Competition, Non-Patented Innovation, and Firm Value*

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Abstract

This paper studies how competition impacts non-patented corporate innovation and firm value by exploiting adoptions of state anti-plug molding laws – laws that prohibit "unscrupulous" reverse engineering by competitors – and their subsequent invalidation by the U.S. Supreme Court. Firms decrease patenting activity following the laws' adoptions while also showing increasing investment spending, profitability, and value. Value gains are larger for firms at greater risk of imitation, and that are more innovative. After the laws are overturned, firms reinitiate patenting whereas prior investment spending, profitability, and value gains dissipate. These results suggest that more intense product market competition disincentivizes value-enhancing corporate innovation.

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1. Introduction

Does competition affect firm value by hindering technological innovation? Prior research has commonly used empirical proxies of product market competition – such as the Herfindahl-Hirschman index, market share, and the Lerner index – to study this question (e.g., Sundaram et al., 1996; Blundell et al., 1999; Aghion et al., 2005; Greenhalgh and Rogers, 2006; Gu, 2016). Yet, findings on the relation between competition, innovation, and firm value tend to be mixed and proxy-dependent (see Cohen, 2010, for a review).[1] Two key empirical obstacles render the identification on the relationship between these three quantities challenging. First, causality could run in the reverse direction if the emergence of concentrated industries is a natural consequence of past innovation conducted by initially successful firms, i.e., with success breeding success. Second, economic conditions and other exogenous factors could also simultaneously codetermine competition, innovation, and firm value.

Theory does not give unambiguous predictions of the direction of competition's impact on corporate innovation and value (e.g., see Aghion et al., 2001, 2015; Gilbert, 2006; Cohen, 2010). On the one hand, consistent with Schumpeterian growth theory, more intense product market competition could reduce the flow of rents to successful innovators and thereby reduce their incentives to innovate and grow (e.g., Gilbert and Newberry, 1982; Aghion and Howitt, 1992; Caballero and Jaffe, 1993). Relatedly, weaker intellectual property protection and lower imitation costs may reduce the expected duration of rents from innovation and thus moderate innovative firms' R&D incentives (e.g., Dasgupta and Stiglitz, 1980; Davidson and Segerstrom, 1998).

On the other hand, firms operating in more competitive industries might receive greater gains from their innovation and thus, have more significant incentives to invest in R&D. This argument is consistent with Arrow's (1962) "replacement effect," where in equilibrium an incumbent monopolist does not innovate since this would partially displace the rents it already earns (e.g., Aghion and Howitt, 1992). By comparison, because firms in a competitive industry are not earning monopoly profits, they have more potential to realize the full return on their innovative activity. Further still, a third prediction suggests a non-linear, "inverted-U" relationship between

[1] For instance, Blundell et al. (1999) find that market share is positively associated with corporate innovation measured by patents, while that of overall market concentration is negative, and that a positive correlation between corporate innovation and market value is stronger for firms with higher market shares. In contrast, Gu (2016) uses a double-sorting portfolio approach on R&D and market concentration and finds that R&D-intensive firms in less concentrated industries earn higher expected returns.

competition and technological innovation, and between competition and growth. Aghion et al. (2005) consider innovation that occurs step-by-step (i.e., a laggard firm has to catch-up with the leader before it can become the leader itself), such that industries are either "neck-and-neck" or "unleveled." In the former case, increased product market competition encourages neck-and-neck firms to innovate to "escape from competition." In the latter case, increasing the intensity of competition in unleveled industries discourages innovation by laggard firms since it reduces any short-run incremental profit from catching the leader.

This paper tests these conflicting predictions by shifting the focus from endogenous proxy variables to a tandem of unique and arguably exogenous events that directly influence the intensity of product market competition. In particular, I exploit the quasi-natural experiments provided by the staggered adoption of anti-plug molding laws (APMLs) by U.S. state legislatures over the period 1978 to 1987 and their subsequent invalidation by the U.S. Supreme Court in 1989 to study how competition affects firm value through its impact on technological innovation.

APMLs decreased product market competition for manufacturing firms headquartered in enacting states by prohibiting rivals (both within and outside of the adopting state) from using an "unscrupulous" form of reverse engineering[2] – the direct molding process – to make an identical but competing product. The direct molding process provides a competitive cost advantage[3] for laggard and new entrant firms by allowing them to use an incumbent's existing product itself as a "plug" to form a mold. From this mold, duplicate items can be mass-produced at a small fraction of the incumbent's total production costs. States that adopt APMLs to exclude competition via the direct molding process pass one of two types of laws: those that protect *all* manufacturing products (All-APMLs) or those that only stipulate coverage for *boat*-related products (Boat-APMLs). This paper focuses on the impact of All-APMLs, using the latter type as a placebo.

These quasi-natural experiments enable the study of the effect of competition on *non-patented* innovation. APMLs (as I will show) provide a compelling partial substitute to the patent system. That is, unlike patents, which require that an invention be formally disclosed in a technically precise and standardized format to receive protection against competitive practices such as reverse engineering, APMLs provide indefinite protection against the direct molding process without any

[2] Reverse engineering's legal definition is described as "starting with the known product and working backward to divine the process which aided in its development or manufacture" (*Kewanee Oil Co.* v. *Bicron Corp.*, 416 U.S. 476).
[3] Namely, by decreasing the costs of developing and introducing an imitative product (Mansfield et al., 1981).

requisite disclosure. The U.S. Supreme Court would cite APMLs' conflict of interest with U.S. federal patent law in its invalidating decision, stating "[*APMLs*] allow petitioner[*s*] to reassert a substantial property right in the idea, thereby constricting the spectrum of public knowledge. Moreover, it does so without the careful protections of high standards of innovation and limited monopoly contained in the federal schemes. We think it clear that such protection conflicts with the federal policy" (*Bonito Boats* v. *Thunder Craft*, 489 U.S. 160).

While patented innovation has been studied extensively, relatively little is known about non-patented innovation (Hall et al., 2014). Yet, according to prior survey evidence, only a small portion of innovative firms depend on patents to protect their intellectual property (IP). Rather, firms report a heavier dependence on alternative mechanisms such as lead time and trade secrecy[4] (e.g., Mansfield, 1986; Levin et al., 1987; Cohen et al., 2000; Arundel, 2001). Similarly, in the most recent publication of the National Science Foundation's Business Research and Development and Innovation Survey (BRDIS), 58.9% of respondents reported that trade secrets were a "very important" type of IP protection, with only 15.4% indicating that it was "not important." By comparison, 44.3% and 18.3% said utility patents and design patents, respectively, were very important, while 36.5% and 54.6%, respectively, reported they were not (BRDIS, 2015).[5]

To the best of my knowledge, this paper is the first to utilize the APML quasi-natural experiments to study changes in product market competition, and, more specifically, to investigate those changes' effect on non-patented innovation and firm value. The study begins by examining whether APML adoptions (and the U.S. Supreme Court's decision to invalidate them) can plausibly be considered as exogenous events. I find that amongst a comprehensive set of predictor variables (e.g., macroeconomic and political factors, and previously enacted corporate laws) that may influence whether state legislatures adopt APMLs, none are associated with the laws' passage. I also find that All-APML-affected firms experience abnormal stock returns of about -0.5% on the day the U.S. Supreme Court's decision to overturn the laws was announced, suggesting that the loss of protection from direct mold process reverse engineering is both costly and unexpected.[6]

[4] Trade secrets can take shape as devices, formulas, practices, processes, designs, or other compilations of information that are not easily ascertainable by others, by which they provide an economic advantage for their holder.
[5] Utility patents protect the functional features of an invention, while design patents protect its appearance.
[6] An additional investigation into the relevancy of APML adoptions for product market competition show that state-specific industry concentration levels and Lerner indices are significantly higher for states that pass All-APMLs.

The effects of APML adoptions on non-patented innovation and firm value are estimated using a difference-in-differences approach in which the group of treated firms are headquartered in states that have adopted the laws, and the group of control firms are headquartered elsewhere. The regressions include firm and industry-by-year fixed effects to ensure that comparisons of within firm changes in outcomes are restricted to treatment and control groups that operate in the same industry. These models also control for the relevant legal context in which APMLs were adopted, local economic and political conditions, and firm characteristics. I find that the reduction in product market competition brought about by the passage of All-APMLs (and not before) significantly reduces affected firms' patenting activity, relative to industry rivals, while increasing their investment spending, profitability, and Tobin's Q. For example, relative to their sample means, patent counts decrease by 6%, R&D spending increases by 9.1%, gross profitability by 3.6%, and Tobin's Q by 6.4%.

The analysis is extended using a triple-difference approach to estimate the separate effects of APML adoptions and their invalidation on non-patented innovation and firm value. Using the same fixed effects and controls, I continue to find decreases in patent outputs and increases in investment spending, profitability, and Tobin's Q when the laws are valid. However, following the U.S Supreme Court's ruling, All-APML-affected firms show a significant resurgence in patenting activity, while the previous increases in investment spending, profitability, and Tobin's Q dissipate. The results related to firm value continue to hold in both the double-difference and triple-difference specifications if, instead of Tobin's Q as a measure of value, the models employ risk-adjusted excess stock returns or Total Tobin's Q (proposed in Peters and Taylor, 2017).

Because a limited number of states adopt laws that protect all manufacturing items, it may be that omitted variables that correlate both with the passage of the laws and the outcomes spuriously drive the main results by influencing post-treatment trends in patent activity, investment spending, profitability and Tobin's Q. Two features of my empirical framework help address this concern. First, because there are very few publicly traded boat-manufacturers in the sample, I am able to exploit the Boat-APMLs as a placebo test since nearly all firms headquartered in these states are non-boat manufacturers and are not affected by their states' adoption of Boat-APMLs. Consistent with All-APMLs being the true source of the results, the coefficients for Boat-APML estimators are always insignificant. Second, the identification strategy is further enriched by the U.S. Supreme Court's invalidation of the laws, as it provides a counter-effect to the APMLs. Thus, a scenario in which omitted variables correlate with the laws' adoptions and the outcomes in one direction and the Supreme Court's ruling and these outcomes in the other direction seems unlikely.

To rule out that the results are explicitly driven by local confounders (e.g., state-level economic conditions or previously enacted corporate laws), I exploit a unique feature of the quasi-natural experiments in which only manufacturing firms with "moldable products" are affected by APMLs. Because the direct molding process uses an existing product to fit a mold, firms that do not have moldable products, such as food and beverage manufacturers, should not be affected by the laws. Using this distinction, I compare APMLs' impact on firms operating in non-moldable products-industries in adopting states with similar firms located elsewhere. The idea behind this comparison is that if there is some state-level confounding factor at play, it should impact all firms headquartered in the state, and not be exclusive to the same subset affected by the laws. Reassuringly, there is no evidence of significant changes in non-patented innovation or firm value for non-moldable product-firms headquartered in adopting states.

To shed further light on the channel underlying the findings on value, I exploit two sources of heterogeneity in firm characteristics. First, because patents already provide protection from reverse engineering, patenting firms headquartered in APML states face a lower risk of product imitation by competitors, and therefore likely benefit less from the laws. Additionally, these same firms also likely have a cost advantage in patenting relative to non-patenting companies (e.g., Kultti et al., 2006, 2007), but because this competitive cost advantage moderates with the laws, it might also be that these firms gain less. Second, since APMLs primarily benefit the innovative incumbents, affected firms with an innate ability to innovate should experience the most substantial increases in value (e.g., Knott, 2008; Cohen et al., 2013). In line with these predictions, the gains in value following the passage of All-APMLs are less pronounced for patenting firms and more pronounced for firms with "patentless R&D," or greater innovative ability, and, following the laws' invalidation, the most innovatively capable firms are most adversely impacted.

Finally, I compare risk-adjusted excess returns of firms headquartered in APML adopting states with those of industry rivals headquartered in non-adopting states on the day that the U.S. Supreme Court's decision to overturn the laws is announced. The results indicate that All-APML-affected firms on average, and especially for those that are less likely to rely on patents for IP protection or that have greater innovative ability, experience significantly negative excess returns of about -0.3% to -0.6% on the day of the announcement. This finding suggests that the loss in protection from the direct molding process, and thus, a reinstatement of more intense product market competition, was costly for innovative firms.

Overall, this study documents that firms increase non-patented innovation when it becomes more costly for competitors to reverse engineer their products and that this, in turn, is valuable for shareholders. These findings are consistent with Schumpeterian growth theory by which less intense product market competition increases rents to innovative incumbents, thereby increasing their incentives to innovate and grow (e.g., Gilbert and Newbery, 1982; Aghion and Howitt, 1992). My research contributes new evidence to the literature on competition and corporate innovation (e.g., Blundell et al., 1995; Aghion et al., 2005; Goettler and Gordon, 2011; Spulber, 2013), and competition and firm value (e.g., Lindenberg and Ross, 1981; Nickell, 1996; Sundaram et al., 1996; Hou and Robinson, 2006; Gu, 2016; Bustamante and Donangelo, 2017).

The results in this paper are related to the studies of Blundell et al. (1999) and Greenhalgh and Rogers (2006). Using U.K. data, both of these studies show that less intense competition is positively associated with innovative firms' valuations. My paper differs from these two in several ways. Most importantly, rather than studying endogenous proxies for competition, the APMLs enable the testing of how innovative firms causally respond to a "shock" in their competitive landscape, and how the market values those responses. Further, because U.S. IP laws can materially differ with those in the U.K.,[7] this paper offers unique insights on how U.S. IP policy affects innovative firms.

More recent related work examines how changes in competition stemming from trade shocks and financial constraints affect an innovative firm's performance and investment behavior (e.g., Hombert and Matray, 2018; Grieser and Liu, 2019; Malamud and Zucchi, 2019). My contribution is to focus on a specific source of competitive pressure, namely, the strength of a firm's IP protection. In particular, this paper shows that the ability of laggard and new entrant firms to reverse engineer a product at low cost is a first-order competitive threat that influences incumbents' patenting and investment activities, and, ultimately, their financial values. Finally, I contribute to prior work on the value of corporate innovation (e.g., Hall et al., 2005; Hsu, 2009; Kogan et al., 2017). However, unlike most previous studies (see Moser, 2012; Simeth and Cincera, 2015; Li et al., 2018, for examples of exceptions), this paper examines how *non-patented* innovation affects value, which survey-based evidence suggests is of utmost importance for most firms.

[7] E.g., see U.S. and U.K. opposing judicial rulings in *Apple Inc.* v. *Samsung Electronics Co.*'s infamous patent dispute.

2. Institutional background

2.1. Reverse engineering and the direct molding process

There are two methods for engineering manufacturing products: forward engineering and reverse engineering. Forward engineering is the conventional process of progressing from high-level ideas to their material implementation. This usually includes the preparation of engineering drawings from which models and then molds are formed for the end-goal of mass production. In contrast, reverse engineering is the process of recreating a finished product without the original plans, drawings, models, or molds (Raja, 2007). Rather, the reverse engineer analyzes the design and components of the existing product to discover and extract its "know-how." In general, reverse engineering is a widely accepted tool for innovation. However, the incentives of forward engineers can be compromised when reverse engineering becomes a relatively costless and quick way to make a competing product (Samuelson and Scotchmer, 2002).

The "direct molding process" is a specific form of reverse engineering that provides an efficient way to duplicate manufacturing products (Brown, 1986). Direct molding involves using an existing product itself as a "plug" to form a mold, upon which imitations of the original product can be manufactured. The typical process entails a competitor spraying an existing product with a mold forming substance (e.g., fiberglass) (that sets quickly and hardens), and then removing the original product and using the remaining mold to produce a replica (i.e., undifferentiated) product (Sganga, 1989); this in turn benefits imitators by allowing them to circumvent the R&D, design and manufacturing costs (i.e., imitation costs) incurred by the originating firm (Devience, 1990). Thus, direct molding process reverse engineering transformed competition in the manufacturing industry by providing laggards and new entrants a competitive cost advantage over incumbents.

2.2. Anti-plug molding laws (APMLs)

On October 1, 1978, California passed an anti-plug molding law (APML), prohibiting the duplication and sale of *all* products manufactured by the direct molding process. The law defined direct molding as "any process in which the original manufactured item was itself used as a plug for the making of the mold which is used to manufacture the duplicate item" (Cal. Bus. & Prof. Code § 17300[c]).[8] Eleven other states followed, enacting similar statutes to protect local

[8] California's APML authorizes injunctions against those found guilty of its violation and provides for actual damages, and mandatory attorney fees and costs for prevailing plaintiffs (Cal. Bus. & Prof. Code § 17301).

consumers and manufacturers from plug molding reverse engineering (conducted within and outside of its state). Although, only Michigan and Tennessee, adopted APMLs identical to California's, which protected *all* manufactured products (Carstens, 1990). The other nine states (Florida, Indiana, Kansas, Louisiana, Maryland, Mississippi, Missouri, North Carolina and Wisconsin) passed legislation only prohibiting plug molding duplication of originally manufactured hulls and components of boats (Crockett, 1990).

In terms of the laws' jurisdictional scope, the history of court cases related to APMLs suggests that the relevant jurisdiction for firms filing lawsuits to protect their products from the direct molding process is generally the state where the plaintiff maintains its principal place of business (Althauser, 1989; Carstens, 1990; Heald, 1990), which is typically interpreted as the firm's headquartering state (e.g., Ribstein and Kobayashi, 1996; Almeling et al., 2010).[9] As a result, APMLs administer reverse engineering protection for a firm even when the accused duplicator is located in a different state that has not enacted one of these laws.[10] Panel A of Table 1 details which states adopted a law, its respective statute name and month/year of adoption, and whether it covers all manufacturing items or only boat-related products. The first state to adopt an APML is California in 1978, and the last state to adopt is Indiana in 1987. The number of states passing APMLs in the interim period of 1978 to 1987 is fairly evenly distributed with four states' passing laws in 1983, one state in 1984, three states in 1985, and two states in 1986.

2.3. Related court decisions leading to the invalidation of APMLs

In July of 1984, the constitutionality of California's APML was brought into question when *Interpart Corp.* (Interpart) sued *Imos Italia* (Italia), *Vitaloni, S.p.A.* (Vitaloni) and *Torino Industries, Ltd.* (Torino), seeking a determination of their rights to copy unpatented products first developed and sold by the defendants (Shipley, 1990). The two firms, Interpart and Italia, competed in the Southern California aftermarket for automobile rearview mirrors. Interpart admitted to copying the mirrors (via a direct molding process) sold by Italia and, co-party, Torino, which were first developed by Vitaloni for notable clientele like Ferrari and Lamborghini (Devience, 1990). Interpart filed its pre-emptive suit against the defendants claiming that its manufacture and sale of automobile rearview mirrors was not in breach of the California Business

[9] The originating or duplicating firm's state of incorporation does not impact these laws' jurisdiction.
[10] APMLs also provide protection against products duplicated via the direct molding process by foreign entities, if the duplicated product is then exported into the U.S. for domestic sale.

and Professional Code. In response, Vitaloni applied and was granted a design patent for their rearview mirrors and subsequently counter-sued Interpart for patent infringement and copying its mirrors using the prohibited (by Californian law) direct molding process (Shipley, 1990).

On July 30, 1984, the Central District Court of California ruled that Vitaloni's design patent was invalid since it had been granted more than one year since its initial sale to the public and that the plug molding claim was unsubstantiated. Further, the district court held that California's APML was preempted by federal patent law (Wong, 1990). The preemption ruling was based on the fact that, unlike APMLs which do not stipulate a trade-off of disclosure for protection, federal patent law requires that the "know-how" of an invention be made public in order to receive formal protection from competitive practices such as reverse engineering. Vitaloni appealed the direct molding claim and the preemption judgment of California's APML. The appeal was transferred to the Court of Appeals for the Federal Circuit, which has exclusive federal appellate jurisdiction in cases arising under patent laws (Shipley, 1990).

In November of 1985, the Federal Circuit upheld the constitutionality of California's APML, reversing the district court's decision. Further, the court found Interpart guilty of copying Vitaloni's products by way of a direct molding process (Devience, 1990). The Federal Circuit reasoned that California's law was not "an obstacle to the accomplishment and execution of the full purposes and objectives of Congress" (*Interpart Corp.*, 777 F.2d at 684) and therefore not preempted by federal patent law. Moreover, the court stated that: "It is clear from the face of the statute that it does not give the creator of the product the right to exclude others from making, using, or selling, the product as does the patent law…The statute prevents…competitors from obtaining a product and using it as the 'plug' for making a mold. The statute does not prohibit copying the design of the product in any other way; the latter, if in the public domain, is free for anyone to make, use, or sell" (*Interpart Corp.*, 777 F.2d at 684, 685).

In addition to *Interpart Corp.* v. *Imos Italia*, there were several other court cases invoking APMLs[11] – some legal scholars believe there would have been many more cases if not for the strong pro-plaintiff bias of the statutes (Sganga, 1989). However, of those that were tried, the most

[11] For example, see *Metro Kane Imports, Ltd.* v. *Rowoco, Inc.* (product: orange juicer); *Gemveto Jewelry Co.* v. *Jeff Cooper, Inc.* (product: jewelry); *Summerford Racing, Inc.* v. *Shadow Boat, Inc.* (product: boat parts); *Power Controls Corp.* v. *Hybrinetics, Inc.* (product: electrical parts); *Brahma, Inc.* v. *Joe Yeargain, Inc.* (product: truck camper shells); *Gladstone* v. *Hillel* (product: jewelry); *Bonito Boats, Inc.* v. *Thunder Craft Boats, Inc.* (product: boat hulls).

notable took place in Florida, where two boat manufacturers battled in the courts over the constitutionality of the state's APML.

In September of 1976, a Florida-based company, *Bonito Boats, Inc.* (Bonito), began the development, design, and manufacture of an original recreational boat hull which, upon completion, was marketed under the trade name "Bonito Boat Model 5VBR" (Heald, 1990). The Model 5VBR was sold to a broad interstate market; however, no patent applications were filed by Bonito to protect either the utilitarian or design aspects of the boat hull (Carstens, 1990). Meanwhile, *Thunder Craft Boats, Inc.* (Thunder Craft) – a company located in Tennessee – observed the success of the Model 5VBR in the open market, and consequently, copied the hull for its own commercial purposes by way of a direct molding process, using the Model 5VBR as its mold to manufacture imitations and distribute as its own creation under the trade name "Capri" (Carstens, 1990).

On May 3, 1983, the Florida legislature adopted an APML prohibiting the use of a direct molding process to duplicate boat hulls or other components for the ends of selling the copied products. Soon thereafter, on December 21, 1984, Bonito brought suit against Thunder Craft for violating the Florida law (Carstens, 1990). However, the Orange County Circuit Court in charge of the case dismissed Bonito's suit, ruling that the state's APML was preempted by federal patent law (Heald, 1990). Bonito eventually appealed to the Florida Supreme Court and on November 12, 1987, in a 4-to-3 ruling, the Court affirmed the lower court's invalidation of the statute (Wong, 1990). The majority four judges reiterated that "when an article is introduced into the public domain, only a patent can eliminate the inherent risk of competition and then but for a limited time" (*Bonito Boats*, 515 So. 2d 220 at 222).

With no other alternatives, Bonito petitioned the U.S. Supreme Court, requesting a resolution in the conflicting judgments between the Florida Supreme Court and the Federal Circuit in *Interpart Corp.* v. *Imos Italia*. The U.S. Supreme Court granted Bonito's petition and heard its appeal on December 8, 1988 (Shipley, 1990). Bonito presented a twofold argument as to why Florida's APML was constitutional, and not preempted by federal patent law. The first argument centered on the assertion that the plug molding law did not afford the same level of protection provided by patents – since the APML only protected against the direct molding process while patents protect against all forms of reverse engineering. The second argument asserted that the

Florida statute was a legitimate exercise of Florida's authority to protect local business interests by regulating and discouraging unfair and "unscrupulous" competition (Carstens, 1990).

On February 21, 1989, the U.S. Supreme Court affirmed the ruling of the Florida Supreme Court and rejected the Federal Circuit's decision in *Interpart*. The Supreme Court concluded that Florida's statute granted substantially similar rights to boat manufacturers as to those conferred to a patentee by excluding competitors from making and selling duplicates procured by the direct molding process (Heald, 1990). Further stating that "the competitive reality of reverse engineering may act as a spur to the inventor, creating an incentive to develop inventions that meet the rigorous requirements of patentability" (*Bonito Boats*, 489 U.S. at 160). Because of this ruling, every states' APML was effectively invalidated (Carstens, 1990).[12] Panel B of Table 1 summarizes essential court decisions related to APMLs, providing information on the jurisdiction, the level of the court hearing the case, case name, the month/year when it was decided, and the eventual ruling.

3. Data

3.1. Independent variables

The main independent variables in this study are indicators that capture whether a firm is headquartered in an All-APML adopting state ($All\ APML$) or in a state with a Boat-APML ($Boat\ APML$). APML adopting states are identified from prior legal scholarship on the topic. In particular, Sganga (1989), Carstens (1990), Crockett (1990), and Heald (1990) provide the enacting states' statute name. With these statute names, I then use the *LexisNexis Academic* "State Statutes and Regulations Search" option to verify the details of both types of APMLs, and to establish the month and year in which they were adopted.

I then match APMLs to the state in which a firm is headquartered (for examples of studies assigning state laws by a firm's headquarters, see Chava et al., 2013; Acharya et al., 2014; Cornaggia et al., 2015; Mukherjee et al., 2017; Klasa et al., 2018) since this is generally where a firm's major plants and operations are located (Henderson and Ono, 2008), and thus the relevant jurisdiction for the law. To ensure that the firms' headquartering states are historically accurate, I use historical location information from the CRSP Historical U.S. Stock database. The CRSP Historical dataset spans the period 1990-2015. To approximate the state of headquarters for the

[12] Congress enacted the Vessel Hull Design Protection Act (VHDPA) in 1998 as part of the Digital Millennium Copyright Act to protect boat hulls and its component parts from the direct molding process. However, the VHDPA was too late for Bonito Boats, as the company shut down in 1990.

sample years 1975-1989, firm-year headquarters are backfilled using the oldest data point of information available.[13] The remaining missing headquartering state values are supplemented with Compustat's data on current states of location.

From the above information, I create the $All\ APML$ and $Boat\ APML$ indicator variables, which are set equal to one for firms headquartered in these respective states in the year of and after its corresponding adoption date, and zero in the years prior to the adoption date, or always zero for firms in states that never enact an APML. In addition, these indicators are adjusted for firms headquartered in either California or Florida dependent on important court decisions validating (*Interpart* v. *Imos Italia*) or invalidating (*Bonito* v. *Thunder Craft*) the laws in their respective jurisdictions (following a similar adjustment approach in Klasa et al., 2018).

The main interacted variables in my tests involve the multiplication of $All\ APML$ and $Boat\ APML$ with the indicator variable, $Post88$, which is set equal to one in the years after 1988, and zero before. These interacted variables capture the change in the legal and competitive environment engendered by the U.S. Supreme Court's ruling (on February 21, 1989) that invalidated both types of APMLs.

Also included is a vector of control variables that can be grouped by their level of exogeneity. I take into account the relevant legal context into which APMLs were adopted, by including dummies for "Other Law Controls," such as antitakeover statutes (FGL; BCL; CSL; DDL; FPL; PPL) (Karpoff and Wittry, 2018), trade secrets law ($UTSA$)[14] (Png, 2017a, 2017b), R&D tax credits ($R\&D\ Credit$) (Wilson, 2009), and wrongful discharge law (GFE) (Serfling, 2016). After showing the main results with these controls, the models then append on less exogenous "State-Level Controls" to account for local economic and political conditions. These include: GDP per capita ($Ln(GDPPC)$) and GDP growth ($GDP\ Growth$), and the percentage of a state's U.S. House of Representatives that belong to the Democratic Party ($Democrat$). Data for these controls comes from either the U.S. Bureau of Economic Analysis or the U.S. House of Representatives.

Finally, in adherence with prior literature, some of the tests also include endogenous "Firm-Level Controls" such as: $Ln(Assets)$, $Ln(Age)$, $Debt$, ROA, OCF, HHI, SG, $Loss$, $FLIQ$,

[13] Backfilling the majority of firm-years in the sample should not be a major concern as prior empirical evidence suggests that firms likely do not switch headquartering states very often (e.g., Pirinsky and Wang, 2006).

[14] Another trade secrets law (IDD) (Klasa et al., 2018) is also considered; however, almost all of the variation associated with its adoption transpires outside of the sample period, prohibiting its estimation with firm fixed effects.

$R\&D/Sales$, and $CAPX/Assets$. Compustat is the main source for these controls and their definitions can be found in Appendix A.

3.2. Sample selection and descriptive statistics

The main sample consists entirely of manufacturing firms in the CRSP/Compustat Merged database that make products that could actually be copied using the direct molding process. To classify firms as manufacturers of "moldable products" I review each two-digit standard industrial classification (SIC) code industry within the manufacturing sector (SIC codes 2000-3999) and exclude those industries where it is unlikely that APMLs would apply. For example, excluded from the sample of firms producing moldable products are companies that operate in the "Food and Kindred Products," "Tobacco Products," "Apparel and other Finished Products Made from Fabrics and Similar Materials," and "Paper and Allied Products" industries in which a competitor would be unable to directly use another firm's product as a plug to fit a mold. Table 2 summarizes the complete list of two-digit SIC code industries included and excluded from the main sample.[15]

Beyond being a company in a moldable products-industry, I also require that the sample firms are headquartered and incorporated in the U.S., are without missing or negative book value of assets and net sales, and have the requisite data to construct the variables used in the main tests. Continuous firm-level variables are winsorized at the 2.5% level in both tails to mitigate the influence of extreme outliers,[16] and dollar values are adjusted for inflation using 2015 dollars. The sample period is from 1975 to 1992, which begins three years before the first state adopts an APML and ends three years after the U.S. Supreme Court invalidates all of these laws. This selection criterion yields a sample of 2,075 firms and 17,600 firm-year observations.

Contained in this final sample are 445 firms and 3,530 firm-years that belong to states that adopt APMLs that protect all manufacturing items (All-APML) from the direct molding process, while 249 firms and 2,169 firm-years correspond to states that pass laws that specifically protect boat hulls and its parts (Boat-APML). Unfortunately, however, given the scarcity of publicly traded boat manufacturers, the sample only contains 3 firms (and 24 firm-year observations) that manufacture boat-related products that are affected by Boat-APMLs. Therefore, given the

[15] In the Internet Appendix, I show that the main results continue to hold if, instead, all firms in the manufacturing sector are used in the tests. Although, as expected, the statistical and economic significance of the coefficients in these tests are generally muted by including firms in which the APMLs do not actually apply.

[16] The findings are similar if, instead, these variables are winsorized at the 1% or 5% level in both tails.

limitations of the data, the main tests focus on the effect of All-APMLs on affected firms' non-patented innovation and value and only considers the Boat-APMLs as a placebo test since the majority of firms headquartered in these states (246-out-of-249) are not boat manufacturers and therefore should not be affected by their states' laws.

Internet Appendix Table A1 presents descriptive statistics on the mean, standard deviation, 25th, 50th, and 75th percentiles, and the total number of observations for the variables used in the main tests over the sample period 1975 to 1992. Overall, the summary statistics for the data are similar to those in prior competition, corporate innovation, and Tobin's Q-based studies.

4. Identification strategy

4.1. Empirical specifications

To identify the implications of a reduction in product market competition, I employ a difference-in-differences ("DD") OLS regression model that compares changes in non-patented innovation or value amongst firms headquartered in states with either an All-APML or Boat-APML with those of firms headquartered somewhere else.

In particular, I estimate

$$y_{ijs(t+n)} = \beta_1 All\ APML_{st} + \beta_2 Boat\ APML_{st} + \alpha' \mathbf{X}_{ijst} + \gamma_i + \lambda_{jt} + \varepsilon_{ijst} \qquad (1)$$

where y is the outcome of interest for firm i, operating in industry j, headquartered in state s, in year $t+n$. $All\ APML$ and $Boat\ APML$, are indicators for whether a firm's headquartering state s has adopted an APML that protects all manufacturing items from the direct molding process or only boat-related products as of the current year t, respectively. Although, as mentioned in subsection 3.2, given the data limitations on publicly traded boat manufacturers, the coefficient on $Boat\ APML$ is interpreted as a placebo estimate since nearly the entirety of firms headquartered in these states (246-out-of-249) are unaffected by the laws. Further, $\mathbf{X}$ represents a vector of controls (defined in Appendix A) while γ represents firm fixed effects, included to control for unobserved, time-invariant heterogeneity within firms, and λ denotes industry-by-year interacted fixed effects, specified to control for time-varying heterogeneity within industries. Lastly, standard errors are clustered by the state of headquarters since $All\ APML$ and $Boat\ APML$ are state-level variables.

These regressions include industry-by-year fixed effects to restrict comparisons of within-firm changes in outcomes for companies headquartered in each type of APML adopting state to firms headquartered in states absent these laws but operating in the same two-digit SIC code industry. This is a necessary restriction as it ensures that inferences are robust to certain industry-level sources of unobserved, time-varying heterogeneity that could bias estimates. This includes M&A activity (as merger waves tend to occur within industries, e.g., Mitchell and Mulherin, 1996; Maksimovic and Phillips, 2001; Rhodes-Kropf et al., 2005) and regional economic conditions (as industries tend to cluster by geography, e.g., Ellison and Glaeser, 1997, 1999; Ellison et al., 2010).

In addition, the identification strategy is further enriched by exploiting the invalidation of APMLs by the U.S. Supreme Court's 1989 decision in *Bonito* v. *Thunder Craft* using the following triple-differences ("DDD") model:

$$y_{ijs(t+n)} = \beta_1 All\ APML_{st} + \beta_2 Boat\ APML_{st} + \beta_3 Post88_t \times All\ APML_{st} + \beta_4 Post88_t \times Boat\ APML_{st} + \alpha'\mathbf{X}_{ijst} + \gamma_i + \lambda_{jt} + \varepsilon_{ijst} \quad (2)$$

where $Post88 \times All\ APML$ and $Post88 \times Boat\ APML$ measure the change in non-patented innovation and value after 1988 for firm i, operating in industry j, headquartered in a state s with either an All-APML or Boat-APML, as of year t. The indicator variable $Post88$ is set equal to one beginning in 1989 and afterward, and otherwise equal to zero, but is excluded from the regression due to multicollinearity with the industry-by-year fixed effects. Besides the addition of the two interaction terms, the empirical specification in model (2) is the same as that in model (1).

4.2. Endogeneity concerns and the APML quasi-natural experiments

The key assumption in using DD and DDD models to exploit the APML quasi-natural experiments is that absent these events, the non-patented innovation and financial value of firms located in states that passed and did not pass the laws would have evolved similarly.[17] To test the validity of this parallel trends assumption, it is critical to rule out two central endogeneity concerns common amongst studies that use state policy changes for identification. The first concern is that the adoption and invalidation of APMLs were brought about by changes in local macroeconomic and political economy conditions, which in turn are the "true" source of changes in a firm's non-patented innovation and value (i.e., omitted variables problem). The second concern is that states passed APMLs and the U.S. Supreme Court later ruled against them with the intention to achieve

[17] Another crucial assumption of the identification strategy is that, by prohibiting laggard and new entrant firms' ability to use direct molding process reverse engineering, the adoption of an APML in a firm's state decreases the intensity of its product market competition. Internet Appendix Table A2 shows that the adoption of All-APMLs significantly increases industry concentration and Lerner indices within those states.

certain technological innovation and firm value implications (i.e., reverse causality). The following subsections evaluate whether these two endogeneity concerns plague the APML quasi-natural experiments.

4.2.1. The passage of APMLs by state legislatures

To assess the seriousness of these concerns for the passage of APMLs, I follow prior literature (e.g., Acharya et al., 2014; Serfling, 2016) and analyze the predictability of their enactment. Specifically, I estimate a linear probability model (LPM), where the dependent variable is the adoption of either an All-APML or a Boat-APML, and where independent variables include state-level macroeconomic, corporate law, political economy, and firm factors that *a prior* might predict these laws' passage. For instance, to gauge the validity of a possible reverse causality problem state-year (SY) median levels and changes (Δ) in the number of patents ($SY\ Ln(1 + Patent)$ and $SY\ \Delta\ Ln(1 + Patent)$) and the Tobin's Q ($SY\ Tobin's\ Q$ and $SY\ \Delta\ Tobin's\ Q$) of all sample firms headquartered in a state are specified.

Further, to explore the likelihood of an omitted variables problem, I include indicators for whether an eventual APML adopting state has already enacted any of the most common antitakeover, trade secrets, or wrongful discharge laws, and/or R&D tax credits. Also included are a number of state-level variables that proxy for local economic and political factors such $Ln(GDPPC)$, $GDP\ Growth$, $Democrat$, a state's level of population ($Ln(Population)$), and rates of unemployment ($Unemployment$), and entry ($Est\ Entry$) and exit ($Est\ Exit$) of local establishments – data for the last four variables comes from either the U.S. Census Bureau or the U.S. Bureau of Labor Statistics. In addition, each LPM specifies year fixed effects to control for transitory U.S.-wide conditions (e.g., macroeconomic) that could impact the likelihood that a state adopts an APML, while supplemental specifications also include headquartering state fixed effects to account for unobserved, time-invariant heterogeneity within adopting states.

The sample period is 1975 to 1988 (i.e., the period when APMLs are legally valid) and each of the predictor variables is measured in the year before the law's adoption. Moreover, states are dropped from the analysis once they pass an APML (i.e., a "failure event" occurs). Finally, for ease of comparison, continuous variables are standardized to have a mean of zero and a standard deviation equal to one, and standard errors are adjusted for clustering at the state of headquarters level. Table 3 presents the results.

The evidence across each of the four columns suggests that the variation created by the adoption of both $All\ APML$s (columns 1-2) and $Boat\ APML$s (columns 3-4) is plausibly exogenous to the then-prevailing local policy conditions in which the laws were passed. For instance, the point estimates on proxies for a state's local economic and political economy conditions are insignificantly different from zero in the LPMs that only include year fixed effects (columns 1 and 3), as well as in specifications that also include headquartering state fixed effects (columns 2 and 4). Moving down the rows of potential determinants to dummies for other state-level corporate laws, I find that the passage of APMLs is not significantly associated with them either. This is an important non-result because many of these other laws are also enacted during the sample period and have the potential to confound the APML quasi-natural experiments. Overall, these findings are consistent with the assumption that economic and political factors and prior corporate law enactments did not significantly impact state legislatures' adoption of APMLs.

Finally, the predictors for a state's median level and change in patent counts and Tobin's Q are also unable to predict the adoption of either an $All\ APML$ or a $Boat\ APML$. This is reassuring evidence that reverse causality is likely not a problem for my strategy that aims to identify the effect of decreased competition in products (via APML adoptions) on a firm's non-patented innovation and value.

4.2.2. The invalidation of APMLs by the U.S. Supreme Court

I next explore the exogeneity of the U.S. Supreme Court's 1989 ruling in *Bonito v. Thunder Craft*. First, I argue that it is highly improbable the U.S. Supreme Court would overturn lower court rulings with the intentions of altering corporate innovation or value. Thus, the concern of a reverse causality problem does not apply in this context. Second, while it is also unlikely the Supreme Court judges would decide the fate of APMLs based on factors outside the merits of the case (e.g., macroeconomic or political economy considerations), I test the plausibility that the ruling was unanticipated by firm and investors (i.e., an exogenous event) and therefore not subject to an omitted variables problem.

Using a short-run event study approach (e.g., Serfling, 2016; Klasa et al., 2018), I study abnormal stock returns around the Supreme Court's decision date, February 21, 1989, for firms located in either $All\ APML$ (columns 1-2) or $Boat\ APML$ (columns 3-4) states. Cumulative abnormal returns (CARs) are estimated using the classic four-factor ($MktRF$, SMB, HML, and

MOM) model (Fama and French, 1993; Carhart, 1997), with both an equally- (odd-numbered columns) and value-weighted (even-numbered columns) market index. The regression parameters are estimated over the trading window [-280,-61], relative to the Supreme Court's ruling date.

However, one important adjustment is required. Since all firms in APML adopting states will be affected by the same event on the same announcement day, the Supreme Court ruling is not independent across firms and correspondingly the standard errors in these regressions will be contaminated by a cross-sectional correlation bias. To deal with this issue, standard errors are corrected following the technique outlined in Kolari and Pynnönen (2010).

The results of these tests are presented in Table 4. Specifically, I show in the pre-announcement period ([-21,-4]) before the U.S. Supreme Court's ruling, the CARs are statistically insignificant for both firms in $All\ APML$ and $Boat\ APML$ states, irrespective of model specification. In contrast, and consistent with the Court's ruling being a surprise to capital markets, firms headquartered in states that pass All-APMLs (columns 1-2) experience negative and statistically significant CARs for all models over varying event windows ([-2,+2], [-0,+0], and [-0,+2]). These estimates vary from -0.50% to -0.65% (-0.48% to -0.57%) in the models with an equally(value)-weighed market index and are the strongest in statistical terms on the actual event date ([-0,+0]).

Unsurprisingly, due to a lack of publicly traded boat manufacturers, the average CAR for a firm headquartered in a Boat-APML state (columns 3-4) is insignificantly different than zero. As mentioned previously, there are only three boat manufacturers in the sample that would be affected by these types of APMLs, yielding the insignificant result (i.e., lack of power). In sum, it appears that investors expected the loss of protection from reverse engineering (and, consequently, an increase in competition) to be detrimental to firm value. Importantly for the identification strategy, the findings provide suggestive evidence that firms and investors did not anticipate the U.S. Supreme Court ruling invalidating APMLs, but rather it was an exogenous event.

5. Main results

5.1. APMLs and non-patented innovation

The analysis of the changes in competition brought about by the adoption and invalidation of APMLs begins by investigating their relationship with non-patented innovation. Because it is difficult to observe this outcome empirically, I rely on three sets of dependent variables to inform the inference on this relation. The first two sets consider how APML adoptions by state legislatures and their subsequent reversal by the U.S. Supreme Court altered affected firms' patent activity ($Patent\ Activity$) and investment spending ($Investment\ Spending$). The third set of dependent variables study the impact of these events on APML-firms' profitability ($Profitability$).

5.1.1. APMLs and patent activity

Panel A of Table 5 reports the DD estimates of the impact of the passage of All-APMLs and Boat-APMLs on the patenting outcomes of firms headquartered in adopting states over the sample period 1975 to 1988. Panel B of this table presents the DDD estimates, extending the first panel's sample period from 1988 to 1992, capturing the effect of both types of APMLs enactments and invalidation on affected firms' patent activity. The dependent variables (as in prior studies on patented innovation, e.g., Hall et al., 2005; Chava et al., 2013; Acharya et al., 2014; Cornaggia et al., 2015; Kogan et al., 2017) include: $Ln(1 + Patent)$, which represents the natural logarithm of one plus a firm's number of patents; $Ln(1 + CW\ Patent)$, measured as the natural logarithm of one plus the citation-weighted value of a firm's patents; and $Ln(1 + SM\ Patent)$, which equals the natural logarithm of one plus the stock market-value of a firm's patents.[18] Data for these measures comes from the KPSS patent dataset.

Further, consistent with prior work (e.g., Fang et al., 2014; Mukherjee et al., 2017; Chemmanur and Tian, 2018), the $Patent\ Activity$ measures are led by two years because the average time it takes a firm to obtain a patent is about 22-25 months (USPTO, 2018). Additionally, each of the specifications include firm and industry-by-year fixed effects and cluster standard errors by a firm's state of headquarters.

The first three columns in Panel A, Table 5 suggest that the adoption of All-APMLs is negatively related to patent counts. For example, in column 1, when only the APML indicators and dummies for "Other Law Controls" are specified, I find that firms headquartered in $All\ APML$ adopting states decrease their use of $Ln(1 + Patent)$ by 1 percentage point relative to industry rivals after their products' risk of being copied by the direct molding process is legally eliminated. Relative to the unconditional mean of $Ln(1 + Patent)$ reported in Internet Appendix Table A1 of 0.17, this represents a decrease of approximately 5.9% relative to that of an industry rival

[18] Following Atanassov (2013), $Patent\ Activity$ measures are only set equal to zero if a firm does not have patents but operates in the same 4-digit SIC industry as a firm that does have patents. Further, these measures have been corrected for a truncation bias inherent in patent data (e.g., see Hall et al., 2005).

headquartered in a non-APML state. The economic magnitude and statistical significance of the effect of $All\ APML$ on $Ln(1 + Patent)$ remains unchanged in columns 2 and 3 that further append "State-Level Controls" and then "Firm-Level Controls."

Inspecting the next three columns of Panel A, Table 5, finds mixed evidence that firms also decrease their use of novel patents (measured using $Ln(1 + CW\ Patent)$ in columns 4-6). For instance, in column 6, which includes all control variables, I find a marginally significant point estimate of -0.05 on $All\ APML$ – representing a reduction of 4.4% (=0.05/1.14) relative to its unconditional mean. However, in models with only "Other Law Controls" (column 4) or "Other Law Controls" plus "State-Level Controls" (column 5), the p-values range from 0.15–0.16, falling just outside of the conventional 10% level. This mixed result might be interpreted as firms still being concerned about other forms of reverse engineering outside of the direct molding process and opting to only selectively decrease their reliance on the patent system for their most novel ideas – indicating that APMLs provide a *partial* substitute to patents. The last three columns of this panel, suggest that firms headquartered in states adopting All-APMLs significantly reduced their use of patents determined by the market to be the most valuable, with an economic significance ranging from 5.9%–6.2% relative to the unconditional mean of $Ln(1 + SM\ Patent)$ of 0.70.

Meanwhile, as expected, the coefficients on the $Boat\ APML$ indicator are never statistically significant or consistent in sign as the majority of firms headquartered in these states do not manufacture boat-related products and are thus not affected by the laws.[19]

Transitioning to Panel B of Table 5, first, I find qualitatively similar coefficients in magnitude and statistical significance on the $All\ APML$ indicator for each of the three $Patent\ Activity$ measures (columns 1-9) over this extended sample period, 1975 to 1992. Next, reviewing the row of point estimates on the $Post88 \times All\ APML$ interaction, shows that in each of the nine columns, the invalidation of APMLs by the U.S. Supreme Court promoted a resurgence in the use of total, novel and valuable patents by firms located in affected states, relative to their industry rivals. For example, in columns 4-6, a firm headquartered in an All-APML adopting states that lost protection from the direct molding process – thus, experiencing a reinstatement of competition in products – via the Supreme Court's ruling increased its use of patents to protect its novel inventions (measured

[19] In order to conserve space, point estimates for the controls are unreported. However, they are generally consistent with prior work and available upon request.

using $Ln(1 + CW\ Patent)$) by 9.5% to 12.5%, relative to its sample mean of 1.14. Whereas, consistent with expectations, the placebo estimators, $Boat\ APML$ and $Post88 \times Boat\ APML$, are always insignificantly related to the three measures of $Patent\ Activity$.

5.1.2. APMLs and investment spending

Table 6 employs a second set of dependent variables to study the effect of changes in competition (via the APML quasi-natural experiments) on affected firms' non-patented innovation. In both panels of this table, these measures include the following six outcomes of $Investment\ Spending$: $R\&D/Sales$, which denotes R&D expenditure divided by sales (e.g., Chan et al., 2001; Eberhart et al., 2004); $CAPX/Assets$, defined as capital expenditures divided by the book value of assets (as in Rauh, 2006); $Intangible\ Capital$, measured as the natural logarithm of the sum of a firm's externally purchased and internally created intangible capital (proposed by Peters and Taylor, 2017); $Advertising$, which represents advertising expenditure divided by the book value of assets (e.g., Coles et al., 2012); $Organizational\ Capital$, measured as a firm's selling, general and administrative expenses divided by the book value of assets (as in Eisfeldt and Papanikolaou, 2013); and $Labor\ Capital$, defined as a firm's total number of employees divided by its real assets (e.g., Dewenter and Malatesta, 2001), where assets are adjusted for inflation using 2015 dollars.[20] Data for these measures comes from Compustat and the Peters and Taylor database on WRDS.

Additionally, $Investment\ Spending$ variables are led by one year, since the APML-related changes likely affect these policies with a lag. Panel A's (B's) sample period is 1975 to 1988 (1975 to 1992). Each of the columns across both panels specify the full set of controls and fixed effects.[21]

Beginning with columns 1 and 2, I find that firms headquartered in states that adopt All-APMLs increase their investments in $R\&D/Sales$ by 9.1% and $CAPX/Assets$ by 10.9% when compared with industry rivals located in states without these laws, relative to their respective sample means of 0.044 and 0.064. Alternatively, specifying $Intangible\ Capital$ (column 3) as a dependent variable yields a similar inference, as the point estimate on $All\ APML$ suggests that

[20] The first three outcomes are viewed as measures of investments in *new* production technologies, whereas the last three capture investments in *existing* production technologies.

[21] Because I am using six separate measures for $Investment\ Spending$, there is insufficient space to also report the only "Law Controls," and only "Law Controls" plus "State-Level Controls" specifications' estimates; the inference, however, is unchanged using either of these alternative models.

these firms – that are shielded from competitors' use of direct molding process reverse engineering – increase their $Investment\ Spending$ in this measure by 2.9 percentage points, relative to firms operating in the same two-digit SIC code industry but headquartered elsewhere. Reviewing the last three columns of Panel A indicates that firms affected by All-APMLs also increase their investments in existing production technologies such as $Advertising$ (column 4), $Organizational\ Capital$ (column 5), and $Labor\ Capital$ (column 6). The point estimates on the $Boat\ APML$ indicator are always statistically insignificant, consistent with the hypothesized ineffectiveness of these laws for non-boat manufacturers.

Next, Panel B, Table 6, investigates how the invalidation of APMLs by the U.S. Supreme Court changed the investment behavior of firms that were protected by these laws before the 1989 ruling. To start, the row of coefficients on $All\ APML$ suggest the same conclusions as in the previous panel – i.e., that the adoption of All-APMLs is significantly related to an increase in own-firm $Investment\ Spending$. In contrast, after the laws are overturned by the Supreme Court, any differentially higher investment spending in the pre-1989 period either dissipates (columns 1–3, and 6) or significantly decreases (columns 4 and 5). Taking column 2 as an example, when the laws protecting firms from the direct molding process of *all* items is legally valid, these companies increase their capital expenditures by 9.4% (=0.006/0.064), relative to industry competitors. However, when APMLs are invalidated in 1989, this same set of firms no longer invest at a differentially higher rate than its industry rivals located elsewhere. Meanwhile, the sample of predominantly non-boat manufacturing (i.e., placebo) firms', that are headquartered in Boat-APML states, $Investment\ Spending$ is unaffected by both the laws' adoptions and invalidation.

5.1.3. APMLs and profitability

Table 7 investigates the impact of the adoptions and invalidation of APMLs on profitability. Consistent with Schumpeterian growth theory, if the adoption of these laws decreases the level of product market competition in the "moldable products" industries, this in-turn should encourage innovation by incentivizing incumbents with larger expected economic rents for their investments in innovative projects. Both panels of this table measure economic rents using the following five $Profitability$-related dependent variables (as in Giroud and Mueller, 2010; Cain et al., 2017): $Gross\ Profit$, represents the ratio of net sales minus the cost of goods sold to net sales; $Operating\ Margin$, which denotes operating earnings divided by total revenue; ROE, or return on equity, is defined as net income divided by common equity; $Loss$, which is an indicator set to one if a firm has negative net income during a fiscal year, and zero otherwise; and $Profitability/Liabilities$, is measured as net sales minus the cost of goods sold and selling, general and administrative expenses scaled by current liabilities. Data for these measures comes from Compustat and the Financial Ratios Suite by WRDS.

These $Profitability$ measures are led by one year, since its likely takes at least one fiscal year for a firm's APML-related policy changes to take effect. As before, Panel A's (B's) sample period is 1975 to 1988 (1975 to 1992), and both panels include the full set of controls and fixed effects.

Each of the five columns in Panel A, reaches a consistent conclusion that All-APMLs are positively related to a firm's profitability. For instance, column 1 shows that, relative to firms operating in the same two-digit SIC code industry but headquartered in a non-APML enacting state, companies protected by $All\ APML$s experience a significant 1.2 percentage point increase in $Gross\ Profit$. The economic significance of this result implies that, relative to the unconditional mean of this variable of 0.34, gross profitability increases by approximately 3.5% relative to that of an industry rival headquartered in a non-APML state. Moving over to column 4, I find that the point estimate of -0.023 on $Loss$ suggests that firms affected by All-APMLs are 2.3% less likely to have negative net income in the upcoming fiscal year when compared with a competitor in the same industry but unprotected by one of these laws.

The first row of coefficients on $All\ APML$ in Panel B of Table 7 are consistent with the prior panel. However, in the post-88 portion of the sample period, firms headquartered in All-APML states no longer experience differentially larger profitability, and in one specification (column 3), are even made significantly less profitable. Moreover, the coefficients on $Boat\ APML$ (in both panels) and $Post88 \times Boat\ APML$ (in Panel B) are insignificantly related to the five measures of $Profitability$.

5.1.4. APMLs and the timing of changes in non-patented innovation

Next, I explore the timing of changes in non-patented innovation relative to the timing of the adoptions of APMLs (following the approach in Bertrand and Mullainathan, 2003, and others). This is done to supplement the earlier findings in Table 3 that suggests reverse causality does not drive the results and to offer additional evidence indicating that the parallel trends assumption is likely satisfied; a finding of insignificant differences between treated and control groups' patent activity, investment spending, and profitability before the laws are passed would lend such support.

Internet Appendix Table A3 presents the results from OLS regressions of $Ln(1 + Patent)$ (column 1), $R\&D/Sales$ (column 2), and $Gross\ Profit$ (column 3) on the following indicators: $All\ APML^{[-1]}$ is set equal to one if a firm is headquartered in a state that will adopt this law next year; $All\ APML^{[0]}$ is set equal to one if a firm is headquartered in a state that adopts this law in the current year; $All\ APML^{[+1]}$ is set equal to one if a firm is headquartered in a state that adopted this law one year ago; $All\ APML^{[2+]}$ is set equal to one if a firm is headquartered in a state that adopted this law two or more years ago. Similar variables are also constructed for Boat-APMLs ($Boat\ APML^{[t]}$). These regressions include the full set of fixed effects and controls and standard errors are clustered by state of headquarters.

The point estimates on $All\ APML^{[-1]}$ are statistically and economically insignificant across these three specifications. In contrast, All-APML-affected firms significantly decrease their reliance on patents immediately after the laws are passed ($All\ APML^{[0]}$) and continue to patent at a significantly lower rate than industry rivals headquartered elsewhere in the following year ($All\ APML^{[+1]}$). Further, these same firms significantly increase their R&D spending ($All\ APML^{[+1\text{ and }2+]}$) and experience heightened gross profitability ($All\ APML^{[0,+1,\text{ and}2+]}$) in the years following their states' adoption of All-APMLs. Meanwhile, as in previous tests, the placebo timing indicators ($Boat\ APML^{[t]}$) for firms headquartered in Boat-APML passing states are always statistically insignificant.[22]

Overall, the evidence from subsection 5.1 suggests that firms affected by All-APMLs decrease their use of patents when the laws provide effective protection (and not before) against an "unscrupulous" type of reverse engineering by competitors, and that they also experience increases in their investments in new and existing production technologies and profitability over the same time period. Then, when the same laws are overturned and the protections become ineffective, this subset of firms increase their reliance on patents to regain IP protection and no longer invest at a significantly higher rate or earn significantly higher profits than industry rivals. Therefore, jointly interpreting the test results from these three sets of dependent variables, I find that a reduction in

[22] Similar tests for parallel trends in non-patented innovation before the U.S. Supreme Court's ruling are not carried out since my main argument is that the APMLs caused it to be significantly higher for treated firms during this period.

product market competition stemming from stronger protections against reverse engineering leads to significant increases in non-patented innovation.

5.2. APMLs and firm value

This section examines the value implication of changes in product market competition by testing how the adoptions and invalidation of APMLs impact the Tobin's Q of firms headquartered in affected states. I then address the concern that Tobin's Q may be an imperfect measure of value by testing the impact of APML adoptions and their subsequent reversal on alternative value-related dependent variables. The heterogeneous value effects of the laws for firms that are ex-ante (i) more at risk of product imitation by rivals or (ii) more innovatively capable are then explored. Finally, an additional analysis is performed by comparing abnormal stock returns of firms affected by APMLs with those of firms unaffected by the laws on the day the U.S. Supreme Court's ruling that overturned APMLs was announced.

5.2.1. APMLs and Tobin's Q

Table 8 reports the DD estimates on the effect of the adoption of APMLs by state legislatures and the DDD estimates on their subsequent invalidation by the U.S. Supreme Court on the Tobin's Q ($Tobin's\ Q$) of firms in enacting states over the periods 1975 to 1988 (columns 1–3) and 1975 to 1992 (columns 4–6). Each of this table's six columns employs $Tobin's\ Q$ as the dependent variable, and specifies indicators for $All\ APML$, $Boat\ APML$ and "Other Law Controls." Consistent with prior empirical work investigating the value implications of competition and corporate innovation (e.g., Lindenberg and Ross, 1981; Blundell et al., 1999; Hall et al., 2005; Giroud and Mueller, 2011; Simeth and Cincera, 2016), $Tobin's\ Q$ is measured as a firm's market value of assets divided by its book value of assets (following Fama and French, 1992).

Further, because the market should respond immediately to the decrease in competition brought about by the adoption of APMLs, $Tobin's\ Q$ is measured as of the current year. Columns (2–3 and 5–6) append on "State-Level Controls" to account for local economic and political conditions, and columns 3 and 6 further tack on "Firm-Level Controls" to account for differences in company characteristics. The standard errors in these regressions are clustered by the state of headquarters.

The adoption of All-APMLs has a positive and statistically significant effect on the Tobin's Q of firms in affected states. The results in the first three columns provides strong support for the key implication that a reduction in competition in products is value enhancing for shareholders. In column 1, with only controls for other plausibly exogenous law adoptions specified, I find that firms headquartered in a state with an All-APML experience an increase in $Tobin's\ Q$ of 8.5 percentage points relative to firms headquartered elsewhere but operating in the same industry. This represent an economically significant increase of 5.9% (=0.085/1.433) relative to the sample mean's $Tobin's\ Q$. Columns 2 and 3 confirms that the passage of these laws are valuable for shareholders even after including controls for other state-level factors and company characteristics, as firms headquartered in these states have Tobin's Qs that are 8.2 to 9.1 percentage points higher, than those of industry rivals headquartered in non-APML adopting states. This suggests that affected firms experienced an economically significant 5.7% to 6.4% increase relative to the sample mean for $Tobin's\ Q$ of 1.433.

Columns 4–6 also indicate that All-APMLs are positively related to $Tobin's\ Q$ when valid (i.e., pre-1989). However, after the U.S. Supreme Court rules against APMLs and effectively invalidates their protection against the direct molding process, their value relevance dissipates. For instance, in column 6, which includes the full set of control variables, the coefficient of -0.072 on $Post88 \times All\ APML$ entirely wipes out the positive point estimate of 0.074 on the (pre-1989) $All\ APML$ indicator. Thus, it appears all of the value gains from the laws are nullified by their ensuing reversal. Reassuringly, the coefficients on the $Boat\ APML$ and $Post88 \times Boat\ APML$ indicators (i.e., the "built-in" placebo estimators) are always insignificant, indicating that neither the adoption of these laws nor their reversal affected the predominantly non-boat-related manufacturers' Tobin's Qs in the sample.

5.2.2. APMLs and the timing of changes in Tobin's Q

Column 4 of Internet Appendix Table A3 examines the timing of changes in Tobin's Q relative to the timing of APML enactments. As before, this analysis is carried out to reinforce the conclusion that reverse causality and pre-treatment trends in $Tobin's\ Q$ do not bias the estimates in the main tests. Following the same approach for the non-patented innovation outcomes, I regress $Tobin's\ Q$ (column 4) on the indicators: $All\ APML^{[-1]}$, $All\ APML^{[0]}$, $All\ APML^{[+1]}$, and $All\ APML^{[2+]}$ (as well as the $Boat\ APML^{[t]}$ analogues). Additionally, the full set of fixed effects and controls are included, and standard errors are clustered by state of headquarters.

From this regression, I find that the coefficients on $All\ APML^{[-1]}$ and $All\ APML^{[0]}$ (as well as on the placebo indicators: $Boat\ APML^{[t]}$) are insignificantly different from zero, while those on $All\ APML^{[+1]}$ and $All\ APML^{[2+]}$ are positive and significant at the 1% level. These results indicate that firms headquartered in All-APML-adopting states experience significant increases in firm value (as measured by Tobin's Q), relative to industry rivals, after the laws are passed, but not before their adoption. This is consistent with the central inference that a reduction in product market competition leads to value enhancements for the affected firms.

5.2.3. APMLs and alternative value measures

Before proceeding to test the heterogenous value implications of APMLs for Tobin's Q, I investigate the reliability of this variable to measure firm value. Tobin's Q could be an imperfect measure of value since it may also correlate with growth opportunities (e.g., Smith and Watts, 1992; Jung et al., 1996; Parise, 2018) and because it is subject to measurement error (e.g., Erickson and Whited, 2000, 2012; Abel, 2018). To address these criticisms, the analysis in Table 8 is replicated but with Tobin's Q replaced by two alternative value-related dependent variables. The first is $Stock\ Return$ (similar to Cohen and Wang, 2013), estimated as the residual from regressions of annual stock returns on the Fama-French four (i.e., $MktRf$, SMB, HML, and MOM) factors (Fama and French, 1993; Carhart, 1997). The second is $Total\ Q$, first proposed in Peters and Taylor (2017), which modifies $Tobin's\ Q$ by explicitly accounting for intangible capital in the firm's replacement cost of total capital.

Internet Appendix Table A4 presents the results from these tests. The first (last) two columns specifies $Stock\ Return$ ($Total\ Q$) as the dependent variable. Further, the odd-numbered (even-numbered) columns are specific to the sample period 1975 to 1988 (1975 to 1992). Each column includes the full set of control variables and fixed effects and the standard errors are clustered at the state of headquarters level. In each of these four columns I find consistent evidence with their $Tobin's\ Q$ analogues, that All-APMLs are positively related to these measures of firm value when valid (pre-1989) and then become statistically insignificant in the ensuing period (post-1988) when the U.S. Supreme Court overturns them. Additionally, the placebo estimators – $Boat\ APML$ and $Post88 \times Boat\ APML$ – are always insignificantly related to both $Stock\ Return$ and $Total\ Q$. In sum, this table reinforces the earlier inference that a reduction in product market competition is valuable for affected firms, and that $Tobin's\ Q$ appears to be a consistent measure of this value.[23]

5.2.3. APMLs and Tobin's Q for patenting and non-patenting firms

Intuitively, businesses that are better protected from direct molding process reverse engineering by patents might not experience the same benefit from APMLs as non-patenting firms. That is, because patenting companies face an ex-ante lower risk of product imitation – i.e., all else equal, competitors of patenting companies have higher imitation costs (Mansfield et al., 1981) – the laws might not matter as much for them. However, given a forward-looking measure like Tobin's Q, which captures long-term anticipated value, this is not to say that the laws will not also be valuable for these patenting companies too, as their current unpatented and future products gain protection. Additionally, these firms also likely have a cost advantage in patenting relative to the non-patenting companies, but since this competitive cost advantage moderates with the laws, it might also be the case that these firms gain less from the reverse engineering protections (Kultti et al., 2006, 2007). Table 9 tests this intuition.

Firm-level $Patent\ Activity$ is measured as before using: $Ln(1 + Patent)$, $Ln(1 + CW\ Patent)$, and $Ln(1 + SM\ Patent)$. However, these variables are supplemented with a fourth measure that should operate in the opposite way: $Patentless\ R\&D$. Following Guernsey et al. (2019), $Patentless\ R\&D$ is as an indicator equal to one if a firm has non-zero R&D in the current or preceding three years, and zero patents granted in the current or next three years, and zero otherwise. Moreover, each column specifies the complete set of controls with firm and industry-by-year fixed effects and standard errors clustered by state of headquarters. Column 1 documents a negative and significant heterogeneous value effect (coefficient = -0.207 and significance level = 1%) for firms protected by All-APMLs but with higher levels of ex-ante patent counts ($All\ APML \times Ln(1 + Patent)$) during the period 1975 to 1988, while an All-APML company without any patents experiences an increase in $Tobin's\ Q$ of 8.9% (=0.127/1.433), relative to the sample average.

However, it is important to be careful of the interpretation here, as the negative and significant point estimate on the interaction term does not suggest that firms with patents were hurt by APMLs, but rather their gains in value are smaller in magnitude since they already had some form

[23] The $Profitability$ results in Table 7 also provide a reinforcing robustness check for the $Tobin's\ Q$ interpretation.

of reverse engineering protection (and have arguably lost their competitive cost advantage in patenting). This is demonstrated formally in column 1 of the bottom of the table in a "Test of joint significance" for a corporation headquartered in an All-APML state with an average level of $Ln(1 + Patent)$. Using this approach, I find the affected company with an average portfolio of patent counts still experiences an increase in firm value of 6.2% (=0.089/1.433), relative to the sample mean $Tobin's\ Q$. The next two columns show similar evidence using $Ln(1 + CW\ Patent)$ (column 2) or $Ln(1 + SM\ Patent)$ (column 3).

Column 4 shows that firms conducting $Patentless\ R\&D$ and headquartered in a state that has adopted a law that protects *all* manufacturing items against the direct molding process experience a heterogenous positive gain in Tobin's Q of 12.8 percentage points, compared to industry rivals with either "patented R&D" or no R&D investments. This signifies an increase of 8.9% (=0.128/1.433) relative to the unconditional mean of $Tobin's\ Q$, while the average increase in Tobin's Q for both patenting and non-patenting firms in All-APML states is 5.6% (=0.080/1.433). Finally, I find some evidence that standalone patent counts and citation-weighted patents, in general, are not value relevant in the 1975 to 1988 period, confirming the findings in Hall (1993), where she shows the stock market's valuation of intangible capital created by manufacturing firm R&D decreased substantially in the mid-1980s.

5.2.4. APMLs and Tobin's Q for firms with greater innovative ability

Table 10 continues my investigation into possible sources of value of All-APMLs by considering their heterogeneous effects on firms that have greater innovative ability. According to Schumpeterian growth theory (e.g., Dasgupta and Stiglitz, 1980; Gilbert and Newberry, 1982; Aghion and Howitt, 1992; Davidson and Segerstrom, 1998), these innovative incumbents can arguably be expected to experience the greatest amount of increase in their investment incentives (i.e., larger expected economic rents stemming from R&D) with the decrease in product market competition. I employ the research quotient (RQ) measure proposed by Knott (2008) and provided on WRDS for the period 1971 to 2015, to capture the innovative ability of the manufacturing firms in the sample.

RQ estimates the output elasticity of R&D (i.e., how successful are corporations at converting R&D into sales revenue). Additionally, I create three indicator variables from the continuous measure RQ: $RQ\ High$, $RQ\ Medium$, and $RQ\ Low$, which are set equal to one if the company's

RQ is above the 66th percentile, in between the 66th and 33rd percentiles, or below the 33rd percentile respectively, and zero otherwise. The $RQ\ Medium$ terms are dropped to avoid perfect multicollinearity. Each specification includes firm and industry-by-year fixed effects and the full set of controls.

The first four columns of Table 10 documents suggestive evidence that the positive value relevancy of All-APMLs is attributable to affected firms with higher levels of innovative ability. For example, in columns 1 and 2, using either RQ or the indicator variables $RQ\ High$ and $RQ\ Low$, I find strong statistical evidence the companies with the greatest ability to convert R&D into sales are heterogeneously benefited by the laws. In particular, in column 2, relative to the sample mean, $Tobin's\ Q$ increases by 3.5% (=0.054/1.433) for firms with the highest levels of innovative ability in All-APML adopting states, while companies with low innovative ability do not experience any value gains. Additionally, the findings in columns 3 and 4 suggest companies with higher levels of output elasticity of R&D (either $Post88 \times All\ APML \times RQ$ or $Post88 \times All\ APML \times RQ\ High$) are significantly adversely affected by the removal of reverse engineering protections. Taking column 3 as an example, firms headquartered in an All-APML passing state after 1989 and with a one-standard deviation higher level of RQ have a 15.9% (=-2.285*0.100/1.433) lower $Tobin's\ Q$, relative to industry rivals with less innovative acumen.

5.3. Abnormal returns on the day the U.S. Supreme Court invalidated APMLs

This section concludes by conducting one last batch of tests on the heterogenous effects of product market competition for innovative firms' value. Specifically, I use a DD approach to compare the stock market returns of subsets of APML-affected-firms on the day the U.S. Supreme Court's decision to invalidate these laws was announced – February 21, 1989 – relative to industry rivals headquartered elsewhere. These models specify risk-adjusted excess returns as the dependent variable to control for differences in risk characteristics between the groups of firms.

Following Cohen and Wang (2013), I compute risk-adjusted excess returns using a two-step procedure. First, each firm's loadings on the standard four (i.e., $MktRf$, SMB, HML, and MOM) factors (Fama and French, 1993, 1996) are estimated over the [-280,-61] trading days relative to the announcement date. Second, I take the residuals from a cross-sectional regression of raw announcement window returns on the estimated factor sensitivities to obtain the dependent variable: 1-$Day\ Risk\text{-}Adjusted\ Excess\ Ann.Return$.

Table 11 presents the results. In particular, it shows the estimates from regressing the risk adjusted one-day excess returns on the $All\ APML$ and $Boat\ APML$ indicators. Also included are industry fixed effects to restrict comparisons between industry rivals and standard errors are clustered at the state of headquarters level. Column 1 shows evidence that invalidating the APMLs and reinstating product market competition yields significantly negative returns for All-APML firms compared with industry rivals headquartered elsewhere. On the event day, All-APML-affected firms, on average, underperformed industry rivals in non-adopting states by 0.37 percentage points.

I then examine how the Supreme Court's ruling affected-All-APML firms with patent counts above ($Patent\ High$) and below the sample median (columns 2-3) and with and without $Patentless\ R\&D$ (columns 4-5). Consistent with the Tobin's Q findings in Table 9, I show that the cost of losing reverse engineering protection for All-APML firms is worse for a subset that are less reliant on patents to protect their IP and R&D. For instance, firms headquartered in states with an All-APML and with patent counts below the sample median underperform industry rivals with similar patent activity on the announcement day by 0.63 percentage points (column 3). Next, columns 6 and 7 indicate that the Supreme Court's invalidating decision was perceived by the market to be more harmful for All-APML firms with greater innovative ability ($RQ\ High$) than similarly innovative firms headquartered elsewhere, by about 0.34 percentage points.

Finally, and as hypothesized, the coefficients on $All\ APML$ are insignificant for firms more reliant on patents, "patented" R&D, or with less innovative ability, and the $Boat\ APML$ point estimates are insignificant in each of the seven columns because the majority of firms headquartered in these states are not affected by their states' laws.

6. Additional tests

6.1. Ruling out local confounding effects

The biggest threat to my identification strategy is that because a limited number of states adopt All-APMLs (even though these same states house a sizeable 3,530 firm-years) my estimates could be spuriously driven by local factors. To assess the seriousness of this threat, I exploit a unique feature of these quasi-natural experiments in which the only firms affected by the laws are those that manufacture "moldable products" (see Table 2). Therefore, for the other firms headquartered in these same states that are not at risk of imitation via the direct molding process, such as food and beverage manufacturers, APMLs should not matter. However, since both types of firms share the same headquarter state, they are both exposed to the same (other) local factors that might confound the study's results.[24]

Table 12 tests whether non-moldable products-firms, headquartered in APML states, experience the same changes in non-patented innovation and firm value as companies with moldable products. As dependent variables, I include $Ln(1 + Patent)$ (columns 1-2), $R\&D/Sales$ (columns 3-4), $Gross\ Profit$ (columns 5-6), and $Tobin's\ Q$ (columns 7-8). Moreover, each regression model includes the full set of fixed effects and controls. The sample period is either 1975 to 1988 (odd-numbered columns) or 1975 to 1992 (even-numbered columns). A quick review of the point estimates indicates that for firms headquartered in APML adopting states but without products that can be reverse engineered by the direct molding process there is no effect of the laws on non-patented innovation or value. This evidence helps to rule out that other local factors (e.g., economic conditions or other corporate laws) spuriously drive the documented relationship between APML adoptions and innovation and value.

6.2. Additional robustness

In the Internet Appendix, I also document that the negative (positive) relation between the passage of All-APMLs and patent activity (investment spending, profitability, and Tobin's Q), and the positive relation (dissipation effect) between the laws' invalidation and patent activity (investment spending, profitability, and Tobin's Q) is robust to: (i) enlarging my sample to include all firms in the manufacturing sector (SIC codes 2000-3999) (Table A5); and (ii) excluding firms headquartered in Boat-APML states entirely (Table A6).

7. Conclusion

Prior research has commonly used empirical proxies of product market competition – such as the Herfindahl-Hirschman index, market share, and the Lerner index – to study whether competition affects firm value by encouraging or hindering technological innovation. This paper takes a different approach by shifting the focus from endogenous proxy variables to a tandem of unique and arguably exogenous events that directly impact the intensity of competition in product markets. In particular, I exploit the quasi-natural experiments provided by the staggered adoption

[24] This test assumes that local factors (not including APML adoptions) should not exclusively affect manufacturing firms with moldable products; although unlikely, I cannot entirely rule out this possibility.

of anti-plug molding laws (APMLs) by U.S. state legislatures over the period 1978 to 1987 and their subsequent invalidation by the U.S. Supreme Court in 1989 to study how product market competition affects firm value through its impact on non-patented innovation.

APMLs decrease competition for manufacturers headquartered in enacting states by prohibiting competitors' use (both within and outside of adopting states) of an "unscrupulous" type of reverse engineering – the direct molding process – to make an identical but competing product. Reverse engineering by way of the direct molding process provides laggard and new entrant firms a competitive cost advantage by allowing them to use an incumbent's existing product as a "plug" to form a mold, upon which duplicate (i.e., undifferentiated) items can be mass-produced at a small fraction of the incumbent's total production costs. However, because APML protection against the direct molding process was indefinite and required no disclosure of the invention's "know-how," the U.S. Supreme Court deemed the laws in direct conflict with U.S. federal patent law and ruled to invalidate them.

Using these quasi-natural experiments and a difference-in-differences design, I compare changes in non-patented innovation and financial value of firms headquartered in states that pass these laws to changes in non-patented innovation and financial value of industry rivals in states that do not adopt them. I find significant declines in firms' patent activity coupled with significant increases in investment spending, profitability, and Tobin's Q following the adoption of APMLs. However, after the laws are overturned by the U.S. Supreme Court, there is a significant resurgence in the firms' use of patents, and a dissipation in the gains in investment spending, profitability, and value from when the APMLs were valid.

Results from robustness and placebo tests aimed at addressing endogeneity concerns support this inference. I also find that the positive relation between the adoption of these laws and firm value are larger for firms that are more at risk of product imitation, that perform more "patentless R&D," and that have greater innovative ability. Further, when the Supreme Court invalidates the laws, these same sets of innovative firms are the most adversely impacted. Overall, these results are consistent with Schumpeterian growth theory by which less intense product market competition increases rents to innovative incumbents, thereby increasing their incentives to innovate and grow.

Appendix A
Control variable definitions.

Control Variables	
FGL	An indicator variable equal to one if a firm is headquartered in a state that adopted a first-generation law, and zero otherwise. I use adoption dates provided by Karpoff and Wittry (2018).
BCL	An indicator variable equal to one if a firm is headquartered in a state that adopts a business combination law, and zero otherwise. I use adoption dates provided by Cain et al. (2017) and Karpoff and Wittry (2018).
CSL	An indicator variable equal to one if a firm is headquartered in a state that adopts a control share law, and zero otherwise. I use adoption dates provided by Cain et al. (2017) and Karpoff and Wittry (2018).
DDL	An indicator variable equal to one if a firm is headquartered in a state that adopts a directors' duties law, and zero otherwise. I use adoption dates provided by Karpoff and Wittry (2018).
FPL	An indicator variable equal to one if a firm is headquartered in a state that adopts a fair price law, and zero otherwise. I use adoption dates provided by Cain et al. (2017) and Karpoff and Wittry (2018).
PPL	An indicator variable equal to one if a firm is headquartered in a state that adopts a poison pill law, and zero otherwise. I use adoption dates provided by Cain et al. (2017) and Karpoff and Wittry (2018).
$UTSA$	The change in state-specific trade secrets protection after the enactment of the Uniform Trade Secrets Act (UTSA) in a firm's state of headquarters, following Png (2017a, 2017b).
$R\&D\ Credit$	An indicator variable set to one if a firm's state of headquarters adopts a tax credit for R&D, and zero otherwise, following Wilson (2009).
GFE	An indicator variable equal to one if a firm is headquartered in a state that adopts a good faith exception, and zero otherwise. I use adoption dates provided by Serfling (2016).
$Ln(GDPPC)$	The natural logarithm of a headquartering state's GDP (in thousands) divided by its total population. I use data from the U.S. Bureau of Economic Analysis.
$GDP\ Growth$	The headquartered state-level GDP growth rate over the fiscal year. I use data from the U.S. Bureau of Economic Analysis.
$Democrat$	The proportion of state-level representatives in the U.S. House of Representatives whom belong to the Democrat party, in a given year. Data comes from the U.S. House of Representatives.
$Ln(Assets)$	The natural logarithm of the value of total book assets in millions, where assets are adjusted using 2015 dollars. Data comes from Compustat.
$Ln(Age)$	The natural logarithm of one plus the number of firm-year observations since the firm's first appearance in Compustat.
$Debt$	Long-term debt divided by book equity. Data comes from Compustat.
ROA	Return on Assets. Income before extraordinary items plus depreciation and amortization divided by book value of assets. Data comes from Compustat.

OCF	Operating cash flow equals the summation of income before extra items, extra items and discontinued operation, depreciation and amortization, deferred taxes, equity in net loss, gains in sale of PPE and investment, other funds from operation, other sources of funds minus the change in working capital, all scaled by last year's book value of assets, following Chang et al. (2014). Data comes from Compustat.
HHI	The Herfindahl-Hirschman Index for a particular industry defined as the sum of squared market shares for all firms in a three-digit SIC industry. The market share of firm *i* is defined as the value of sales of firm *i* divided by the total value of sales in the industry of firm *i*. Data comes from Compustat.
SG	The natural logarithm of the value of sales in millions in year *t* divided by the value of sales in millions in year *t*-1. Data comes from Compustat.
$Loss$	An indicator variable set to one if a firm has negative net income during a fiscal year, and zero otherwise. Data comes from Compustat.
$FLIQ$	Current assets minus current liabilities divided by the value of total book assets. Data comes from Compustat.
$R\&D/Sales$	Research and development expense divided by the value of sales. Data comes from Compustat.
$CAPX/Assets$	Capital expenditures divided by the value of total book assets. Data comes from Compustat.

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Table 1
State-level anti-plug molding laws.

The table lists the month and year when each anti-plug molding law (APML) adopting state enacted its respective statute (Panel A) as well as the month and year when an important court ruling related to the validity of a respective APML was decided (Panel B). The states omitted from Panel A did not adopt an APML and therefore do not have a related court decision. The states included in Panel A but omitted in Panel B may have had a related court decision but the initial (and, if applicable, final) ruling(s) validated the statute.

Panel A: The month and year of APML adoption

State	Statute	Month/Year Adopted	Covered Products
California	*CAL. BUS. & PROF. CODE § 17300*	10/1978	All items
Florida	*FLA. STAT. § 559.94*	05/1983	Boat hulls
Indiana	*IND. CODE §§ 24-4-8-1*	08/1987	Boat hulls
Kansas	*KAN. STAT. ANN. § 50-802*	07/1984	Boat hulls
Louisiana	*LA. REV. STAT. ANN. § 51: 462.1*	07/1985	Boat hulls
Maryland	*MD. COM. LAW CODE ANN. § 11-1001*	04/1986	Boat hulls
Michigan	*MICH. COMP. LAWS §§ 445.621*	03/1983	All items
Mississippi	*MISS. CODE ANN. § 59-21-41*	03/1985	Boat hulls
Missouri	*MO. REV. STAT. § 306.900*	04/1986	Boat hulls
North Carolina	*N.C. GEN. STAT. §§ 75A-27*	07/1985	Boat hulls
Tennessee	*TENN. CODE ANN. § 47-50-111*	07/1983	All items
Wisconsin	*WIS. STAT. ANN. § 134.34*	06/1983	Boat hulls

Panel B: The month and year of an important APML related court decision

Jurisdiction	Court	Case	Month/Year Decided	Decision
California (CA)	District Court	*Interpart Corp. v. Imos Italia*	07/1984	Invalidates CA statute
California	Federal Circuit	*Interpart Corp. v. Imos Italia*	11/1985	Validates CA statute
Florida (FL)	District Court	*Bonito Boats, Inc. v. Thunder Craft Boats, Inc.*	12/1984	Invalidates FL statute
Florida	Supreme Court	*Bonito Boats, Inc. v. Thunder Craft Boats, Inc.*	11/1987	Invalidates FL statute
United States	Supreme Court	*Bonito Boats, Inc. v. Thunder Craft Boats, Inc.*	02/1989	Invalidates all statutes

Table 2
Describing industries with "Moldable Products."

This table provides a description of the moldable products-industries included and excluded from the main sample. *Source*: siccode.com

Two-digit SIC codes	Description	"Moldable Products" industry
20	Food and Kindred Products	No
21	Tobacco Products	No
22	Textile Mill Products	No
23	Apparel and other Finished Products Made from Fabrics and Similar Materials	No
24	**Lumber and Wood Products, except Furniture**	**Yes**
25	**Furniture and Fixtures**	**Yes**
26	Paper and Allied Products	No
27	Printing, Publishing, and Allied Industries	No
28	Chemicals and Allied Products	No
29	Petroleum Refining and Related Industries	No
30	**Rubber and Miscellaneous Plastics Products**	**Yes**
31	**Leather and Leather Products**	**Yes**
32	**Stone, Clay, Glass, and Concrete Products**	**Yes**
33	Primary Metal Industries	No
34	**Fabricated Metal Products, except Machinery and Transportation Equipment**	**Yes**
35	**Industrial and Commercial Machinery and Computer Equipment**	**Yes**
36	**Electronic and other Electrical Equipment and Components, except Computer Equipment**	**Yes**
37	**Transportation Equipment**	**Yes**
38	**Measuring, Analyzing, and Controlling Instruments; Photographic, Medical and Optical Goods; Watches and Clocks**	**Yes**
39	**Miscellaneous Manufacturing Industries**	**Yes**

Table 3
Determinants of anti-plug molding law adoptions.

This table reports results from linear probability models analyzing the determinants of a state adopting an anti-plug molding law (APML). The sample period is 1975 to 1988. A "failure event" is the adoption of either an all item ($All\ APML$) (columns 1-2) or boat specific ($Boat\ APML$) (columns 3-4) APML in a given state. States are excluded from the sample once they adopt an APML. Predictor variables are measured at the state-level in year t-1. Those that proxy for state economic and political conditions include: $Ln(GDPPC)$; $GDP\ Growth$; $Democrat$; $Ln(Population)$; $Unemployment$; $Est.Entry$; $Est.Exit$. Those that account for previously enacted corporate laws in the headquarter state include: FGL; BCL; CSL; DDL; FPL; PPL; IDD; $UTSA$; $R\&D\ Credit$; GFE. Also included are state-year (SY) variables to test a reverse causality concern: $SY\ Ln(1 + Patent)$ is the median value of the natural logarithm of one plus patent counts across all firms in a state during a given year; $SY\ \Delta\ Ln(1 + Patent)$ is the median value of the one-year change (Δ) in the natural logarithm of one plus patent counts across all firms in a state during a given year; $SY\ Tobin's\ Q$ is the median value of $Tobin's\ Q$ across all firms in a state during a given year; $SY\ \Delta\ Tobin's\ Q$ is the median value of the one-year change in $Tobin's\ Q$ across all firms in a state during a given year. All predictor variables, except indicators are standardized to have a mean of zero and unit variance. Standard errors in parentheses are clustered by state of headquarters. State fixed effects are defined using historical headquarter states. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	$All\ APML_{[t]}$		$Boat\ APML_{[t]}$	
	(1)	(2)	(3)	(4)
$Ln(GDPPC)_{[t-1]}$	0.009	0.006	-0.010	-0.007
	(0.011)	(0.014)	(0.056)	(0.076)
$GDP\ Growth_{[t-1]}$	-0.010	0.086	0.095	0.090
	(0.023)	(0.096)	(0.122)	(0.190)
$Democrat_{[t-1]}$	0.000	0.008	0.024	0.034
	(0.004)	(0.009)	(0.023)	(0.035)
$Ln(Population)_{[t-1]}$	-0.001	-0.089	0.006	0.043
	(0.003)	(0.099)	(0.006)	(0.152)
$Unemployment_{[t-1]}$	0.008	0.010	0.006	0.011
	(0.007)	(0.011)	(0.012)	(0.022)
$Est.Entry_{[t-1]}$	-0.001	0.002	0.003	0.003
	(0.002)	(0.004)	(0.007)	(0.012)
$Est.Exit_{[t-1]}$	-0.001	0.001	-0.003	0.011
	(0.001)	(0.002)	(0.004)	(0.014)
$FGL_{[t-1]}$	0.009	0.019	0.010	0.012
	(0.010)	(0.020)	(0.010)	(0.014)
$BCL_{[t-1]}$	-0.003	0.002	-0.146	-0.076
	(0.006)	(0.003)	(0.168)	(0.159)
$CSL_{[t-1]}$	0.002	0.003	-0.020	-0.006
	(0.004)	(0.004)	(0.024)	(0.018)
$DDL_{[t-1]}$	0.005	0.003	0.022	0.011
	(0.006)	(0.005)	(0.037)	(0.029)
$FPL_{[t-1]}$	-0.001	0.000	0.135	0.062
	(0.004)	(0.002)	(0.170)	(0.162)
$PPL_{[t-1]}$	-0.004	-0.002	-0.009	0.008
	(0.006)	(0.004)	(0.049)	(0.041)
$IDD_{[t-1]}$	0.022	0.003	0.011	0.132
	(0.020)	(0.006)	(0.034)	(0.117)
$UTSA_{[t-1]}$	-0.005	-0.006	0.013	-0.006
	(0.006)	(0.007)	(0.033)	(0.028)
$R\&D\ Credit_{[t-1]}$	-0.005	-0.002	0.041	0.058
	(0.006)	(0.004)	(0.073)	(0.062)
$GFE_{[t-1]}$	0.003	0.000	-0.027	-0.012
	(0.004)	(0.003)	(0.022)	(0.015)

$SY\ Ln(1 + Patent)_{[t-1]}$	0.036	0.001	0.159	0.192
	(0.052)	(0.042)	(0.240)	(0.280)
$SY\ \Delta\ Ln(1 + Patent)_{[t-1]}$	-0.001	-0.027	-0.048	0.008
	(0.036)	(0.047)	(0.160)	(0.273)
$SY\ Tobin's\ Q_{[t-1]}$	0.002	0.001	-0.005	-0.013
	(0.004)	(0.003)	(0.011)	(0.017)
$SY\ \Delta\ Tobin's\ Q_{[t-1]}$	0.001	0.004	0.001	0.001
	(0.002)	(0.005)	(0.008)	(0.012)
Year FE	Yes	Yes	Yes	Yes
State FE	No	Yes	No	Yes
Observations	417	417	414	414
Adjusted R^2	0.004	0.067	0.002	0.098

Table 4
CARs around the U.S. Supreme Court's invalidation of anti-plug molding laws.

This table reports the cumulative abnormal returns (CARs) surrounding the U.S. Supreme Court's ruling in *Bonito Boats v. Thunder Craft Boats* on February 21, 1989 that invalidated anti-plug molding laws (APMLs) for firms headquartered in either All-APML ($All\ APML$) (columns 1-2) or Boat-APML ($Boat\ APML$) (columns 3-4) adopting states. CARs are estimated over the event windows [-2,+2], [-0,+0], and [-0,+2] and pre-event window [-21,-4]. CARs are estimated using the 4-factor model where the market factor is based on either CRSP equal-weighted returns (odd-numbered columns) or CRSP value-weighted returns (even-numbered columns), and the remaining three factors include: small-minus-big (SMB), high-minus-low (HML), and momentum (MOM). The parameters of the 4-factor model are estimated over the window [-280,-61] relative to the announcement date. Only firms in "moldable products" industries (Table 2) are included in these tests. The estimated *t*-statistics have been corrected for cross-sectional correlation (following Kolari and Pynnönen, 2010) and are shown in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	$All\ APML$		$Boat\ APML$	
	EW Index	VW Index	EW Index	VW Index
CAR Window:	(1)	(2)	(3)	(4)
[-21,-4]	-1.29%	-0.88%	-0.03%	0.34%
	(-1.38)	(-0.75)	(0.11)	(0.56)
[-2,+2]	-0.65%**	-0.57%*	-0.05%	-0.00%
	(-2.05)	(-1.81)	(-0.30)	(-0.13)
[-0,+0]	-0.52%**	-0.49%**	0.37%	0.39%
	(-2.35)	(-2.14)	(1.23)	(1.32)
[-0,+2]	-0.50%**	-0.48%*	-0.27%	-0.30%
	(-2.04)	(-1.89)	(-0.65)	(-0.61)
Observations	346	346	192	192

Table 5
The effect of anti-plug molding laws on patent activity.

This table reports results from OLS regressions of patent activity on indicators for whether a state has adopted either an all item APML ($All\ APML$) or a boat specific APML ($Boat\ APML$). The sample period in Panel A (B) is 1975 to 1988 (1975 to 1992). The dependent variables are $Ln(1 + Patent)$ (columns 1-3), $Ln(1 + CW\ Patent)$ (columns 4-6), and $Ln(1 + SM\ Patent)$ (columns 7-9). $Ln(1 + Patent)$ is the natural logarithm of one plus patent counts. $Ln(1 + CW\ Patent)$ is the natural logarithm of one plus citation-weighted patents. $Ln(1 + SM\ Patent)$ is the natural logarithm of one plus stock market-weighted patents. $All\ APML$ ($Boat\ APML$) is an indicator variable equal to one if the firm is headquartered in a state that adopts an APML that covers all (only boat related) products. Panel B interacts $ALL\ APML$ and $Boat\ APML$ with an indicator for whether the sample year is post-1988 ($Post88$). Other law controls include: FGL, BCL, CSL, DDL, FPL, PPL, $UTSA$, $R\&D\ Credit$ and GFE. State-level controls include: $Ln(GDPPC)$, $GDP\ Growth$, and $Democrat$. Firm-level controls include: $Ln(Assets)$, $Ln(Age)$, $Debt$, ROA, OCF, HHI, SG, $Loss$, $FLIQ$, $R\&D/Sales$, and $CAPX/Assets$. Controls are defined in Appendix A. Control variable coefficients are unreported to conserve space. Industry fixed effects are defined at the two-digit SIC level. Continuous variables, except state-level variables, are winsorized at their 2.5th and 97.5th percentiles. Standard errors (reported in parentheses) are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: The effect of APML adoptions on patent activity over the period 1975-1988

	$Ln(1 + Patent)_{t+2}$			$Ln(1 + CW\ Patent)_{t+2}$			$Ln(1 + SM\ Patent)_{t+2}$		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$All\ APML_{[t]}$	-0.010***	-0.010***	-0.010***	-0.049	-0.049	-0.050*	-0.041**	-0.043***	-0.043***
	(0.003)	(0.003)	(0.003)	(0.034)	(0.033)	(0.026)	(0.019)	(0.016)	(0.013)
$Boat\ APML_{[t]}$	-0.002	-0.001	0.002	-0.006	-0.005	0.018	-0.048	-0.049	-0.029
	(0.013)	(0.013)	(0.012)	(0.056)	(0.055)	(0.049)	(0.035)	(0.034)	(0.032)
Other Law Controls$_{[t]}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Level Controls$_{[t]}$	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Firm-Level Controls$_{[t-1]}$	No	No	Yes	No	No	Yes	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,139	13,139	13,139	13,139	13,139	13,139	13,139	13,139	13,139
Adjusted R^2	0.923	0.923	0.925	0.840	0.840	0.843	0.926	0.926	0.929

Table 5
Continued.

Panel B: The effect of APML adoptions on patent activity over the period 1975-1992									
	$Ln(1 + Patent)_{t+2}$			$Ln(1 + CW\ Patent)_{t+2}$			$Ln(1 + SM\ Patent)_{t+2}$		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$All\ APML_{[t]}$	-0.010***	-0.009***	-0.009***	-0.042	-0.043	-0.045*	-0.049**	-0.051***	-0.054***
	(0.003)	(0.003)	(0.003)	(0.034)	(0.032)	(0.025)	(0.019)	(0.017)	(0.015)
$Post88_{[t]} \times All\ APML_{[t]}$	0.041***	0.041***	0.036***	0.143**	0.141**	0.108**	0.108**	0.109**	0.084*
	(0.009)	(0.010)	(0.009)	(0.057)	(0.058)	(0.050)	(0.049)	(0.048)	(0.042)
$Boat\ APML_{[t]}$	0.001	0.001	0.005	-0.003	-0.005	0.016	-0.032	-0.035	-0.013
	(0.011)	(0.011)	(0.010)	(0.060)	(0.060)	(0.050)	(0.037)	(0.037)	(0.031)
$Post88_{[t]} \times Boat\ APML_{[t]}$	0.012	0.013	0.011	0.078	0.078	0.070	0.041	0.036	0.031
	(0.012)	(0.012)	(0.012)	(0.034)	(0.075)	(0.069)	(0.045)	(0.44)	(0.041)
Other Law Controls$_{[t]}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Level Controls$_{[t]}$	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Firm-Level Controls$_{[t-1]}$	No	No	Yes	No	No	Yes	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,600	17,600	17,600	17,600	17,600	17,600	17,600	17,600	17,600
Adjusted R^2	0.905	0.905	0.908	0.822	0.822	0.828	0.906	0.906	0.912

Table 6
The effect of anti-plug molding laws on investment spending.

This table reports results from OLS regressions of investment spending on indicators for whether a state has adopted either an all item APML ($All\ APML$) or a boat specific APML ($Boat\ APML$). The sample period in Panel A (B) is 1975 to 1988 (1975 to 1992). The dependent variables are $R\&D/Sales$ (column 1), $CAPX/Assets$ (column 2), $Intangible\ Capital$ (column 3), $Advertising$ (column 4), $Organizational\ Capital$ (column 5), and $Labor\ Capital$ (column 6). $R\&D/Sales$ is a firm's research and development expenditure (*xrd*) divided by its sales (*sale*). $CAPX/Assets$ is a firm's capital expenditures (*capx*) expenditure divided by its value of total book assets (*at*). $Intangible\ Capital$ is the natural logarithm of a firm's externally purchased and internally created intangible capital (*k_int*) – *k_int* is measured as in Peters and Taylor (2017). $Advertising$ is a firm's advertising expenditure (*xad*) divided by its value of total book assets. $Organizational\ Capital$ is measured as a firm's selling, general and administrative expenses (*xsga*) divided by its value of total book assets. $Labor\ Capital$ is a firm's number of employees (*emp*) divided by its real assets, where assets are adjusted using 2015 dollars. $All\ APML$ ($Boat\ APML$) is an indicator variable equal to one if the firm is headquartered in a state that adopts an APML that covers all (only boat related) products. Panel B interacts $ALL\ APML$ and $Boat\ APML$ with an indicator for whether the sample year is post-1988 ($Post88$). Control variables, unless specified as a dependent variable, include: FGL, BCL, CSL, DDL, FPL, PPL, $UTSA$, $R\&D\ Credit$, GFE, $Ln(GDPPC)$, $GDP\ Growth$, $Democrat$, $Ln(Assets)$, $Ln(Age)$, $Debt$, OCF, HHI, SG, $Loss$, $FLIQ$, $R\&D/Sales$, and $CAPX/Assets$. Controls are defined in Appendix A. Control variable coefficients are unreported to conserve space. Industry fixed effects are defined at the two-digit SIC level. Continuous variables, except state-level variables, are winsorized at their 2.5th and 97.5th percentiles. Standard errors (reported in parentheses) are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: The effect of APML adoptions on investment spending over the period 1975-1988

	$R\&D/Sales_{[t+1]}$	$CAPX/Assets_{[t+1]}$	$Intangible\ Capital_{[t+1]}$	$Advertising_{[t+1]}$	$Organizational\ Capital_{[t+1]}$	$Labor\ Capital_{[t+1]}$
	(1)	(2)	(3)	(4)	(5)	(6)
$All\ APML_{[t]}$	0.004***	0.007***	0.029***	0.011***	0.001***	0.002*
	(0.001)	(0.002)	(0.010)	(0.002)	(0.000)	(0.000)
$Boat\ APML_{[t]}$	-0.000	0.002	-0.019	0.000	-0.001	-0.000
	(0.002)	(0.003)	(0.027)	(0.005)	(0.001)	(0.000)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,019	13,019	13,018	13,019	13,019	8,717
Adjusted R^2	0.742	0.444	0.909	0.823	0.838	0.821

Table 6
Continued.

Panel B: The effect of APML adoptions on investment spending over the period 1975-1992	$R\&D/Sales_{[t+1]}$	$CAPX/Assets_{[t+1]}$	$Intangible\ Capital_{[t+1]}$	$Advertising_{[t+1]}$	$Organizational\ Capital_{[t+1]}$	$Labor\ Capital_{[t+1]}$
	(1)	(2)	(3)	(4)	(5)	(6)
$All\ APML_{[t]}$	0.003*	0.006***	0.037***	0.011***	0.001***	0.001*
	(0.001)	(0.001)	(0.011)	(0.002)	(0.000)	(0.000)
$Post88_{[t]} \times All\ APML_{[t]}$	0.006	0.000	0.011	-0.010*	-0.002***	-0.000
	(0.005)	(0.003)	(0.014)	(0.005)	(-0.000)	(0.000)
$Boat\ APML_{[t]}$	0.000	-0.001	-0.008	-0.003	-0.001	-0.000
	(0.002)	(0.003)	(0.027)	(0.006)	(0.001)	(0.000)
$Post88_{[t]} \times Boat\ APML_{[t]}$	-0.001	0.002	-0.014	-0.009	-0.001	-0.000
	(0.003)	(0.003)	(0.023)	(0.008)	(0.001)	(0.00)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,476	17,476	17,473	17,476	17,476	13,158
Adjusted R^2	0.794	0.445	0.909	0.811	0.817	0.798

Table 7
The effect of anti-plug molding laws on profitability.

This table reports results from OLS regressions of profitability on indicators for whether a state has adopted either an all item APML ($All\ APML$) or a boat specific APML ($Boat\ APML$). The sample period in Panel A (B) is 1975 to 1988 (1975 to 1992). The dependent variables are $Gross\ Profit$ (column 1), $Operating\ Margin$ (column 2), ROE (column 3), $Loss$ (column 4), and $Profit/Liabilities$ (column 5). $Gross\ Profit$ is net sales (*sale*) minus the cost of goods sold divided by net sales. $Operating\ Margin$ is operating earnings (*oibdp*) minus the cost of good sold (*cogs*) divided by total revenue (*sale*). ROE is net income (*ni*) divided by common equity (*ceq*). $Loss$ is an indicator set to one if a firm has negative net income during a fiscal year, and zero otherwise. $Profit/Liabilities$ is profit before depreciation (*sale-cogs-xsga*) divided by current liabilities (*lct*). $All\ APML$ ($Boat\ APML$) is an indicator variable equal to one if the firm is headquartered in a state that adopts an APML that covers all (only boat related) products. Panel B interacts $ALL\ APML$ and $Boat\ APML$ with an indicator for whether the sample year is post-1988 ($Post88$). Control variables, unless specified as a dependent variable, include: FGL, BCL, CSL, DDL, FPL, PPL, $UTSA$, $R\&D\ Credit$, GFE, $Ln(GDPPC)$, $GDP\ Growth$, $Democrat$, $Ln(Assets)$, $Ln(Age)$, $Debt$, OCF, HHI, SG, $Loss$, $FLIQ$, $R\&D/Sales$, and $CAPX/Assets$. Controls are defined in Appendix A. Control variable coefficients are unreported to conserve space. Industry fixed effects are defined at the two-digit SIC level. Continuous variables, except state-level variables, are winsorized at their 2.5th and 97.5th percentiles. Standard errors (reported in parentheses) are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: The effect of APML adoptions on profitability over the period 1975-1988					
	$Gross\ Profit_{[t+1]}$	$Operating\ Margin_{[t+1]}$	$ROE_{[t+1]}$	$Loss_{[t+1]}$	$Profit/Liabilities_{[t+1]}$
	(1)	(2)	(3)	(4)	(5)
$All\ APML_{[t]}$	0.012***	0.018***	0.016***	-0.023*	0.017**
	(0.004)	(0.006)	(0.006)	(0.013)	(0.007)
$Boat\ APML_{[t]}$	-0.004	0.000	-0.012	0.005	-0.005
	(0.003)	(0.005)	(0.029)	(0.028)	(0.009)
Control Variables	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes
Observations	9,700	9,699	13,112	13,093	9,701
Adjusted R^2	0.764	0.728	0.238	0.304	0.788

Table 7
Continued.

Panel B: The effect of APML adoptions on profitability over the period 1975-1992					
	$Gross\ Profit_{[t+1]}$	$Operating\ Margin_{[t+1]}$	$ROE_{\{t+1]}$	$Loss_{[t+1]}$	$Profit/Liabilities_{[t+1]}$
	(1)	(2)	(3)	(4)	(5)
$All\ APML_{[t]}$	0.010***	0.014***	0.017***	-0.022*	0.014*
	(0.004)	(0.004)	(0.005)	(0.012)	(0.008)
$Post88_{[t]} \times All\ APML_{[t]}$	-0.003	-0.000	-0.025**	0.008	0.004
	(0.005)	(0.011)	(0.012)	(0.016)	(0.008)
$Boat\ APML_{[t]}$	-0.003	0.002	-0.003	0.001	0.003
	(0.004)	(0.005)	(0.023)	(0.027)	(0.011)
$Post88_{[t]} \times Boat\ APML_{[t]}$	-0.001	-0.007	0.010	-0.013	-0.003
	(0.006)	(0.013)	(0.041)	(0.041)	(0.012)
Control Variables	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes
Observations	14,149	14,148	17,560	17,531	14,150
Adjusted R^2	0.752	0.699	0.226	0.288	0.770

Table 8
The effect of anti-plug molding laws on Tobin's Q.

This table reports results from OLS regressions of Tobin's Q on indicators for whether a state has adopted either an $All\ APML$ or a $Boat\ APML$. The sample period is either 1975 to 1988 (columns 1-3) or 1975 to 1992 (columns 4-6). The dependent variable is $Tobin's\ Q$. $Tobin's\ Q$ is the market value of assets (*at* – book equity + market equity (*prcc_f***csho*)) divided by the book value of assets (*at*). The book equity calculation, and this measure in general, follows Fama and French (1992). $All\ APML$ ($Boat\ APML$) is an indicator variable equal to one if the firm is headquartered in a state that adopts an APML that covers all (only boat related) products. Columns 4-6 interact $ALL\ APML$ and $Boat\ APML$ with an indicator for whether the sample year is post-1988 ($Post88$). Other law controls include: FGL, BCL, CSL, DDL, FPL, PPL, $UTSA$, $R\&D\ Credit$ and GFE. State-level controls include: $Ln(GDPPC)$, $GDP\ Growth$, and $Democrat$. Firm-level controls include: $Ln(Assets)$, $Ln(Age)$, $Debt$, ROA, OCF, HHI, SG, $Loss$, $FLIQ$, $R\&D/Sales$, and $CAPX/Assets$. Controls are defined in Appendix A. Industry fixed effects are defined at the two-digit SIC level. Continuous variables, except state-level variables, are winsorized at their 2.5th and 97.5th percentiles. Standard errors (reported in parentheses) are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	$Tobin's\ Q_{[t]}$					
	(1)	(2)	(3)	(4)	(5)	(6)
$All\ APML_{[t]}$	0.085***	0.082***	0.091***	0.074***	0.069***	0.074***
	(0.011)	(0.012)	(0.011)	(0.012)	(0.013)	(0.015)
$Post88_{[t]} \times All\ APML_{[t]}$				-0.121*	-0.121*	-0.072
				(0.064)	(0.061)	(0.064)
$Boat\ APML_{[t]}$	0.053	0.047	0.035	0.050	0.039	0.036
	(0.044)	(0.046)	(0.044)	(0.043)	(0.041)	(0.040)
$Post88_{[t]} \times Boat\ APML_{[t]}$				0.001	-0.013	-0.021
				(0.045)	(0.047)	(0.036)
Other Law $Controls_{[t]}$	Yes	Yes	Yes	Yes	Yes	Yes
State-Level $Controls_{[t]}$	No	Yes	Yes	No	Yes	Yes
Firm-Level $Controls_{[t-1]}$	No	No	Yes	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,139	13,139	13,139	17,600	17,600	17,600
Adjusted R^2	0.681	0.682	0.705	0.662	0.663	0.683

Table 9
The effect of anti-plug molding laws on Tobin's Q for patenting and non-patenting firms.

This table reports results from OLS regressions of $Tobin's\ Q$ on indicators for whether a state has adopted either an $All\ APML$ or a $Boat\ APML$. The sample period is 1975 to 1988. $All\ APML$ ($Boat\ APML$) is an indicator variable equal to one if the firm is headquartered in a state that adopts an APML that covers all (only boat related) products. The table examines the heterogeneous treatment effect of an APML for ex-ante patenting and non-patenting firms. The $Patent\ Activity$ interaction terms include: $Ln(1 + Patent)$ (column 1), $Ln(1 + CW\ Patent)$ (column 2), $Ln(1 + SM\ Patent)$ (column 3), and $Patentless\ R\&D$ (column 4). $Patentless\ R\&D$ is an indicator equal to one if a firm has non-zero R&D in the current or preceding three years, and zero patents granted in the current or next three years, and zero otherwise. Interactions of $Patent\ Activity$ with $Boat\ APML$ are unreported to conserve space. FGL, BCL, CSL, DDL, FPL, PPL, $UTSA$, $R\&D\ Credit$, GFE, $Ln(GDPPC)$, $GDP\ Growth$, $Democrat$, $Ln(Assets)$, $Ln(Age)$, $Debt$, OCF, HHI, SG, $Loss$, $FLIQ$, $R\&D/Sales$, and $CAPX/Assets$. Controls are defined in Appendix A. Control variable coefficients are unreported to conserve space. The row "Test for joint significance" shows the result from a test of whether the effect of $ALL\ APML$ on $Tobin's\ Q$ for a firm with an average level of $Patent\ Activity$ is different from zero during the period. Industry fixed effects are defined using two-digit SICs. Continuous variables, except state-level variables, are winsorized at their 2.5th and 97.5th percentiles. Standard errors (reported in parentheses) are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	$Tobin's\ Q_{[t]}$			
	(1)	(2)	(3)	(4)
$All\ APML_{[t]}$	0.127***	0.134***	0.139***	-0.048
	(0.012)	(0.013)	(0.013)	(0.060)
$All\ APML_{[t]} \times Ln(1 + Patent)_{[t-1]}$	-0.207***			
	(0.036)			
$All\ APML_{[t]} \times Ln(1 + CW\ Patent)_{[t-1]}$		-0.035***		
		(0.006)		
$All\ APML_{[t]} \times Ln(1 + SM\ Patent)_{[t-1]}$			-0.061***	
			(0.009)	
$All\ APML_{[t]} \times Patentless\ R\&D_{[t-1]}$				0.128**
				(0.049)
$Boat\ APML_{[t]}$	0.035	0.035	0.032	-0.003
	(0.044)	(0.044)	(0.045)	(0.028)
$Ln(1 + Patent)_{[t-1]}$	-0.001			
	(0.044)			
$Ln(1 + CW\ Patent)_{[t-1]}$		-0.002		
		(0.010)		
$Ln(1 + SM\ Patent)_{[t-1]}$			0.058***	
			(0.016)	
$Patentless\ R\&D_{[t-1]}$				-0.097**
				(0.042)
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
Observations	13,139	13,139	13,139	9,909
Adjusted R^2	0.705	0.705	0.706	0.713
$Patent\ Activity$ mean (std. deviation):	0.182	1.179	0.682	0.866
	(0.397)	(1.557)	(1.368)	(0.341)
Test for joint significance:				
$[All\ APML_{[t]} \times Patent\ Activity_{[t-1]}] +$	0.089***	0.092***	0.097***	0.080***
$[All\ APML_{[t]}]$	(0.011)	(0.011)	(0.012)	(0.021)

Table 10
The effect of anti-plug molding laws on Tobin's Q for firms with greater innovative ability.

This table reports results from OLS regressions of $Tobin's\ Q$ on indicators for whether a state has adopted either an $All\ APML$ or a $Boat\ APML$. The sample period is either 1975 to 1988 (columns 1-2) or 1975 to 1992 (columns 3-4). $All\ APML$ ($Boat\ APML$) is an indicator variable equal to one if the firm is headquartered in a state that adopts an APML that covers all (only boat related) products. The table examines the heterogeneous treatment effect of an APML for firms with greater ex-ante innovative ability. The $Innovative\ Ability$ interaction terms include: RQ (columns 1 and 3), and $RQ\ High$ and $RQ\ Low$ (columns 2 and 4). RQ is short for "research quotient" (as proposed in Knott, 2008) and measures the percentage increase in revenue from a 1% increase in R&D – i.e., the output elasticity of R&D. $RQ\ High$ ($RQ\ Low$) is an indicator equal to one if a firm's RQ is in the top (bottom) tercile of its distribution, in a given year, and zero otherwise. $RQ\ Medium$ and its interactions are omitted due to multicollinearity. Point estimates on $Boat\ APML$ terms and interactions of $Innovative\ Ability$ with $Post88$ are unreported to conserve space. Control variables include: FGL, BCL, CSL, DDL, FPL, PPL, $UTSA$, $R\&D\ Credit$, GFE, $Ln(GDPPC)$, $GDP\ Growth$, $Democrat$, $Ln(Assets)$, $Ln(Age)$, $Debt$, OCF, HHI, SG, $Loss$, $FLIQ$, $R\&D/Sales$, and $CAPX/Assets$. Controls are defined in Appendix A. Control variable coefficients are unreported to conserve space. Industry fixed effects are defined at the two-digit SIC level. Continuous variables, except state-level variables, are winsorized at their 2.5th and 97.5th percentiles. Standard errors (reported in parentheses) are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	$Tobin's\ Q_{[t]}$			
	(1)	(2)	(3)	(4)
$All\ APML_{[t]} \times RQ_{[t-1]}$	0.364***		0.269**	
	(0.091)		(0.115)	
$All\ APML_{[t]} \times RQ\ High_{[t-1]}$		0.054***		0.128***
		(0.019)		(0.026)
$All\ APML_{[t]} \times RQ\ Low_{[t-1]}$		0.018		0.031
		(0.013)		(0.026)
$Post\ 88_{[t]} \times All\ APML_{[t]} \times RQ_{[t-1]}$			-2.285***	
			(0.472)	
$Post\ 88_{[t]} \times All\ APML_{[t]} \times RQ\ High_{[t-1]}$				-0.210**
				(0.084)
$Post\ 88_{[t]} \times All\ APML_{[t]} \times RQ\ Low_{[t-1]}$				0.099
				(0.112)
$All\ APML_{[t]}$	-0.056**	-0.029	-0.064**	-0.090**
	(0.024)	(0.022)	(0.028)	(0.039)
$Post\ 88_{[t]} \times All\ APML_{[t]}$			0.219***	-0.060
			(0.058)	(0.072)
$RQ_{[t-1]}$	0.096		0.148	
	(0.118)		(0.117)	
$RQ\ High_{[t-1]}$		0.013		0.007
		(0.018)		(0.017)
$RQ\ Low_{[t-1]}$		-0.026		-0.031
		(0.024)		(0.026)
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
Observations	6,546	6,546	8,619	8,619
Adjusted R^2	0.665	0.665	0.653	0.653

Table 11
Abnormal returns around the U.S. Supreme Court's invalidation of anti-plug molding laws.

This table reports results from OLS regressions of one-day risk-adjusted excess announcement returns on indicators for whether a state has adopted either an $All\ APML$ or a $Boat\ APML$. The event date is February 21, 1989. $All\ APML$ ($Boat\ APML$) is an indicator variable equal to one if the firm is headquartered in a state that adopts an APML that covers all (only boat related) products. Following Cohen and Wang (2013), I compute risk-adjusted excess returns using a two-step procedure. First, I estimate each firm's loadings on the standard four (i.e., MktRf, SMB, HML, and MOM) factors (Fama and French, 1993, 1996) over the [-280,-61] trading days relative to the announcement date. Second, I take the residuals from a cross-sectional regression of raw announcement window returns on the estimated factor sensitivities to obtain my dependent variable: $1\text{-}Day\ Risk\text{-}Adjusted\ Excess\ Ann.Return$. Column 1 considers the average effect for $All\ APML$ and $Boat\ APML$ firms, while the remaining columns (2-7) consider the heterogenous effect for such firms split by the following cross-sectional characteristics: $Patent\ High$ is an indicator equal to one for firms with patents counts above its median sample value, and zero otherwise; $Patentless\ R\&D$; and $RQ\ High$. Industry fixed effects are defined at the two-digit SIC level. Risk-adjusted excess returns are winsorized at their 2.5th and 97.5th percentiles. Standard errors (reported in parentheses) are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

				$1\text{-}Day\ Risk\text{-}Adjusted\ Excess\ Ann.Return_{[t]}$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$All\ APML_{[t]}$	-0.365**	-0.049	-0.630***	-0.283*	-0.188	-0.339**	-0.065
	(0.137)	(0.160)	(0.160)	(0.159)	(0.166)	(0.157)	(0.171)
$Boat\ APML_{[t]}$	0.206	0.346	0.109	0.378	0.340	-0.121	0.075
	(0.200)	(0.249)	(0.234)	(0.343)	(0.805)	(0.388)	(0.250)
Sample:	N/A	$Patent\ High = 1$	$Patent\ High = 0$	$Patentless\ R\&D = 1$	$Patentless\ R\&D = 0$	$RQ\ High = 1$	$RQ\ High = 0$
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,299	528	771	774	213	223	475
Adjusted R^2	0.007	0.001	0.010	0.005	0.001	0.008	0.001

Table 12

The non-effect of anti-plug molding laws on firms in non-"moldable products" industries.

This table reports falsification test results from OLS regressions of patent counts, R&D spending, gross profitability, and Tobin's Q on indicators for whether a state has adopted either an $All\ APML$ or a $Boat\ APML$ on a sample of firms that operate in industries without "Moldable Products" (see Table 2). The sample period is either 1975 to 1988 (odd-numbered columns) or 1975 to 1992 (even-numbered columns). The dependent variables include: $Ln(1 + Patent)$ (columns 1-2), $R\&D/Sales$ (columns 3-4), $Gross\ Profit$ (columns 5-6) or $Tobin's\ Q$ (columns 7-8). $All\ APML$ ($Boat\ APML$) is an indicator variable equal to one if the firm is headquartered in a state that adopts an APML that covers all (only boat related) products. The even-numbered columns interact $ALL\ APML$ and $Boat\ APML$ with an indicator for whether the sample year is post-1988 ($Post88$). Control variables include, unless specified as a dependent variable: FGL, BCL, CSL, DDL, FPL, PPL, $UTSA$, $R\&D\ Credit$, GFE, $Ln(GDPPC)$, $GDP\ Growth$, $Democrat$, $Ln(Assets)$, $Ln(Age)$, $Debt$, OCF, HHI, SG, $Loss$, $FLIQ$, $R\&D/Sales$, and $CAPX/Assets$. Controls are defined in Appendix A. Control variable coefficients are unreported to conserve space. Industry fixed effects are defined at the two-digit SIC level. Continuous variables, except state-level variables, are winsorized at their 2.5th and 97.5th percentiles. Standard errors (reported in parentheses) are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	$Ln(1 + Patent)_{[t+2]}$		$R\&D/Sales_{[t+1]}$		$Gross\ Profit_{[t+1]}$		$Tobin's\ Q_{[t]}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$All\ APML_{[t]}$	-0.002	-0.002	0.001	0.001	-0.003	-0.004	-0.003	0.007
	(0.006)	(0.005)	(0.001)	(0.001)	(0.011)	(0.009)	(0.054)	(0.054)
$Post88_{[t]} \times All\ APML_{[t]}$		-0.003		-0.000		-0.012		-0.004
		(0.003)		(0.001)		(0.009)		(0.055)
$Boat\ APML_{[t]}$	-0.005	-0.007	0.000	0.001	0.004	-0.002	0.044	0.038
	(0.005)	(0.005)	(0.001)	(0.001)	(0.007)	(0.009)	(0.039)	(0.045)
$Post88_{[t]} \times Boat\ APML_{[t]}$		-0.004		0.000		0.010		-0.048
		(0.005)		(0.001)		(0.013)		(0.030)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,160	25,023	18,160	25,023	11,643	22,073	18,160	25,023
Adjusted R^2	0.951	0.945	0.837	0.881	0.656	0.684	0.735	0.703