calculated the predicted probability of a Michigan firm becoming an acquisition target from the Full Model. We predicted outcomes at the mean of all covariates, grouped by treatment status and time period (before/after); moderator variables are mean-centered at zero. Greene (2010: 291) recommends that researchers use graphical representation to interpret higher-order interaction effects in nonlinear models; we therefore followed Zelner (2009) and used a combination of simulation and graphing techniques to assess the boundary conditions of the predicted difference-in-differences effect at different levels of each moderating variable. Our objective is to predict the before-to-after effect of MARA in Michigan, adjusting for changes in the comparison states that represent what would have happened in Michigan in the absence of MARA. We present our predictions in Figure 2 and explain the details of our simulation and graphing technique in Appendix B.

Insert Figure 2 about Here

Panel A. Panel A in Figure 2 shows the predicted before-to-after change in the likelihood of being an acquisition target at different values of each moderating variable (*Knowledge Workers*, *In-state Competition*, and *IP Protection*). The 'naïve' effect of MARA is the difference between the bottom, 'Before' line and the top, 'After' line. The 'difference-in-differences' effect is represented by the shaded region between the 'Before' line and the middle, 'After Adjusted' line.

Panel B. Panel B in Figure 2 shows the magnitude of the predicted DD effect as well as the confidence-interval and range over which the DD effect is statistically significant and different from zero. The magnitude of the DD effect is calculated by subtracting the value of each 'Before' line from the corresponding value of each 'After Adjusted' line in Panel A, and the result is then plotted as a solid black line in Panel B. For each effect hypothesized in H2-H4, we simulated 1,000 predictions, for both the before-MARA period and the after-MARA period, for both Michigan and the comparison states, and for 60 separate levels of each moderating variable, resulting in a total of 240,000 simulations for each effect. From these simulations we constructed a 95% confidence interval around the predicted 'difference-in-differences' line.

We now interpret the effects presented in Figure 2. As seen at the mean-centered-zero-point of each interaction graph in Panel A, after adjusting for changes in comparison states and effects of the covariates, the sudden enforcement of non-compete agreements increased the likelihood of an average firm in Michigan becoming an acquisition target by 3.35 percentage points (i.e., the "After Adjusted" line minus the "Before" line, as read at the center point of the graphs). Similar magnitude of the effect can also be seen at the center point of the graphs in Panel B. Further, by examining the left or right tails of the predicted DD effect for each moderating effect in Panel B, we are able to obtain a richer interpretation of our hypothesized effects. Specifically, we find that MARA did not affect acquisition likelihood significantly where we would not expect it to matter that much: namely, when firms have low levels of *Knowledge Workers* and *In-state Competition*, or high levels of *IP Protection*; but that MARA did affect acquisition likelihood significantly where we would expect it to matter the most to the firm: namely, when firms have high levels of *Knowledge Workers* and *In-state Competition*, but low levels of *IP Protection*.

Robustness checks

We performed a series of robustness checks of the Full Model used for hypotheses testing (Column 4 of Table 4) and report these results in Table 5. We begin by testing the counter-factual comparison made in our difference-in-differences analysis. Whereas we follow prior research (Marx *et al.*, 2009; Stuart & Sorenson, 2003b) and assume that firms in states that did not enforce non-compete agreements (before and after MARA) should provide the best counterfactual comparison with respect to the enforcement of non-competes, such firms may not provide the best comparison for other economic factors. Therefore, in Column 1 of Table 5, we change the DD comparison group to firms headquartered in states near Michigan (i.e., Ohio, Indiana, Illinois, Wisconsin, and Pennsylvania) to control for trends in the Midwest economies. We find similar results in Column 1 as we do in the Full Model, with statistical significance at $p<0.01$ or better for all of our hypotheses. In Column 2, we test industry fixed effects at the 3-digit SIC level and find results similar to the Full Model, indicating that between-industry differences at finer levels do not drive our results. In Column 3, we use backwards elimination (Lindsey & Sheather, 2010) to remove controls that reduce an optimal Bayesian Information Criterion; this procedure retains the following controls: *Ind. Computers & Communication, Business Combination Laws*, *Ind-State Tobin's q*, *Ind-State Herfindahl, Ind-State Acquisition Rate, Ind-State Acquisition Rate Squared, Ind-State Delisting Rate, Assets (log), Years Public, Reports Segments,* and *Prior Bids (log)*. Results in Column 3 are consistent with the Full Model, suggesting that our findings are not sensitive to the inclusion of irrelevant controls. In Column 4, we check whether our specification of the 'before' and 'after' periods affects our results. So far we have assumed a two-year lag in how knowledge of MARA was diffused and acted upon by potential acquirers, yet it is possible that this process might take less time. We test the one-year lag time break (1986-1987) in Column 4 and find similar results as the Full Model.

Column 5 expands the sample to include observations dropped by our matching procedure and finds very similar results. In Column 6, we check whether limiting our sample selection to firms in existence prior to MARA affects our results. While our selection of a prior-only population rules out the possibility that the sample selection is endogenous to MARA, and this practice follows prior research (Marx *et al.*, 2009), the selection of a prior-only population might cause our results to be influenced by certain characteristics of firms that survived into the later years of the analysis. Therefore, in Column 6, we analyze a broader sample that also includes new firms emerging after MARA, and find very similar results as we do with a prior-only sample. In Column 7, we check whether non-independence of repeated observations at the state level affects our results. While we cluster standard errors in all models at the firm-level, recent 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 state-level clusters (Bertrand, Duflo, & Mullainathan, 2004). Clustered block-bootstrapping methods are often used in this situation to correct for non-independence (Cameron & Trivedi, 2009). In Column 7, we re-estimate the standard errors of the Full Model using a robust block-bootstrapping procedure, clustered at the state level, and find similar or higher levels of statistical significance.[1]

Insert Table 5 about Here

We conduct several other robustness tests but do not tabulate the results due to space constraint (results available upon request). First, the automotive industry accounts for 13% of firms in Michigan in our sample; the importance of the automotive industry raises the concern that particular characteristics of the industry might explain differences in the likelihood of acquisition, independent of NCA enforcement. While we control for the automotive industry in all of our models with an indicator variable, it is possible that our results are sensitive to time-trends in the automotive industry. As an additional robustness check,

[1] Although block-bootstrapping should not, in and of itself, change coefficient estimates, the block-bootstrapping procedure conflicts with the application of CEM weights. We therefore conducted our block-bootstrapping procedure on the sample of matched observations, but without the application of CEM weights.

therefore, we added control variables for *Michigan * Auto*, *After * Auto*, and *Michigan * After * Auto* to the Full Model to control for the before-to-after trend in autos. The addition of these variables did not substantively change the effect size or statistical significance of any of our hypothesized effects (with the exception that the significance level for H2 dropped from $p<0.001$ to $p<0.01$). Second, we examine whether acquirers biding for poorly-performing targets to upgrade the targets may explain our results (Makaew, 2011). Thus, in another robustness check, we add variables *Michigan * ROA1*, *After * ROA1*, and *Michigan * After * ROA1* to the Full Model, with ROA1 being defined as operating income divided by assets in t-1. We find that the *Michigan * After * ROA1* term is positive and significant, suggesting that companies with better performance are more likely to be acquisition targets due to MARA; in addition, the results for the hypothesized effects are substantively identical. Third, it is possible that certain unobserved and unmeasured acquirer characteristics or motivations may drive our results. For instance, employee departure may not be a significant concern for an acquirer if it aims to acquire the target's patents or revenue base. To test such possibilities, we add to the Full Model two "triple-difference" interaction variables, *Michigan * After * Patent Stock* and *Michigan * After * Revenue*, as well as their lower-order interactions. We find that *Michigan * After * Patent Stock* is negative and moderately significant, indicating that firms with more patents are less likely to become an acquisition target due to MARA; *Michigan * After * Revenue* is non-significant, however. Perhaps more importantly, the results for the hypothesized effects continue to be highly significant with these additional control variables.

DISCUSSION

Drawing on a difference-in-differences analysis of a natural experiment in Michigan, we have shown that the state's reversal of its previous policy proscribing the enforcement of non-compete agreements *causes* an increase in the likelihood of a firm becoming an acquisition target. Because research has shown that NCA enforcement reduces employee mobility (Fallick, Fleischman, & Rebitzer, 2006; Garmaise, 2011; Marx *et al.*, 2009) and because strategic factor market theory argues that firms make acquisition decisions based on their expectations about the value of a resource (Barney, 1986), our results suggest that decreases in anticipated employee mobility due to the policy reversal affect acquirers' expectations about the value of a firm and increase the attractiveness of the firm as an acquisition target. We have also found strong support for the three moderating hypotheses centered around knowledge-based arguments, highlighting that employee mobility is indeed an important factor affecting acquirers' decision to use M&As as a strategy to source knowledge and human talents. Taken as a whole, across multiple models and robustness tests, our study shows a consistent pattern of results suggesting a negative *causal* relationship between the anticipated employee departure from a firm and the likelihood of the firm being an acquisition target, and we demonstrate further that this relationship is contingent on the consequences of employee departure for acquirers.

Our paper makes several important contributions to theory and research. First, our study contributes to a prominent stream of M&A research that focuses on the human capital aspect of acquisitions. Prior research has pointed to the significant challenges acquirers face in retaining the employees of the acquired company post-acquisition (Coff, 2002; Hambrick & Cannella, 1993; Haspeslagh & Jemison, 1991; O'Reilly & Pfeffer, 2000; Ranft & Lord, 2000; Ranft & Lord, 2002). Our study complements this research by studying how the anticipated risk of employee departure from a firm following an acquisition *ex post* may affect acquirers' decision regarding whether to bid for the firm *ex ante*. Prior research has emphasized the importance of acquiring and developing human capital for a firm to gain a competitive advantage (Barney & Wright, 1998; Coff, 1997; Lado & Wilson, 1994). The results we presented demonstrate that acquirers are sensitive to employee mobility in their acquisition decisions and that "people-related problems" can present a challenge for firms to use acquisitions as a strategy to source human capital (Bruner, 2004; Coff, 1999a, 2002; O'Reilly & Pfeffer, 2000). While acquirers may not be able to influence states' policy of NCA enforcement, our research suggests that acquirers may rely on certain *ex-ante* institutional mechanisms to mitigate the costs of employee departure post-acquisition. Though acquirers can retain employees through others means during acquisition integration (Larsson & Finkelstein, 1999; Pablo, 1994; Ranft & Lord, 2000; Ranft & Lord, 2002), we are not able to examine integration directly in this study, given our focus on acquisition likelihood as our outcome of interest and our research design requiring inclusion of both acquisition events and "non-events" in the sample (Field & Karpoff, 2002; Ragozzino & Reuer, 2011; Schildt & Laamanen, 2006; Song & Walkling, 2000). We encourage future research to use other methods such as surveys or field experiments to examine, from the acquirers' viewpoint, how their acquisition decisions may be affected by their integration plans or the integrative mechanisms they will put in place to retain key employees and protect proprietary knowledge post-acquisition.

Second, our study contributes to a foundational theory in strategic management, strategic factor market theory, which assumes that firms formulate a strategy based on expectations about future returns from that strategy (Barney, 1986). This assumption, while straight forward, has not been the focus of much empirical research, perhaps because firms' expectations are largely unobserved. We model acquisition decisions as strategic choices based on variations in acquirers' *ex ante* expectations about the outcome of potential acquisitions. We note that this modeling approach departs from the majority of extant M&A research, which focuses on *realized* acquisition deals, in that we focus on the entire population of publicly-listed firms that could become a target for an acquisition bid (see Ambrose & Megginson, 1992; Field & Karpoff, 2002; Palepu, 1986; Song & Walkling, 1993, 2000). While our approach avoids problems of sample selection bias (Heckman, 1979) and allows us to examine target-side factors that shape acquirer-side expectations, it limits our ability to directly incorporate characteristics on the acquirer side. Extensions to our study could sample on realized acquisition deals before and after MARA to examine other important questions such as how the policy reversal may affect acquirers' bidding strategies and integration plans, and how acquisition performance may vary based on acquirers' capabilities to retain employees post-acquisition (Coff, 1999a; Cording *et al.*, 2008; Ellis *et al.*, 2011; Larsson & Finkelstein, 1999; O'Reilly & Pfeffer, 2000; Ranft & Lord, 2000; Ranft & Lord, 2002). Finally, while our focus on the policy change in Michigan and acquisitions of public-listed targets helps us better link to strategic factor market theory and provides an important advantage in identifying causal effects, we encourage future research to use other research designs and sample on private companies to improve the generalizability of our results.

Third, our study expands existing research on employee non-compete agreements. Prior research on NCAs has examined the relationship between NCA enforcement and the mobility of individual employees (Fallick *et al.*, 2006; Garmaise, 2011; Marx *et al.*, 2009) and has studied how this relationship may affect new venture founding rates and innovation rates at the regional level (Franco & Mitchell, 2008; Gilson, 1999; Samila & Sorenson, 2011; Saxenian, 1994; Stuart & Sorenson, 2003b). Our study departs from extant research by investigating how anticipated employee mobility, due to the reversal of a policy that governs NCA enforcement, affects the likelihood of firms becoming acquisition targets. This approach links together interorganizational employee mobility to firms' interorganizational strategic choices. Our findings are consistent with recent research on the relationship between individual-level employee mobility and firm-level strategies and outcomes (Agarwal *et al.*, 2004; Stuart & Sorenson, 2003b), and we contribute to that research by explicitly considering how individual mobility and considerations about inalienable human capital may shape firms' boundary decisions through acquisitions. Future research can extend our study's focus to examine how corporate development activities may affect employee mobility and the role NCAs may play in this process.

Fourth, this study advances the use of several new methodologies in strategic management research. We exploit a natural experiment and a difference-in-differences analysis to control for endogeneity, a frequent concern in strategy research; in doing so, we aim to establish *causal* evidence on the antecedents of strategic choices. We introduce the use of coarsened exact matching (Iacus *et al.*, 2009b) as a new procedure in strategy research to provide better counterfactual comparisons in studies using observational data. Finally, we apply simulation techniques developed for nonlinear models (King, Tomz, & Wittenberg, 2000; Zelner, 2009) to difference-in-differences analysis to better evaluate "triple difference" interaction effects, and our graphs aid the interpretation by showing the specific domain in which the interaction effects are statistically significant.

In conclusion, this study uses a natural experiment to demonstrate that anticipated employee departure from a firm causes a significant and economically important increase in the likelihood of the firm becoming an acquisition target. Our results further suggest that employee mobility is an important factor affecting acquirers' decision to use M&As as a strategy to source knowledge and human capital from target firms. As human capital grows in prominence in today's economy and firms rely more on M&As to source knowledge and talents, understanding the relationship between employee mobility and corporate acquisitions will likely take on greater importance.

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Table 1: Summary statistics and correlations (n=18,713).

	Summary Statistics	**Mean**	**Std. Dev.**	**Min**	**Max**
1	State GDP (log)	12.49	1.11	9.18	13.90
2	State Biz Combination Laws	0.49	0.50	0.00	1.00
3	State Establishment Entry	13.61	2.18	8.80	28.80
4	State Establishment Exit	11.39	1.69	7.80	25.90
5	Ind-State Tobin's q	1.76	0.92	0.00	12.60
6	Ind-State Herfindahl	0.55	0.33	0.00	3.81
7	Ind-State Acquisition Rate	0.07	0.09	0.00	1.00
8	Ind-State Acq. Rate Instate	0.01	0.03	0.00	0.50
9	Ind-State Delisting Rate	0.06	0.14	0.00	1.00
10	Ind-State Sales Growth	23.52	2126.37	-4.71	234313.50
11	Beale Urban Index	0.73	1.65	0.00	9.00
12	Assets (log)	4.50	2.18	0.00	10.71
13	Liquidity	0.01	11.65	-1293.00	16.24
14	ROA	-0.09	1.62	-247.08	86.32
15	Sales Growth	2.45	64.85	-3.73	9376.00
16	Patents	15.45	87.90	0.00	1525.00
17	Years Public	14.12	13.49	0.00	73.00
18	Reports Segments	0.95	0.21	0.00	1.00
19	Prior Bids (log)	0.20	0.42	0.00	2.48
20	Knowledge Workers (KW)	0.00	0.20	-0.34	0.61
21	In-state Competition (IC)	0.00	0.22	-0.18	0.82
22	IP Protection (IP)	0.00	0.34	-0.33	0.67
23	Michigan	0.10	0.30	0.00	1.00
24	After	0.60	0.49	0.00	1.00

	Correlations	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
1	State GDP (log)	**1.00**											
2	State Biz Combination Laws	**0.10**	**1.00**										
3	State Establishment Entry	**0.16**	**-0.35**	**1.00**									
4	State Establishment Exit	**0.31**	**-0.17**	**0.34**	**1.00**								
5	Ind-State Tobin's q	**0.23**	**0.07**	**0.11**	**0.12**	**1.00**							
6	Ind-State Herfindahl	**-0.44**	**0.04**	**-0.19**	**-0.23**	**-0.39**	**1.00**						
7	Ind-State Acquisition Rate	**0.15**	**0.15**	**-0.02**	0.01	**0.04**	**-0.06**	**1.00**					
8	Ind-State Acq. Rate Instate	**0.11**	**0.02**	**0.05**	**0.05**	-0.01	**-0.08**	**0.38**	**1.00**				
9	Ind-State Delisting Rate	0.01	**-0.06**	**0.04**	**0.04**	**-0.05**	**0.02**	**0.06**	**0.02**	**1.00**			
10	Ind-State Sales Growth	-0.01	0.01	0.00	-0.01	0.00	**0.01**	-0.01	0.00	0.00	**1.00**		
11	Beale Urban Index	**-0.43**	**-0.06**	**0.03**	**0.05**	**-0.12**	**0.16**	**-0.04**	**-0.02**	**0.02**	0.00	**1.00**	
12	Assets (log)	**0.04**	**0.16**	**-0.06**	**-0.03**	**-0.12**	**0.06**	**0.06**	**0.04**	**-0.05**	0.01	**-0.07**	**1.00**
13	Liquidity	0.01	-0.01	0.01	0.01	0.01	**-0.02**	0.01	0.00	0.00	0.00	0.00	**0.04**
14	ROA	**-0.02**	0.01	-0.01	-0.01	**-0.02**	**0.02**	0.01	0.00	-0.01	0.00	0.00	**0.08**
15	Sales Growth	0.00	0.01	0.00	0.01	0.00	0.00	0.00	0.00	0.01	0.01	0.00	**-0.02**
16	Patents	**0.03**	**0.09**	**-0.07**	**-0.05**	**0.12**	**0.02**	0.01	0.00	**-0.02**	0.00	**-0.05**	**0.34**
17	Years Public	**-0.02**	**0.15**	**-0.19**	**-0.14**	**-0.17**	**0.14**	**0.02**	**0.02**	**-0.04**	-0.01	**-0.02**	**0.54**
18	Reports Segments	0.01	**0.13**	0.01	-0.01	**0.06**	0.00	-0.01	0.00	**-0.24**	0.00	**-0.03**	**-0.02**
19	Prior Bids (log)	**0.03**	**-0.10**	**0.06**	**0.05**	**-0.07**	-0.01	**0.18**	**0.10**	**0.06**	0.00	**-0.03**	**0.14**
20	Knowledge Workers (KW)	**0.13**	**0.06**	**0.09**	**0.10**	**0.30**	**-0.29**	-0.01	0.00	**-0.08**	0.00	**-0.05**	**-0.06**
21	In-state Competition (IC)	**0.25**	**0.06**	**0.05**	**0.08**	**0.21**	**-0.19**	**0.06**	**0.04**	**-0.06**	0.00	**-0.16**	**0.23**
22	IP Protection (IP)	**0.10**	**0.06**	**-0.08**	**-0.06**	**0.29**	**-0.11**	**0.03**	-0.01	**-0.09**	0.00	**-0.12**	**-0.03**
23	Michigan	**-0.11**	**0.03**	**-0.27**	**-0.32**	**-0.19**	**0.16**	**-0.04**	**-0.03**	**-0.02**	0.00	**0.04**	0.01
24	After	**0.22**	**0.75**	**-0.38**	**-0.10**	**0.07**	**-0.02**	**0.20**	**0.04**	0.01	0.01	**-0.01**	**0.11**
	Correlations, continued	**(13)**	**(14)**	**(15)**	**(16)**	**(17)**	**(18)**	**(19)**	**(20)**	**(21)**	**(22)**	**(23)**	**(24)**
13	Liquidity	**1.00**											
14	ROA	**0.10**	**1.00**										
15	Sales Growth	0.00	-0.01	**1.00**									
16	Patent Applications	0.00	0.01	-0.01	**1.00**								
17	Years Public	0.00	**0.03**	**-0.02**	**0.29**	**1.00**							
18	Reports Segments	0.00	0.00	0.00	**0.04**	0.00	**1.00**						
19	Prior Bids (log)	0.00	**0.02**	0.00	0.01	**0.13**	**-0.02**	**1.00**					
20	Knowledge Workers (KW)	0.01	0.00	**0.02**	0.01	**-0.14**	**0.19**	**-0.02**	**1.00**				
21	In-state Competition (IC)	0.01	0.01	-0.01	**0.26**	**0.09**	**0.11**	**0.05**	**0.06**	**1.00**			
22	IP Protection (IP)	**0.02**	0.00	-0.01	**0.17**	**0.04**	**0.18**	**-0.03**	**-0.18**	**0.18**	**1.00**		
23	Michigan	0.00	0.01	-0.01	**-0.02**	**0.07**	**-0.02**	**-0.05**	**-0.11**	**-0.03**	**0.05**	**1.00**	
24	After	**-0.01**	0.00	0.00	**0.06**	**0.15**	**-0.04**	**-0.16**	-0.01	0.01	**0.02**	0.00	**1.00**

Notes: Values in bold are statistically significant at the p<.05 level.

Table 2: Rates of acquisitions of firms in Michigan and comparison states.

Panel A: 1987-1988

	+/– 1 year window		
	Before	After	*Diff.*
Michigan	6.77	19.69	12.92
Comparison	9.00	10.70	1.70
Difference	-2.23	8.99	**11.22**

Panel B: 1983-1992

+/– 5 year window		
Before	After	*Diff.*
5.61	10.85	5.25
6.83	9.22	2.39
-1.22	1.64	**2.86**

Panel C: 1982-1998

All data		
Before	After	*Diff.*
5.18	8.87	3.69
6.29	9.35	3.07
-1.10	-0.48	**0.62**

Notes: All values are in percent and are calculated from the *matched* population of firms listed in 1987 or earlier. The "difference-in-differences" value appears in the lower-right cell of each panel.

Table 3: Logit models of the likelihood of being an acquisition target, by time window.

	(1) 1987-88	(2) 1986-89	(3) 1985-90	(4) 1984-91	(5) 1983-92	(6) 1982-93	(7) 1982-98
Ind. Auto	1.8467**	1.0887*	0.8806*	0.7255*	0.4866	0.4617	0.2111
	(0.573)	(0.434)	(0.371)	(0.349)	(0.327)	(0.305)	(0.275)
Ind. Drugs	0.8475	0.6505+	0.3429	0.2260	0.1316	0.2915	0.1788
	(0.565)	(0.341)	(0.285)	(0.232)	(0.218)	(0.235)	(0.197)
Ind. Chemicals	0.7223+	0.3398	0.2195	0.1434	0.0393	0.0198	0.0501
	(0.417)	(0.357)	(0.278)	(0.217)	(0.199)	(0.195)	(0.189)
Ind. Computers & Comm.	-0.0656	0.2420	0.3294	0.3589+	0.3593*	0.3666*	0.4533**
	(0.381)	(0.244)	(0.225)	(0.194)	(0.177)	(0.167)	(0.144)
Ind. Electrical	0.3327	0.1556	0.2065	0.1537	0.1578	0.1762	0.1502
	(0.335)	(0.234)	(0.193)	(0.169)	(0.153)	(0.144)	(0.138)
Ind. Wholesale	-0.2757	0.1340	0.1481	0.1411	0.0969	0.0632	0.1480
	(0.392)	(0.302)	(0.243)	(0.231)	(0.215)	(0.192)	(0.164)
Ind. Retail	-0.1507	0.0905	0.0739	0.1364	0.1486	0.1038	0.0469
	(0.304)	(0.223)	(0.194)	(0.176)	(0.164)	(0.161)	(0.157)
State GDP (log)	0.2191	0.1948*	0.0389	0.0578	0.0464	0.0433	0.0313
	(0.145)	(0.097)	(0.082)	(0.076)	(0.071)	(0.065)	(0.053)
State Business Combination Laws	-0.2254	-0.3712*	-0.2486	-0.3412*	-0.3291*	-0.3964**	-0.5020***
	(0.249)	(0.182)	(0.159)	(0.146)	(0.139)	(0.136)	(0.118)
State Establishment Entry	-0.0811	-0.2152**	-0.0185	-0.0288	-0.0199	-0.0151	0.0081
	(0.101)	(0.071)	(0.040)	(0.038)	(0.036)	(0.034)	(0.021)
State Establishment Exit	-0.0245	0.0412	0.0110	0.0053	0.0074	0.0121	0.0138
	(0.144)	(0.039)	(0.040)	(0.040)	(0.037)	(0.036)	(0.028)
Ind-State Tobin's q	0.1426	0.1941*	0.2437***	0.2670***	0.2131**	0.2072***	0.2475***
	(0.096)	(0.079)	(0.074)	(0.070)	(0.065)	(0.060)	(0.052)
Ind-State Herfindahl	-0.5367	-0.5262*	-0.4959*	-0.2657	-0.3480+	-0.3836*	-0.3929*
	(0.358)	(0.259)	(0.226)	(0.206)	(0.188)	(0.177)	(0.167)
Ind-State Acquisition Rate	-3.9226+	-0.8580	1.6881	2.1544*	2.2106*	2.6390**	4.7407***
	(2.272)	(1.336)	(1.113)	(1.047)	(0.976)	(0.964)	(0.800)
Ind-State Acq. Rate Squared	4.5929	-1.3189	-5.0767**	-5.9696**	-5.4482**	-5.5206**	-8.1051***
	(4.319)	(2.116)	(1.907)	(1.981)	(1.815)	(1.820)	(1.712)
Ind-State Acq. Rate Instate	-4.5221	-2.8843	-2.5852	-1.0976	-1.1459	-1.3888	-1.2310
	(3.905)	(2.172)	(1.765)	(1.453)	(1.294)	(1.213)	(1.008)
Ind-State Delisting Rate	2.4229***	2.1463***	2.0944***	2.1966***	2.1634***	2.0767***	2.2618***
	(0.382)	(0.276)	(0.242)	(0.219)	(0.207)	(0.196)	(0.172)
Ind-State Sales Growth	-0.0169	-0.0003	-0.0003	-0.0003	-0.0002	-0.0002	-0.0002
	(0.042)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Beale Urban Index	0.0848	0.0075	-0.0217	-0.0185	-0.0191	-0.0047	-0.0029
	(0.087)	(0.051)	(0.043)	(0.039)	(0.036)	(0.033)	(0.028)
Assets (log)	0.0805	0.0965*	0.1050**	0.1012**	0.0909**	0.0782**	0.0632**
	(0.054)	(0.040)	(0.036)	(0.034)	(0.031)	(0.028)	(0.023)
Liquidity	0.7245**	0.1793+	0.2128*	0.0800	0.0889	0.1167*	0.1540**
	(0.244)	(0.109)	(0.101)	(0.083)	(0.065)	(0.059)	(0.059)
ROA (above ind. avg.)	0.6071+	0.5497*	0.2938*	0.1299	0.0024	0.0005	0.0017
	(0.319)	(0.235)	(0.141)	(0.108)	(0.008)	(0.006)	(0.007)
Sales Growth	-0.0016	0.0005	0.0006	0.0008	0.0007	0.0007	0.0000
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Patents	-0.0099	-0.0072+	-0.0016	-0.0027	-0.0021	-0.0022	-0.0011
	(0.009)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)
Years Public	-0.0341**	-0.0238**	-0.0184**	-0.0172***	-0.0172***	-0.0165***	-0.0193***
	(0.012)	(0.009)	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)
Reports Segments	-2.3416***	-2.2484***	-2.3376***	-2.2972***	-2.1984***	-2.1434***	-2.1392***
	(0.258)	(0.212)	(0.193)	(0.181)	(0.173)	(0.168)	(0.165)
Prior Bids (log)	3.0159***	2.4711***	2.1897***	2.1351***	2.0532***	2.0083***	1.6789***
	(0.231)	(0.146)	(0.127)	(0.114)	(0.103)	(0.094)	(0.080)
Knowledge Workers (KW)	-1.5706**	-1.7175***	-1.5390***	-1.3002***	-1.2521***	-1.2144***	-1.3713***
	(0.531)	(0.381)	(0.340)	(0.304)	(0.289)	(0.273)	(0.255)
In-state Competition (IC)	-1.3148**	-1.3140***	-1.3102***	-0.9558***	-0.8080***	-0.7875***	-0.6975**
	(0.467)	(0.305)	(0.290)	(0.234)	(0.221)	(0.219)	(0.234)
IP Protection (IP)	-0.6283+	-0.3908+	-0.4653*	-0.3487+	-0.2739	-0.3449*	-0.7032***
	(0.326)	(0.233)	(0.212)	(0.191)	(0.180)	(0.171)	(0.157)
Michigan	-0.5554	-0.5134	-0.3408	-0.2799	-0.3110	-0.2418	-0.1044
	(0.558)	(0.335)	(0.262)	(0.207)	(0.210)	(0.199)	(0.185)
After	0.2595	0.8274**	1.1998***	0.4813+	1.0367***	1.2035**	0.9227***
	(0.316)	(0.289)	(0.261)	(0.265)	(0.298)	(0.376)	(0.125)
Michigan * After	**1.4993****	**1.0742****	**0.9084****	**0.8278****	**0.8105****	**0.6753***	**0.4528+**
	(0.573)	(0.383)	(0.325)	(0.303)	(0.302)	(0.287)	(0.246)
Constant	-2.5997	-1.7856	-2.8188**	-2.6829**	-3.1068***	-3.5757***	-2.5997
	(1.777)	(1.186)	(0.993)	(0.875)	(0.771)	(0.762)	(1.777)
Observations	2,833	5,483	7,966	10,332	12,553	14,566	2,833
Log likelihood	-570.62	-1172.88	-1691.51	-2182.59	-2604.90	-2897.03	-570.62

***Notes**:* Standard errors clustered by firm and reported in parentheses. All models include year indicators; two-tailed tests: *** p<0.001, ** p<0.01, * p<0.05, + p<0.10.

Table 4: Moderated logit models of the likelihood of being an acquisition target.

	(1)	(2)	(3)	(4) Full Model
Ind. Auto	0.2111	0.1703	0.1687	0.1603
	(0.275)	(0.285)	(0.297)	(0.294)
Ind. Drugs	0.1788	0.1960	0.1761	0.1872
	(0.197)	(0.199)	(0.199)	(0.199)
Ind. Chemicals	0.0501	0.0299	0.0329	0.0473
	(0.189)	(0.186)	(0.189)	(0.190)
Ind. Computers & Comm.	0.4533**	0.4634**	0.4645**	0.4703**
	(0.144)	(0.146)	(0.146)	(0.147)
Ind. Electrical	0.1502	0.1487	0.1543	0.1619
	(0.138)	(0.140)	(0.138)	(0.139)
Ind. Wholesale	0.1480	0.1429	0.1544	0.1492
	(0.164)	(0.166)	(0.162)	(0.162)
Ind. Retail	0.0469	0.0616	0.0796	0.0734
	(0.157)	(0.160)	(0.162)	(0.162)
State GDP (log)	0.0313	0.0614	0.0638	0.0653
	(0.053)	(0.061)	(0.061)	(0.061)
State Business Combination Laws	-0.5020***	-0.5420***	-0.5198***	-0.5127***
	(0.118)	(0.117)	(0.117)	(0.118)
State Establishment Entry	0.0081	-0.0237	-0.0263	-0.0265
	(0.021)	(0.029)	(0.029)	(0.029)
State Establishment Exit	0.0138	-0.0259	-0.0226	-0.0235
	(0.028)	(0.037)	(0.036)	(0.037)
Ind-State Tobin's q	0.2475***	0.2519***	0.2533***	0.2522***
	(0.052)	(0.054)	(0.054)	(0.054)
Ind-State Herfindahl	-0.3929*	-0.4091*	-0.4213*	-0.4253*
	(0.167)	(0.170)	(0.170)	(0.170)
Ind-State Acquisition Rate	4.7407***	4.1457***	4.2177***	4.1739***
	(0.800)	(0.801)	(0.798)	(0.806)
Ind-State Acq. Rate Squared	-8.1051***	-7.2458***	-7.4213***	-7.3449***
	(1.712)	(1.687)	(1.687)	(1.709)
Ind-State Acq. Rate Instate	-1.2310	-1.3674	-1.3319	-1.3708
	(1.008)	(1.048)	(1.046)	(1.046)
Ind-State Delisting Rate	2.2618***	2.1871***	2.2122***	2.2095***
	(0.172)	(0.172)	(0.171)	(0.172)
Ind-State Sales Growth	-0.0002	-0.0003	-0.0003	-0.0003
	(0.000)	(0.001)	(0.001)	(0.001)
Beale Urban Index	-0.0029	0.0128	0.0132	0.0140
	(0.028)	(0.029)	(0.029)	(0.029)
Assets (log)	0.0632**	0.0699**	0.0711**	0.0719**
	(0.023)	(0.024)	(0.024)	(0.024)
Liquidity	0.1540**	0.1534*	0.1532*	0.1518*
	(0.059)	(0.061)	(0.060)	(0.059)
ROA	0.0017	0.0026	0.0023	0.0019
	(0.007)	(0.008)	(0.007)	(0.007)
Sales Growth	0.0000	0.0000	0.0000	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)
Patents	-0.0011	-0.0011	-0.0010	-0.0010
	(0.001)	(0.001)	(0.001)	(0.001)
Years Public	-0.0193***	-0.0194***	-0.0195***	-0.0195***
	(0.004)	(0.004)	(0.004)	(0.004)
Reports Segments	-2.1392***	-2.1471***	-2.1463***	-2.1627***
	(0.165)	(0.167)	(0.167)	(0.169)
Prior Bids (log)	1.6789***	1.7232***	1.7333***	1.7331***
	(0.080)	(0.086)	(0.087)	(0.088)

Results continue on next page.

Table 4, continued: Moderated logit models of the likelihood of being an acquisition target.

	(1)	(2)	(3)	(4) Full Model
	Results continued from previous page.			
Knowledge Workers (KW)	-1.3713***	-1.6298***	-1.7488***	-1.7507***
	(0.255)	(0.301)	(0.302)	(0.302)
In-state Competition (IC)	-0.6975**	-0.7231**	-0.0375	-0.0216
	(0.234)	(0.240)	(0.233)	(0.236)
IP Protection (IP)	-0.7032***	-0.7200***	-0.7088***	-0.7697***
	(0.157)	(0.158)	(0.159)	(0.204)
Michigan	-0.1044	-1.0214**	-1.2382***	-1.4453***
	(0.185)	(0.333)	(0.352)	(0.376)
After	0.9227***	1.4959***	1.4567***	1.4547***
	(0.125)	(0.312)	(0.311)	(0.311)
Michigan * After	**0.4528+**	**1.2919*** **	**1.5611*** **	**1.7662*** **
	(0.246)	(0.366)	(0.387)	(0.409)
Michigan * KW		-5.3385***	-5.7644***	-5.2838***
		(1.384)	(1.296)	(1.415)
After * KW		0.4474	0.6435	0.6513
		(0.408)	(0.418)	(0.419)
Michigan * After * KW		**5.6845*** **	**6.1893*** **	**5.6104*** **
		(1.615)	(1.569)	(1.664)
Michigan * IC			-2.8237*	-3.5472*
			(1.325)	(1.411)
After * IC			-1.1443**	-1.1619**
			(0.433)	(0.448)
Michigan * After * IC			**3.8674***	**4.6799***
			(1.545)	(1.645)
Michigan * IP				1.8504***
				(0.505)
After * IP				0.0602
				(0.264)
Michigan * After * IP				**-2.0622***
				(0.668)
Constant	-2.8476***	-3.0032***	-3.0313***	-3.0249***
	(0.681)	(0.718)	(0.723)	(0.726)
Observations	18,713	18,713	18,713	18,713
Log likelihood	-3992.61	-3954.88	-3947.00	-3943.99

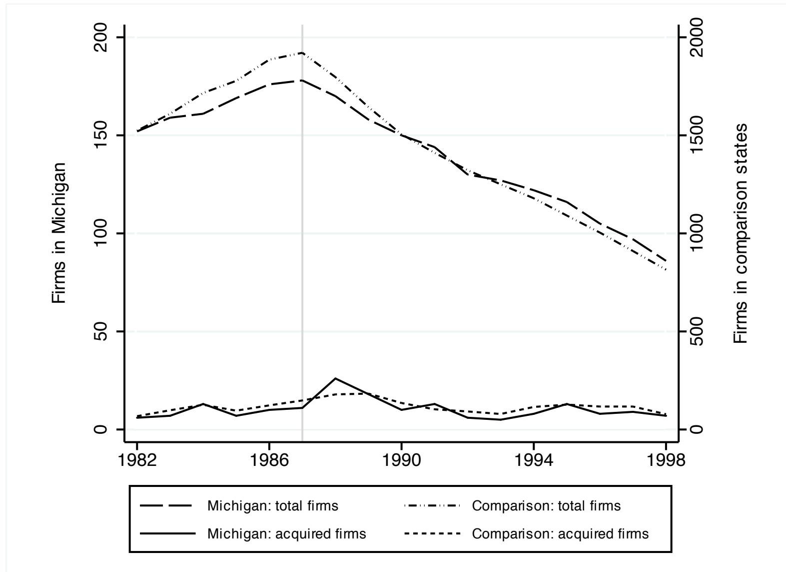
Notes*:* Column 4 is the Full Model. Standard errors are clustered by firm and reported in parentheses. All models include year indicators; two-tailed tests: *** p<0.001, ** p<0.01, * p<0.05, + p<0.10.

Table 5: Robustness checks of the Full Model.

	(1) Midwest States	(2) Industry Fixed Effects	(3) BIC Controls	(4) *After* 1986/1987	(5) No Matching	(6) New Firms	(7) Bootstrap Cluster SE
Knowledge Workers (KW)	-2.3835***	-2.5656***	-1.6539***	-1.9141***	-1.9428***	-1.8495***	-1.9465***
	(0.523)	(0.493)	(0.297)	(0.417)	(0.285)	(0.279)	(0.433)
In-state Competition (IC)	-0.0785	-0.4492	-0.0231	-0.0953	-0.1173	-0.1259	-0.1345
	(0.277)	(0.307)	(0.229)	(0.268)	(0.227)	(0.232)	(0.394)
IP Protection (IP)	-0.2324	-1.1063***	-0.6371***	-0.8438**	-0.6887***	-0.7537***	-0.6939**
	(0.217)	(0.325)	(0.177)	(0.266)	(0.176)	(0.187)	(0.263)
Michigan	-0.7883**	-1.3042***	-1.3793***	-1.5608***	-1.3644***	-1.5751***	-1.2847***
	(0.256)	(0.312)	(0.379)	(0.329)	(0.360)	(0.385)	(0.236)
After	1.7799***	1.6049***	1.5790***	1.3920***	1.5548***	1.3636***	0.9427***
	(0.382)	(0.342)	(0.293)	(0.336)	(0.261)	(0.299)	(0.255)
Michigan * After	**1.2058*** **	**1.6654*** **	**1.8027*** **	**1.8704*** **	**1.7588*** **	**1.6229*** **	**1.7816*** **
	(0.294)	(0.354)	(0.413)	(0.358)	(0.395)	(0.405)	(0.119)
Michigan * KW	-5.2022***	-3.9974***	-5.3707***	-5.8974***	-5.2718***	-5.4353***	-5.1589***
	(1.202)	(1.184)	(1.451)	(1.337)	(1.360)	(1.377)	(0.432)
After * KW	0.8585	1.0267*	0.6190	0.5791	0.4458	0.6568*	0.4698
	(0.617)	(0.414)	(0.423)	(0.474)	(0.374)	(0.333)	(0.447)
Michigan * After * KW	**6.0245*** **	**4.1642** **	**5.6405*** **	**6.1031*** **	**5.9982*** **	**5.6882*** **	**5.8515*** **
	(1.488)	(1.449)	(1.698)	(1.623)	(1.618)	(1.551)	(0.455)
Michigan * IC	-3.1998*	-3.0171*	-3.4636**	-2.7257+	-3.5228**	-3.7856*	-3.6122***
	(1.286)	(1.496)	(1.340)	(1.397)	(1.364)	(1.598)	(0.411)
After * IC	-0.7807	-1.1369**	-1.1887**	-1.0752*	-1.0993*	-0.8655*	-1.0536
	(0.491)	(0.416)	(0.455)	(0.433)	(0.427)	(0.357)	(0.868)
Michigan * After * IC	**4.0291** **	**4.4765** **	**4.6352** **	**3.5699* **	**4.6734** **	**4.5295* **	**4.7417*** **
	(1.526)	(1.707)	(1.590)	(1.631)	(1.607)	(1.760)	(0.811)
Michigan * IP	0.8004+	1.5126*	1.7893***	2.1578***	1.8122***	1.7407***	1.7796***
	(0.471)	(0.597)	(0.500)	(0.637)	(0.486)	(0.509)	(0.286)
After * IP	-0.1776	0.0608	0.0339	0.0148	-0.1093	0.1618	-0.1114
	(0.285)	(0.282)	(0.265)	(0.310)	(0.232)	(0.217)	(0.280)
Michigan * After * IP	**-1.7293** **	**-2.2411** **	**-2.0040** **	**-2.1296** **	**-1.9190** **	**-2.2213*** **	**-1.9297*** **
	(0.663)	(0.773)	(0.658)	(0.790)	(0.654)	(0.619)	(0.259)
Constant	-1.4737	-1.4926	-2.7879***	-3.1538***	-3.1154***	-3.0746***	-2.9288
	(1.986)	(1.140)	(0.367)	(0.706)	(0.674)	(0.620)	(2.208)
Observations	13,618	18,116	18,713	18,791	19,020	24,865	18,713
Log likelihood	-2493.56	-3723.89	-3958.82	-3954.75	-3983.65	-6584.24	-3939.70

Notes: All models estimate robust standard errors, clustered by firm and presented in parentheses, except Column 7, which bootstraps and clusters robust standard errors by state. All models include year indicators and use a CEM matched sample, except Column 5, which uses all available observations. All models include the complete set of control variables reported in Table 3 and 4, except Column 3, which uses backwards elimination (Lindsey & Sheather, 2010) to remove controls that reduce an optimal Bayesian Information Criterion. Column 1 replaces the comparison group with firms headquartered in Midwest states near Michigan (Ohio, Indiana, Illinois, Wisconsin, and Pennsylvania). Column 2 drops the seven broad industry indicators and instead includes a complete set of indicators at the three-digit SIC level. Column 3 includes the following controls in the model: *Ind. Computers & Communication*, *Business Combination Laws*, *Ind-State Tobin's q*, *Ind-State Herfindahl, Ind-State Acquisition Rate, Ind-State Acquisition Rate Squared, Ind-State Delisting Rate, Assets (log), Years Public, Reports Segments, Prior Bids (log)*. Column 4 moves the *After* year forward a year to split between 1986 and 1987. Column 5 expands the sample to include all available observations, including those dropped by our matching procedure. Column 6 includes new firms listed after 1987. Column 7 bootstraps and clusters standard errors at the state level. *KW=Knowledge Workers*; *IC=In-state Competition*; *IP=IP Protection*. Two-tailed tests: *** p<0.001, ** p<0.01, * p<0.05, + p<0.10.

Panel A:
Base population and number of acquisitions in Michigan and comparison states

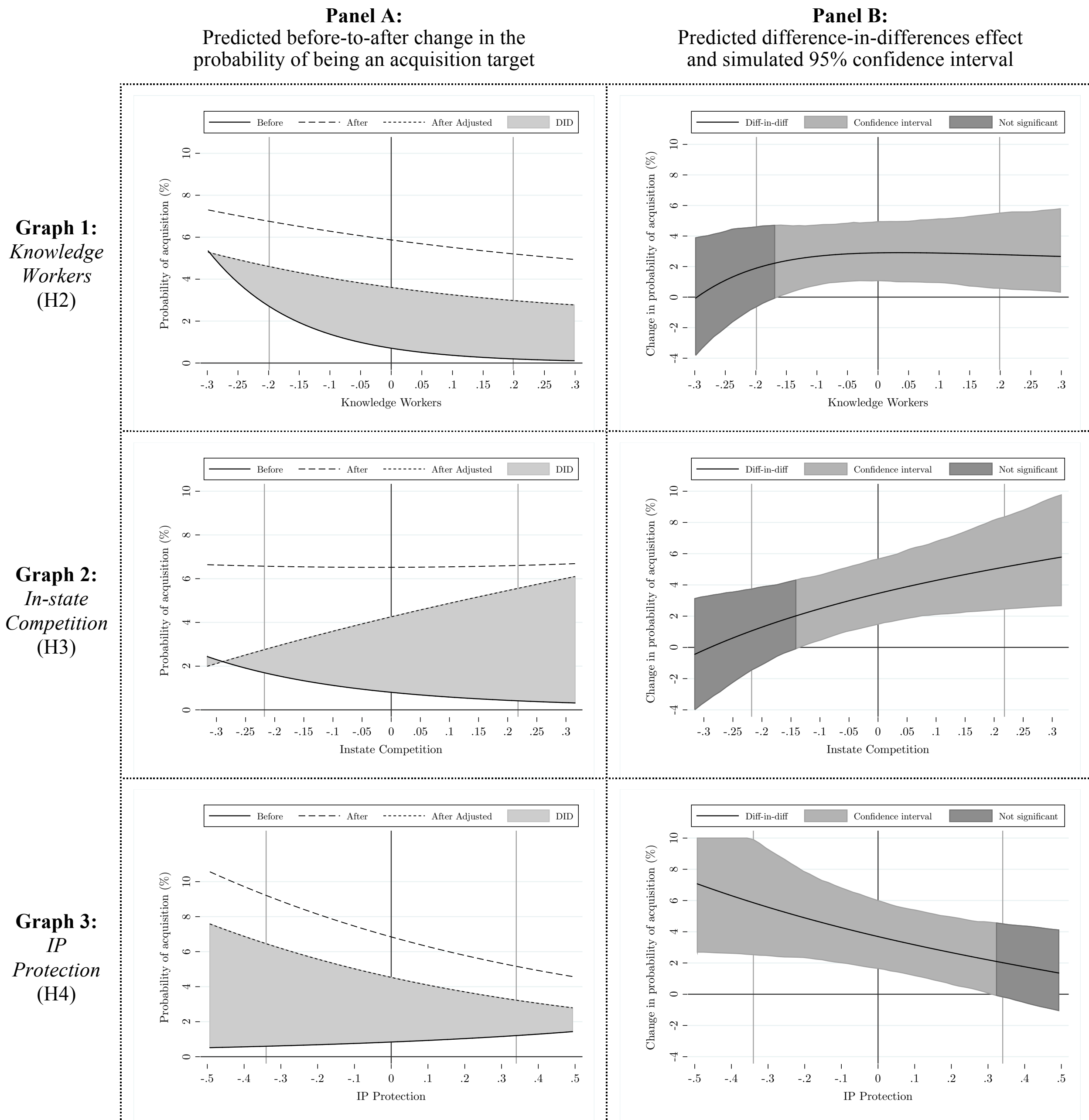


Notes**:** We restricted our base population to firms that were publically listed prior to MARA. This population declined after 1987 in both groups (Michigan and comparison states) as firms were acquired or delisted due to firm failure. The vertical line in the figure denotes 1987; the top two lines represent the total count of firms by group; and the bottom two lines represent the count of acquisition events by group.

Panel B:
Rate of acquisition in Michigan and comparison states

Notes**:** The rate of acquisition is calculated as the number of acquisitions expressed as a percentage of the number of firms in either Michigan or the group of comparison states.

Figure 1: Descriptive trends in Michigan and comparison states



Notes: Figure 2 shows the predicted effect of an increase in the enforcement of non-compete agreements on the likelihood of firms becoming a target of acquisition, by level of *Knowledge Workers* (H2), *In-state Competition* (H3), and *IP Protection* (H4) in target firms. Vertical lines appear at zero and +/–1 SD around the mean of each moderator (interactions are mean-centered). Panel A plots the naïve effect of MARA as the difference between the 'Before' line and the 'After' line; a difference-in-differences analysis, however, removes the effect of coinciding changes observed in comparison states by subtracting counterfactual changes from the 'After' line and arriving at the 'After Adjusted" line, and the magnitude of the difference-in-differences effect is therefore represented by the shaded region between the 'Before' line and the 'After Adjusted' line. Panel B plots the magnitude of the predicted difference-in-differences increase in the probability of being a target (i.e., the shaded region of Panel A) as a solid black line in Panel B, and simulates a 95% confidence interval around the predicted difference-in-differences effect using simulation techniques described in Appendix B. The non-significant range of the difference-in-differences effect is plotted in a darker shade where the confidence interval falls below zero. As predicted in H2, Panel B-Graph 1 indicates that the effect of MARA is stronger for firms that employ a greater proportion of knowledge workers. As predicted in H3, Panel B-Graph 2 indicates that the effect of MARA is stronger for firms that face greater in-state competition. As predicted in H4, Panel B-Graph 3 indicates that the effect of MARA is weaker for firms that are protected by a stronger IP regime.

Figure 2: The predicted effect of MARA by level of moderating variable

APPENDIX A: PLACEBO TESTS

	(1) OH	(2) IL	(3) IN	(4) PA	(5) WI	(6) CT	(7) FL	(8) NY	(9) OR	(10) TFEAA
Ind. Auto	-0.0257	0.0010	0.3381	0.3708	0.3476	0.2474	0.3521	0.0999	0.2631	0.0247
	(0.312)	(0.303)	(0.368)	(0.277)	(0.238)	(0.306)	(0.299)	(0.249)	(0.308)	(0.317)
Ind. Drugs	0.1130	0.1727	0.1342	0.2401	0.0934	0.1281	0.4525*	0.1318	0.1508	0.0540
	(0.195)	(0.182)	(0.199)	(0.193)	(0.202)	(0.206)	(0.193)	(0.180)	(0.204)	(0.171)
Ind. Chemicals	0.1218	0.2408	0.0604	0.1292	0.0669	0.0718	0.1427	-0.0121	0.1064	-0.0661
	(0.199)	(0.232)	(0.194)	(0.181)	(0.196)	(0.198)	(0.200)	(0.160)	(0.194)	(0.148)
Ind. Comp. & Comm.	0.6020***	0.5220***	0.5142***	0.4341***	0.5386***	0.5111***	0.6583***	0.3614**	0.4985***	0.4308***
	(0.129)	(0.128)	(0.134)	(0.128)	(0.135)	(0.138)	(0.137)	(0.126)	(0.134)	(0.126)
Ind. Electrical	0.1747	0.1576	0.1237	0.1522	0.1366	0.1217	0.1250	0.0514	0.1336	0.0975
	(0.119)	(0.132)	(0.125)	(0.121)	(0.124)	(0.131)	(0.123)	(0.115)	(0.128)	(0.123)
Ind. Wholesale	0.1786	0.0972	0.1577	0.1838	0.2072	0.1725	0.0864	-0.0502	0.1363	0.0604
	(0.156)	(0.159)	(0.172)	(0.167)	(0.181)	(0.185)	(0.156)	(0.149)	(0.182)	(0.145)
Ind. Retail	0.1882	0.0205	0.0329	-0.0256	-0.0039	-0.0255	0.0683	0.0306	0.0327	-0.0531
	(0.146)	(0.154)	(0.163)	(0.155)	(0.167)	(0.169)	(0.143)	(0.132)	(0.163)	(0.135)
State GDP (log)	0.0309	0.0327	0.0279	0.0224	0.0204	0.0230	0.0323	0.0648	0.0203	0.0479
	(0.053)	(0.053)	(0.053)	(0.052)	(0.053)	(0.055)	(0.051)	(0.052)	(0.053)	(0.047)
State Biz Comb Laws	-0.5955***	-0.5233***	-0.5204***	-0.5361***	-0.5342***	-0.4764***	-0.4178***	-0.5071***	-0.4722***	-0.3378***
	(0.103)	(0.104)	(0.106)	(0.105)	(0.108)	(0.112)	(0.099)	(0.098)	(0.111)	(0.096)
State Estab. Entry	-0.0167	-0.0217	-0.0245	-0.0265	-0.0201	-0.0262	-0.0138	-0.0229	-0.0226	-0.0145
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.026)	(0.023)	(0.024)	(0.025)	(0.022)
State Estab. Exit	-0.0265	-0.0124	-0.0116	-0.0033	-0.0120	-0.0101	-0.0100	-0.0195	-0.0097	-0.0129
	(0.030)	(0.029)	(0.030)	(0.028)	(0.030)	(0.031)	(0.028)	(0.029)	(0.029)	(0.025)
Ind-State Tobin's q	0.3220***	0.2797***	0.2742***	0.2942***	0.2894***	0.2770***	0.1792**	0.2486***	0.2744***	0.2948***
	(0.048)	(0.048)	(0.050)	(0.048)	(0.050)	(0.052)	(0.060)	(0.046)	(0.050)	(0.046)
Ind-State Herfindahl	-0.4264**	-0.3515*	-0.4721**	-0.3371*	-0.4711**	-0.5846***	-0.3692*	-0.2656*	-0.5412**	-0.4147**
	(0.151)	(0.150)	(0.160)	(0.149)	(0.158)	(0.168)	(0.146)	(0.133)	(0.165)	(0.136)
Ind-State AcqRate (AR)	3.6971***	4.4979***	3.8736***	4.7795***	4.1623***	4.2213***	4.9173***	3.9645***	4.3016***	3.7232***
	(0.692)	(0.730)	(0.747)	(0.754)	(0.776)	(0.813)	(0.743)	(0.670)	(0.801)	(0.652)
Ind-State AR Squared	-5.2896***	-7.9574***	-5.9766***	-7.8271***	-7.0284***	-7.0716***	-8.0050***	-6.0142***	-7.5345***	-5.6217***
	(1.410)	(1.637)	(1.633)	(1.677)	(1.652)	(1.800)	(1.749)	(1.489)	(1.821)	(1.422)
Ind-State AR Instate	-2.0469*	-1.7161	-1.9721*	-2.2246*	-1.7396	-1.8495	-2.2749*	-1.7815*	-1.6426	-2.0931*
	(0.916)	(0.980)	(0.980)	(0.978)	(1.008)	(1.032)	(0.930)	(0.825)	(1.026)	(0.946)
Ind-State Delist Rate	2.3404***	2.1066***	2.2346***	2.1603***	2.2257***	2.1962***	2.1108***	2.1510***	2.2041***	2.0818***
	(0.148)	(0.145)	(0.150)	(0.146)	(0.150)	(0.158)	(0.139)	(0.140)	(0.153)	(0.147)
Ind-State Sales Growth	-0.0002	-0.0002	-0.0002	-0.0002	-0.0002	-0.0002	-0.0000	-0.0002	-0.0002	-0.0009
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Beale Urban Index	0.0039	0.0004	0.0054	-0.0150	-0.0035	0.0045	0.0019	0.0201	0.0046	-0.0040
	(0.025)	(0.025)	(0.026)	(0.025)	(0.026)	(0.028)	(0.025)	(0.024)	(0.026)	(0.016)
Assets (log)	0.0829***	0.0815***	0.1008***	0.0982***	0.1013***	0.1115***	0.0942***	0.1249***	0.1052***	0.0939***
	(0.021)	(0.021)	(0.022)	(0.021)	(0.022)	(0.023)	(0.022)	(0.019)	(0.023)	(0.019)
Liquidity	0.1009	0.0878	0.0936	0.0904	0.0930	0.0823	0.0871	0.0804	0.0856	0.0947
	(0.058)	(0.055)	(0.059)	(0.058)	(0.060)	(0.057)	(0.054)	(0.048)	(0.057)	(0.052)
ROA	0.0052	0.0086	0.0060	0.0033	0.0051	0.0050	0.0144	-0.0012	0.0049	0.0084
	(0.007)	(0.010)	(0.008)	(0.006)	(0.007)	(0.007)	(0.012)	(0.003)	(0.007)	(0.008)
Sales Growth	0.0000	-0.0001	0.0000	0.0000	0.0000	0.0000	-0.0001	-0.0001	0.0000	-0.0001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Patent Stock	-0.0004	0.0001	-0.0007	-0.0004	-0.0007	-0.0007	-0.0007	-0.0002	-0.0006	-0.0004
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Years Public	-0.0126**	-0.0110**	-0.0165***	-0.0157***	-0.0199***	-0.0191***	-0.0185***	-0.0181***	-0.0191***	-0.0216***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.005)	(0.004)
Reports Segments	-1.9055***	-2.0525***	-1.9866***	-2.0093***	-2.0556***	-2.1260***	-1.9965***	-2.3048***	-2.1082***	-2.1184***
	(0.145)	(0.145)	(0.155)	(0.150)	(0.155)	(0.169)	(0.144)	(0.153)	(0.167)	(0.139)
Prior Bids (log)	1.8060***	1.7669***	1.7514***	1.7783***	1.8193***	1.7766***	1.7767***	1.7681***	1.7803***	1.8246***
	(0.073)	(0.075)	(0.079)	(0.076)	(0.081)	(0.082)	(0.077)	(0.070)	(0.080)	(0.067)
Knowledge Workers	-1.8229***	-1.6157***	-1.6264***	-1.5294***	-1.7139***	-1.6306***	-1.4917***	-1.3607***	-1.5866***	-1.8578***
	(0.223)	(0.216)	(0.233)	(0.214)	(0.232)	(0.237)	(0.215)	(0.197)	(0.231)	(0.195)
In-state Competition	-0.7127**	-0.8238***	-0.6756**	-0.8460***	-0.8270**	-0.8355**	-0.7909***	-0.6166**	-0.8080**	-0.8681***
	(0.219)	(0.224)	(0.253)	(0.242)	(0.254)	(0.264)	(0.235)	(0.194)	(0.259)	(0.188)
IP Protection	-0.7275***	-0.6910***	-0.7132***	-0.7512***	-0.6907***	-0.7599***	-0.6865***	-0.6369***	-0.7802***	-0.7535***
	(0.135)	(0.131)	(0.141)	(0.137)	(0.141)	(0.146)	(0.137)	(0.126)	(0.144)	(0.125)
Placebo State	-0.2611	-0.0365	0.0834	-0.2825	-0.1522	0.0352	0.0730	0.0141	0.3063	0.1538
	(0.150)	(0.142)	(0.214)	(0.166)	(0.230)	(0.173)	(0.144)	(0.113)	(0.274)	(0.161)
After	1.8509***	1.5087***	1.5663***	1.6522***	1.7190***	1.6370***	1.5967***	1.4683***	1.5995***	1.5811***
	(0.262)	(0.243)	(0.257)	(0.254)	(0.265)	(0.275)	(0.247)	(0.219)	(0.266)	(0.230)
Placebo State * After	**0.0508**	**-0.0517**	**-0.3119**	**0.1878**	**-0.1861**	**-0.0308**	**-0.0077**	**0.0615**	**-0.6583**	**-0.1528**
	(0.185)	(0.175)	(0.313)	(0.196)	(0.242)	(0.236)	(0.182)	(0.136)	(0.427)	(0.184)
Constant	-3.4564***	-3.1514***	-3.0675***	-3.2463***	-3.1187***	-2.9801***	-3.2399***	-3.2372***	-2.9439***	-3.4193***
	(0.658)	(0.651)	(0.674)	(0.657)	(0.677)	(0.750)	(0.656)	(0.644)	(0.687)	(0.624)
Observations	20,509	20,669	18,314	20,640	18,510	17,216	20,598	25,120	17,862	24,500
Log likelihood	-4245.83	-4393.93	-3924.45	-4293.72	-3884.80	-3655.16	-4479.53	-5197.07	-3779.50	-5191.37

***Notes**:* Standard errors are clustered by firm and reported in parentheses. All models include year indicators; two-tailed tests: *** p<0.001, ** p<0.01, * p<0.05, + p<0.10.

APPENDIX B:
THE INTERPRETATION OF NONLINEAR, DIFFERENCE-IN-DIFFERENCES

We faced several challenges in the interpretation of our nonlinear, difference-in-differences results. First, our analysis included several continuous variables as moderators of the difference-in-differences model, and we therefore had several triple-interactions to interpret. Second, our analysis employed a nonlinear (logit) model, and nonlinear interactions are a function of not only the coefficient for the interaction variable, but also the coefficients for the variables being interacted, as well as the values of *all* the variables in the model (Ai & Norton, 2003; Greene, 2010; Hoetker, 2007; Puhani, 2012). Third, our theory suggested that we should examine the range over which our hypothesized effects actually matter. In other words, we wanted to know in economic terms when and where our hypothesized moderators mattered, not just that they mattered on average.

To address the above concerns, we followed the simulation and prediction approach developed by King and colleagues (King *et al.*, 2000), and recently advanced in the strategy literature by Zelner (2009). In particular, we used *Clarify*, an add-on program for Stata developed by Tomz, Whittenberg, and King (2003), to interpret statistical results in terms of predicted outcomes. This Appendix summarizes the operation of *Clarify*, and then explains the construction of the graphs presented in Figure 2 in the main paper.[2]

To begin, we estimated the Full Model in Stata 12.1. Next, we used *Clarify* to predict outcomes for the model. Because effects in nonlinear models depend upon the coefficient estimates and levels of all the variables in the model, *Clarify* follows a simulation approach accounting for all sources of uncertainty in the prediction. First, *Clarify* incorporates uncertainty with respect to the estimation of the predictor variables used in the predictions; that uncertainty is represented in the variance-covariance matrix of the parameters and used by *Clarify* in the simulation. Second, *Clarify* incorporates stochastic uncertainty in general (i.e., the model error) into the prediction. Third, *Clarify* accounts for the link-function (in nonlinear models) in making the prediction. *Clarify* addresses the above three concerns by bootstrapping a distribution for each of the parameters in the model based on the estimated variance-covariance matrix of the parameters, and by including uncertainty about the model error into the bootstrap. The procedure appeals to the Central Limit Theorem to assume that coefficient estimates can be

[2] The following paragraphs paraphrase explanations of *Clarify* presented in (King *et al.*, 2000) and (Tomz *et al.*, 2003); we refer readers to the original works for an exposition of the working of *Clarify*.

drawn from a multivariate-normal distribution (note that this assumption is separate from the distributional assumptions of the underlying model itself). *Clarify* then uses the bootstrapped coefficient estimates to calculate a range of predictions at a given level of all the variables in the model.

As the final step in *Clarify*, we manipulated the levels at which predictions are made in order to obtain a distribution of predicted outcomes for different conditions of interest. Ultimately we simulated 1,000 predictions, for both the before-MARA period and the after-MARA period, for both Michigan and the comparison states, and for 60 separate levels of each moderating variable, resulting in a total of 240,000 simulations for each effect hypothesized in H2 - H4. Finally, we plotted these results in graphical form, as seen in Figure 2 of the main paper. Using the spread of simulated outcomes at each level of our variables of interest, we constructed and graphed a 95% confidence interval for the predicted outcome across a range of each moderating variable in our analysis. When the outer-bounds of the confidence interval rise above and do not cross the zero level, we can infer with greater confidence that our predicted effects are in fact statistically significant at the given level of the moderating variable.