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

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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. Copyright © 2014 John Wiley & Sons, Ltd.

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 and Mitchell, 1998; Hall, 1988). Academic research and anecdotal evidence suggests that acquisitions often are driven by firms' desire to acquire the human talents of the target companies (Buono and Bowditch, 1989; Coff, 1999, 2002; Ranft and Lord, 2000; Wysocki, 1997a, b). 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 *et al.*, 2011; Haspeslagh and Jemison, 1991; Heimeriks, Schijven, and 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) and risks of employees departing the target company after an acquisition (Buono and Bowditch, 1989; Jemison and 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, 1999) and how acquirers can work to reduce ex post employee departure from the acquired company (e.g., Larsson and Finkelstein, 1999; Ranft and Lord, 2000). To our knowledge, however, no research has investigated how the anticipated departure of employees from a firm ex post

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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 and Helfat, 1991) that can affect the future outcome of an acquisition (Coff, 1999, 2002; Ranft and 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 noncompete agreements (NCAs) provides an observable, exogenous source of variation in employee mobility (Marx, Strumsky, and Fleming, 2009). Using a difference-in-differences approach, we find causal evidence that constraints on employee mobility, due to an increase in NCA enforcement, raise the likelihood that a Michigan firm becomes an acquisition target, compared to firms in nonenforcing 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 and Gambardella, 1990; Cohen and 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, b). 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 and Katila, 2001; Capron, 1999; Puranam, Singh, and Zollo, 2006).

Despite these potential benefits, significant challenges exist for firms that pursue acquisitions as a knowledge sourcing strategy. One important stream of research examines the ex ante problems of information asymmetry (e.g., Akerlof, 1970) related to the acquisition of human capital-intensive targets and the strategic choices acquirers make before an acquisition deal is concluded. When faced with difficulties ex ante, research finds that acquirers may employ contractual clauses such as earnouts (Datar, Frankel, and Wolfson, 2001), use a greater proportion of equity as payment, lengthen negotiation time (Coff, 1999), select more geographically proximate targets (Chakrabarti and Mitchell, 2013), rely on information from alliances (Schildt and Laamanen, 2006), or simply choose not to close the deal (Coff, 2002). In addition to ex ante difficulties, acquiring firms also face challenges in retaining human talents and protecting the embedded knowledge and skills of companies *post*-acquisition (Buchholtz, Ribbens, and Houle, 2003; Hambrick and Cannella, 1993; Walsh, 1988). Accordingly, a second stream of research examines how acquirers may retain and motivate the employees of acquired companies during acquisition integration and how such efforts affect acquisition performance (Ashkenas, DeMonaco, and Francis, 1998; Haspeslagh and Jemison, 1991; Pablo, 1994; Schweiger and DeNisi, 1991; Shrivastava, 1986). Research has shown that employee retention is a critical part of acquirers'

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integration plan, contributing significantly to synergy realization and acquisition performance (Cannella and Hambrick, 1993; Cording, Christmann, and King, 2008; Larsson and Finkelstein, 1999; Zollo and Singh, 2004).

This study examines how anticipated employee departures from a firm ex post may affect acquirers' decision to bid for the firm ex ante. Studying this question links together the two streams of M&A research reviewed 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 if critical knowledge or other assets are lost as employees leave the target firm after the acquisition, or if employee departures negatively affect the performance of others who remain with the firm (O'Reilly and 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). 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). In this study, we 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 because it is difficult to observe acquirers' expectations about post-acquisition employee departure. Furthermore, there is a great deal of uncertainty about the likely magnitude of such an event, making it difficult for acquirers to form accurate expectations about such turnover and about the future value of the potential target. However, *observable* institutional factors exist that can reduce acquirers' uncertainty about employee turnover and thus can inform their acquisition decisions, as we explain in greater detail below.

Employee mobility and acquisition likelihood

A prevalent finding in the M&A literature is that acquisitions are disruptive to the people involved (Haspeslagh and Jemison, 1991; Jemison and Sitkin, 1986; Larsson and Finkelstein, 1999). All else equal, "people-related problems" increase the turnover of individuals in the target company (Jemison and 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. Acquirers may seek stability in the workforce after an acquisition in order to learn more about which employees they would prefer to retain or to let go. Employee turnover also 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.

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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 and Ungson, 1991), especially when such knowledge is tacit or hard to articulate (Kogut and 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, and 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, and 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 5–8 percent decline in the collaborators' quality-adjusted publication rates in the years that follow (Azoulay, Zivin, and 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 and 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 and Drazin, 2002), and key technical knowledge (Rosenkopf and Almeida, 2003) by recruiting talent from rivals. Spin-offs founded by former employees also pose competition to the firm in the future (Agarwal *et al.*, 2004; Stuart and 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, 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.

Our study focuses on the role of employee noncompete agreements (NCAs) in constraining employee mobility. Employee noncompete 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 noncompete agreements with their employers (Kaplan and Stromberg, 2003; Leonard, 2001). Theoretical research has long suggested that varying levels of enforcement of noncompetes contribute to the differential employee mobility and patterns of knowledge diffusion observed in different states (e.g., Franco and Mitchell, 2008; Gilson, 1999; Saxenian, 1994). Recent empirical studies have confirmed the negative relationship between noncompete 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 noncompete 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

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value of a target firm and that acquirers' acquisition decisions reflect such expectations. Specifically, as the enforcement of noncompetes 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 noncompete agreements will increase the likelihood that a firm will become an acquisition target.

Hypothesis 1 is our baseline hypothesis. The strength of Hypothesis 1, 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. 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 (at least in the short run) from an increase in the enforcement of noncompetes: 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 and 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 key capabilities. They are, therefore, more likely to take that knowledge with them to a competitor when they depart or use that knowledge to generate spin-offs to compete with their ex-employer in the future (Bhide, 2000). Third, legal theory and the justification for noncompete agreements is rooted in the concept that workplace knowledge is a form of employer intellectual property (Fisk, 2009; Hyde, 2010). Employers apply noncompete 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 noncompete agreement, compared to other types of employees (Kaplan and Stromberg, 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 noncompete agreements will increase the

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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 and Stuart, 2001; Stuart and 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 and 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. 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 (Garmaise, 2011; Gilson, 1999). 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 noncompetes 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 noncompete 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 and Hambrick, 1993; Coff, 2002; O'Reilly and Pfeffer, 2000; Ranft and Lord, 2000), such consequences may vary across individual companies based on the knowledge protection mechanisms at their disposal. 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, and Walsh, 2000; Levin *et al.*, 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 patenting 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, and employee departure is more likely to reduce their knowledge stock directly, as well as transfer 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

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acquisition targets to a lesser degree, compared to firms operating in a weaker IP regime:

Hypothesis 4: An increase in the enforcement of noncompete 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 rarely changes; and when it does change, it usually changes by a modest amount (Garmaise, 2011). 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). However, research suggests that, in passing MARA, legislators also inadvertently repealed Michigan statute 445.761, a statute that previously prohibited the enforcement of noncompete 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. Because stronger enforcement of antitrustregulations, especially at the federal level, is unlikely to cause an increase in M&A activity (Brodley, 1995), antitrustaspects of MARA should work against us finding our hypothesized effects. It would therefore appear that the repeal of 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 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 noncompetes when conducting due diligence in M&As (Deloitte, 2010; Garmaise, 2011). In addition, we believe that the policy change, being publicly available information, would be reflected in acquirers' acquisition decisions in the highly competitive M&A market.

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 and 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) 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. In our analysis, we assigned firms in Michigan to the "treated group" 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, and Pedowitz, 2002; Marx *et al.*, 2009; Stuart and 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 also removes differences between treatment and

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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 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.

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, and Porro, 2011) and, as such, reduces causal estimation error, model-dependence, bias, and inefficiency (Iacus, King, and Porro, 2009a, b). We matched on the pre-MARA average value of *Assets*, *Liquidity*, and *ROA* (Return on Assets), in that these covariates have been shown in prior research (e.g., Field and Karpoff, 2002) to affect the likelihood of acquisition. 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 included observations dropped by CEM back into the sample in a robustness check 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 and Walkling, 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, spin-offs, carve-outs, self-tenders, and recapitalizations.

Figure 1 (panel A) 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 1 (panel B) 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.

Measures

Dependent variable

Consistent with prior research on acquisition likelihood (e.g., Song and Walkling, 1993, 2000), our dependent variable, *Acquisition*, is a dichotomous variable that equals 1 if the focal firm is the target of an acquisition in a given year based on the acquisition announcements reported by SDC and 0 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.

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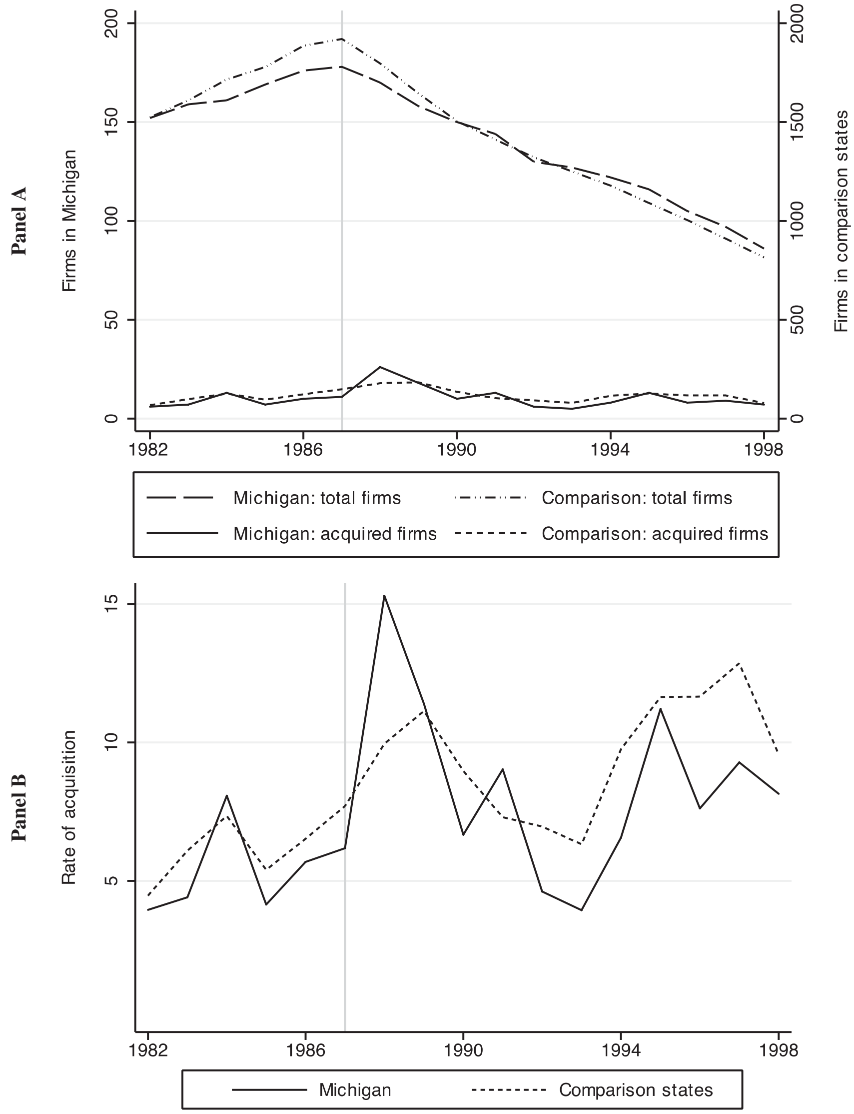

Figure 1. Descriptive trends in Michigan and comparison states. (**Panel A**) Base population and number of acquisitions in Michigan and comparison states. ***Notes***: We restricted our base population to firms that were publically listed prior to MARA. This population declined after 1987 in both groups (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

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 and Megginson, 1992; Field and Karpoff, 2002; Palepu, 1986; Song and 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 1 if a firm was located in Michigan based on the historical location of the firm's corporate headquarters. The DD "after" variable, *After*, is an indicator variable that equals 1 for 1988 or thereafter and 0 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 and additional time for the knowledge to be absorbed by corporate managers. It would then take more time for potential acquirers to act upon the knowledge, given the significant requirements for 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 acquirers officially contact targets to the date of public announcement (Boone and Mulherin, 2007), not including the private, unobservable part of the process. We therefore selected a break between 1987 and 1988 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 Hypothesis 1.

To test the moderating effects proposed in Hypotheses 2–4, 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 0 to simplify interpretation of the interaction effects.

For *Knowledge Workers*, we followed prior research (e.g., Coff, 1999, 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 three-digit standard industrial classification (SIC) industry by OES occupational code. From the OES data, we calculated the proportion of the total workforce being knowledge workers for a given three-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 three-digit SIC industries. Because the COMPUSTAT Segments file provides more comprehensive coverage of firms' sales data by NAICS, we extracted the data by the four-digit NAICS designation and then converted it to the three-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 three-digit SIC industries. The variable *In-state Competition* is a continuous measure from 0 to 1.

For *IP Protection*, we followed Cohen *et al.* (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. 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 0 to nonmanufacturing firms, as the measure is not relevant to those firms. We rescaled the measure to vary continuously from 0 to 1 to be consistent with the other two moderating 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* (three-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

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Table 1. Summary statistics and correlations ($n = 18{,}713$)

	Summary statistics	Mean	Std. dev	Min	Min
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

category (Marx *et al.*, 2009); results are robust to the use of three-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 and 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 three-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* $q = ((\text{PRCC\_F} \times \text{CSHO}) + \text{AT} - \text{CEQ})/\text{AT}$ (Chung and 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 and Seldeslachts, 2013); *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, Clougherty, and Barros, 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 and Seldeslachts, 2013). 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.

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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 and Karpoff, 2002; Song and 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 three-year trailing average return on assets (*ROA*) for each firm that is in excess of the three-year trailing average return on assets for the focal firm's three-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 1 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 three-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 Securities and Exchange Commission (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 treatment and comparison groups, our measure of firm location should work against us finding our hypothesized results.

Models

We estimated logit models in a difference-in-differences configuration to test whether the Michigan increase in NCA enforcement affects the likelihood of a Michigan firm becoming an acquisition target (Hypothesis 1), and to examine the conditions under which this relation is strengthened or weakened (Hypotheses 2–4). Instead of estimating the before-to-after change in outcomes for the treatment group (i.e., Michigan) and assuming 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. The DD model does this by estimating the change in Michigan and the change in comparison states and then taking the difference of those 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 repeated observations by 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 A in Table 2 indicates that the rate of acquisition rose by 12.92 percentage points in Michigan, from 6.77 percent in 1987 before MARA to 19.69 percent in 1988 after MARA. Not all of this change, however, can be attributed to

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

	Panel A: 1987–1988 +/− one year window			**Panel B**: 1983–1992 +/− five year window			**Panel C**: 1982–1998 All data		
	Before	After	*Diff*.	Before	After	*Diff*.	Before	After	*Diff*.
Michigan	6.77	19.69	12.92	5.61	10.85	5.25	5.18	8.87	3.69
Comparison	9.00	10.70	1.70	6.83	9.22	2.39	6.29	9.35	3.07
Difference	−2.23	8.99	**11.22**	−1.22	1.64	**2.86**	−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.

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% points) from the difference in the treated state, Michigan (12.92% 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 A in Table 2: The treatment effect of MARA was an 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, and Stulz, 2004). As shown in panels B and C in Table 2, 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.

Hypotheses testing

Moving to a regression framework, Table 3 reports results of a moderated difference-in-differences analysis. Each model adjusts for 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*. Column 1 estimates the interaction of *Michigan* × *After* as the basic difference-in-differences effect in a multivariate framework. Columns 2–4 interact *Michigan* × *After* with the moderating conditions hypothesized in Hypotheses 2–4. Concerning control variables, we find that *Industry Computers and Communications*, *Industry-State Tobin's q*, *Industry-State Acquisition Rate*, *Industry-State Delisting Rate*, *Assets*, *Liquidity*, and *Prior Bids* are associated with an increase in acquisition likelihood, whereas *State Business Combination Laws*, *Years Public*, and *Reporting Segments* are associated with a decrease in acquisition likelihood. Due to space constraints, we interpret the effects of control variables in Appendix S1. Concerning the main effects of DD variables, 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 the likelihood of acquisition generally increases from the before period to the after period for firms in both Michigan and comparison states ($p < 0.001$); these findings are consistent with our univariate analysis and graphs shown in Figure 2.

Next we turn to the hypotheses testing results. In our baseline hypothesis (Hypothesis 1), we posit that an increase in the enforcement of NCAs 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

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Table 3. 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)
Knowledge workers (KW)	−1.3713***	−1.6298***	−1.7488***	−1.7507***

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Table 3. Continued

	(1)	(2)	(3)	(4) Full model
	(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: ***$p < 0.001$; **$p < 0.01$; *$p < 0.05$; +$p < 0.10$

of acquisition of firms in the comparison states. Hypotheses 2–4 further identify several conditions under which constraints on employee mobility due to the enforcement of NCAs will be more or less important in shaping firms' likelihood of being a target. Consistent with the prediction in Hypothesis 2, 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 Hypothesis 3; 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, Hypothesis 4 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

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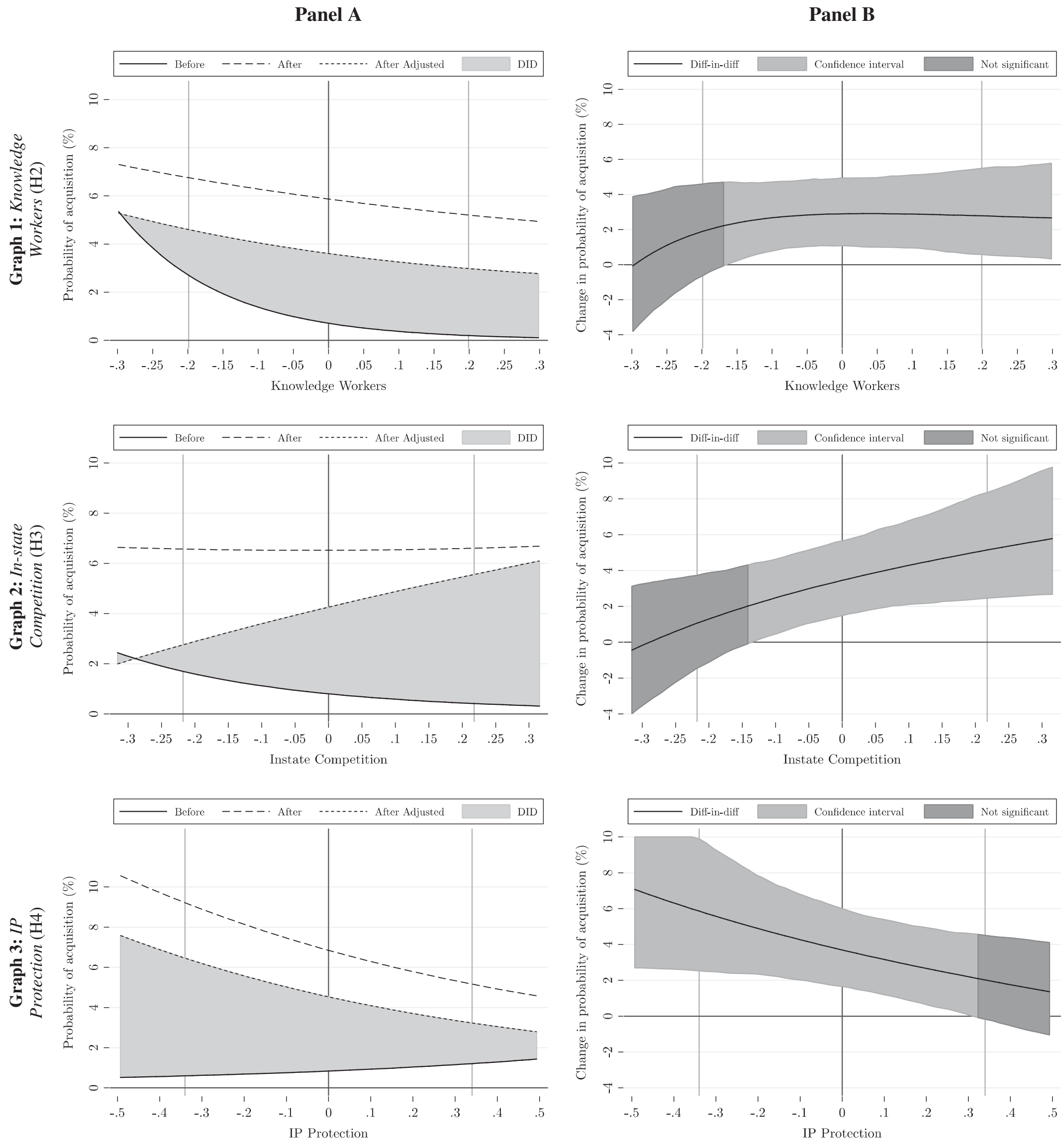

Figure 2. The predicted effect of MARA by level of moderating variable. (**Panel A**) Predicted before-to-after change in the probability of being an acquisition target. (**Panel B**) Predicted difference-in-differences effect and simulated 95 percent confidence interval. ***Notes***: This figure shows the predicted effect of an increase in the enforcement of noncompete agreements on the likelihood of firms becoming a target of acquisition, by level of *Knowledge Workers* (Hypothesis 2), *In-state Competition* (Hypothesis 3), and *IP Protection* (Hypothesis 4) in target firms. Vertical lines appear at 0 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 percent confidence interval around the predicted difference-in-differences effect using simulation techniques described in Appendix S1. The nonsignificant range of the difference-in-differences effect is plotted in a darker shade where the confidence interval falls below 0. As predicted in Hypothesis 2, 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 Hypothesis 3, panel B-graph 2 indicates that the effect of MARA is stronger for firms that face greater in-state competition. As predicted in Hypothesis 4, panel B-graph 3 indicates that the effect of MARA is weaker for firms that are protected by a stronger IP regime

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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) recommends using graphical representations 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 S1.

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 effect is statistically different from zero. The magnitude of the DD effect is calculated by subtracting the value of each "Before" line from the 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 Hypotheses 2–4, 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 percent 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 in panel A, after adjusting for changes in comparison states and effects of the covariates, the sudden enforcement of noncompete 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 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 (Column 4 of Table 3) and report these results in Table 4. We begin by testing our counter-factual comparison. Whereas we assume that firms in states that did not enforce noncompete agreements (before and after MARA) provide the best comparison with respect to the enforcement of noncompetes, such firms may not provide the best comparison for other economic factors. Therefore, we change the comparison group in Column 1 to firms headquartered in states near Michigan (i.e., Ohio, Indiana, Illinois, Wisconsin, and Pennsylvania) to control for trends in the Midwest economies and find similar results in Column 1 as we do in the Full Model. In Column 2, we test industry fixed effects at the three-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 and Sheather, 2010) to remove controls that reduce an optimal Bayesian Information Criterion and find results that are consistent with the Full Model, suggesting that our findings are not sensitive to the

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Table 4. 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, except Column 3, which uses backwards elimination (Lindsey and 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: ***$p < 0.001$; **$p < 0.01$; *$p < 0.05$; +$p < 0.10$

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, the selection

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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 nonindependence 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 autocorrelation and within-group dependence, because the indicator variable for "treatment" (i.e., Michigan) is highly correlated between periods within state-level clusters (Bertrand, Duflo, and Mullainathan, 2004). Clustered block-bootstrapping methods are often used in this situation to correct for nonindependence (Cameron and Trivedi, 2009). In Column 7, we reestimate 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]

Finally, we perform a series of other robustness checks and report the results in Appendix S1 due to space constraint. In brief, we test other "placebo" states in the Midwest and around the country to examine whether our results might reflect developments in the Midwest but did not find an increase in the likelihood of acquisition in any of the placebo states. We also investigate whether our results might be due to changes in Michigan's antitrust laws (instead of the reversal of NCA enforcement) by testing antitrust legislation passed in Texas in 1983 and as expected found no change in the likelihood of acquisition. We conduct several other robustness tests and report the results in Appendix S1.

DISCUSSION

Drawing on a difference-in-differences analysis of a natural experiment in Michigan, we have shown that Michigan's enforcement of noncompete agreements caused an increase in the likelihood of a firm becoming an acquisition target. Because research has shown that NCA enforcement reduces employee mobility (Fallick *et al.*, 2006; Garmaise, 2011; Marx *et al.*, 2009) and because strategic factor market theory argues that firms make acquisition decisions based on expectations about the value of a resource (Barney, 1986), our results suggest that decreases in anticipated employee mobility increase the attractiveness of the firm as an acquisition target. We also find strong support for three, knowledge-based interactions, demonstrating that employee mobility is indeed an important factor in M&A strategies to source knowledge and human talents. Taken as a whole, 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 and Cannella, 1993; Haspeslagh and Jemison, 1991; O'Reilly and Pfeffer, 2000; Ranft and Lord, 2000). 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 and Wright, 1998; Coff, 1997; Lado and Wilson, 1994). Our results 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, 1999, 2002; O'Reilly and Pfeffer, 2000). While acquirers may not be able to influence states' policy of NCA enforcement, this study suggests that acquirers may rely on 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 and Finkelstein, 1999; Pablo, 1994; Ranft and Lord, 2002), we are not able to examine integration directly in this

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

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study, given our focus on acquisition likelihood and our research design requiring the inclusion of both acquisition events and "nonevents" in the sample (Field and Karpoff, 2002; Schildt and Laamanen, 2006; Song and Walkling, 2000).

Second, our study contributes to strategic factor market theory (Barney, 1986), a foundational theory in strategic management. The assumption that firms formulate a strategy based on expectations about future returns from that strategy, while straightforward, 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. Our 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 and Megginson, 1992; Field and Karpoff, 2002; Palepu, 1986; Song and Walkling, 1993, 2000). While our approach avoids problems of sample selection bias and allows us to examine target-side factors that shape acquirer-side expectations, it limits our ability to incorporate directly 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, 1999; Cording *et al.*, 2008; Ellis *et al.*, 2011; Larsson and Finkelstein, 1999; O'Reilly and Pfeffer, 2000; Ranft and Lord, 2000, 2002). Finally, while our focus on the policy change in Michigan 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 noncompete agreements. Prior research has examined the relationship between NCA enforcement and the mobility of 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 and Mitchell, 2008; Gilson, 1999; Samila and Sorenson, 2011; Saxenian, 1994; Stuart and 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. Our approach links interorganizational employee mobility to firms' interorganizational strategic choices. Our findings are consistent with research on the relationship between individual-level employee mobility and firm-level strategies and outcomes (Agarwal *et al.*, 2004; Stuart and Sorenson, 2003b), and we contribute to that research by examining how expectations about individual mobility 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.

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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SUPPORTING INFORMATION

Additional supporting information may be found in the online version of this article:

Appendix S1. How anticipated employee mobility affects acquisition likelihood: evidence from a natural experiment

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