Journal of Economic Behavior and Organization 150 (2018) 162–181

Contents lists available at ScienceDirect

Journal of Economic Behavior and Organization

journal homepage: www.elsevier.com/locate/jebo

Competitive pressure on the rate and scope of innovation☆

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ARTICLE INFO

Article history:
Received 3 June 2017
Revised 31 March 2018
Accepted 31 March 2018

Keywords:
Competitive pressure
Innovation
Merger & acquisition
Takeover
Organization
Hierarchy
Incentive
Patents

ABSTRACT

While executives play an important role in leading firm innovation, they may economize on efforts to innovate when protected from takeover threat. Middle managers may curtail the rate and scope of innovation when executives are expected to reduce their innovation involvement. We test our prediction by exploiting a natural experiment in Delaware where court rulings increased takeover protection for Delaware firms. Difference-in-differences estimates show that increased takeover protection reduced the *rate* of innovation by firms, and that it also reduced the *scope* of innovation across several key dimensions (technological, temporal, organizational, and international). Consistent with our argument, we find that the negative effect of takeover protection on innovation was weaker for larger firms, where innovation decision making authority is more likely to be delegated to middle managers and executive involvement is lower. Finally, we examine the substitutive relationship between competitive pressures from the takeover market and the product market, and find that the negative effect of takeover protection on innovation was stronger for firms facing low competitive pressure from the product market.

1. Introduction

A prominent body of research in economics and management examines the effect of competition on innovation (Schumpeter, 1939; Hart, 1983; Porter, 1990). Theoretical work has proposed a positive relationship (e.g., Arrow, 1962), a negative relationship (e.g., Dasgupta and Stiglitz, 1980), and an inverted-U relationship (Aghion et al., 2005) between product market competition and innovation; empirical evidence, however, remains inconclusive (Cohen, 2010). Understanding how competitive pressure shapes innovation remains critical for understanding the interplay between management strategy and innovation.

This study departs from previous research in several ways: First, our primary focus is on the effect of competition in the takeover market on innovation, rather than the effect of competition in the product market. While research has long suggested that competitive pressure in the takeover market shapes managerial behavior (Jarrell et al., 1988), we study the effect on innovation of a change in Delaware takeover law that significantly increased the protection of executives from the threat of takeover (Subramanian, 2004; Low, 2009). Second, we move beyond existing research and show that increased takeover protection affects not only the rate of innovation by firms (Atanassov, 2013; Sapra et al., 2014), but also the scope of innovation. Given the growing interest in understanding the *direction* of inventive activity (Lerner and Stern, 2012), our

☆ We acknowledge the National Natural Science Foundation of China (Nos. 71272131, 71672021 and 71632002) for partial support for this research.
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https://doi.org/10.1016/j.jebo.2018.03.026
0167-2681/

study opens an important new line of inquiry and develops several new measures of innovation scope. Third, it is long received that organizational hierarchies affect delegation of authority and resource allocation decisions (Williamson, 1975, 1981). Confirming this idea, we show that takeover protection has a lesser effect on the rate and scope of innovation for larger firms where innovation decision making authority is more likely to be delegated down the hierarchy and is thus less affected by competitive pressure in the takeover market. Finally, while early work conjectures that the effects of competition in the takeover and product markets interact (e.g., Hart, 1983), there has been little empirical study of the proposition with respect to innovation. We find a substitutive relationship between competitive pressures from the takeover market and the product market, and show that these forces interact to shape both the rate and the scope of firm innovation.

Innovation involves a continual search for new knowledge components and new means of recombination (Schumpeter, 1939; Nelson and Winter, 1982). Such search is often characterized by considerable uncertainty (Levinthal, 1997; Fleming, 2001), and as a result, researchers have maintained that the management of innovation requires "substantial human effort" (Holmstrom, 1989: 309) and keen attention by managers (Van de Ven, 1986). Despite such requirements, managers in an imperfectly competitive environment may prefer to enjoy a "quiet life" (Hicks, 1935) and, as Hart (1983) points out particularly, "other things equal, a manager may prefer not to innovate since this involves effort." Empirical research confirms the idea and shows that top executives tend to economize on effort when the threat of hostile takeovers is reduced (Bertrand and Mullainathan, 2003), because their positions become less threatened and they might prefer to avoid undertaking cognitively difficult activities (Giroud and Mueller, 2010).

We argue that when executives expect to economize on effort because of reduced takeover pressure, their involvement in innovative activity will likely decrease. This in turn affects the types of projects that middle managers will select for review and pursuit, and therefore the firm's subsequent innovation outcomes. Specifically, executives looking to economize on effort may rely more on existing standards and procedures in decision making and resource allocation, suggesting a preference for activities that are easier to understand and predict, and that can be directed with less investment in information collection and evaluation ex post (Holmstrom, 1989). Accordingly, middle managers are more likely to propose projects that can be more easily explained and justified to an inattentive boss ex ante. Because projects with a broader scope are more likely to involve unexpected contingencies, demanding greater attention and oversight from executives, we expect middle managers to prioritize searching for projects with a narrower scope and propose such projects to executives when their time with those executives is more constrained. For example, we argue that it may be easier for executives to communicate and understand information in projects that combine fewer technological components, build on well-established prior art, and derive from inventors already working at the firm from within the same country—all of which are proxies of the scope of innovation that we examine in this study.

The kinds of effort expended by executives on innovative activity are very difficult to measure and may be endogenous to many aspects of the firm and its environment. We address this empirical challenge by exploiting an exogenous increase in takeover protection in Delaware in 1995. A series of court rulings in Delaware legitimized the use of a poison pill in conjunction with a staggered board in 1995, giving firms incorporated in Delaware (but usually located and operated throughout the United States) a potent defense against takeover, making them virtually immune to hostile acquisition. A large literature suggests that by reducing the threat of hostile takeovers, the change in Delaware takeover law weakened executives' incentive to expend effort (Bebchuk and Cohen, 2003; Low, 2009), though no study has examined the effect of the change on firm innovation. Using the regime change in Delaware as a natural experiment, we investigate the effect of increased takeover protection on both the rate and scope of firms' innovation. Consistent with existing research, we predict that Delaware firms will reduce the rate of innovation after the regime change. Extending prior research, we hypothesize that Delaware firms will prioritize innovative projects with a smaller scope, thus also reducing the scope of innovation. We then examine the moderating role of organizational hierarchy and predict that increased takeover protection will have a lesser influence on the rate and scope of innovation for Delaware firms of a larger size, because in larger firms innovation decision making authority is more likely delegated to middle managers ex ante and the effect of executives economizing on effort is smaller. Finally, we argue that competition in product markets may interact with pressure from takeover markets, and hypothesize that takeover protection should have a stronger effect on the rate and scope of innovation for Delaware firms that also face low competition in the product market.

Our findings, based on a difference-in-differences methodology, multiple measures of innovation rate and scope, and a series of robustness tests, provide strong support for all of our hypotheses. Our research introduces the scope of innovation as a new topic of study and responds to calls for more research on the direction of inventive activity. We also contribute to an emerging literature linking management strategy and innovation by providing causal evidence that takeover protection affects both the rate as well as scope of firm innovation.

2. Related literature and hypotheses

A stream of economics and strategy research on innovation has focused on incentive design issues. For example, Holmstrom (1989) suggests that innovative projects are risky, uncertain, and require substantial human effort; however, such activities are difficult to measure, thus the incentives needed to induce innovation should be different from those for routine activities that are easy to measure (e.g., Manso, 2011). In contrast to this stream, a body of literature on innovation and knowledge search has given great emphasis to the idea of bounded rationality (Simon, 1955). Since Schumpeter (1939), researchers have accepted the view of innovation as an ongoing search for new knowledge components and new methods of recombination (Nelson and Winter, 1982; Simon, 1996; Fleming, 2001). While innovation requires substantial attention to new ideas and methods, research has long suggested that human beings are limited in their capacity to process and absorb new sources of information (Simon, 1947), and that they may in response limit the number of (new) elements or choices in their search and investigation.

The two streams of research may be usefully combined to explain organizational decision making on innovative projects and subsequent innovation outcomes. A firm is a multi-layered organization consisting of boundedly rational top executives and middle managers such as R&D managers (as well as employees) (March and Simon, 1958). Significant research by organizational economists (e.g., Milgrom, 1988) and management scholars (e.g., Bower, 1970) has improved our knowledge about decision making dynamics in large organizations. In an organizational hierarchy, although authority confers top executives the ultimate decision right over project investment and resource allocation, middle managers and top executives are engaged in a mutual relationship during the decision process. In the case of innovation, middle managers matter a great deal (Bower, 1970; Holmstrom, 1989), as it is often the case that such managers search and select project ideas for top executives to review, who have the decision right over which ones to eventually pursue. After a project starts, this relationship then evolves into one in which middle managers are responsible for execution and top executives excise oversight.

Given such relationships, middle managers' search and selection of innovation projects ex ante is likely affected by the kind of effort top executives are willing to expend on projects ex post. Middle managers' bounded rationality in their search effort, as well as the low-powered incentives in hierarchies (Williamson, 1975), may lead them to prioritize or select projects that are "easier" or have less innovative potential (Holmstrom, 1989). While such projects may not be in the long-term interest of the firm, they may be in the interest of top executives if they face little pressure to improve performance, because such projects may be easier to evaluate and monitor using conventional standards and routines and are therefore less likely to tax their attention. Because "attention is a scarce resource" (Banerjee and Mullainathan, 2008: 489), human beings' limited attention can provide an explanation for executives' preference for limiting the number of projects, or new components in projects, for their consideration. Such decision making dynamics may be particularly true when middle managers can *anticipate* top executives to give less attention and effort to the proposed projects ex post, such as when observable signals or public events indicate that top executives are likely to do so.

2.1. Rate of innovation

Extant research has suggested that top executives often prefer to enjoy a quiet life (Bertrand and Mullainathan, 2003), especially when they face little competitive pressure from the takeover market. Indeed, the threat of hostile takeover has long been considered one of the strongest market mechanisms for inducing executive effort. When the threat of a takeover looms large, top executives who are not putting out best efforts run the risk of being dismissed when their companies are taken over by an acquirer. By contrast, when the market for corporate control is inefficient, the fear of a hostile takeover and job loss is reduced; accordingly, executives may seek to economize on effort and avoid undertaking cognitively difficult innovation activities. Substantial research has used antitakeover laws as a proxy for weakened market for corporate control and has documented strong evidence of executives economizing on effort after passage of such laws. For instance, research has shown that, subsequent to states' passing business combination laws in the 1980s, firms' investment activities decrease and employee wages and other input costs increase—presumably to economize on executives' effort and avoid difficult bargaining with labor unions and other stakeholders (Bertrand and Mullainathan, 1999, 2003).

We argue that protection from takeover threat will have a negative effect on firms' rate of innovation. When public events indicate that the threat of takeover will reduce, such as when states pass antitakeover laws or court rulings change the takeover regime, middle managers observing such events can ancitipate that top executives may reduce effort levels following the events. Anticipating such changes, for example, R&D managers may undertake more limited search for innovative projects and propose fewer projects or "easier" projects to executives for review. Executives may also see this as being in their interest because they may prefer to invest less effort overseeing projects when takeover pressures reduce. This may result in a drop in firms' rate of innovation output. In our study's context, the change in Delaware's takeover regime significantly increased Delaware firms' protection from hostile takeovers, reducing the competitive pressure the executives will experience. We therefore predict a drop in the rate of innovation for Delaware firms following the change in Delaware's takeover regime.[1] While this prediction is consistent with an earlier finding that passing state business combination laws reduces firms' rate of innovation (Atanassov, 2013; Sapra et al., 2014), we propose and empirically test the prediction to examine if it also holds in a different context.

Hypothesis 1. A decrease in competitive pressure from the takeover market will lead to a reduction in the rate of innovation.

One concern with Hypothesis 1 is that some research suggests a curvilinear relationship between product market competition and innovation (Aghion et al., 2005).[2] Although competitive pressure may induce firms to innovate as a means of

[1] One might interpret Manso's (2011) model—a model of the optimal incentive scheme to motivate innovation—as suggesting that protection from hostile takeovers will encourage exploration. However, we show below that Delaware firms see their scope of innovation become narrower and less exploratory following the takeover regime change.

[2] To the extent that firms under less competitive pressure will have more internal slack resources, and that slack has an inverted U-shaped relationship with the rate of innovation (Nohria and Gulati, 1996), competition will have a curvilinear relationship with the rate of innovation.

escaping competition, this mechanism is theorized to be strongest in leveled or neck-and-neck sectors where competitors have technological parity. Moreover, a decrease in competition under the Aghion et al. (2005) model should raise innovation only for the subset of firms that are at the highest-level of competition, but that also are not in neck-and-neck competition. Finally, our hypothesis focuses on competitive pressure from the takeover market rather than the product market. For the above reasons, we do not hypothesize a curvilinear relationship.

2.2. Scope of innovation

Executives' anticipated effort levels may not only affect how much innovation search middle managers will conduct, but also where and the direction in which they search for new knowledge components and new methods for recombination, thus shaping the scope of firms' innovation activities. Specifically, anticipating reduced attention and oversight by top executives following a takeover regime change, middle managers may choose to simplify how they search for and select innovative ideas. Middle managers may, for example, prioritize projects with a narrower scope with fewer innovative components and less recombinative potential, and propose such projects for review and approval by executives. Given limited capacity and the unpredictability of environments, human beings have a natural tendency to simplify (Simon, 1955). Simplification by middle managers can be also seen as valuable by top executives, in part because projects with a broader scope may require executives to learn new evaluation metrics and apply non-routine decision rules, imposing a tax on their limited attention at a time when they may prefer to reduce involvement in innovation because of takeover protection.

To expand a firm's scope of innovation requires conducting exploration in broader, newer, and unfamiliar areas. Such exploration requires executive attention over, and oversight of, a larger technological, temporal, or geographic space (March 1991). For instance, starting new R&D projects helps to broaden a firm's technological scope, but it requires paying keen attention to managing "people problems", including recruiting new creative personnel (Amabile, 1996), discovering new personnel abilities and complementarities (Prescott and Visscher, 1980), and matching new talents to new projects (Jovanovic, 1979). Human beings, however, are shown to rationally prefer to search for opportunities in areas familiar to them, because familiarity gives a level of comfort with known solutions and helps to economize on one's cost of evaluating opportunities (Huberman, 2001). To expand the scope of innovation, executives also need to deal with various interdependencies in the innovation process, such as coordinating cross-functional collaboration, or interfacing with R&D personnel and customers (Nelson and Winter, 1982; Clark, 1991). All these enlarge the scope of executive oversight and responsibility, which they might prefer to avoid when competitive threats from the takeover market are removed. Anticipating this dynamic, middle managers may be more likely to simplify search and prioritize and select projects with a smaller scope to report to executives.[3] We therefore predict that the change in Delaware's takeover regime, which significantly decreases competitive threats to executives, will cause a drop in the scope of innovation for Delaware firms.

Hypothesis 2. A decrease in competitive pressure from the takeover market will lead to a reduction in the scope of innovation.

2.3. Role of organizational size

Our predictions so far assume a firm with a more centralized structure where top executives are typically involved in the innovation decision making process, but would otherwise prefer to limit their involvement in innovation because of increased takeover protection. Such centralized decision making is more likely to apply to smaller organizations with more direct contact between middle managers and executives. In larger organizations, however, decision making authority is more likely to be delegated to middle managers, and the innovation process and incentives are being pushed down the hierarchy (e.g., Rajan and Wulf, 2006). This is because as firms grow in size, limits of hierarchical control become apparent, and the value and need of decentralization increase accordingly. Indeed, organizational economists have long maintained that decentralization of organizational hierarchies is responsible for the growth of the modern corporation (Williamson, 1975, 1985).

We believe that organizational hierarchy, as proxied by organizational size, plays a critical role in shaping how takeover protection affects innovation. Although firm size has been argued to affect various aspects of innovation at least since Schumpeter (1934, 1942), evidence on the impact of firm size on innovation remains mixed (see Cohen, 2010, for a review of this literature). Our intent is not to examine the main effect of firm size on innovation outcomes, but rather to examine whether firm size strengthens or weakens the effect of takeover protection on innovation. Given that in larger organizations, innovation decisions are more likely to be delegated down the hierarchy and further removed from executives' direct oversight, middle managers will be more incentivized to play a larger role in leading and directing innovation. We therefore predict that the negative effect of Delaware's takeover regime change on innovation, as hypothesized in H1 and H2 above, will be weaker for larger firms and, correspondingly, stronger for smaller firms.

[3] Possible explanations may relate to the demand (customer) side, in addition to the supply (innovation process) side discussed here. For example, when competitive threats from the takeover market are removed, managers are not under pressure to expand their business and thus may slow down their search for new customers. As a result, the firm may devote less effort to exploring new technologies, leading to a smaller scope of innovation (Hui et al., 2017).

Hypothesis 3. The negative effect of a decrease in competitive pressure from the takeover market on the rate (H1) and scope of innovation (H2) will be weaker for larger firms and stronger for smaller firms.

2.4. Role of product market competition

The effect of competitive pressure from takeover markets on innovation may also depend on the presence or absence of other sources of pressures. In addition to hostile takeovers, prior research has emphasized product market competition as another, prominent, market-based mechanism for shaping executive behavior. While executives may prefer to avoid expending effort (Hicks, 1935), research suggests that product market competition provides a source of pressure and forces executives to work hard to maximize efficiency and increase productivity (e.g., Hart, 1983). Among other scholars, Hart (1983) in particular suggests research to integrate competition in product markets and competition in capital markets as different mechanisms for increasing executive effort. Recent research begins to empirically examine how antitakeover laws and product market competition interact to influence operating performance (Giroud and Mueller, 2010), but there is scant research on how such forces may interact to affect innovation.

Given that the threat of hostile takeovers and the pressure from product market competition both act to induce executive effort, the two sources of competitive pressure may substitute for each other in increasing executives' involvement in innovation and oversight of innovative projects. Specifically, for firms facing low competitive pressure from the product market, executives enjoying takeover protection will feel more comfortable being detached from the innovation process, and the negative effect of takeover protection on innovation will be stronger. Yet for firms already subject to high product market competition, the effect of takeover protection on innovation will be weaker, since executives are still under great pressure to increase firms' competitiveness and maintain job security. These arguments suggest that the change in Delaware's takeover regime should have a greater effect on the rate and scope of innovation for those Delaware firms that face lower levels of competition in the product market. We therefore propose:

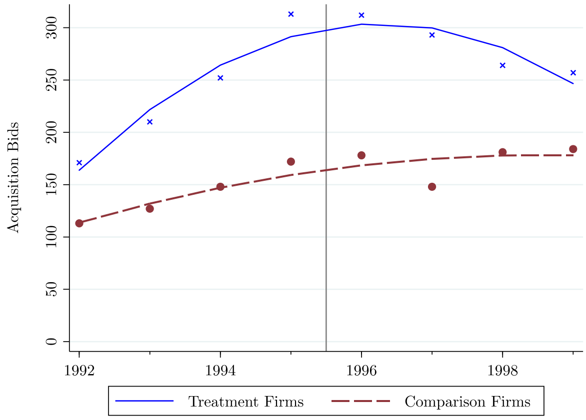
Hypothesis 4. The negative effect of a decrease in competitive pressure from the takeover market on the rate (H1) and scope of innovation (H2) will be weaker for firms with more competitive pressure from the product market and stronger for firms with less competitive pressure from the product market.

3. Data and methods

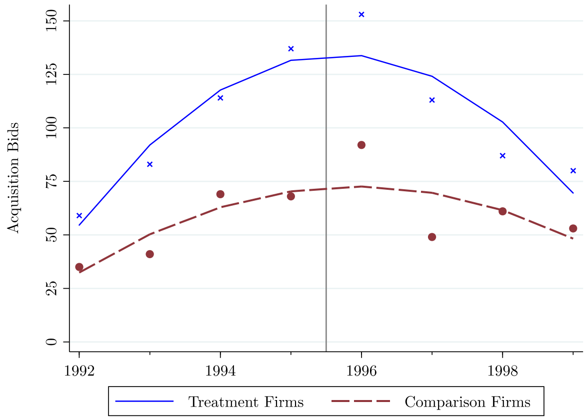
3.1. Research design and identification strategy

We empirically examine our hypotheses by exploiting a natural experiment in Delaware where court rulings changed the corporate takeover regime for firms incorporated in Delaware and therefore produced exogenous variation in the competitive pressure firms experienced from the takeover market. Before the mid-1990s, Delaware laws had been relatively friendly to hostile takeovers (Daines, 2001). However, several court rulings in Delaware in the mid-1990s, such as *Unitrin Inc. v. American General Corp.* (1995) and *Moore v. Wallace Computer* (1995), resulted in a significant change in Delaware's takeover regime. The court rulings legitimized the use of poison pills in conjunction with a staggered board for Delaware firms to solidify the power of managers to "just say no" to hostile bidders (Gilson, 2001). Poison pills are a widely adopted takeover defense that can dilute the share ownership of the acquirer by allowing the target firm's existing shareholders to purchase shares at a discount. A staggered board restricts the replacement of a firm's directors to a certain fraction in a year, and the election route entails a minimum of two years to change control of the board. These provisions gave Delaware firms a very effective defense against hostile acquirers since the acquirer cannot immediately remove the incumbent directors, who in the meantime may adopt a poison pill to reduce the target firm's value should a takeover occur. Consistent with this argument, Rauh (2006) reports a decrease in the probability of takeover for Delaware firms after 1996. Our own assessment of the effect of Delaware's takeover regime change using data from Thomson Financial's SDC Platinum gives a similar picture. As shown in Fig. 1, while the number of all acquisition bids for Delaware firms tapered off after 1996 (Panel A), the number of hostile bids for Delaware firms dropped sharply (Panel C). This significant decline stands in contrast with the trend of a steady increase in the number of all acquisition bids as well as the number of hostile bids for comparison firms shown in the same panels.

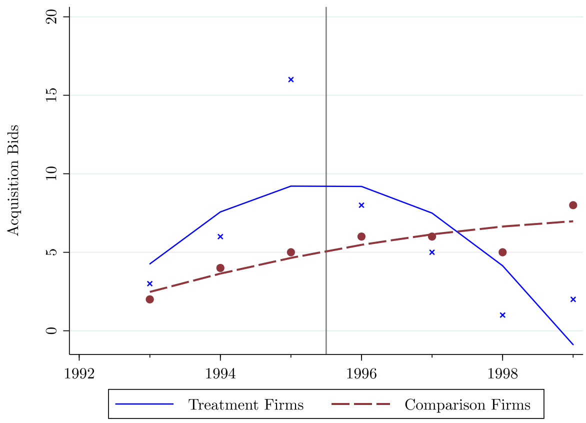
Given that state antitakeover laws are typically supported by a small number of exclusive groups of firms, rather than by large business or political coalitions (Romano, 1987), we assume that the legal changes in Delaware are exogenous events for most firms and therefore can be used as a natural experiment. In particular, following the takeover regime change, the fear of a hostile takeover and job loss was reduced significantly for executives of Delaware firms, who according to prior research (Low, 2009) exhibit a preference for seeking a quiet life and avoidance of efforts to increase firm value. In addition, using Delaware's legal change as a natural experiment is attractive for our purpose because Delaware's legal change occurred in the mid-1990s, which was separate from other major changes in takeover laws, such as the second-generation antitakeover laws in late 1980s (Rauh, 2006). The Delaware experiment therefore mitigates identification problems due to clustering of various legal changes in a short period (Yun, 2009). In addition, because firms rarely change their state of incorporation (Bertrand and Mullainathan, 2003), and all Delaware-incorporated firms were affected by the legal change regardless of their state of operation, we are able to control for shocks specific to a state of operation and year.



Panel A: All acquisition bids



Panel B: Unfriendly acquisition bids



Panel C: Hostile acquisition bids

Fig. 1. Number of acquisition bids targeting firms in the treatment and control groups**.**
Notes: The above figures plot data from SDC Platinum showing the count of acquisition bids targeting firms in the treatment set (public firms incorporated in Delaware with a staggered board), compared to firms in the control set (all other public firms). Actual counts are marked with a dot (for "treatment firms") or cross (for "comparison firms"); trends are plotted with a kernel-weighted local linear regression and Epanechnikov kernel. Unfriendly bids are deals where SDC did not rate the deal as "friendly." Hostile bids are deals where SDC did rate the deal as "hostile.".

Table 1
Variable definitions.

	Variable	Definition	Data source
1	*New patents*	Count of new patent applications (eventually granted) in focal year	NBER/Hall et al.
2	*Forward citations*	Total number of citations received from other patents in next 5 years	NBER/Sampat
3	*Patent classes*	Total number of ICL Patent classes filed by firm in focal year	NBER/Li et al.
4	*Back1 ratio*	Ratio of back-cites to patents within 1 year to total back-cites	NBER/Li et al.
5	*New inventors*	Number of new-to-the-firm inventors filing a patent in focal year	NBER/Li et al.
6	*Cross-border*	Indicator for firms with at least one foreign inventor in focal year	NBER/Li et al.
7	*State employment*	Change in employment level in state of corporate headquarters	BLS
8	*State income*	Aggregate personal income in state of corporate headquarters	BLS
9	*State job growth*	New job creation in state of corporate headquarters	BLS
10	*State BC law*	Indicator for business combination law in state of incorporation	Giroud & Mueller
11	*Labor market rivals*	Number of competitors within the same focal labor market area	Compustat/BLS
12	*Labor market population*	Number of people living within the same focal labor market area	Compustat/BLS
13	*Assets (log)*	Natural logarithm of total firm assets	Compustat
14	*Employees (log)*	Natural logarithm of total number of firm employees	Compustat
15	*R&D stock (log)*	Accumulated "stock" of R&D - prior 5 years (logged)	Compustat
16	*Patent stock (log)*	Accumulated "stock" of patents - prior 5 years (logged)	NBER/Li et al.
17	*Inventor stock*	Accumulated "stock" of inventors - prior 10 years	NBER/Li et al.
18	*Firm age*	Proxy for firm age based on years firm has been publically listed	Compustat
19	*CapEx intensity*	Ratio of Capital Expenditure to Total Assets	Compustat
20	*Revenue growth*	Percentage change in gross revenue over prior 3 years	Compustat
21	*Net income*	Net income	Compustat
22	*Return on assets*	Return on assets	Compustat
23	*Liquidity*	(Current Assets – Current Liabilities) / Total Assets	Compustat
24	*Leverage*	Ratio of Book Value of Debt to Market Value of Equity	Compustat
25	*Beta*	12-month correlation of stock returns to market returns	CRSP
26	*OrgSize*	Average count of employees	Compustat
27	*Competition*	Median Lerner Index by SIC-3, avg. and constant in after period	Compustat
28	*DE*	Indicator for firms incorporated in DE with a staggered board	RiskMetrics
29	*After*	Indicator for the years 1996–1999	Compustat

The Delaware 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 one is unable to randomly assign subjects into a treatment group versus a control group. In this analysis, we assign firms to the 'treatment group' when data from *RiskMetrics* indicates that they were incorporated in Delaware and had a staggered board of directors at the approximate time of the policy change; all other firms were assigned to the 'comparison group.' By assuming that trends in the comparison group represent trends in what would have happened to firms in the treatment group in the absence of treatment, the DD identifies a causal treatment effect as the before-to-after difference for the treatment group, netting out trends from the comparison group. As such, a DD analysis removes observed and/or unobserved differences between treatment and control, provided that those differences remain fixed over time (Wooldridge, 2002).

3.2. Sample and data

The sample includes annual, firm-level data from Compustat for 1992 through 1999, the 4-year window before and 4-year window after the regime change in Delaware. We include all U.S. firms publically listed prior to 1996; firms listed for the first time in 1996 or later are excluded from the sample to exclude the possibility that the experiment might affect firms' decision to go public. Financial instruments (e.g., ADRs and ETFs) and securities used internally within firms (i.e., CUSIPs ending in 990–999 or 99A-99Z) are excluded from the sample. The final sample is comprised of 6759 observations and is approximately balanced between observations for the treatment group ($n = 3376$) and observations for the control group ($n = 3383$), as well as between observations in the before period 1992–1995 ($n = 3185$) and observations in the after period 1996–1999 ($n = 3574$). To supplement the Compustat sample with additional data for analysis, we merge in patent data from the Electronic Information Products Division of the USPTO (Sampat, 2011), patent-to-corporation disambiguation data from the NBER patent file (Hall et al., 2001), inventor disambiguation data from the Patent Network Dataverse at Harvard University (Li et al., 2014), state and labor-market data from the Bureau of Labor Statistics (BLS), and other data noted in Table 1.

3.3. Dependent variables

In line with the hypotheses set forth earlier, we examine two types of outcomes: (1) Outcomes related to the *rate* of innovation by firms; and (2) Outcomes related to the *scope* of innovation by firms. We analyze multiple dependent variables in each area to establish a consistent pattern of evidence for each hypothesis.

Rate of innovation. In our first section of analysis (corresponding to our Hypothesis 1), we test how the change in takeover threat affected the rate of innovation for treated firms. Here we test two outcomes frequently examined in the literature:

innovation *quantity* and innovation *quality*. For innovation quantity, we follow Griliches (1981) and Jaffe (1986) and examine *New Patents*, the count of (eventually granted) new patent applications in a focal year (Hall et al., 2001; Czarnitzki and Toole, 2011). For innovation quality, we follow (Trajtenberg 1990; Hall et al., 2005) and examine *Forward Citations*, the count of patent forward citations received by a focal patent from subsequent patents over the following 5 years.[4] In this first analysis, we focus on the two variables widely used in the innovation literature and recently also used in research on antitakeover laws (Atanassov, 2013). In the next section, we focus on several newer variables that shed more light on the direction of innovation.

Scope of innovation. In our second section of analysis (corresponding to Hypothesis 2), we test how the change in takeover threat affected the scope of innovation for treated firms. Consistent with our argument that executives protected from takeover threats may see their firms explore less broadly in technological, temporal, and or geographic spaces, we test four outcomes: technological breadth, innovation timeliness, inventor recruitment, and innovation offshoring. To assess technological breadth, we measure *Patent Classes*, the number of different (unique) international patent classifications on patents applied for by a focal firm in a focal year. A decrease in the measure of *Patent Classes* indicates that the firm is innovating in fewer technological areas. To assess the timeliness of innovation, we calculate *Back1 Ratio*, the ratio of the count of backward citations made by a focal patent to other patents filed within the previous year (numerator of ratio) to the total count of backward citations made by a focal patent (denominator of ratio). A decrease in *Back1 Ratio* indicates that a firm is referencing less of the cutting-edge innovation that is happening in the technology space.[5] To assess the utilization of new innovative human capital in the firm, we measure *New Inventors*, the count of inventors listed on firm patents in a given year who have never been observed as patenting at that firm in prior years. A decrease in *New Inventors* indicates that a firm utilizes less new human capital in the pursuit of innovation. To assess the degree of offshoring innovation to foreign R&D operations, we measure *Cross-Border* as a binary indicator variable that equals one when one or more patents for the firm list an inventor as currently residing outside of the U.S. A decrease in *Cross-Border* indicates that a (U.S.) firm is reducing its effort to coordinate multinational R&D across geographic spaces. Our use of patent-based innovation scope measures is consistent with recent research (Gao et al., 2018).

3.4. Explanatory variables

Difference-in-differences. The DD 'treatment group' variable, *Delaware* (or "*DE*"), is an indicator variable that equals one if a firm was incorporated in Delaware and had a staggered board of directors. The state of incorporation was based on data from Compustat (historical files) and crosschecked with data from RiskMetrics; we identified firms with staggered boards based on data from RiskMetrics. The DD 'after' variable, *After*, is an indicator variable that equals zero for the years 1992 through 1995, and one for the years 1996 through 1999. *After* is collinear with a full set of year fixed effects and therefore does not appear directly in our model specifications, but the variable is used in constructing higher-level interactions. Following prior DD research (Meyer, 1995), we create an interaction variable *Delaware***After* and use this variable to identify the main treatment effect of the Delaware natural experiment.

Organization Size (triple-difference-in-differences). Next, to test the moderating effect of organizational size captured in H3, we compute the variable *OrgSize* as the count of firm employees as reported in Compustat, and interact it with the DD treatment effect identified above, giving rise to a triple interaction variable defined as *Delaware***After***OrgSize*. We compute the variable *OrgSize* as the average count of *Employees* over the DD 'before' period, and then hold that value constant in the 'after' period to prevent endogeneity in the measure due to the natural experiment itself. *OrgSize* is mean centered to simplify the interpretation of interaction effects.

Market competition (triple-difference-in-differences). Finally, to test the moderating effect of product market competition in H4, we compute the variable *Competition* and interact it with the DD treatment effect identified above, giving rise to a triple interaction variable defined as *Delaware***After***Competition*. We compute *Competition* as the median, industry-adjusted, Lerner Index (Lerner, 1934) at the 3-digit SIC level, based on Hou and Robinson's (2006) suggestion that 3-digit SIC classifications provide an appropriate level of aggregation to allow firms in related businesses to be grouped together. Using the Lerner Index as our product market competition measure is consistent with prior work on the relationship between product market competition and firm performance (Nickell, 1996) as well as more recent work on competition and innovation (Aghion et al., 2005; Aghion et al., 2013). Following this research, we calculate the price cost margin (*PCM*) for each firm using data provided by Compustat[6] as:

$$\mathrm{PCM}_i = (\mathrm{SALE}_i - \mathrm{COGS}_i - \mathrm{XSGA}_i)/\mathrm{SALE}_i, \quad (1)$$

[4] We also examined outcomes for patent forward citations over 10, 15, and 20 years and obtained similar results. In addition, when we calculated an alternative measure of forward citations that are demeaned from the average forward citation number of all patents from the same filing year and same Hall et al. (2001) technology class, we obtained highly consistent results.

[5] Average backward citation lag has been used in prior research as a measure of organizational obsolescence (Sørensen and Stuart, 2000). We find consistent results across the board when using average backward citation lag, but at levels that are not statistically significant or marginally significant in several model specifications. Given the short longitudinal timeframe of our study, however, we use *Back1 Ratio* as a more sensitive measure of how firms are staying current with cutting-edge technological developments.

[6] Compustat variables are defined as: `SALE` = sales/turnover (net); `COGS` = cost of goods sold; `XSGA` = sales, general and administrative expenses; `OIBDP` = operating income before depreciation.

substituting $\text{PCM}_i = \text{OIBDP}_i/\text{SALE}_i$ in cases where there was missing data for COGS or XSGA. We winsorize *PCM* to + 1 and −1 around the mean of *PCM* to mitigate the influence of outliers. Next, we compute a sales-weighted, industry-adjusted, firm-specific measure of market power for each firm, as:

$$\text{MarketPower}_i = \text{PCM}_i - \sum\nolimits_J^N \omega_j \text{PCM}_j \qquad (2)$$

PCM_i is the price cost margin for firm *i* calculated above; ω_j is the proportion of total revenue contributed by each firm *j* in the same SIC-3 industry as firm *i*; PCM_j is the price cost margin for each firm *j* in the same SIC-3 industry as firm *i*; the sales-weighted price cost margins are summed over the N firms in the same SIC-3 industry as firm *i*. We take the median value of *MarketPower* by SIC-3 to derive an industry-level measure of product market competition. In computing *MarketPower*, we use the entire sample of Compustat firms in each industry, not only those in our patenting sample. Finally, we compute the variable *Competition* as the average level of *MarketPower* over the DD 'before' period, and then hold that value constant in the 'after' period to prevent endogeneity in the measure due to the natural experiment itself. *Competition* is mean centered to simplify the interpretation of interaction effects.[7]

Control variables. We add covariate controls into our models to adjust for differences in trends between treated firms and comparison firms. While trends over time in the comparison set of firms serve as the basic control in a difference-in-differences specification, additional covariates can control for specific state, labor market, and firm differences (Belderbos et al., 2008). At the state level (based on the physical headquarters of each firm, *not* the state of incorporation), we control for state employment (*State Employment*), the logged level of state income (*State Income*), and the rate of new job creation (*State Job Growth*). We also control (by state of incorporation) for the effect of earlier changes in business combination laws by including an indicator variable (*State BC Law*) using data reported in prior studies (Bertrand and Mullainathan, 2003; Giroud and Mueller, 2010). At the labor market level, we control for the population size of the focal firm's labor market (*Labor Market Population*) and the number of competitors within the same labor market (*Labor Market Rivals*) based on data provided by the Bureau of Labor Statistics. At the firm level, we control for firm size with *Assets* (logged), *Employees* (logged), *R&D Stock* (logged), *Patent Stock* (logged), and *Inventor Stock* (logged), measured as the count of unique inventors observed at the firm over the previous 10 years. We include a proxy for *Firm Age* based on the number of years since the public listing of the firm. Research suggests that firms' financial conditions also affect innovation (Almeida et al., 2017), thus we control for the following financial variables: *Capital Expenditure Intensity, Revenue Growth, Net Income, Return on Assets, Liquidity, Leverage*, and *Beta*. Finally, we control for year fixed effects and industry fixed effects at the SIC-3 level (Clougherty and Seldeslachts, 2013). In alternative specifications where we include firm fixed effects, we obtain consistent results (for details, see Section 4.5 *Robustness Checks*). All variable definitions and data sources are summarized in Table 1.

3.5. Model specifications

We follow a difference-in-differences interaction technique to identify the following effects:

$$\beta_0 + \beta_1 (DE^*After) + \beta_2 (DE^*After^*Moderator) + \alpha \qquad (3)$$

where β_0 is an intercept; β_1 identifies the DD treatment effect; β_2 identifies the triple-DD treatment effect of the moderator (*Organization Size*, or *Market Competition*); and α represents all other predictors in the model (lower-order interaction terms, covariate controls, and fixed effects). However, given the different types of dependent variables in our analysis (i.e., *New Patents, Forward Citations, Patent Classes, Back1 Ratio, New Inventors*, and *Cross-Border*), we employ several different estimation models to test these effects. We use the same set of right-hand-side predictors in each model. We cluster robust standard errors by firm to account for heteroskedasticity and the non-independence of repeated observations by firm. Each type of model is described below in more detail.

OLS models. We use ordinary least squares (OLS) to analyze *Back1 Ratio* and *Cross-Border* with the following equation:

$$Y_{it} = \beta_0 + \beta_1 (DE_i{}^*After_t) + \beta_2 (DE_i{}^*After_t{}^*Moderator_{it}) + \boldsymbol{D}_{it} + \boldsymbol{X}_{it} + \boldsymbol{T}_t + \boldsymbol{I}_j + \boldsymbol{\varepsilon}_{it} \qquad (4)$$

$\mathbf{Y}_{it}$ is the dependent variable for firm *i* in year *t*; $\boldsymbol{\beta_0}$ is the intercept; $\boldsymbol{\beta_1}$ is the main DD treatment effect; $\boldsymbol{\beta_2}$ is the triple-DD treatment effect; $\boldsymbol{D_{it}}$ are the lower-order DD interactions; $\boldsymbol{X_{it}}$ is a vector of covariate controls for firm *i* in year *t*, $\boldsymbol{I_j}$ is a vector of industry fixed effects at the SIC-3 industry level, $\boldsymbol{T_t}$ is a vector of year fixed effects, and $\boldsymbol{\varepsilon_{it}}$ is the error term. *Back1 Ratio* ($mean = 0.065$, $sd = 0.099$, $skew = 3.740$) is a ratio with a right-skewed distribution; we therefore also run log-linear models of these outcomes with a log-transformed dependent variable. *Cross-Border* is a binary dependent variable; we therefore checked the predicted values of the model and found 97% of predicted outcomes to fall within the interval (−0.1, 1.1).

[7] Prior research has used both the *Herfindahl Index* and the *Price Cost Margin* to measure product market competition. We use the *Price Cost Margin* for two reasons: First, the *Herfindahl Index* is calculated at an industry level, whereas the *Price Cost Margin* is calculated at the firm level and the rest of our analysis is at the firm level. Furthermore, competitive pressure may originate from between industries, as well as within industries, for the threat of new-entry can be a potent source of competitive pressure (Raith, 2003). Second, it is often the case that firms compete in global product markets even though a disproportionate amount of R&D is conducted in the home country. The *Price Cost Margin* implicitly accounts for product market competition in both home and foreign markets.

Negative binomial models. We use negative binomial count models to analyze *New Patents, Forward Citations, Patent Classes*, and *New Inventors*. The negative binomial model is estimated by the following equation:

$$\mu_{it} = \mathbf{exp}\left(\beta_0 + \beta_1 (DE_i{}^{*}After_t) + \beta_2 (DE_i{}^{*}After_t{}^{*}Moderator_{it}) + \boldsymbol{D}_{it} + \boldsymbol{X}_{it} + \boldsymbol{T}_t + \boldsymbol{I}_j + \boldsymbol{\varepsilon}_{it}\right) \tag{5}$$

μ_{it} is the count rate of the dependent variable for firm i in year t; all other variables are the same as defined earlier. In the case of *Patent Classes*, we use a zero-truncated negative binomial model to account for the fact that every patent must be assigned to at least one patent class and the count distribution for *Patent Classes* must therefore start at one (not zero).[8] In each model we test for overdispersion and find the negative binomial regression model to be preferred over a Poisson regression model. We also check the validity of solutions arrived at by MLE.[9]

Log-linear models. Finally, we estimate log-linear OLS models for all of our outcome variables (with the exception of *Cross-Border*, where we continue to use a binary dependent variable and a linear probability model) with the following equation:

$$ln(1 + Y_{it}) = \beta_0 + \beta_1 (DE_i{}^{*}After_t) + \beta_2 (DE_i{}^{*}After_t{}^{*}Moderator_{it}) + \boldsymbol{D}_{it} + \boldsymbol{X}_{it} + \boldsymbol{T}_t + \boldsymbol{I}_j + \boldsymbol{\varepsilon}_{it} \tag{6}$$

We cross-check the robustness of our results by testing log-linear models because simulation evidence suggests that log models and log-linear models can sometimes vary substantially in terms of bias and precision when data are skewed, especially in cases of positive-valued outcomes with heavy right-tails (Lerner, 1994; Manning and Mullahy, 2001).

4. Empirical results

Table 2 reports descriptive statistics for the dependent variables (Panel A) and independent variables (Panel B). Table 3 reports univariate DD analyses of the dependent variables representing the rate of innovation (Columns 1 and 2), and scope of innovation (Columns 3–6). The numbers in the first row represent the difference in the mean values in the 'before' period and the 'after' period for each dependent variable for the treatment group (firms incorporated in Delaware with a staggered board), while the numbers in the second row represent such difference for the comparison group (all other firms). The numbers in the bottom row report the difference in the two differences (i.e., difference-in-differences) above, and they all take on a negative value. This shows that Delaware's takeover regime change caused an across-the-board drop in the rate and scope of the treated firms' innovations.

4.1. Rate of innovation

We now move on to multivariate DD analyses to examine our hypotheses using regressions. Table 4 first reports results for two dependent variables related to the rate of innovation by firms: *New Patents* (Column 1) and *Forward Citations* (Column 2). All models include a full set of control variables as well as year fixed effects and industry fixed effects at the 3-digit SIC level. While the comparison group serves as the basic control mechanism in a DD specification, inclusion of specific covariates can help to strengthen the equal trends assumption (Blundell and Costa-Dias, 2000). Our first hypothesis predicts that the Delaware takeover regime change, which increases takeover protection for Delaware firms, will cause a drop in the rate of innovation for firms. The results provide strong support for this hypothesis, as indicated by the negative and significant coefficients for the interaction term *DE*After* in both columns. Thus, the evidence indicates that Delaware firms have fewer patent grants and receive fewer citations following the takeover regime change.

4.2. Scope of innovation

Our second hypothesis predicts that Delaware's takeover regime change will also reduce firms' scope of innovation, because search over an expanded technological, temporal, or geographic space places additional strain on managers' limited capacity. To test this hypothesis, we examine four dependent variables related to firms' scope of innovative activities, also presented in Table 4: *Patent Classes, Back1 Ratio, New Inventors*, and *Cross-Border*. The results provide strong support for the prediction as well: the coefficients for *DE*After* are negative and significant in all four columns. Specifically, we find that following the takeover regime change, Delaware firms innovate in fewer technological areas (Column 3), reference less to cutting-edge technological developments in their innovative efforts (Column 4), have fewer new inventors to develop innovations (Column 5), and reduce their efforts to coordinate cross-border multinational R&D (Column 6).

4.3. Organization size

Our third hypothesis predicts that the negative effect of takeover protection on innovation will be weaker for larger firms where innovation tasks are more likely to be delegated down the organizational hierarchy to middle managers and thus top executives are less involved in innovation activity ex ante. We test this hypothesis in Table 5, where we regress each of

[8] Using a zero-truncated model strengthens the magnitude of the effect because the conditional count rate starts with at least one patent class; doing so lowers statistical significance and makes the analysis more conservative.

[9] Maximum likelihood estimation can converge to spurious solutions in count models when there are perfectly collinear regressors for a subsample of observations where the dependent variable equals zero (Santos Silva and Tenreyro, 2010). In such cases, we calculate starting values with a log-linear model.

Table 2
Descriptive statistics.

		Mean	SD	Min	Max	
Panel A: Dependent variables ($n = 6759$).						
1	New patents	31.73	118.27	1	2285	
2	Forward citations	174.50	778.06	0	16,140	
3	Patent classes	14.19	43.79	1	714	
4	Back1 ratio	0.07	0.10	0	1	
5	New inventors	14.52	45.46	0	813	
6	Cross-border	0.22	0.42	0	1	
Panel B: Independent variables						
		Mean overall	SD overall	Mean 1992–95	Mean treated 92–95	Mean control 92–95
7	State employment	0.02	0.01	0.02	0.01	0.02
8	State income (log)	19.40	0.90	19.28	19.42	19.15
9	State job growth	2.14	2.38	1.68	1.50	1.86
10	State BC law	0.92	0.27	0.92	1.00	0.85
11	LMA rivals	12.37	19.00	10.66	12.62	8.81
12	LMA population	23,553	15,297	23,559	24,023	23,125
13	Assets (log)	5.63	1.83	5.50	5.21	5.78
14	Employees (log)	1.28	1.18	1.28	1.05	1.51
15	R&D stock (log)	2.93	1.95	2.77	2.55	2.98
16	Patent stock (log)	2.84	1.49	2.75	2.48	3.00
17	Inventor stock	106.52	388.18	110.63	70.87	147.90
18	Firm age	12.48	7.34	11.66	9.64	13.55
19	CapEx intensity	0.06	0.05	0.06	0.06	0.07
20	Revenue growth	2.27	27.48	2.34	4.16	0.63
21	Net income	90.82	510.20	61.80	36.17	85.82
22	ROA	−0.01	0.22	−0.01	−0.04	0.03
23	Liquidity	0.34	0.23	0.35	0.37	0.33
24	Leverage	0.39	0.22	0.39	0.39	0.38
25	Beta	1.25	2.03	1.37	1.46	1.29
26	OrgSize	0.00	28.15	0.13	−3.27	3.31
27	Competition	0.00	0.06	0.00	0.00	0.00
28	DE	0.50	0.50	0.48	1	0
29	After	0.53	0.50	0	0	0
	Observations	6759	6759	3185	1541	1644

Notes: Sample includes firm-level data from Compustat for 1992 through 1999 for public firms that filed at least one patent during the sample period. The sample is approximately balanced between treated observations ($n = 3376$) and control observations ($n = 3383$), as well as between observations in the before period 1992–1995 ($n = 3185$) and observations in the after period 1996–1999 ($n = 3574$). The variables *OrgSize* and *Competition* were mean-centered to zero to facilitate interpretation of interactions.

Table 3
Univariate difference-in-differences.

	Rate of innovation			Scope of innovation			
	New patents (1)	*Forward citations* (2a)	*Forward citations*[a](2b)	*Patent classes* (3)	*Back1 ratio* (4)	*New inventors* (5)	*Foreign innovation* (6)
Before-to-after differences							
Delaware (DE staggered boards)	2.070	31.73	4	0.529	0.003	1.114	0.049
Comparison (all other firms)	10.338	92.61	5	1.983	0.012	3.813	0.069
Difference-in-differences							
	−8.268	**−60.88**	**−1**	**−1.454**	**−0.009**	**−2.699**	**−0.020**

Notes: This table summarizes the mean before-to-after difference for firms incorporated in Delaware with a staggered board, as compared to all other firms; the 'difference-in-differences' reflects the difference in those two differences.
*[a] Given skewed distribution of forward citations, we report median differences for patent forward-5-year citations in Column 2b.

our six dependent variables on a triple DD interaction term *DE*After*OrgSize*, the corresponding two-way DD interaction terms, as well as a full set of control variables and year and industry fixed effects. In Table 5, we see that the coefficients for *DE*After* OrgSize* are positive and significant in all columns (except for Column 6, where the *p*-value is 0.12), suggesting that the negative effect of Delaware's takeover regime change on firms' rate and scope of innovation is weaker (stronger) for larger (smaller) firms in Delaware. We also find the coefficients for *DE*After* to be negative and significant in all columns (except for Column 4, where the *p*-value is 0.13), consistent with the results reported earlier.

4.4. Product market competition

Our last hypothesis focuses on the role of product market competition in mitigating executives' tendencies to economize on effort and thus alleviating the negative effect of takeover protection on innovation. We test this hypothesis in Table 6,

Table 4
Difference-in-differences in the rate and scope of innovation.

	New patents (1)	*Forward citations* (2)	*Patent classes* (3)	*Back1 ratio* (4)	*New inventors* (5)	*Cross-border* (6)
State employment	−0.4565	1.1268	−0.5036	0.1619	−0.6488	−0.4299
	(1.093)	(1.988)	(1.234)	(0.112)	(1.321)	(0.531)
State income (log)	0.0129	0.0577+	0.0042	0.0031+	0.0244	0.0203*
	(0.019)	(0.032)	(0.019)	(0.002)	(0.021)	(0.009)
State job growth	−0.0021	0.0050	−0.0046	0.0002	0.0010	0.0007
	(0.004)	(0.006)	(0.005)	(0.000)	(0.005)	(0.002)
State BC law	−0.0273	0.0483	−0.0796	−0.0113*	−0.0897	−0.0047
	(0.054)	(0.090)	(0.063)	(0.006)	(0.065)	(0.026)
Labor market rivals	0.0015	0.0036*	0.0010	0.0003*	0.0022+	−0.0004
	(0.001)	(0.002)	(0.001)	(0.000)	(0.001)	(0.001)
Labor market pop.	0.0000	−0.0000	0.0000	0.0000	−0.0000	−0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Assets (log)	0.1813***	0.2378***	0.2389***	0.0073**	0.2727***	0.0090
	(0.023)	(0.038)	(0.025)	(0.003)	(0.028)	(0.011)
Employees (log)	0.0142	−0.0042	−0.0026	−0.0070+	0.1062**	0.0471*
	(0.032)	(0.058)	(0.033)	(0.004)	(0.041)	(0.018)
R&D stock (log)	0.0335**	0.0567**	0.0375**	0.0013	0.0737***	0.0078
	(0.013)	(0.019)	(0.013)	(0.001)	(0.015)	(0.006)
Patent stock (log)	0.7340***	0.6660***	0.7362***	−0.0014	0.3656***	0.1032***
	(0.017)	(0.025)	(0.018)	(0.002)	(0.020)	(0.008)
Inventor stock	0.0001*	0.0002**	−0.0000	0.0000+	0.0002***	0.0001*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Firm age	−0.0291***	−0.0446***	−0.0277***	−0.0004	−0.0270***	−0.0029*
	(0.002)	(0.004)	(0.003)	(0.000)	(0.003)	(0.001)
CapEx intensity	1.2439***	2.0556***	1.2413***	0.0628*	1.0660***	0.0093
	(0.249)	(0.381)	(0.259)	(0.032)	(0.314)	(0.108)
Revenue growth	0.0014*	0.0012	0.0005	0.0001	0.0006	0.0002
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)
Net income	0.0000	0.0000	−0.0000	0.0000*	0.0000	−0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ROA	−0.1449+	−0.1922+	−0.1501*	−0.0197*	−0.1321+	0.0281
	(0.075)	(0.114)	(0.073)	(0.008)	(0.077)	(0.028)
Liquidity	0.2640**	0.6083***	0.2716**	0.0103	0.1698	0.0327
	(0.089)	(0.153)	(0.094)	(0.009)	(0.107)	(0.040)
Leverage	−0.0490	−0.2143+	−0.0841	0.0020	−0.0763	0.0517
	(0.078)	(0.125)	(0.077)	(0.008)	(0.083)	(0.037)
Beta	−0.0085	−0.0058	−0.0097	0.0001	0.0040	0.0034+
	(0.006)	(0.013)	(0.007)	(0.001)	(0.006)	(0.002)
DE	0.1235***	0.1574*	0.1326***	0.0083*	0.0735+	0.0113
	(0.035)	(0.062)	(0.038)	(0.004)	(0.042)	(0.018)
DE * After	**−0.1526*****	**−0.2512*****	**−0.1135***	**−0.0098***	**−0.1424****	**−0.0417***
	(0.046)	(0.068)	(0.048)	(0.005)	(0.050)	(0.019)
Constant	−1.5452*	−0.1260	−1.6616***	−0.0890**	−1.0064*	−0.6504**
	(0.755)	(0.667)	(0.429)	(0.034)	(0.450)	(0.199)
Observations	6759	6759	6759	6759	6759	6759
Model	NB	NB	T-NB	OLS	NB	OLS
R^2 or [Log likelihood]	[−20,887]	[−31,844]	[−15,527]	0.117	[−18,920]	0.329

Notes: All models include year fixed effects and industry SIC-3 fixed effects. The main effect of *After* is collinear with year fixed effects. Robust standard errors are reported in parentheses and clustered by firm. Columns 1, 2, and 5 are estimated with a negative binomial model. Column 3 is estimated with a Truncated Negative Binomial model to adjust for count outcomes that start at one (not zero); Columns 4 and 6 are estimated by OLS. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.10$, two-tailed tests.

where we regress each of our six dependent variables on a triple DD interaction term *DE*After*Comp*, the corresponding two-way DD interaction terms, as well as a full set of control variables and year and industry fixed effects. In Table 6, we indeed see that the coefficients for *DE*After*Comp* are positive and significant in all columns (except for Column 5, where the *p*-value is 0.11), suggesting that the negative effect of Delaware's takeover regime change on firms' rate and scope of innovation is stronger (i.e., more negative) for Delaware firms that faced lower levels of product market competition before the change. Note that the difference in the magnitudes of the two triple-DD coefficients (*DE*After*OrgSize* and *DE*After*Comp*) is due to the different magnitudes and distributions of the two moderating variables (*OrgSize* and *Comp*, respectively). Finally, we also find the coefficients for *DE*After* to be negative and significant in all columns, consistent with the results reported earlier.

4.5. Robustness checks

Inventor concentration. In Hypothesis 3, we theorized that the main effect on the rate and scope of innovation is likely to be attenuated in larger organizations, where innovation decisions are more often pushed down the hierarchy to middle

Table 5
Triple difference-in-differences by *OrgSize.*

	New patents (1)	*Forward citations* (2)	*Patent classes* (3)	*Back1 ratio* (4)	*New inventors* (5)	*Cross- border* (6)
State employment	−0.4055	0.7976	−0.3936	0.1606	−0.1949	−0.3971
	(1.111)	(1.995)	(1.251)	(0.114)	(1.337)	(0.537)
State income (log)	0.0108	0.0479	0.0001	0.0029+	0.0277	0.0208*
	(0.019)	(0.032)	(0.020)	(0.002)	(0.022)	(0.009)
State job growth	−0.0021	0.0044	−0.0049	0.0003	0.0010	0.0008
	(0.004)	(0.006)	(0.005)	(0.000)	(0.005)	(0.002)
State BC law	−0.0521	0.0347	−0.0981	−0.0101+	−0.1218+	−0.0131
	(0.056)	(0.092)	(0.064)	(0.006)	(0.067)	(0.027)
Labor market rivals	0.0018+	0.0027	0.0010	0.0003**	0.0022+	−0.0003
	(0.001)	(0.002)	(0.001)	(0.000)	(0.001)	(0.001)
Labor market pop.	0.0000	0.0000	0.0000	0.0000	−0.0000	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Assets (log)	0.1723***	0.2392***	0.2377***	0.0078**	0.2680***	0.0022
	(0.024)	(0.039)	(0.026)	(0.003)	(0.029)	(0.011)
Employees (log)	0.0367	−0.0026	0.0048	−0.0067	0.1293**	0.0689***
	(0.037)	(0.064)	(0.039)	(0.004)	(0.046)	(0.020)
R&D stock (log)	0.0391**	0.0639***	0.0402**	0.0013	0.0795***	0.0094
	(0.013)	(0.018)	(0.014)	(0.001)	(0.015)	(0.006)
Patent stock (log)	0.7336***	0.6624***	0.7377***	−0.0019	0.3626***	0.1012***
	(0.017)	(0.025)	(0.018)	(0.002)	(0.021)	(0.008)
Inventor stock	0.0001***	0.0003**	−0.0000	0.0000	0.0003***	0.0001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Firm age	−0.0288***	−0.0451***	−0.0273***	−0.0004	−0.0269***	−0.0028*
	(0.003)	(0.004)	(0.003)	(0.000)	(0.003)	(0.001)
CapEx intensity	1.2444***	2.0056***	1.2505***	0.0625+	1.1430***	−0.0079
	(0.252)	(0.388)	(0.266)	(0.033)	(0.327)	(0.109)
Revenue growth	0.0014+	0.0009	0.0002	0.0000	0.0005	0.0002
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)
Net income	0.0000	0.0000	−0.0000	0.0000*	0.0000	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ROA	−0.0827	−0.1428	−0.1409+	−0.0204*	−0.1314+	0.0285
	(0.062)	(0.114)	(0.073)	(0.009)	(0.078)	(0.028)
Liquidity	0.2625**	0.6352***	0.2917**	0.0112	0.2099+	0.0367
	(0.088)	(0.147)	(0.096)	(0.009)	(0.111)	(0.042)
Leverage	−0.0470	−0.1406	−0.0611	−0.0029	−0.0532	0.0525
	(0.077)	(0.126)	(0.079)	(0.008)	(0.086)	(0.038)
Beta	−0.0054	0.0130	−0.0079	−0.0000	0.0017	0.0035
	(0.006)	(0.008)	(0.007)	(0.001)	(0.006)	(0.002)
DE	0.1239***	0.1516*	0.1387***	0.0074*	0.0701+	0.0097
	(0.035)	(0.061)	(0.039)	(0.004)	(0.043)	(0.018)
DE * After	−0.1431**	−0.2214**	−0.1207*	**−0.0076**	−0.1303**	−0.0344+
	(0.047)	(0.067)	(0.049)	(0.005)	(0.050)	(0.019)
Org. size	−0.0015*	−0.0009	−0.0007	0.0000	−0.0023**	−0.0010+
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
DE * OrgSize	−0.0037*	−0.0033	−0.0023	−0.0003*	−0.0022	−0.0018+
	(0.002)	(0.002)	(0.002)	(0.000)	(0.002)	(0.001)
After * OrgSize	−0.0000	−0.0004	−0.0003	0.0000	0.0001	−0.0005
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
DE*After*OrgSize	0.0063**	0.0091***	0.0044*	0.0003*	0.0059*	**0.0015**
	(0.002)	(0.003)	(0.002)	(0.000)	(0.002)	(0.001)
Constant	−1.4797*	−0.0175	−1.6166***	−0.0866*	−1.0962*	−0.6429**
	(0.754)	(0.675)	(0.440)	(0.035)	(0.466)	(0.205)
Observations	6,566	6,566	6,566	6,566	6,566	6,566
Model	NB	NB	T-NB	OLS	NB	OLS
R^2 or [LL]	[−20,880]	[−31,826]	[−15,482]	0.111	[−18,899]	0.322

Notes: All models include year and SIC-3 fixed effects. The main effect of *After* is collinear with year fixed effects. Robust standard errors are reported in parentheses and clustered by firm. Columns 4 and 6 are estimated by OLS; Column 6 is a linear probability model; Columns 1, 2, 5 are estimated by Negative Binomial; Column 3 uses a Truncated Negative Binomial model to adjust for a count DV that starts at one, not zero. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.10$, two-tailed tests.

managers. Nevertheless, large firms are sometimes highly centralized, and they may also be less likely to be targeted for acquisition in the first place. Therefore, as a robustness check, we also test H3 with an alternative measure for organizational hierarchy: we calculate the variable *IC* (short for Inventor Concentration) as the Herfindahl index of the distribution of inventors across different U.S. states in the 'before' period (1992–1995). *IC* ranges between 0 and 1, where 1 represents complete concentration of all inventors into just one state. The expected sign for the interaction of *DE*After*IC* is negative (instead of positive as it is for *DE*After*OrgSize*), because we expect the effect of top executives to be even greater

Table 6
Triple difference-in-differences by *competition*.

	New patents (1)	*Forward citations* (2)	*Patent classes* (3)	*Back1 ratio* (4)	*New inventors* (5)	*Cross-border* (6)
State employment	−0.3181	1.2702	−0.3021	0.1697	−0.4712	−0.4135
	(1.086)	(1.989)	(1.224)	(0.112)	(1.314)	(0.529)
State income (log)	0.0147	0.0574+	0.0058	0.0031+	0.0259	0.0207*
	(0.019)	(0.032)	(0.019)	(0.002)	(0.021)	(0.009)
State job growth	−0.0026	0.0049	−0.0047	0.0002	0.0007	0.0007
	(0.003)	(0.006)	(0.005)	(0.000)	(0.005)	(0.002)
State BC law	−0.0444	0.0456	−0.0931	−0.0109*	−0.0973	−0.0071
	(0.055)	(0.090)	(0.063)	(0.006)	(0.065)	(0.026)
Labor market rivals	0.0013	0.0035*	0.0008	0.0003*	0.0021+	−0.0004
	(0.001)	(0.002)	(0.001)	(0.000)	(0.001)	(0.001)
Labor market po.	0.0000	−0.0000	0.0000	0.0000	−0.0000	−0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Assets (log)	0.1831***	0.2329***	0.2388***	0.0070**	0.2700***	0.0088
	(0.023)	(0.039)	(0.025)	(0.003)	(0.029)	(0.011)
Employees (log)	0.0126	0.0004	−0.0014	−0.0066+	0.1083**	0.0477**
	(0.032)	(0.058)	(0.033)	(0.004)	(0.041)	(0.018)
R&D stock (log)	0.0358**	0.0592**	0.0406**	0.0014	0.0762***	0.0079
	(0.013)	(0.018)	(0.013)	(0.001)	(0.015)	(0.006)
Patent stock (log)	0.7323***	0.6666***	0.7341***	−0.0014	0.3656***	0.1036***
	(0.017)	(0.025)	(0.018)	(0.002)	(0.021)	(0.008)
Inventor stock	0.0001*	0.0002**	−0.0000	0.0000	0.0002***	0.0001*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Firm age	−0.0292***	−0.0450***	−0.0280***	−0.0004	−0.0273***	−0.0030*
	(0.002)	(0.004)	(0.003)	(0.000)	(0.003)	(0.001)
CapEx intensity	1.2820***	2.0585***	1.2666***	0.0633*	1.0657***	0.0051
	(0.246)	(0.382)	(0.258)	(0.032)	(0.312)	(0.107)
Revenue growth	0.0012**	0.0011	0.0005	0.0001	0.0005	0.0002
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Net income	0.0000	0.0000	−0.0000	0.0000*	0.0000	−0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ROA	−0.1096	−0.1634	−0.1129	−0.0175*	−0.1008	0.0310
	(0.073)	(0.114)	(0.071)	(0.008)	(0.076)	(0.028)
Liquidity	0.2523**	0.5883***	0.2691**	0.0090	0.1626	0.0336
	(0.089)	(0.156)	(0.095)	(0.009)	(0.107)	(0.040)
Leverage	−0.0368	−0.2161+	−0.0713	0.0025	−0.0696	0.0517
	(0.078)	(0.127)	(0.077)	(0.008)	(0.083)	(0.037)
Beta	−0.0065	−0.0052	−0.0075	0.0002	0.0056	0.0036*
	(0.006)	(0.013)	(0.006)	(0.001)	(0.006)	(0.002)
DE	0.1016**	0.1463*	0.1163**	0.0075*	0.0605	0.0099
	(0.035)	(0.061)	(0.038)	(0.004)	(0.042)	(0.018)
DE * After	**−0.1157****	**−0.2276*****	**−0.0826+**	**−0.0082+**	**−0.1171***	**−0.0391***
	(0.044)	(0.067)	(0.046)	(0.005)	(0.048)	(0.018)
Competition	−1.3612*	−0.7933	−2.1743**	0.0642	−1.2252**	−0.1631
	(0.639)	(0.486)	(0.736)	(0.040)	(0.390)	(0.141)
DE * Comp	−2.8377***	−1.1911	−2.0501**	−0.2280**	−1.7679*	−0.4331
	(0.745)	(1.038)	(0.790)	(0.079)	(0.800)	(0.271)
After * Comp	0.6455	1.0068	1.4514+	0.0383	1.1833	−0.0970
	(0.768)	(1.068)	(0.790)	(0.046)	(1.010)	(0.139)
DE * After * Comp	**2.8431****	**2.8244***	**2.0516***	**0.2018***	**1.9471**	**0.7424***
	(1.014)	(1.358)	(1.033)	(0.097)	(1.209)	(0.295)
Constant	−1.3547+	−0.0971	−1.6981***	−0.0894**	−1.0288*	−0.6271**
	(0.729)	(0.666)	(0.431)	(0.034)	(0.450)	(0.200)
Observations	6,759	6,759	6,759	6,759	6,759	6,759
Model	NB	NB	T-NB	OLS	NB	OLS
R^2 or [Log Likelihood]	[−20,835]	[−31,826]	[−15,482]	0.121	[−18,899]	0.330

Notes: All models include year and SIC-3 fixed effects. The main effect of *After* is collinear with year fixed effects. Robust standard errors are reported in parentheses and clustered by firm. Columns 4 and 6 are estimated by OLS; Column 6 is a linear probability model; Columns 1, 2, 5 are estimated by Negative Binomial; Column 3 uses a Truncated Negative Binomial model to adjust for a count DV that starts at one. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.10$, two-tailed tests.

when the innovation workforce is more concentrated.[10] In Table 7, we checked the consistency of results for this alternative specification against results reported earlier for *DE*After*OrgSize*, and found strong evidence that is consistent with H3. The

[10] Inverting Hypothesis 3, the negative effect of a decrease in competitive pressure from the takeover market on the rate (H1) and scope of innovation (H2) will be stronger for firms with a geographically concentrated workforce.

Table 7
Triple difference-in-differences by *inventor concentration (IC)*.

	New patents (1)	*Forward citations* (2)	*Patent classes* (3)	*Back1 ratio* (4)	*New inventors* (5)	*Cross-border* (6)
State employment	−0.4347	1.1388	−0.4574	0.1623	−0.6157	−0.4364
	(1.092)	(1.990)	(1.233)	(0.112)	(1.320)	(0.531)
State income (log)	0.0133	0.0586+	0.0048	0.0031+	0.0254	0.0200*
	(0.019)	(0.032)	(0.019)	(0.002)	(0.021)	(0.009)
State job growth	−0.0022	0.0048	−0.0048	0.0002	0.0008	0.0007
	(0.004)	(0.006)	(0.005)	(0.000)	(0.005)	(0.002)
State BC law	−0.0278	0.0460	−0.0810	−0.0113*	−0.0909	−0.0042
	(0.054)	(0.090)	(0.063)	(0.006)	(0.065)	(0.026)
Labor market rivals	0.0015	0.0035*	0.0010	0.0003*	0.0022+	−0.0004
	(0.001)	(0.002)	(0.001)	(0.000)	(0.001)	(0.001)
Labor market po.	0.0000	−0.0000	0.0000	0.0000	−0.0000	−0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Assets (log)	0.1807***	0.2378***	0.2384***	0.0072**	0.2721***	0.0084
	(0.023)	(0.039)	(0.025)	(0.003)	(0.029)	(0.011)
Employees (log)	0.0153	−0.0030	−0.0008	−0.0070+	0.1068**	0.0476**
	(0.032)	(0.058)	(0.033)	(0.004)	(0.041)	(0.018)
R&D stock (log)	0.0336**	0.0565**	0.0371**	0.0013	0.0738***	0.0082
	(0.013)	(0.019)	(0.013)	(0.001)	(0.015)	(0.006)
Patent stock (log)	0.7342***	0.6661***	0.7367***	−0.0014	0.3657***	0.1033***
	(0.017)	(0.025)	(0.018)	(0.002)	(0.020)	(0.008)
Inventor stock	0.0001*	0.0002**	−0.0000	0.0000+	0.0002***	0.0001*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Firm age	−0.0292***	−0.0447***	−0.0279***	−0.0004	−0.0271***	−0.0029*
	(0.002)	(0.004)	(0.003)	(0.000)	(0.003)	(0.001)
CapEx intensity	1.2388***	2.0556***	1.2444***	0.0624*	1.0532***	0.0071
	(0.249)	(0.383)	(0.260)	(0.032)	(0.314)	(0.109)
Revenue growth	0.0014*	0.0012	0.0005	0.0001	0.0006	0.0002
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)
Net income	0.0000	0.0000	−0.0000	0.0000*	0.0000	−0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ROA	−0.1436+	−0.1928+	−0.1491*	−0.0197*	−0.1304+	0.0290
	(0.075)	(0.114)	(0.073)	(0.008)	(0.077)	(0.028)
Liquidity	0.2621**	0.6078***	0.2693**	0.0102	0.1669	0.0324
	(0.089)	(0.153)	(0.094)	(0.009)	(0.107)	(0.040)
Leverage	−0.0476	−0.2138+	−0.0818	0.0020	−0.0750	0.0518
	(0.078)	(0.125)	(0.077)	(0.008)	(0.083)	(0.037)
Beta	−0.0085	−0.0059	−0.0099	0.0001	0.0040	0.0034+
	(0.006)	(0.013)	(0.007)	(0.001)	(0.006)	(0.002)
DE	0.1233***	0.1590*	0.1337***	0.0083*	0.0732+	0.0110
	(0.035)	(0.062)	(0.038)	(0.004)	(0.042)	(0.018)
DE * After	−0.1526***	−0.2537***	−0.1181*	−0.0097*	−0.1391**	−0.0408*
	(0.046)	(0.068)	(0.048)	(0.005)	(0.050)	(0.019)
IC	−0.0362	0.5787*	0.2572	−0.0146	−0.2402	−0.2490*
	(0.156)	(0.285)	(0.221)	(0.016)	(0.209)	(0.122)
DE * IC	0.8429***	−0.2914	0.8877***	0.0309	0.6691**	0.7205**
	(0.206)	(0.364)	(0.243)	(0.028)	(0.240)	(0.221)
After * IC	0.5071+	−0.1112	0.1826	0.0354	0.9414***	0.3034*
	(0.266)	(0.234)	(0.476)	(0.037)	(0.280)	(0.132)
DE * After * IC	−2.1027***	−1.5736***	−3.9098*	0.0180	−0.9455**	−0.3850*
	(0.294)	(0.299)	(1.621)	(0.040)	(0.318)	(0.157)
Constant	−1.5510*	−0.1404	−1.6713***	−0.0887*	−1.0211*	−0.6412**
	(0.755)	(0.668)	(0.428)	(0.035)	(0.450)	(0.199)
Observations	6,759	6,759	6,759	6,759	6,759	6,759
Model	NB	NB	T-NB	OLS	NB	OLS
R^2 or [Log Likelihood]	[−20,885]	[−31,842]	[−15,525]	0.118	[−18,918]	0.329

Notes: All models include year and SIC-3 fixed effects. The main effect of *After* is collinear with year fixed effects. Robust standard errors are reported in parentheses and clustered by firm. Columns 4 and 6 are estimated by OLS; Column 6 is a linear probability model; Columns 1, 2, 5 are estimated by Negative Binomial; Column 3 uses a Truncated Negative Binomial model to adjust for a count DV that starts at one. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.10$, two-tailed tests.

coefficient on *DE*After*IC* in Column 6 (*Cross Border*) of Table 7 now becomes statistically significant (whereas it was not in Table 5), however the coefficient in Column 4 (*Back1Ratio*) is insignificant.

R&D intensity. While *New Patents* and *Forward Citations* provide a relatively fine-grained proxy for the rate of innovation (Griliches, 1990), other research follows Cohen and Levinthal (1989) and examines R&D *intensity* as a coarser measure of the inputs to innovation. We therefore also tested *R&D Intensity* – defined as R&D expenditure divided by total revenue (Czarnitzki and Kraft, 2004) – as an alternative measure of the rate of innovation and found similar results as those reported for *New Patents* and *Forward Citations* (results available from authors).

Table 8
Alternative time windows.

	(1) ±4 years	(2) ±3 years	(3) ±2 years	(4) Prior period
1. New patents				
DE * After	−0.1526	−0.1677	−0.1685	0.0149
p-value	(0.00)	(0.00)	(0.00)	(0.77)
DE * After * OrgSize	0.0063	0.0077	0.0097	0.0027
p-value	(0.01)	(0.00)	(0.00)	(0.15)
DE * After * Competition	2.8431	3.0070	3.7208	−0.1585
p-value	(0.01)	(0.01)	(0.00)	(0.89)
2. Forward citations				
DE * After	−0.2512	−0.2305	−0.1655	−0.1366
p-value	(0.00)	(0.00)	(0.03)	(0.07)
DE * After * OrgSize	0.0091	0.0108	0.0120	0.0011
p-value	(0.00)	(0.00)	(0.00)	(0.73)
DE * After * Competition	2.8244	3.3085	3.9748	−0.7923
p-value	(0.04)	(0.02)	(0.00)	(0.63)
3. Patent classes				
DE * After	−0.1135	−0.1321	−0.1554	0.0813
p-value	(0.02)	(0.01)	(0.01)	(0.14)
DE * After * OrgSize	0.0044	0.0055	0.0075	0.0014
p-value	(0.04)	(0.01)	(0.00)	(0.38)
DE * After * Competition	2.0516	2.1844	3.1977	−0.5652
p-value	(0.05)	(0.05)	(0.01)	(0.63)
4. Back1 ratio				
DE * After	−0.0098	−0.0087	−0.0080	0.0031
p-value	(0.04)	(0.12)	(0.24)	(0.65)
DE * After * OrgSize	0.0003	0.0006	0.0005	−0.0002
p-value	(0.13)	(0.02)	(0.05)	(0.21)
DE * After * Competition	0.2018	0.2099	0.1563	0.0063
p-value	(0.04)	(0.04)	(0.15)	(0.96)
5. New inventors				
DE * After	−0.1424	−0.1630	−0.1356	0.0577
p-value	(0.00)	(0.00)	(0.02)	(0.33)
DE * After * OrgSize	0.0059	0.0069	0.0087	0.0019
p-value	(0.02)	(0.01)	(0.01)	(0.44)
DE * After * Competition	1.9471	2.4023	3.6595	−2.0194
p-value	(0.11)	(0.05)	(0.00)	(0.12)
6. Cross-border				
DE * After	−0.0417	−0.0419	−0.0180	−0.0210
p-value	(0.02)	(0.03)	(0.40)	(0.35)
DE * After * OrgSize	0.0015	0.0017	0.0022	0.0018
p-value	(0.12)	(0.08)	(0.05)	(0.12)
DE * After * Competition	0.7424	0.8411	0.6925	−0.0875
p-value	(0.01)	(0.01)	(0.03)	(0.80)
Observations	6,759	5,249	3,589	3,132

Notes: All models include year fixed effects, SIC-3 industry fixed effects, and all control variables reported earlier. Column 1 consolidates results from Tables 4–6. The effect of *DE * After* is estimated as the base diff-in-diff effect without the interaction of *OrgSize* or *Competition*. The effects of *DE * After * OrgSize* and *DE * After * Competition* are estimated from the full model with all covariates and lower interactions. Robust standard errors are reported in parentheses and clustered by firm; *p*-values reflect two-tailed tests. Column 4 performs a DD analysis of the 'before' period (1992–1995), moving the 'after' split to 1994, such that the two sub-periods in the 'before' period are: 1992–1993 and 1994–1995.

Alternative windows. In Table 8 we checked the sensitivity of our analysis to our sampling timeframe. Column 1 consolidates results from Tables 4–6 for ease of comparison. In Column 2, we limited our timeframe to the period from 1993 to 1998, with a 3-year window before and a 3-year window after the takeover regime change in Delaware, cutting the sample size to 5249 observations. As shown in this column, the *p*-values of the coefficient estimates for *DE***After, DE***After***OrgSize*, and *DE***After***Competition* are qualitatively similar to, and sometimes stronger than, those reported in Column 1; the only result that becomes weaker is the coefficient for *DE***After* in the *Black1 Ratio* model ($p = 0.12$). In Column 3, we further reduced the timeframe to two years before and two years after the regime change (i.e., 1994–1997), cutting the full sample size by almost 50% to 3589 observations. The results in this column again show a pattern consistent with that in the first two columns. However, because of the considerably smaller sample size, three out of the 21 coefficient estimates now become non-significant: the *p*-values for the coefficients for *DE***After* in the *Black1 Ratio* model and the *Cross-Border* model drop to 0.24 and 0.40, respectively; and the *p*-value for the coefficient for *DE***After***Competition* in the *Black1 Ratio* model drops to 0.15. Overall, this set of analysis shows that our results are robust to different time windows.

Endogeneity in the timing of the experiment. Next, we checked if the policy change in Delaware was itself an endogenous result of changes already underway in the economy. For example, it might be the case that Delaware firms with staggered boards pressured the courts to reinterpret the law and change the takeover regime. If that were the case, then we might expect to see a change in the dependent variable(s) just prior to the observed policy change. In Column 4 of Table 8, we

Table 9
Log-linear models.

	(1) Log-linear SIC-3 FE	(2) Collapsed SIC-3 FE	(3) Bootstrapped SIC-2 FE	(4) Panel firm FE
1. New patents				
DE * After	−0.1104	−0.1204	−0.1126	−0.0698
p-value	(0.00)	(0.00)	(0.00)	(0.09)
DE * After * OrgSize	0.0037	0.0047	0.0036	0.0033
p-value	(0.24)	(0.07)	(0.03)	(0.33)
DE * After * Competition	2.6586	2.6596	2.3555	2.0706
p-value	(0.00)	(0.00)	(0.00)	(0.03)
2. Forward citations				
DE * After	−0.2030	−0.2526	−0.2033	−0.112
p-value	(0.00)	(0.00)	(0.00)	(0.08)
DE * After * OrgSize	0.0067	0.0082	0.0075	0.0052
p-value	(0.07)	(0.01)	(0.01)	(0.15)
DE * After * Competition	4.0765	4.398	3.7327	2.8878
p-value	(0.00)	(0.00)	(0.00)	(0.03)
3. Patent classes				
DE * After	−0.0913	−0.0989	−0.0904	−0.0542
p-value	(0.02)	(0.01)	(0.02)	(0.19)
DE * After * OrgSize	0.0034	0.0042	0.0031	0.003
p-value	(0.25)	(0.11)	(0.06)	(0.33)
DE * After * Competition	2.4434	2.6081	2.2467	1.6639
p-value	(0.00)	(0.00)	(0.00)	(0.06)
4. Back1 ratio				
DE * After	−0.0079	−0.0130	−0.0081	−0.0053
p-value	(0.05)	(0.05)	(0.01)	(0.19)
DE * After * OrgSize	0.0003	0.0003	0.0003	0.0003
p-value	(0.13)	(0.10)	(0.08)	(0.06)
DE * After * Competition	0.1608	0.2433	0.1570	0.1621
p-value	(0.04)	(0.00)	(0.01)	(0.06)
5. New inventors				
DE * After	−0.1307	−0.1473	−0.1275	−0.1173
p-value	(0.00)	(0.00)	(0.00)	(0.01)
DE * After * OrgSize	0.0050	0.0071	0.0049	0.0036
p-value	(0.16)	(0.02)	(0.03)	(0.34)
DE * After * Competition	2.2560	2.6765	1.9527	1.7481
p-value	(0.02)	(0.03)	(0.02)	(0.13)
6. Cross-border				
DE * After	−0.0417	−0.0326	−0.0361	−0.0318
p-value	(0.02)	(0.08)	(0.02)	(0.11)
DE * After * OrgSize	0.0015	0.0010	0.0016	0.002
p-value	(0.12)	(0.22)	(0.20)	(0.02)
DE * After * Competition	0.7424	0.5113	0.6584	0.6042
p-value	(0.01)	(0.05)	(0.03)	(0.04)
Observations	6759	2590	6759	5920
Standard errors	Clustered by firm	Clustered by state	Bootstrapped	Robust

Notes: All models log the DV (except *Cross-Border*), are estimated by OLS, include all controls reported earlier, and use robust SE. Column 1 fixes effects by year & SIC-3, and clusters SE by firm. Column 2 collapses observations by firm to before and after, and clusters SE by state. Column 3 fixes effects by year & SIC-2 and block bootstraps SE (there are insufficient observations to bootstrap at SIC-3). Column 4 uses a fixed effects panel (xtreg) with a minimum of 6 or more observations per firm and robust SE.

tested whether there were any significant difference-in-differences in any of our dependent variables between the first half of the ‘before’ period (1992–1993) as compared to the second half of the ‘before’ period (1994–1995). As can be seen in the column, the results show little evidence of significant difference-in-differences in any of our dependent variables between the two sub-periods in the ‘before’ period (1996) used in our main analysis.

Log-linear models. We cross-checked all of our main results against alternative OLS specifications where we transformed the dependent variable to the natural logarithm of 1 + each dependent variable (with the exception of *Cross-Border* where the dependent variable is binary and we continued to use a linear probability model); we then re-estimated each model by OLS. Table 9 reports four sets of results based on log-linear models. In Column 1, we clustered the standard errors by firm, and included year fixed effects as well as industry fixed effects at the 3-digit level (and all controls reported earlier). Results in the column are generally consistent with our main findings, with similar or better levels of statistical significance, except that *p*-values for *DE*After*OrgSize* are generally not statistically significant (but are consistent in sign), perhaps suggesting that the log-linear model is a poor fit for this test. In Column 2, we examined alternative techniques for handling the potential non-independence of repeated observations in difference-in-differences research designs (Bertrand et al., 2004): we collapsed all observations by firm to an averaged observation for the before period and an averaged observation for the after period, and then clustered the standard errors by state. All results are consistent with our main findings, except that the *p*-values for *DE*After*OrgSize* again are generally weaker. In Column 3, we block bootstrapped the standard errors and

Table 10
Predicted difference-in-differences as a percentage change.

	(1) Predicted at the mean of all covariates	(2) Predicted at −1 S.D. level of *OrgSize*	(3) Predicted at −1 S.D. level of *competition*
New patents	−0.4%	−7.4%	−5.5%
	[−2.5% 1.8%]	[−10.4% −4.4%]	[−9.2% −1.8%]
Forward citations	−1.8%	−9.2%	−7.5%
	[−4.3% 0.7%]	[−12.8% −5.7%]	[−11.8% −3.2%]
Patent classes	0.6%	−10.0%	−7.3%
	[−3% 4.2%]	[−15.1% −4.9%]	[−13.5% −1.1%]
Back1 ratio	−11.3%	−24.3%	−20.9%
	[−19.6% −2.9%]	[−36.1% −12.4%]	[−35.4% −6.5%]
New inventors	−5.2%	−12.9%	−13.7%
	[−8.5% −1.8%]	[−17.6% −8.2%]	[−19.4% −7.9%]
Cross-border	−16.8%	−42%	−38.1%
	[−30% −3.6%]	[−61.5% −22.5%]	[−62.2% −14%]

Notes: This table reports the predicted difference-in-differences effect as a percentage change for each outcome variable (listed in the left column) based on the log-linear models reported in Column 1 of Table 9 (except *Cross-Border*, which is a linear probability model). Column 1 of this table reports the predicted difference-in-differences effect, holding covariates at their mean, and subtracting the average before-to-after difference for control firms from the average before-to-after difference for treated firms. Columns 2 and 3 report the predicted triple-difference-in-differences effect as a percentage change, setting the level of *OrgSize* and *Competition* to one standard deviation below the mean, holding covariates at their mean, and subtracting the predicted average before-to-after effect for control firms from the predicted average before-to-after effect for treated firms. The predicted 90% confidence interval for each result is shown in square brackets.

also included year fixed effects as well as industry fixed effects; however, due to insufficient observations at the SIC-3 level for bootstrapping, we fixed industry effects at the SIC-2 level. The results are again substantively identical to those reported earlier.

Finally, while the previous models included industry fixed effects and thus estimated between-firm effects, we also ran a firm-fixed-effects panel model to examine within-firm effects in Column 4.[11] In this model, we required firms to have a minimum of 6 observations over the 8-year period, dropping 12% of the sample due to insufficient observations. By fixing effects by firm, we remove omitted variable bias that is invariant over time and directly attributable to the firm (Cameron and Trivedi, 2009). As a result, variables that do not vary over time by firm (such as treatment group status *DE*, or *BC Law*) dropped out of the estimation. The reduction in sample size, plus the removal of all fixed, between-firm variation reduces the power of the analysis in Column 4; nevertheless, we continued to find results that are generally in line with our main findings, albeit sometimes at lower levels of statistical significance.

4.6. Predicted effects

To interpret the economic significance of our results, we predicted outcomes at different levels of the *DE*After* and *DE*After*Competition* interactions for the log-linear models reported in Column 1 of Table 9. We used log-linear models for our predictions to avoid well-known complications with interpreting interaction effects in non-linear models (Ai and Norton, 2003; Greene, 2010). To arrive at our predicted difference-in-differences effects, we calculated the average before-to-after difference for treated firms and subtracted the average before-to-after difference for control firms.[12] We then calculated the percentage change for each diff-in-diff outcome, measured relative to the average outcome level of treated firms in the before period. We report the results in Table 10 along with the 90% confidence interval for each effect.

Consistent with our regression results, we find a negative average predicted DD treatment effect for each outcome in Column 1 of Table 10, except for *Patent Classes*, which was positive but near zero. The predicted effect was statistically and economically significant for *Back1 Ratio, New Inventors* and *Cross-Border*, but not statistically significant for *New Patents, Forward Citations*, or *Patent Classes* when predicted at the average of all other covariates and interactions. To test our prediction that the effects of takeover protection will be stronger for smaller organizations, we calculated the percentage change for the difference-in-differences effect at the minus one standard deviation level of *OrgSize* (including all other lower-level interactions and covariates reported in Table 5) and report the results in Column 2. Here we find that all of our predicted effects are substantially stronger for smaller firms and that all effects are statistically significant. Finally, to test our theory that effects will be stronger when product market competition does not impose strong pressure on firms, we assess the average treatment effect for each outcome at the minus one standard deviation level of *Competition*. Again we calculated the percentage change for the difference-in-differences effect and report the results in Column 3. We find that all of our predicted effects to be substantially stronger for firms facing little product market competition and that all effects are statistically significant.

[11] We ran our panel regressions with log-linear models due to persistent problems with MLE convergence in negative binomial and Poisson panel models.

[12] We predicted effects at the mean of all covariates, and used a complete set of orthogonal polynomial contrast codes for year and fixed effects (instead of indicator variables with an omitted category) to capture variation between years and SIC-3 industries. A contrast coding approach is equivalent to the use of dummy variables (Davis, 2010) but enabled us to calculate an average prediction across fixed effects.

5. Conclusion

In this paper, we examine how competitive pressure, or the lack thereof, shapes innovation. We depart from prior research on product market competition and innovation by focusing on the competitive pressure due to takeovers, and by studying the effect of such pressure on the rate as well as the scope of firm innovation. We argue that innovation decision making in an organizational hierarchy reflects a mutual relationship between boundedly rational top executives and middle managers: to the extent that middle managers anticipate top executives to economize on efforts to innovate, they may conduct less search or search less broadly for innovative projects; executives anticipating reduced involvement in innovation ex post may also find such projects to be in their interest ex ante. Thus, increased protection of executives from takeover threat may reduce the rate and scope of innovation. Such effects, however, should be weaker for larger firms where decision making authority is more likely to have been delegated to middle managers and thus the influence of top executives has been smaller, and for firms already facing greater competition in the product market that provides an alternative source of pressure to increase executive involvement. Using a difference-in-differences methodology, our analysis of the effect of Delaware's takeover regime change, which significantly reduces takeover pressure for Delaware firms, provides strong evidence supporting these predictions.

Our study makes contributions to several streams of related literature. While takeover pressure has long been argued to shape managerial behavior and financial performance, there is scant empirical analysis of how such pressure affects firm innovation. In addition, existing studies on takeover pressure and innovation rarely consider that innovation decision making in a corporation is a process being shaped by organizational hierarchies and management incentives. We explicitly incorporate this consideration in making our arguments and empirically show that the effect of protection from takeover threat on innovation is moderated by organizational size. This finding therefore points to the value to examine in finer-grained terms how particular aspects of organizational hierarchies, such as the depth and span of control (e.g., Rajan and Wulf, 2006), may directly or indirectly affect innovation. Relatedly, while extant research has examined the direct relationship between product market competition and innovation, we show a substitutive relationship between competitive pressures from the product market and the takeover market in affecting firm innovation outcomes. Empirically, we go behind existing studies' focus on the rate of firm innovation by introducing a new set of variables related to the scope of innovation. We believe such variables warrant more research to improve our understanding of the direction of firms' inventive activities. In an economy in which innovation is increasingly recognized as an engine of corporate growth, how competitive pressures and managerial factors interact to affect firm innovation will continue to be an interesting and important research agenda.

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