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Penalizing the Underdogs? Employment Protection and the Competitive Dynamics of Firm Innovation

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Abstract

This paper examines how constraining a firm's ability to adjust resources affects the competitive dynamics of firm innovation. By limiting the pace and efficiency with which firms can replace resources that turn out to be unproductive, a constraint reduces laggards' ability to experiment with new resources and challenge industry leaders through increasing innovation. To explore this theory empirically, I exploit staggered adoptions of employment protection laws by U.S. state courts that increase the cost of employee dismissal. In addition to showing that increasing employment protection results in fewer and safer yet lower quality of innovation by laggards but not leaders, I also find that the negative effects are highly asymmetrical, penalizing more heavily firms operating in sectors with high technological velocity or labor intensity, as well as firms with limited financial slack. Using proprietary data on firm-level patent transactions, I also find that employment protection constrains a firm's ability to assimilate external knowledge.

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This paper examines how constraining a firm's ability to adjust resources affects the competitive dynamics of firm innovation. By limiting the pace and efficiency with which firms can replace resources that turn out to be unproductive, a constraint reduces laggards' ability to experiment with new resources and challenge industry leaders through increasing innovation. To explore this theory empirically, I exploit staggered adoptions of employment protection laws by U.S. state courts that increase the cost of employee dismissal. In addition to showing fewer and safer yet lower quality of innovation by laggards (but not leaders) resulting from the constraint, I find that the negative effects are highly asymmetrical, penalizing more heavily firms operating in sectors with high technological velocity or labor intensity, as well as firms with limited financial slack. Using proprietary data on firm-level patent transactions, I also find that employment protection constrains a firm's ability to assimilate external knowledge.

The essential point to grasp is that in dealing with capitalism we are dealing with an *evolutionary process*... that incessantly revolutionizes the economic structure from within, incessantly destroying the old one, incessantly creating a new one. [Joseph A. Schumpeter, *Capitalism, Socialism, and Democracy* (1942), p.82]

1. Introduction

Scholars have long recognized organizational adaptation as a competitive process that requires dynamic adjustments to resources (Cyert and March, 1963; Nelson and Winter, 1982; Penrose, 1959; Utterback and Abernathy, 1975). Changes in the competitive environment require adjustment to firm resources but because of adjustment costs and complex interdependencies across resources, attempts at adaptation also increase the risk of obsolescing other firm resources (Aghion and Howitt, 1998; Miller, 1992; Siggelkow, 2001). An increasing number of studies has examined various impediments to a firm's ability to manage this adjustment process as a micro-foundation that can account for various firm and industry dynamics, such as persistence of firm performance, direction of firm growth, and fluctuations in the demand for resources (Doraszelski and Pakes, 2007; Helfat et al., 2007; Sakhartov and Folta, 2014). I examine how the ability to adjust resources, specifically its constraint from increasing employment protection, affects the competitive dynamics of firm innovation and performance.

There are two competing effects to employment protection that complicate whether it is good or bad for innovation. More obviously, employment protection reduces the pace and effectiveness with which firms can adapt their innovation strategies to changes in the external environment by releasing obsolescent employees and hiring new employees with requisite skills ("adjustment effects"). However, there are also studies that find increased job security to motivate higher employee efforts and improve the productivity of existing resources, increasing returns to investing in innovation ("resource effects"). This tradeoff is central to current policy and academic debates; a number of European policy makers and academics have criticized the

stringent employment protection in the EU for its lagging high-tech and venture capital industries, particularly in comparison to Silicon Valley in the U.S. (Bozkaya and Kerr, 2014; Saint-Paul, 2002; Samila and Sorenson, 2011), while advocates have responded with evidence of significant and positive resource effects (Acharya et al., 2014; Griffith and Macartney, 2014). From a broader theoretical perspective, the tradeoff between being able to dynamically adjust resources and efficiently utilize extant resources represents a fundamental theme in management and economics research, underpinning choices of organic versus mechanistic structure, exploitation versus exploration, the optimal degree of a firm's strategic commitment, and the strength of anti-trust regulations among others (Ghemawat, 1991; Hannan and Freeman, 1977; March, 1991; Schumpeter, 1942).

Given the tradeoff, prior theoretical research suggests the overall effect of increasing employment protection to depend critically on how much a firm needs to adjust its resources, having a positive effect in a stable environment but a negative effect in a volatile environment where firm resources quickly become obsolete (Eisenhardt and Martin, 2000; Winter, 2003). While building on this contingency perspective that compares across industries, I look within industries and exploit the competitive dynamics in a firm's innovation strategy that help to explicate these competing effects on innovation performance. Notably, both industrial organizations and behavioral research theorize a firm's innovation strategy to depend on its competitive position and vary across laggards and leaders (Anderson and Cabral, 2007; Cyert and March, 1963; Greve, 2003; Lerner, 1997; Reinganum, 1989). With less to lose (or more to gain), laggards challenge the leaders by increasing investment in innovation and also pursuing high-variance, disruptive technologies whereas leaders focus on safer yet more incremental innovation that relies on existing resources.

My main argument is that because employment protection decreases the pace and efficiency with which firms can displace resources that turn out to be unproductive, it constrains laggards in their ability to experiment with risky new resources and challenge industry leaders resulting in fewer and safer yet lower quality innovations. In contrast, employment protection is expected to have limited or null effects for leaders who focus on exploiting existing resources with limited adjustment requirements.

To test these predictions empirically, I leverage staggered restrictions to the “employment-at-will” doctrine by the U.S. state courts from 1973 to 1999 that prohibited firms from dismissing their employees without due cause and thereby increased the cost of adjusting their employee base (Autor et al., 2007). In examining the interactions between the adoption of these legislative shocks and a firm’s competitive position as a laggard or leader, I employ a difference-in-differences and triple-differences approach (Giroud and Mueller, 2010). Such specification requires an alternative mechanism both to coincide with the staggered adoptions of employment protection laws and account for their differential effects based on the direction and strength of a firm’s competitive position (i.e., laggard or leader) and increases confidence in the causal interpretation of the findings.

I find several sets of findings in support of the proposed theory based on patent-based measures of innovation. As my baseline, I first show that increasing employment protection, specifically the adoption of implied contract exceptions that increases the cost of employee dismissal (simply Implied Contract hereafter), indeed decreases the quantity of innovation in high velocity sectors with rapid adoptions of new technology but has the opposite positive effect in low velocity sectors. The null effect of Implied Contract on the overall sample emphasizes the contingent nature of the tradeoff that masks the significance of the two underlying effects and the

importance of accounting for differences in requisite resource adjustments. Next, looking at firm-level competitive dynamics, I find a 17% increase in the number of patent applications (or 1.02 more patents) on average in response to falling behind but Implied Contract fully moderates this increase while having limited effects on the patents of leaders. The effects are also non-linear and increase in magnitude with negative performance up to a certain point but plateau afterwards, matching the observed patterns in resource adjustments generated by the convex increase in adjustment costs (Hamermesh and Pfann, 1996; refer to Appendix A for related results). The findings are robust across various specifications, firm subsamples, and alternative measures of a firm's competitive position.

I then conduct a series of additional analyses to further explicate the contingent effects of constraining the adjustment ability. The negative effects from increasing employment protection are larger for laggards operating in sectors with high technological velocity or labor intensity, as well as firms with limited financial slack (Zhang, 2005). Looking across different types of patents in a given firm, I find that the decline in the number and quality of a laggard's patents is also more pronounced for novel technologies compared to more traditional technologies. Lastly, I use data on patent inventors (Lai et al., 201) and a proprietary dataset on firm-level patent transactions to examine the effect of the constraint on utilizing external knowledge. Consistent with the prior findings on the internal innovative, laggards increase participation in the external market for technology (Arora et al., 2014), but Implied Contract fully moderates the increase in hiring new inventors or buying external patents while having limited effects on a firm's ability to sell its own internal patents.

This study contributes primarily to three streams of research. First, it builds upon prior studies that explore how employment protection may affect firm productivity and innovation

(Acharya et al., 2014; Autor et al., 2007; Griffith and Macartney, 2014), but highlights its heterogeneous effects based on industry and firm-level competitive dynamics. Second, the paper extends the largely theoretical research on technology and R&D races in industrial organizations research (Anderson and Cabral, 2003; Reinganum 1989; cf. Lerner, 1997) by underscoring that adjustment ability is a critical contingency necessary to uncover the theorized competitive dynamics in the data. More generally, these findings suggest that adjustment ability is a key determinant of how much risk a laggard takes in responding to negative performance (Cyert and March, 1963) and helps to reconcile prior empirical inconsistencies on the inertia of lagging firms (O'Reilly CA and Tushman, 2008). Third, this paper relates to research on resource management interested in understanding how firms can better manage their resources and sustain their competitive advantage over time (Helfat et al. 2007; Sirmon et al., 2007). My findings suggest the critical role played by the broader contractual and legal environments of the labor market and complement prior studies that focus on factors internal to the firm (e.g., Gavetti, 2005; Teece et al., 1997; Felin et al., 2007). Lastly, from a policy perspective, it address whether employment protection is good or bad for firm innovation. In contrast to prior studies that find positive effects (Acharya et al., 2014; Griffith and Macartney, 2014), this study points to the complex tradeoff across industries and different segment of firms and cautions against taking a singular position on this important and controversial issue based on a partial understanding of the economic consequences.

2. Related work and hypothesis

2.1. Competing effects of adjustment ability

Implementing innovation entails disrupting multiple resources at different parts of the value chain, including the skill mix of employees, the production process, and broader tangible

and intangible organizational resources (Aghion and Howitt, 1998; Benner and Tushman, 2002; Van de Van, 1986). The difficulty in managing this adjustment process underpins the failure of dominant (or incumbent) firms in responding to innovation by competitors (or new entrants), and extensive research investigates a variety of technological, managerial, transactional, and institutional factors that enable certain firms to successfully make adjustments while most others fail (for a review, refer to Ahuja et al., 2008). For example, pointing to the complexity and multiplicity of necessary transactions, transaction cost economics advocate vertical integration in introducing a new technology that enables more effective and timely adjustments in the preceding or subsequent stages of the production (Amour and Teece, 1980). Unlike routine tasks, innovation also involves a significant degree of trial-and-error and experimenting with risky new resources (Holmstrom, 1989) and requires minimizing the cost of failure by quickly releasing unproductive resources and acquiring new resources that better reflect the demands of the changing environment. As a result, constraining a firm's ability to adjust resources increases both the cost and risk of innovation and in turn, decreases its quantity with a bias towards projects that do not require drastic adjustments to existing resources. I refer to these negative effects as "adjustment effects."

In contrast, there are studies that find constraints on resource adjustments to enhance firm innovation because of their effect on the resources themselves, especially human resources. Agency research suggests that increased job security from restricting a firm's ability to dismiss employees can motivate greater employee effort, increasing returns to investing in innovation (Acharya et al., 2014; Griffith and Macartney, 2014). The consequent reduction in employee turnover also increases a firm's incentive to invest in employee training and the willingness of employees to develop more firm-specific, specialized knowledge and undertake riskier projects

(Manso, 2011; Samila and Sorenson, 2011). Models of employer-employee matching (e.g., Mortensen and Pissarides, 1994) provide more nuanced arguments that distinguish between the effects on the quantity and quality of innovation. Given higher adjustment costs, firms are likely to set higher thresholds for expected payoffs in screening for new resources and projects (Autor et al., 2007; Kugler and Saint-Paul, 2004). As a result, the quality of innovation is expected to increase but at the cost of reduced quantity. More generally beyond human resources, Bloom et al. (2013) propose that low adjustment ability that “traps” resources, combined with a negative demand shock, can actually encourage innovation by reducing the opportunity cost of experimenting with the affected resources. While recognizing that the resource effects are varied and can differ across specific resources, I term the overall positive effects on existing resources as “resource effects.”¹

This tension between dynamic efficiency in adjusting and creating resources and the static efficiency in utilizing existing resources remains a central theme across multiple research streams and policy debates (Benner and Tushman, 2002; Burns and Stalker, 1961; Helfat et al., 2007; March, 1991; Nelson and Winter, 1982; Schumpeter, 1942). One of the recurring challenges to assessing the tradeoff arises from the contingent importance of the two competing effects based on how much a firm needs to adjust resources (Eisenhardt and Martin, 2000). For example, the ability to adjust resources confers limited advantage to a firm in a stable environment that relies on existing resources (Winter, 2003). Conversely, in a disruptive environment, increasing efficiency in utilizing existing resources provides limited benefits and can even be harmful by delaying the adoption of new requisite resources (Eggers, 2012). The

¹ For example, Bradley, Kim, and Tian (2015) argue for an alternative view that emphasizes the increased risk of moral hazard and slack in the context of unionization. While there is no clear consensus, job security is generally considered a necessary evil in managing the highly idiosyncratic and risky innovation process (Manso, 2011).

few existing empirical studies also suggest the overall effects to depend on the characteristics of the task at hand. Using the same set of legislative shocks that increase employment protection, Autor et al. (2007) find negative effects on plant-level productivity whereas Acharya et al. (2014) find positive effects on firm-level innovation and entrepreneurship. Acharya et al. (2014) attribute the differences to the highly disruptive and uncertain nature of the innovation process that increases the severity of employer-employee agency conflicts and in turn, the benefits of increasing job security.

Despite the recognition of the heterogeneous effects based on a firm's requirements for resource adjustments (Burns and Stalker, 1961; Helfat et al., 2007), there is no systematic empirical study that explicates the contingent nature of the two underlying effects.² Notably, extant studies on employment protection employ a series of fixed effects (e.g., year \times industry and/or region) to simply suppress any industry level dynamics and focus on estimating shifts in the "equilibrium" level of firm innovation from horseracing the two competing effects (e.g., Acharya et al., 2014). This approach raises at least two important concerns. First, when looking at the average effect of the adjustment ability over time, the two competing effects are likely to cancel each other and underestimate the importance of the adjustment ability. Griffith and Macartney (2014) address this issue by using the country-level variations in employment protection of multi-national firms and looking for differences in incremental patents versus radical patents that cite scientific journals. They find positive effects on incremental patents but negative effects on radical patents, and the effects to be also more pronounced in industries with

² There is an extensive number of studies that employs computational methods to simulate the interactions between firm strategy and the external environment (e.g., Ericson and Pakes, 1995; Levinthal, 1997; March, 1991; Nelson and Winter, 1982). While they examine a similar theme of the tradeoff in the static-dynamic efficiency, I do not review them here because they consider resources to be largely independent from a firm's adjustment ability and as a result, do not consider the resource effects (for a review, refer to Chang and Harrington, 2006).

high-levels of layoffs. Second, there is a disconnect with the extensive research that characterizes innovation as a competitive process of “creative destruction” (Schumpeter, 1942) where innovation by one firm critically affects the resource allocation and strategy of competing firms, for example, by altering investment incentives or managerial perceptions of opportunities and threat (Aghion et al., 2013; Cyert and March, 1963; Knott, 2003). Addressing this theoretical and empirical gap represents the main objective of this paper.

2.2. Competitive dynamics of firm innovation and adjustment ability

The central argument of this paper is that it is precisely the interaction between a firm’s ability to adjust resources and the omitted firm-level competitive dynamics that has the potential to account for the heterogeneity in a firm’s innovation strategy and its change over time. Notably, current research on firm innovation, even after including a host of control variables, typically accounts for less than twenty percent of the overall variation among publicly traded firms (e.g., Acharya et al., 2014), in contrast with R^2 values on R&D intensity or spending that range above seventy percent (e.g., Chen and Miller, 2007).³ The fact that the inclusion of firm fixed effects brings only marginal improvement in the overall fit suggests firm innovation strategy to be highly dynamic, shifting the relative importance of resource and adjustment effects over time. To unpack this tradeoff, I build upon research on the competitive dynamics of firm innovation that suggests varying requirements for adjusting resources across laggard and leader firms.

Complementing a vast body of innovation research that relates competition and investment in innovation at the industry level (e.g., Aghion et al., 2005; Schumpeter, 1942), a subset of industrial organizations research suggests that a firm’s incentive to invest in R&D and innovation is not constant but varies over time as a function of its performance relative to

³ Industry-specific studies tend to produce higher R^2 values that range above 0.3 (e.g., Lerner, 1997; Cardinal, 2001).

competitors.⁴ Notably, models of R&D race and quality ladders examine the strategic interactions across two (groups of) competing firms where a firm's payoff depends on its distance to the other, and provide a formal analysis of their optimal investment strategy. With less to lose (or more to gain), laggards become more risk-seeking and invest in disruptive innovations whereas leaders become risk-averse and invest in more incremental innovation (Anderson and Cabral, 2007). When the payoff from investing in innovation is stochastic or does not depend on prior investments, it is also rational for laggards to invest more heavily in innovation, causing technological leadership to cycle across different firms (Reinganum, 1983; for a review of various contingencies, refer to Reinganum, 1989 and Gilbert, 2006).

While starting from a contrasting set of assumptions, behavioral models of risk-taking and change also suggest consistent dynamics.⁵ Extensive empirical research in this tradition documents that the risk propensity of managers and firms varies based on the distance of a firm's performance to certain reference points, notably the industry performance benchmark (Cyert and March, 1963; for a review, refer to Shinkle, 2012). When a firm falls behind the average competitor in its industry, there is increased risk-taking and search for new resources, including increased investment in R&D, CEO turnover, and divestment of business units, whereas getting ahead results in increased risk-aversion and inertia (Chen and Miller, 2007; Lant et al., 1992; Shimizu, 2007). Such variable search strategy based on a firm's competitive position also forms one of the core assumptions in evolutionary models of organizational adaptation and innovation (Nelson and Winter, 1982).

⁴ Refer to Ahuja et al. (2008) and Cohen (2010) for a review of determinants of investment in innovation.

⁵ Notably, behavioral research rejects the premise of risk neutrality, constant risk preference, profit maximization, and equilibrium analysis. In generating the discussed dynamics of firm innovation, convexity in risk-preference serves a similar role as the convexity in the pay-off or value function in game-theoretic models within industrial organizations research. Refer to Cabral (2003) for illustration.

The increased investment in innovation by laggards relative to leaders presents one of the key insights from behavioral and industrial organizations research on innovation that draws a stark contrast with broader patterns of firm investment. Namely, leaders grow in size by increasing investment in resources, such as employment and capital expenditure, while laggards shrink. However, with few notable exceptions (Lerner, 1997; Greve, 2003), extant research on the competitive dynamics of firm innovation remains largely theoretical with limited empirical support, and several scholars suggest the need to better account for the heterogeneity in firms' ability to make the requisite resource adjustments to uncover the theorized leader-laggard dynamics in the actual data (Chen and Miller, 2012; Sirmon et al., 2007).

My main argument is that employment protection has negative effects on the innovation by laggards. In addition to decreasing the cost of replacing obsolescent resources already in place, a firm's ability to adjust resources plays a critical role in determining the extent to which new investments are reversible and in turn, the cost of risk-taking. As a result, I expect employment protection to constrain the ability of laggards to experiment with risky new resources and challenge the leaders in response to falling behind. In contrast, I predict employment protection to have limited effect on leaders that tend to pursue more incremental innovation and have limited adjustment requirements. Based on this theory, I test the following prediction.

P1: Increasing employment protection is detrimental to the innovation performance of laggards but has limited effect on that of leaders.

With the constraint on requisite resource adjustment as the primary underlying mechanism, I expect the magnitude of the negative effects to closely reflect patterns of resource adjustments in the unconstrained state, prior to the implementation of employment protection. Specifically, extensive finance and economic research documents adjustment costs to be highly convex and asymmetrical with respect to the reduction and expansion in resources (Hamermesh

and Pfann; Zhang, 2005).⁶ As a result, laggards are highly responsive in reducing and adjusting resources at low ranges of negative performance but constrained in making further adjustments beyond certain ranges of negative performance. Put differently, laggards are unable to increase investment in innovation indefinitely, and theoretical models have incorporated this resource or cost constraint by capping the technology or investment gap between laggards and leaders (e.g., Aghion et al., 2005; Anderson and Cabral, 2007). Accordingly, I expect the negative effects of employment protection to increase in the magnitude of negative performance but plateau beyond certain ranges.

P2: The negative effects of employment protection on the innovation performance of laggards are non-linear, increasing in negative performance up to a certain point but plateauing afterwards.

There are substantial empirical challenges to testing these predictions. The ability to adjust resources is not directly observable, does not leave paper trails like patents, and as a result, it has been inferred indirectly from (un)successful changes to firm resources that are often confounded with other events and incentives to invest in innovation, such as the maturity of relevant technology. In addition, assessing its effects requires observing firm responses to a series of low to high adjustment requirements over time to make the distinction from ad-hoc or random adjustments (Winter, 2003). These limitations effectively qualify studies based on firm responses to a one-time shock in the external environment, such as technological discontinuity or trade liberalization (e.g., Bloom et al., 2013; Tushman and Anderson, 1986). To deal with these issues, I exploit a series of legislative shocks that increase the cost of employee dismissal.

3. Data and Empirical Approach

⁶ This asymmetry arises in part from the limited reversibility of investments in resources, for example from the lack of secondary markets and the specialized nature of resources (Ramey and Shapiro, 2001), and is further reinforced by some government policies, such as mandatory severance pay, advance notice for layoffs, or subsidies to new capital investments. Refer to Zhang (2005) for a discussion.

3.1. Employment protection

Generally, employment protection can be described as “restrictions placed on the ability of the employer to utilize labor (Addison and Teixeira, 2003:85).” The U.S. historically supported an “employment-at-will” doctrine, which allowed employers to fire their employees without any cause, advance notice, or restriction. Since the late 1970s through 1990s, however, different states have gradually adopted common law exceptions that restrict an employer’s ability to fire at-will. These restrictions are commonly referred to as “wrongful discharge laws” and consist of three different classes: implied contract exception, good faith exception, and public policy exception. I provide a brief summary of these exceptions here and refer the reader to Autor, Donohue, and Schwab (2006) for more detailed descriptions and adoption timelines for each state.

Under the implied contract exception, an employee can only be dismissed for “good cause.” The promise of such a contract by the employer can be implied and may include: personnel manuals stating that the employer’s policy is to terminate employees only for just cause; expectations arising from a worker's longevity of service or history of promotions and salary increases; and usual company practices that preclude terminating workers without good cause. The good faith exception prohibits employers from firing workers to deprive them of earned benefits, such as sales commissions or pension bonuses. The public policy exception provides workers with protections against discharges that would prevent them from acting in accordance with public policy, such as serving jury duty or whistleblowing an employer’s illegal activity.

Of the three exceptions, the implied contract exception has been shown to have the strongest effect on employment patterns and firm performance with null results for public policy

(Autor et al., 2006; Bird and Knopf, 2009; Keum, 2016a). Some studies also find significant results for good faith exceptions (Acharya et al., 2014), and I focus on estimating the effects of the implied contract exception while controlling for the good faith exception. As labor contracts are governed by state laws, we follow previous research in using the location of the CEO's office, reported in Compustat, as governing the firm's overall contracts (Bird and Knopf, 2009).

Several prior studies within legal, finance, and economic research has argued the legislative shocks from the adoptions of these wrongful discharge laws to be orthogonal to firm performance, as judicial decisions aim for objectives other than economic performance, such as enhancing procedural justice in employment relationships (Walsh and Schwartz, 1996). Acharya et al. (2014) also conduct a battery of empirical tests to address remaining concerns about omitted variable bias and reverse causality with respect to firm innovation and find limited evidence. Moreover, the use of triple-differences as my empirical strategy, described below, relaxes the strong identifying assumption of parallel pre-trends in the treated and control samples and replaces it with a much weaker requirement that the treatment does not systemically correlate with a variable that creates the third difference, whether firm is a laggard or leader.

3.2 Firm performance

Our main measure of a firm's relative competitive position is based on industry adjusted ROA. This measure has accumulated robust evidence of its influence on firm risk-taking as well as external evaluation of firm and managerial performance (e.g., Chen and Miller, 2007; Greve, 2003; Shinkle, 2012), and has also been used in previous research that explores the effects of employment protection (Bird and Knopf, 2009) as well as other policy changes (e.g., Giroud and Muller, 2010; Ljungqvist, Zhang, and Zuo, 2015). I also test the robustness to using alternative

measures of firm performance based on stock market performance and total factor productivity (Imrohoroglu and Tüzel, 2014) and find consistent results.

In operationalizing a firm's competitive position based on its relative financial performance, I follow the approach of Learner (1997) and Moliterno et al. (2014) that forms quartiles of firm performance relative to certain reference competitors. I start by assigning a firm as a laggard or a leader based on the difference between a firm's performance (P_{it}) and the industry benchmark set by the median competitor (IB_{it}): $P_{it} - IB_{it}$. IB_{it} is defined as the median ROA at four-digit SIC level for each fiscal year. I first use a simple binary measure where a firm with negative industry adjusted return is set to 1. In most of the analyses, however, I use a continuous measure with two additional knots at the 25th and 75th percentiles that result in four continuous variables covering each quartile of the annual performance. For brevity, I label the four linear splines created as Leader High (top 25th percentile), Leader Low (25-50th percentile), Laggard High (50-75th percentile), Laggard Low (bottom 25th percentile). Laggard High and Laggard Low take negative values by construction, and I take their absolute values for the ease of interpretation.

I adopt this specification for two primary reasons. First, it helps to capture the asymmetrical (Leader vs. Laggard) and convex increase in adjustment costs (High vs. Low) that can cause marginal effects of Implied Contract to be non-linear across Laggard High and Laggard Low (P2). In addition, I directly verify that this operationalization captures actual patterns of firm resource adjustments, including employment level and capital investments (for related results, refer to Appendix A and Keum, 2016a), and justify its use on both empirical and theoretical grounds. The use of a median firm within each SIC code or Fama-French industry classification as the benchmark has drawn significant criticism because of the increasing

obsolescence of SIC categorization, the diversified nature of firms, and significant heterogeneity across various firm attributes, such as firm size. While these concerns are valid, my empirical approach does not require a precise identification of a firm's representative competitor and rests on a much weaker assumption that the median firm serves as its reasonable correlate in terms of the direction and magnitude of a firm's relative competitive position. While less than ideal, the noisy measurement biases towards null findings and should result in more conservative estimates.

3.3. Proxies for innovation

I follow related research and use patent-based measures of firm innovation in assessing the effects of employment protection (e.g., Acharya et al., 2014). I use patent count for each firm-application year to proxy for the quantity of innovation, and the number of citations received to proxy for its quality. Patent citation counts have been shown to be a reasonable proxy for patent quality, with only a small number of citations accumulating to incremental patents (Hall et al., 2005). I also weigh the citation received with a truncation index created by Hall et al. (2001) that econometrically adjusts for different paces of citation accumulation for different application years and take its log value given the skewedness in the number of citations received. The number of citations received is averaged across patents for each firm-application year.

3.4. Control variables

In addition to firm fixed effects, all specifications include year fixed effects to control for any industry level adjustment dynamics, such as technological uncertainty or product life cycle of the industry. I control for factors related to a firm's innovation performance, including firm size (log of sales), R&D spending, average industry R&D intensity (R&D spending / sales), and industry growth rates. I also control for factors related to a firm's financial resources that can influence its risk preference and investment behavior, including distance from bankruptcy based

on Altman's Z score (1983), financial leverage based on its debt ratio, and financial slack measured with the current ratio (current assets divided by current liabilities) and working capital to sales ratio (Chen and Miller, 2007). All industry level controls are constructed at the four-digit SIC level in line with the prior construction of a firm's relative competitive position. When the dependent variable is patent citations, I also additionally control for the total number of patents applied by a firm each year. In one of the robustness tests, I also check whether the findings are robust to controlling for a firm's performance relative to its own past performance (Cyert and March, 1963), industry concentration based on Herfindahl index and its square term (Aghion et al., 2005), SIC2×Year fixed effects to capture industry specific time varying shocks, or the omission of all control variables other than firm and year fixed effects.

3.5. Empirical Approach

We use the following specification in testing the interaction between a firm's competitive position (CP) and legislative shocks from the adoptions of Implied Contract (IC)

$$Y_{ist} = \alpha_1 + \alpha_t + \beta_1 IC_{st-n} + \beta_2 CP_{it-n} + \beta_3 IC_{st-n} \times CP_{it-n} + X_{ist} + \varepsilon_{ist} \quad (1)$$

where i and t index a firm and year. n is the number of lags before the current time period t because the effects are expected to arise with some lags, usually ranging between one to three years, given the nature of legislative shocks and patents as the dependent variable (e.g., Atanassov, 2014; Acharya et al., 2014). s indexes the state in which the headquarters are located and IC_{st} is an indicator variable that equals 1 if an implied contract exception has been adopted in state s by year t. CP is the four competitive position variable (Leader High, Leader Low, Laggard High, and Laggard Low). This four regime asymmetric linear adjustment model (Enders and Granger, 1998) allows coefficients to vary based on the direction and strength of a firm's competitive position and has been used to examine the effects of adverse economic shocks

on factor demands (Hamermesh, 1993; Addison and Teixeira, 2003). In this triple-difference specification (Giroud and Mueller, 2010), the first difference compares the effect on the dependent variable before and after the legislative shock. The second difference takes the difference in the first difference across firms headquartered in the treated and non-treated states (IC_{st}). The interaction term $IC \times CP$ creates the third difference that estimates whether the effect of Implied Contract varies for leaders and laggards.⁷

3.6. Data

My starting sample is the universe of Compustat firms from 1973 to 1999 and their patent portfolio recorded in the latest NBER patent database (Hall et al., 2001). The two are matched using NBER algorithm by Hall, Jaffe, and Trajtenberg (2001). Since performance variable ROA is a ratio that can take extreme values, I start with firm-years with ROA less than 100% and greater than -100% and trim the sample at the 99th and 1st percentile. In addition, I follow prior research in excluding firms with R&D spending larger than their revenues as they represent firms specializing in R&D (Chen and Miller, 2007). All of the results are robust to their inclusion. Finally, I drop financial firms (SIC 6000-6999) and government enterprises (SIC>7999) from the sample since they are subject to different regulatory rules from manufacturing firms. My main sample consists of 35,367 firm-year observations. Prior studies on employment protection have focused on the manufacturing sector with SIC codes between 2000-4000 (e.g., Autor et al., 2007), which represents 77% of my overall sample. I adopt coding by Autor et al. (2006) for the timing of the passage of wrongful discharge laws in each state. Table 1 reports the overall descriptive statistics.

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

⁷ I also experiment with different lags (m) of the legislative shocks (IC_{st-m}) and find consistent results.

4. Results

4.1. Industry level dynamics based on pace of technological change

As a baseline, Figure 1 depicts the effects of Implied Contract on the quantity of firm innovation across the overall sample and two subsamples based on the industry's pace of technological change. The y-axis shows the logarithm of the number of patents applied for in a given year; the x-axis shows the time relative to the year that Implied Contract was adopted, ranging from two years prior to the adoption until five years after. Each 4-digit SIC code is assigned as a high or low velocity sector based on the mean value of the speed at which patents accumulate citations.⁸ This measure closely resembles the notion of a half-life, commonly used in science, and intends to proxy for the technological dynamism of the industry by measuring the speed at which a new technology is adopted.

In the overall sample shown in Figure 1a, Implied Contract is insignificant across all years. However, across the two subsamples in Figure 1b, Implied Contract is negative and significant in high velocity sectors but positive and significant in low velocity sectors. Consistent with the notion that the effect of the constraint manifests as existing resources obsolete, the negative effect appears with a two year lag and persists over time, whereas the positive resource effect becomes significant in the year of adoption with the strongest effect in t-1. These effect sizes are also economically significant, affecting patent counts on average **by 16-17% (or decrease and increase of 1.1 patents)**. These results verify that increasing employment protection

⁸ Specifically, I calculate i) the share of citations accumulated by a patent within the first three years of its grant and check robustness to using ii) the share of citations made to patents granted in the last three years. I use a three year window because it is the median value of the average citation lag. Using longer or shorter lags yields qualitatively consistent results. Three largest high velocity SIC code are 2834 (pharmaceuticals), 3674 (semiconductor), and 4911 (electric devices). Three largest low velocity SIC code are 1311 (crude petroleum and natural gas), 5411 (grocery stores), and 5812 (eating places).

creates both negative and positive effects but their relative magnitudes vary based the industry's speed of technological change.

4.2. Competitive dynamics of firm innovation

Models (1) – (3) in Table 2 estimate the effects of Implied Contract and its interaction with a firm's competitive position on the number of patents (log) with one, two, and three-year lags.⁹ Laggard High_{t-2} and Laggard Low_{t-2} are positive and significant and provide empirical support for the theorized increase in innovation when firms fall into a laggard position.

Consistent with P2, the coefficient is also larger for Laggard High_{t-2} than Laggard Low_{t-2}. The negative and significant Laggard High_{t-2} × IC_{t-2} confirms the main prediction of this paper that employment protection constrains the increase in the quantity of innovation by laggards. The negative and significant Leader High_{t-1} has not been explicitly hypothesized but is consistent with the reduced innovation by leader firms discussed in IO and behavioral research (Anderson and Cabral, 2007; Cyert and March, 1963). The timing and magnitude of the effects provide strong support for the causal interpretation of these results. Laggard High_{t-n} peaks with two-year lags (Model 2, n=2), consistent with the expectation that any actions on firm innovation and patents should take some time to appear (Atanassov, 2013). The weakening of the effects over time (Model 3, n=3) reflects the increase in innovation as a response to a time specific performance shock and mean reversion in performance over time (Wiggins and Ruefli, 2005). Laggard High_{t-n} × IC_{t-n} also shows similar patterns across all three lags and closely matches Laggard High_{t-n} in magnitude. Using a simple binary measure for being a laggard instead of the four linear splines also provides consistent results with insignificant coefficients for Implied Contract_{t-2} (0.029, p=0.28), positive coefficients for Laggard_{t-2} (0.028, p=0.04), and negative

⁹ Almost all of the results turn insignificant after year 3. Results on longer lags are available upon request.

coefficients for their interaction (-0.038, $p=0.03$). In terms of the economic significance, firms on average file for 1.02 additional patents in response to falling behind, which represents a 17% increase over the annual average (5.98 patents per year), and Implied Contract fully moderates this increase. Models (4) – (6) repeat the same analyses but weigh the patent count by the number of citations received. All results remain consistent. Lastly, I verify that the results are robust to a Poisson model as well as the inclusion of SIC2 \times Year fixed effects (untabulated).

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

4.3. Are all industries and firms equally affected?

Table 3 explores whether the negative effect on laggards differs based on firm and industry-level characteristics. I first test the previous results on the competitive dynamics across high and low velocity sectors in a 2×2 setting that jointly tests the industry (high vs. low velocity) and firm-level dynamics (leader vs. laggard). To conserve space, I only report the results with two-year lags ($n=2$) but all results show empirical patterns similar to those in Table 2. In a subsample of high velocity sectors in Model (1), the previously negative and significant Implied Contract _{$t-2$} (Figure 2a) turns insignificant once I control for its interaction with a firm's competitive position, but there is an increase in the magnitude and significance of Laggard High _{$t-2$} and Laggard High _{$t-2$} \times IC _{$t-2$} . This suggests that the previously negative effect of Implied Contract _{$t-2$} in high velocity sectors arises from constraining the ability of laggards to adjust resources and challenge the leaders. However, in Model (2) that examines a subsample of low velocity sectors, controlling for the competitive dynamics makes little difference in the significance and magnitude of Implied Contract _{$t-2$} . In addition, while still negative in magnitude, Laggard _{$t-n$} \times IC _{$t-n$} does not achieve statistical significance. This contrast provides additional support for the dominance of resource effects in low velocity sectors versus the dominance of

adjustment effects in high velocity sectors. Controlling for the competitive dynamics also generates sizable increases in R^2 from 0.17 to 0.22 and 0.14 to 0.18 in Models (1) and (2), which highlight the importance of accounting for the competitive dynamics in firm innovation. Note that controlling for the size of the firm (log of sales) only increases R^2 by 0.1 in the baseline specification in Figure 2.

Models (3) and (4) test whether high financial slack can moderate the constraint from employment protection by allowing firms to employ new resources without having to release existing resources. I divide the sample based on the median value of a firm's debt ratio (0.20), and find significant moderation of $\text{Laggard High}_{t-2} \times \text{IC}_{t-2}$ in the high slack subsample in Model (4). Subsampling based on the median value of the Kaplan-Zingales index (1997) yields qualitatively consistent results (untabulated). Models (5) and (6) test whether the effect of $\text{Implied Contract}_{t-2}$ is more pronounced in more labor intensive industries, measured as 4-digit SIC codes with above mean employee-to-sales ratio, and find support. Subsampling based on firm level labor-intensity yields qualitatively consistent results (untabulated).

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

4.5. Quantitative aspects of innovation

Models (1) and (2) of Table 4 examine how employment protection differentially affects the quality of patents based on the speed of technological change. Matching models predict that firms will be more careful in selecting potential resources and projects, which serves as an additional source of resource effect through which increased employment protection can enhance the quality of innovation. Consistent with the presence of this selection effect unique to the quality of innovation, $\text{Implied Contract}_{t-2}$ is positive and significant in Model (1), in contrast to the null effect in low velocity sectors in Model (2) as well as the prior null results on the number

of patents (Table 2). The magnitude of the effect is also sizable, increasing the average number of citations received by 10.5% (or 1.12 more citations). Similar to prior findings on the number of patents, $\text{Laggard High}_{t-2}$ and $\text{Laggard High}_{t-2} \times \text{IC}_{t-2}$ are significant only in high velocity sectors. This contrast again suggests the dominance of the adjustment effects in responding to specific competitive shocks.

Next, I test whether the negative effect on the quantity and quality of innovation is more pronounced for radical technologies that draw from novel resources.¹⁰ A patent is considered to contain novel or “new” technology if it cites technology classes that have not been cited by other patents in the same USPTO’s technology class (3-digit nclass), and I divide each patent to be “new” or “old” based on the median value of this citation-based measure of novelty (Aseem and Eggers, 2016). Refer to the Data Appendix for a more detailed description of this measure. Across Models (3) and (4), $\text{Laggard High}_{t-2}$ increases the number of new technology patents but decreases old technology patents, consistent with the theory that laggards shift towards more radical projects in response to falling behind (Anderson and Cabral, 2007). The positive and significant Leader High_{t-2} in Model (3) contradicts the predicted increase in the technological inertia of leaders but the magnitude is substantially smaller compared to that of $\text{Laggard High}_{t-2}$. The interactions between being a laggard and IC_{t-2} are negative and significant only for the new technology in Model (3), in line with the argument that employment protection discourages experimenting with risky resources and generates a bias towards incremental innovations that do not require substantial adjustments to existing resources. Models (4) and (5) repeat the analysis with patent citations (log) as the dependent variable. I estimate the effects on the quality of patents at the level of individual patents that permits more detailed controls, including 421

¹⁰ The requirement for calculating the historical patterns in citation reduces the time period of the data to 1976-1997.

technology class fixed effects. Consistent with the selection effect, Implied Contract_{t-2} is positive for new technology patents but weakly negative and significant for old technology patents. The negative effect on competitive dynamics (Laggard High_{t-2} × IC_{t-2}) comes from new technology with limited effect on more traditional technology. Taken together, these results suggest that the constraint on resource adjustment not only reduces a firm's pursuit of radical technologies but also decrease their quality when they are actually pursued.

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

4.6 External market for technology

Lastly, I examine the effects of employment protection on a firm's ability to utilize the external market for technology (Arora, Belenzon, and Rios, 2014) using three different outcome measures: the hiring of new external inventors, buying of external patents, and selling of internal patents. Refer to Data Appendix for descriptions of inventor data (Lei et al., 2010) and the proprietary dataset on patent transactions. Autor (2003) finds employment protection to increase the outsourcing of simple tasks, but it is less clear how employment protection will affect a firm's ability to utilize complex external knowledge (Cohen and Levinthal, 1990) because it also constrains the adjustments necessary for assimilation and implementation. The effects on the external market for technology closely resemble prior findings on a firm's internal innovative activity. In response to falling behind, laggards actively reorganize their human and intellectual resources by increasing participation in the market for technology: hiring more external inventors (Model 1), buying external patents (Model 2), and also selling their own internal patents (Model 3). However, Laggard High_{t-n} × Implied Contract_{t-n} again fully moderates these increases with the exception of selling their own patents, suggesting the importance of the ability to adjust labor and other downstream resources in assimilating external knowledge. The more immediate effect

on the buying and selling of patents (t-1) and the longer lags on hiring external inventors (t-2) are also consistent with the characteristics of the respective transactions and databases in use.¹¹

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

4.7. Robustness checks

The triple difference specification makes alternative mechanisms very unlikely, and for robustness checks, I focus on the stability of the results across various subsamples. I first test whether the results are robust to both excluding each of the three largest states – California, New York, and Texas – from the sample and using the 55th or 60th percentile firm as the industry performance benchmark. I also try restricting the sample to firms with at least ten or fifteen years of data that provides a longer panel and running multiple time lags simultaneously ranging between t+2 and t-3. In addition, I also test a restricted sample that only includes firm-years five years before and after the adoption of the implied contract exception. I find all of the results to remain qualitatively consistent (untabulated). Dividing the sample into high or low patent firm samples to better align the average level of the dependent variable produces sharper and consistent results. As a placebo test, I try assigning the location of a firm based on its state of incorporation as well as randomly. Using the state of incorporation produces insignificant or marginally significant results while a random assignment produces null results (untabulated).

5. Conclusion

How does the ability to adjust resources affect firm adaptation and innovation?

Theoretical research challenges the intuitive positive relationship; being able to adjust and create new resources more dynamically (adjustment effects) comes at the cost of losing efficiency in

¹¹ Refer to the Data Appendix for details. In short, the hiring of an external inventor can only be tracked when the inventor files for a new patent in the new organization, and as a result, longer lags are expected compared to patent transaction.

utilizing existing resources (resource effects). In fact, the few prior empirical studies on firm innovation point to the dominance of the less obvious resource effects (Acharya et al., 2014; Griffith and Macartney, 2014). I explicate this tradeoff in the context of the competitive dynamics of firm innovation. By studying the effects of staggered adoptions of wrongful discharge laws, I find that increasing employment protection impedes laggards' ability to experiment with new resources and challenge industry leaders through increasing innovation. However, consistent with the tradeoff, the overall effects are also highly heterogeneous based on a firm's adjustment requirements, for example, industry-level characteristics such as its technological dynamism or labor intensity as well as firm-level financial resources. Taken together, these findings demonstrate that a firm's ability to adjust resources is a critical but highly contingent determinant of its innovation performance.

I note some important limitations to these findings. In particular, there are several sources of potential downward bias, including noisy identification of a firm's competitive position and partial exposure to legislative shocks from multi-location firms. Hence, the observed effects of employment protection (both positive and negative) should be considered as falling closer to their lower bounds. Given the noisy nature of patent transaction data, some of the findings on the external market for technology should be interpreted as preliminary results that warrant further verification, although there are no obvious reasons to suspect any correlation between the cleaning and matching algorithms I use with the exogenous increase in employment protection. In motivating the predictions, I use models of technology race from industrial organizations research and behavioral models interchangeably. However, the two differ in important aspects that have not been fully discussed in this paper, and a more careful mapping of these differences appear to be a productive direction for future inquiry, for example, exploring varying levels of

competitive intensity or governance quality that likely impose varying degrees of economic efficiency and rationality on managers. Lastly, employment protection represents an important but rather specific form of constraint. There is no shortage of shocks to the factor market, including mandatory severance pay, R&D tax credits, and subsidies to capital investments to list a few (Bloom, Griffith, and van Reenen, 2002), and looking at their effects on the dynamics of firm innovation can better inform related policy debates.

While admittedly an incomplete examination of the role of resource adjustment in a firm's ability to adapt and innovate, this study has important implications for policy design. The highly contingent importance of the adjustment and resource effects points to a complex trade-off between increasing employment protection and firm innovation performance. This raises both new and old questions to policy makers, including those related to industry composition, industry concentration, and redistribution. In the case of this study, protecting employees from the unlimited power of employers came at the cost of penalizing some underdog firms. Given the wide-ranging and significant yet unintended consequences of legislative shocks on economic performance, the role of U.S. state courts raises interesting questions. As Schumpeter (1942) observed, innovation seems to be at the heart of the inextricable relationship between economic growth and social institutions of democracy.

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Table 1**Summary statistics.**

The sample consists of the universe of Compstat firms between 1973-1999 matched to the latest NBER patent database (Hall et al., 2001).

Variables	Obs.	Mean	SD	Min	Max
Number of patents _t	35,367	5.98	16.96	0.00	136.00
Citations received _t	16,591	2.35	0.83	0.00	5.82
Implied Contract _t	35,367	0.62	0.49	0.00	1.00
Good faith _t	35,367	0.25	0.43	0.00	1.00
Industry adjusted ROA _t	35,367	-0.02	0.14	-0.79	0.33
Laggard High _{t-2} : ROA top quartile	35,367	0.03	0.06	0.00	0.88
Laggard Low _{t-2} : ROA second quartile	35,367	0.01	0.02	0.00	0.34
Laggard High _{t-2} : ROA third quartile	35,367	0.01	0.03	0.00	0.76
Laggard High _{t-2} : ROA bottom quartile	35,367	0.04	0.13	0.00	1.03
Industry revenue growth _t	35,367	0.14	0.14	-1.33	1.17
Debt ratio _t	35,367	0.22	0.20	0.00	4.98
Financial slack: Current ratio _t	35,367	3.17	11.61	0.00	1719.25
Financial slack: Sales ratio _t	35,367	0.38	1.16	-37.33	61.74
Distance from Bankruptcy _t	35,367	5.37	12.53	-90.02	924.23
Total asset (log) _t	35,367	4.53	1.92	0.06	11.91
R&D spending (log) _t	35,367	1.59	1.45	-0.05	7.92
Average industry R&D intensity _t	35,367	0.06	0.14	0.00	10.01

Figure 1: The two figures show a visual difference-in-differences examining the effects of implied contract exceptions (IC) on innovation in firms located in adopting states relative to firms in nonadopting states. The y-axis plots the logarithm of the number of patents filed; the x-axis shows the time relative to the year of adoption, ranging from two years prior to adoption until five years after the adoption. The dashed lines represent a 95% confidence interval of the regression Patent counts_{ist} = $\alpha_i + \alpha_t + \beta_1 IC_{st-n} + X_{ist} + \varepsilon_{ist}$.

Figure 1a: overall sample

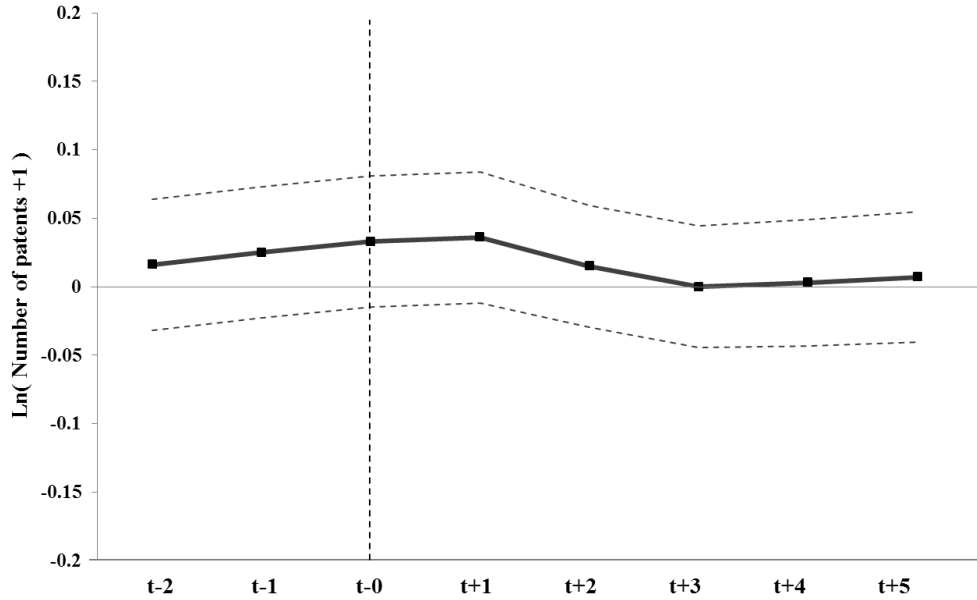


Figure 1b: High vs. Low velocity sectors

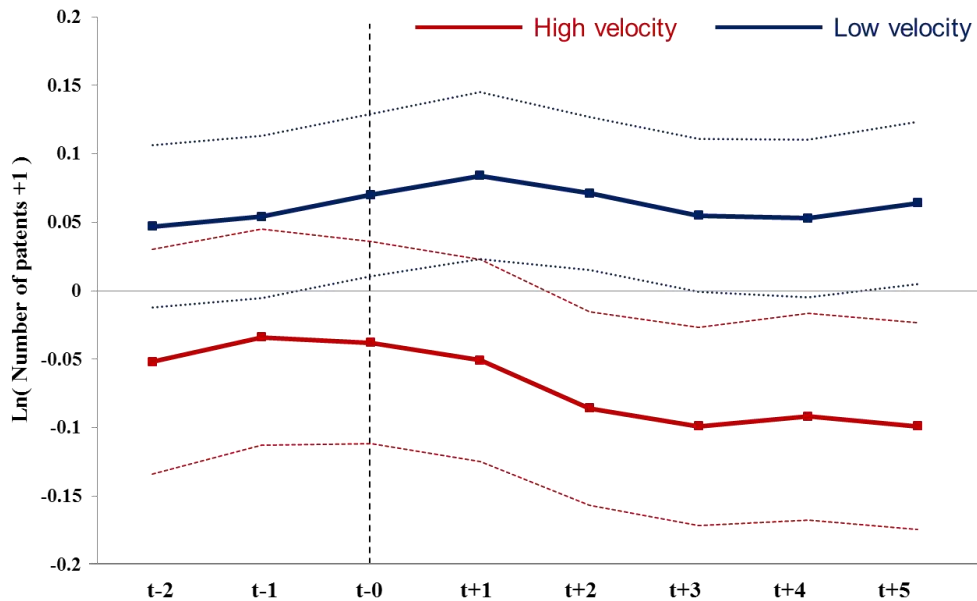


Table 2**Effects of firm competitive position and implied contract exceptions on the number of patents**

OLS regression result. Robust standard errors are clustered at the firm level and shown in brackets. *, **, and *** indicate statistical significance at 10%, 5%, and 1% levels, respectively. The sample is the universe of Compustat firms between 1973-1999.

	Number of patents (log)			Citation weighted number of patents (log)		
	n = 1 (1)	n = 2 (2)	n = 3 (3)	n = 1 (4)	n = 2 (5)	n = 3 (6)
Implied Contract (IC) _{t-n}	0.035 [0.029]	0.030 [0.028]	0.016 [0.027]	-0.236 [1.325]	-0.443 [1.268]	-0.185 [1.265]
Leader High _{t-n}	-0.295* [0.163]	-0.090 [0.160]	-0.199 [0.150]	3.516 [5.423]	3.079 [4.850]	4.146 [4.611]
Leader Low _{t-n}	-0.511 [0.411]	0.013 [0.450]	0.628 [0.432]	-10.873 [19.828]	-7.684 [22.132]	0.044 [15.842]
Laggard High _{t-n}	0.066 [0.205]	0.547** [0.221]	0.315 [0.192]	0.065 [5.725]	15.355** [5.340]	21.290** [5.707]
Laggard Low _{t-n}	0.082 [0.058]	0.136** [0.064]	0.157** [0.055]	3.751** [1.715]	6.142** [1.713]	7.846** [1.628]
Leader High _{t-n} x IC _{t-n}	0.181 [0.202]	-0.120 [0.209]	-0.127 [0.200]	-5.284 [6.965]	-7.348 [7.019]	-9.087 [6.596]
Leader Low _{t-n} x IC _{t-n}	0.430 [0.522]	0.144 [0.546]	-0.725 [0.535]	13.835 [22.854]	10.258 [25.235]	-15.430 [20.123]
Laggard High _{t-n} x IC _{t-n}	-0.212 [0.241]	-0.663** [0.263]	-0.489* [0.267]	-1.381 [6.933]	-15.952** [6.895]	-25.405** [7.975]
Laggard Low _{t-n} x IC _{t-n}	-0.045 [0.072]	-0.098 [0.076]	-0.133* [0.070]	-1.731 [2.106]	-4.801** [2.129]	-5.559** [2.027]
Controls	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
Adj. R ²	0.19	0.19	0.19	0.12	0.19	0.19
Obs.	35,367	34,758	33,888	35,367	34,758	33,888

Table 3**Contingent effects based on industry technological velocity, financial slack, and labor intensity**

OLS regression results. Robust standard errors are clustered at the firm level and shown in brackets. *, **, and *** indicate statistical significance at 10%, 5%, and 1% levels, respectively. The sample is the universe of Compustat firms between 1973-1999.

	Number of patents (log)					
	High tech. velocity (1)	Low tech. velocity (2)	Low financial slack (3)	High financial slack (4)	High labor intensity (5)	Low labor intensity (6)
Implied Contract (IC) _{t-2}	-0.067 [0.042]	0.076** [0.034]	0.046 [0.035]	0.049 [0.041]	0.030 [0.033]	0.025 [0.044]
Leader High _{t-2}	-0.015 [0.210]	-0.165 [0.193]	0.171 [0.157]	-0.250 [0.246]	0.246 [0.160]	-0.321 [0.224]
Leader Low _{t-2}	-0.078 [0.687]	0.046 [0.572]	0.917 [0.590]	-0.803 [0.679]	0.213 [0.550]	-0.017 [0.690]
Laggard High _{t-2}	0.786*** [0.263]	0.070 [0.298]	1.013*** [0.278]	0.298 [0.350]	0.358 [0.226]	0.807*** [0.376]
Laggard Low _{t-2}	0.047 [0.070]	0.225** [0.104]	0.179** [0.082]	0.038 [0.109]	0.058 [0.066]	0.189* [0.103]
Leader High _{t-2} x IC _{t-2}	-0.119 [0.266]	0.092 [0.265]	-0.194 [0.224]	-0.037 [0.297]	-0.213 [0.200]	0.097 [0.286]
Leader Low _{t-2} x IC _{t-2}	-0.056 [0.790]	0.611 [0.740]	0.298 [0.780]	0.480 [0.784]	0.656 [0.718]	-0.242 [0.810]
Laggard High _{t-2} x IC _{t-2}	-0.824*** [0.331]	-0.350 [0.358]	-1.354*** [0.359]	-0.342 [0.398]	-0.754*** [0.291]	-0.657 [0.417]
Laggard Low _{t-2} x IC _{t-2}	0.003 [0.092]	-0.157 [0.111]	-0.200* [0.104]	0.036 [0.121]	-0.039 [0.085]	-0.088 [0.114]
Controls	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
Adj. R ²	0.22	0.18	0.18	0.18	0.16	0.23
Obs.	15,126	21,234	16,796	17,962	17,831	18,529

Table 4**Quality of patents and new vs. old technology**

OLS regression results. Robust standard errors are clustered at the firm level and shown in brackets. *, **, and *** indicate statistical significance at 10%, 5%, and 1% levels, respectively. The sample for Models (1) and (2) consists of the universe of Compustat firms between 1973-1999, and 1976-1997 for Models (3)-(6). Models (5) and (6) are estimated at the patent level.

	Number of citations (log)		Number of patents (log)		Number of citations (log)	
	High tech. velocity (1)	Low tech. velocity (2)	New technology (3)	Old technology (4)	New technology (5)	Old technology (6)
Implied Contract (IC) _{t-2}	0.117** [0.054]	0.028 [0.034]	0.030 [0.026]	-0.019 [0.024]	0.066** [0.033]	-0.005 [0.042]
Leader High _{t-2}	-0.302 [0.537]	-0.076 [0.390]	0.350** [0.138]	-0.414** [0.129]	-0.072 [0.407]	-0.968** [0.475]
Leader Low _{t-2}	1.710 [1.079]	-0.052 [0.993]	0.005 [0.550]	-0.251 [0.545]	0.315 [0.928]	1.367 [1.133]
Laggard High _{t-2}	1.493** [0.746]	0.464 [0.662]	0.581** [0.208]	-0.403** [0.205]	0.664 [0.615]	0.584 [0.866]
Laggard Low _{t-2}	0.225 [0.355]	0.058 [0.249]	0.074 [0.050]	-0.001 [0.055]	-0.077 [0.291]	0.031 [0.249]
Leader High _{t-2} x IC _{t-2}	0.171 [0.583]	-0.310 [0.461]	-0.419** [0.158]	0.298* [0.164]	-0.274 [0.432]	0.504 [0.553]
Leader Low _{t-2} x IC _{t-2}	-1.439 [1.206]	-1.244 [1.222]	-0.006 [0.687]	0.293 [0.664]	-1.132 [1.052]	-1.616 [1.358]
Laggard High _{t-2} x IC _{t-2}	-1.898* [0.976]	-1.201 [0.885]	-0.584** [0.261]	0.286 [0.239]	-1.699** [0.762]	0.194 [1.085]
Laggard Low _{t-2} x IC _{t-2}	0.023 [0.377]	0.051 [0.273]	-0.109* [0.065]	0.034 [0.061]	0.312 [0.299]	0.162 [0.320]
Controls	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
Tech. class FE	no	no	no	no	yes	yes
Adj. R ²	0.15	0.06	0.04	0.01	0.33	0.40
Obs.	7,366	9,015	29,220	29,220	144,101	69,594

Table 5**Effects on utilizing the external market for technology**

OLS regression results. Robust standard errors are clustered at the firm level and shown in brackets. *, **, and *** indicate statistical significance at 10%, 5%, and 1% levels, respectively. The sample for Model (1) consists of the universe of Compustat firms between 1973-1999, and 1980-1997 for Models (2)-(3).

	Hiring external inventor (log) n=2 (1)	Buying external patents (log) n=1 (2)	Selling internal patents (log) n=1
Implied Contract (IC) _{t-n}	-0.054** [0.026]	0.003 [0.021]	0.004 [0.020]
Leader High _{t-n}	0.573** [0.218]	0.187 [0.136]	0.192 [0.128]
Leader Low _{t-n}	0.183 [0.516]	0.187 [0.394]	0.193 [0.403]
Laggard High _{t-n}	0.716** [0.315]	0.364* [0.206]	0.316** [0.157]
Laggard Low _{t-n}	0.222** [0.104]	0.177** [0.057]	0.104** [0.047]
Leader High _{t-n} x IC _{t-n}	-0.219 [0.269]	-0.237 [0.146]	-0.066 [0.149]
Leader Low _{t-n} x IC _{t-n}	0.805 [0.749]	-0.380 [0.459]	-0.022 [0.509]
Laggard High _{t-n} x IC _{t-n}	-0.820** [0.400]	-0.160 [0.232]	-0.018 [0.180]
Laggard Low _{t-n} x IC _{t-n}	-0.203* [0.108]	-0.147** [0.074]	-0.044 [0.061]
Controls	yes	yes	yes
Year FE	yes	yes	yes
Firm FE	yes	yes	yes
Adj. R ²	0.19	0.07	0.11
Obs.	34,758	28,004	28,242

Data Appendix

A. New vs. Old Technologies (Table 4)

To measure the novelty of a technology contained in a patent, I follow the method of Aseem and Eggers (2016), Jaffe (1986, 1989), and Breschi et al. (2003) and construct relatedness among patent technology classes based on the frequency of cross citations. I assume that technology classes A and B are closely related if there are frequent citations between the two in the prior five years (t-5 to t-1). Specifically, I adopt the following measure from Aseem and Eggers (2016) in which novelty is calculated by taking the maximum of the following variable:

$$\text{Novelty}_{ij} = 1 - \frac{\sum_{t=-5}^{-1} \text{Citations}_{ij}}{\sum_{t=-5}^{-1} \text{Citations}_{i}}$$

Novelty takes a higher value if a patent cites one of the 421 technology classes (nclass) that has not been cited by other patents in the same class in the past five years. I divide individual patents to contain “new” or “old” technologies based on the median values of Novelty (0.987). The time period of the data is reduced to 1976-1997 due to the requirement for calculating the historical patterns in citations.

B. Disambiguated inventor database (Table 5)

I start with the database by Lei et al. (2010) that disambiguates the names of inventors contained in all of the USPTO patents and assigns a unique identification to each inventor. I first merge this DB with the latest NBER patent DB (Hall et al., 2001) to identify i) the assignee firm for each patent and ii) GVKEY. Some inventors file multiple patents in a given year, and I take the assignee firm associated with the last patent filed in a given year as the employer of the inventor. I record firm A (based on GVKEY) to have hired a new external inventor if the inventor has worked for a different company previously or produced patents as an independent inventor between year t-3 and t-1. There are two notable exceptions. First, in the case of firm A (date 1) – “missing” (date 2) - firm A (date 3), I replace missing (date 2) with firm A. “Missing” (date 2) is likely from an assignment issue and does not represent access to a new resource. Second, I exclude cases where an inventor is considered a new hire because of transitioning from missing GVKEY to non-missing GVKEY despite sharing the same PDPASS across the transitioning years.

While providing a complete coverage of all inventors that file for patents, the precise date of an inventor’s move from firm A to firm B cannot be identified based on this dataset. An inventor’s employer is revealed only when the inventor files for a patent (as assignees), and unless an inventor files for patents consecutively without a gap year, the precise year of the movement cannot be identified. For example, it is unclear which year (2001 vs. 2002 vs. 2003) inventor A moved to firm Y from firm X in the following case.

Inventor A – Patent 1 – year 2000 – firm X

Inventor A – Patent 2 – year 2003 – firm Y

I record the year of application for the new patent (2003) as the year of movement. Note that this is an upper bound for the year of the movement. All of the results are robust to using a mid-point year (2002, rounded up).

C. Patent transaction (Table 5)

There are three primary challenges to tracking the buying and selling of patents.

- i) Parsing the USPTO PAIR DB into a readable format (available for years between 1980-2015 in bulk format from <https://www.google.com/googlebooks/uspto-patents-assignments.html>)
- ii) The large majority of patents are first assigned to an inventor (inventor is both assignor and assignee) then reassigned to an organization of employment (inventor is the assignor and the organization is the assignee). Separating an actual external transaction or “reassignment” (e.g., between an independent inventor and firm X) from a simple internal reassignment (e.g., from a hired inventor who is simply transferring patents to an employer organization) presents a substantial challenge.
- iii) The patent transaction must be matched to Compustat but because of inconsistencies in naming, the large majority of obvious matches will not be made.

Dealing with ii): I follow a cleaning approach described in the NBER working version of Serrano (2008) with some modifications, noted below.

- a) I only include cases of “patent reassignments” in the conveyance text and do not include cases of mergers and acquisitions. While there are some cases of acquiring a whole company for its patent portfolio, I consider these cases to be a byproduct rather than purposeful transactions aimed at acquiring certain technologies.
- b) Close to 40% of traded patents are traded more than once. With each additional transaction, there is a substantial decrease in the accuracy with which sellers and buyers can be identified (i.e., GVKEY or CUSIP). Hence, in the analysis, I only include the first reassignment that can be identified with reasonable accuracy but including the second, third, or fourth reassignments make limited difference.
- c) I do not exclude cases in which the execution date precedes application date. Serrano excludes them for reasons of “pre-arrangement.”

Dealing with iii): Matching to Compustat has to be based on company names because it is the only common information across the three databases necessary for the analysis (Compustat, USPTO patent DB, and USPTO PAIR DB). It is comparatively straightforward to identify who the first time seller is (vs. buyer) because the seller is also the producer of the patent. I first identify all of the different names (e.g., IBM, I.B.M, International B.M.) used for selling patents along with its GVKEY (6066). In identifying GVKEY of the buyer, I use the previous list of names used to sell patents. For example, if the buyer is “International B.M.,” I consider the buyer of this transaction to be IBM and assign 6066 for GVKEY. I also augment this initial matching using the NBER standardization algorithm (available at <https://sites.google.com/site/patentdatapoint>).