Three essays in Corporate Finance and Innovation

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Abstract

Innovation is the engine of economic growth for developed countries. In my dissertation, I study three different factors that affect firms' innovation outcome. The first chapter of my dissertation shows that when courts systematically favor plaintiffs over defendants, their biased attitudes stifle innovation and reduce the financial value of innovative firms under their jurisdiction. The second chapter of my dissertation looks at another aspect of innovation output, namely the inventors. I show that inventors who work in cities with higher level of labor market concentration produce more patents while compensating that with lower quality of innovation. The third chapter of my paper shows that in firms where the extensive monitoring is absent; innovation output could also be the outcome of firms' attempt to time the market.

In the first chapter of my dissertation, I study the effect of district courts on innovative firms. District courts have exclusive legal power to make final decisions in patent ownership and infringement lawsuits. This paper shows that when district courts systematically favor plaintiffs over defendants, their biased attitudes stifle innovation and reduce the financial value of firms under their jurisdiction. To arrive at this conclusion, I exploit the 2017 Supreme Court ruling that limits the venues where plaintiffs can file a patent litigation case to defendants' local district courts only. Firms which, following the ruling, have become subject to the jurisdiction of plaintiff-friendly district courts generate fewer patents with lower total market value and invest less in R&D compared to firms subject to the jurisdiction of defendant-friendly district courts.

In the second chapter, I examine the effect of labor market concentration on innovation output. To establish causality, I compare successful versus failed mergers that limit the number of potential employers in a local labor market. I find that following an increase in labor market concentration inventors file a larger number of patents; yet the patents are of lower dollar value and more firm-specific. Furthermore, inventors in consolidated labor markets produce less exploratory innovation and instead build on their employers' existing technological knowledge. The results suggest that firms manage their skilled workers' tasks in a way that increases inventors' dependency on their employer, thereby allowing firms in concentrated markets to share a smaller portion of innovation-driven surplus with their skilled labor.

The third chapter of my dissertation shows that firms engage in window dressing by timing the date of their patent applications. In support of this argument, I find that almost 40% of firms' yearly patents are filed in the last quarter of the calendar year. To establish window dressing as the key mechanism behind this pattern, I use the adoption of America Invents Act as the quasi-natural experiment that has increased the costs of window dressing. Following the Act, the window dressing behavior has become less prevalent. I also find that patents applied for at the last quarter have lower quality, and the window dressing effect is most pronounced in firms with the same fiscal and calendar year-ends. Finally, the pattern is prevalent among firms subject to lower external monitoring, further supporting the incentive to window dress as the underlying channel.

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1 District Courts and Innovation

1.1 Introduction

Promotion of innovation is a desirable goal both for individual firms and for society in general. As a result, identifying potential obstacles to the creation of valuable new ideas is crucially important. Patent rights are a key instrument that stimulates innovation activity by ensuring that inventors can monetize their innovation. However, the intangible nature of the patents and the fact that patents protect ownership over ideas rather than physical assets make patent enforcement a noisy process with substantial room for legal interpretation (Jaffe and Lerner (2011)). Moreover, loopholes of the legal system provide some entities with the opportunity to exploit it by filing frivolous patent infringement cases. As a result, the threat of litigation can be so high that it could potentially stifle innovation.

Despite introduction of new regulations, aimed to reduce expected costs of patent litigation for defendants (Appel et al. (2019), Mezzanotti (2021)), frivolous patent lawsuits still overwhelm dockets in certain courts. A body of law literature argues that district courts, known for their pro-plaintiff practices, are to blame.[1] District courts are part of the U.S. judicial system and have the authority to interpret the patent rules and make the final calls for plaintiffs to assert their ownership over their patents. If district courts systematically favor plaintiffs over defendants (or vice versa), their biased attitudes may encourage frivolous lawsuits and increase the chances that the plaintiff wins the case even if no infringement has occurred. This, in turn, will have an impact on innovation and financial value of firms that fall under their jurisdiction. Yet, despite the role of district courts as a main apparatus within the patent litigation system, to the best of our knowledge, none of the existing studies have examined the effect of district courts on corporate innovation. This paper aims to close this gap. Specifically, it studies whether plaintiff versus defendant-friendly attitude of a district court shapes innovation of firms that operate under its jurisdiction.

[1] In this work we focus solely on patent ownership and infringement lawsuits. As a result, our definition of plaintiff refers only to patent owners who seek to establish that the defendant has committed patent infringement by unlawfully using the patented claim in the defendant's own innovation.

To address this question empirically, we use the 2017 Supreme Court decision in *TC Heartland v. Kraft Foods* ruling as a regulatory shock that has significantly limited plaintiff control over the choice of jurisdiction. Prior to 2017, plaintiffs filed litigation lawsuits in district courts of their choice. As a result, over one-third of all U.S. cases were filed in the district courts of Texas, notoriously known for their plaintiff-friendly attitude (Eldar and Sukhatme (2018), Frye and Ryan (2017) and Miller (2018a)). Texas district courts were particularly popular among non-practicing entities – patent trolls, whose source of profits relies on claiming license fees and litigating against infringement. The Supreme Court ruling of May 2017 has limited the plaintiffs' choice of potential venues to the state where defendant was incorporated or had an established place of business. We use this decision as a natural experiment to study the impact of district court attitudes on firms' innovation output in a difference-in-differences setting.

Our main prediction is as follows. If district courts are impartial, or if their attitudes are economically unimportant, innovation activity of Texas relative to non-Texas firms should not change following the ruling. However, if district courts exhibit systematic plaintiff-friendly biases that give rise to a market friction, we expect to find that Texas-exposed firms will have lower levels of innovation activity compared to their non-Texas peers after the Supreme Court ruling.

The first element of our hypothesis development is the argument that following the *TC Heartland* case ruling, expected patent litigation costs have increased for Texas firms compared to non-Texas ones. This happens for two reasons. First, prior to the ruling, patent trolls could essentially litigate any firm in plaintiff-friendly Texas courts. Following the ruling, trolls had to litigate non-Texas firms outside Texas. This change has reduced the chances of winning an opportunistic lawsuit against non-Texas firms and encouraged patent trolls to shift their attention to Texas firms as relatively more profitable lawsuit targets. As a result, the expected costs of patent litigation for Texas-based firms have increased. Second, following the ruling, non-Texas defendants fell under the jurisdiction of their home state district court, which has reduced their probability to be litigated in Texas and their expected litigation costs. Since Texas firms are the treatment group in our diff-in-diff analysis, this channel also implies that the costs of litigation have become higher for

Texas firms *relative* to expected litigation costs for the rest of the sample.

Next, we tie the increase in expected litigation costs to innovation activity. To that end, we build on Mezzanotti (2021), who demonstrates that higher expected costs of litigation reduce innovation activity of prospective defendants. This happens because strict patent enforcement that helps plaintiffs obtain large settlements for frivolous claims increases the hold-up costs of litigation, and reduces profitability of future research and development (R&D) investments. Based on this finding, we hypothesize that if plaintiff-friendly attitude leads to higher expected litigation cost for prospective Texas defendants, following the ruling Texas-exposed firms will exhibit lower levels of innovation activity compared to non-Texas firms.

Before turning to the main empirical analysis, we validate the central assumption of our setting and verify that the ruling has transferred litigation activity away from plaintiff-friendly Texas courts. To this end, we look at the number of new patent cases filed in each court using Stanford Non-Practicing Entity (NPE) Litigation Database. Before May 2017, the share of patent cases filed in Texas district courts out of the total cases filed nation-wide was around 35%. However, just one month after *TC Heartland* this number dropped to 19%, and has remained at this level until the end of the sample in 2019. Furthermore, when we split the number of filings by plaintiff type, we find that the decrease in Texas courts filings is almost fully attributed to a decline in filings by non-practicing entities.

To test the main hypothesis, for every firm we identify all the district courts it can be potentially exposed to using the following algorithm. First, we consider the district court located in the state of firm's incorporation as one venue in which it can be litigated. Second, districts in which defendants have substantial business can be another venue where plaintiffs can file a case. Since there is no unique interpretation of the term from the legal standpoint, we consider the broadest possible definition. To that end, we include the district courts of the headquarter state under the assumption that firms often conduct a significant portion of their overall operation in the headquarter location. We also consider cases where firms have major business operations outside of their headquarter state. For every firm in the Compustat universe we count the number of times each state is mentioned in a 10-k report.[2] We then define the state that has been mentioned most often as the state in which the firm has substantial business.

We employ Kogan, Papanikolaou, Seru and Stoffman (2017) (hereafter, KPSS) dataset to extract patent information for publicly traded firms. Using KPSS, we obtain the number of patent applications each firm produces per year, as well as the dollar value of each applied patent based on the market reaction to the news of a patent grant.

We enrich the regressions with numerous control variables. In particular, we use an indicator variable that captures whether firms can be litigated in the state of Delaware. Around 70 percent of U.S. public firms are incorporated in Delaware and after the ruling, Delaware saw a significant jump in the number of new patent cases filed at its district court. However, it is a-priori unclear whether Delaware is more pro-plaintiff friendly compared to other non-Texas district courts. Although Delaware is known as the hub of corporate litigation, it is often argued that it is because of its reputation for impartial and thorough analysis of cases (Eldar and Sukhatme (2018)). As a result, it is difficult to gauge a prediction regarding the expected costs of litigation of Delaware firms after the ruling.

In our baseline empirical analysis, we estimate the effect of *TC Heartland* indicator variable, which takes on a value of one starting from 2018, interacted with *TexasExposed* indicator variable, on the overall number of patents filed using a difference-in-differences setting. We control for standard firm characteristics, including size, profitability and growth opportunities, as well as time and firm-fixed effects. All the estimated coefficients for the interaction term are negative and statistically significant. The results show that after the ruling Texas firms face a 5.5% drop in their patenting volume relative to the average levels of patenting by all sample firms. Next, we examine the total dollar value of innovation, and find that following the ruling the economic value of innovation produced by Texas firms has also declined. Finally, we look at changes in R&D and ask

[2]The methodology of using 10-k files to identify the extent of a firm's economic presence in a given state is well established in the literature (Garcia and Norli (2012); Cohen et al. (2012); Mukherjee et al. (2017), Addoum et al. (2020)).

whether the drop in innovation output could result from lower levels of investment in research and development of Texas firms compared to other firms. Consistent with this idea, we find that Texas firms reduce their R&D activity in response to the ruling. Taken together, the baseline findings support the hypothesis that increased ability of plaintiffs to sue defendants in plaintiff-friendly states dampen both innovation input and innovation output.

Next, we address alternative explanations. For example, it is possible that the decline in innovation activity of Texas firms could be attributed to other events that have occurred in Texas around the time of the ruling. To address this concern, we employ an alternative empirical setting. Specifically, we turn to a patent-level analysis and look at the changes in the dollar value of patents that were granted in the narrow window of a few months around the ruling. Because it usually takes a patent 18 to 24 months to get approved since the initial application, patents that were granted right before/after the ruling were likely developed by the same firm around the same time will have similar quality and technological characteristics. As a result, a change in the patent value following the ruling can be solely attributed to changes in the expected costs of litigation that the newly patented product may trigger in the future. Consistent with this prediction, we find a significant drop in the dollar value of patents of Texas-based firms that were granted right after the ruling. The relation is robust to controlling for firm- and technological class-fixed effects. The results of this test confirm the idea that jurisdiction of a plaintiff-friendly [defendant-friendly] court reduces [increases] the economic value of innovation.

We further address the concern that other events at play may be the drivers of our results by performing an event study analysis and examining stock market response to the Supreme Court final ruling announcement in the case of *TC Heartland*. Specifically, we calculate cumulative abnormal returns of all firms during the [0,+5] day window around the ruling announcement and test for a differential response of Texas versus non-Texas firms. The results show a negative effect of the ruling on the market value of Texas firms, which is particularly pronounced among innovating firms, as measured by the scope of R&D and patenting activity prior to the ruling. These results further corroborate the idea that firms subject to jurisdiction of plaintiff-friendly firms incur higher litigation expenses, which reduce their financial value. The statistically significant market response also alleviates a number of endogeneity concerns, and indicates that the outcome of the ruling has not been known to market participants prior to the official announcements.

After addressing endogeneity concerns, we turn to a cross-sectional analysis. Our goal is to further pinpoint the expected litigation costs channel as the underlying mechanism. To this end, we ask whether the impact of the ruling on Texas firms is stronger within the subsample of firms that are more likely to become litigation targets, especially by patent trolls. To define the subsample of prospective defendants, we rely on Cohen et al. (2021), who show that the probability of litigation is higher among innovation-intensive firms, as measured by R&D activity and Tobin's Q, as well as among smaller firms, who are less able to defend themselves against possible lawsuits.[3] We split our sample into two groups based on firm size and the scope of innovation activity prior to the ruling, and re-estimate the baseline regression. We find that the impact of *TC Heartland* ruling is particularly pronounced among firms with high Tobin's Q and R&D activity, as well as smaller firms, as measured by asset size. Thus, Texas firms that are most likely to become potential litigation targets are the ones that have reduced their innovation the most.

In the last part of the paper, we further test the economic implications of district court attitudes. To this end, we look at the geographical dispersion of patenting activity across states in response to *TC Heartland*. Following the ruling, all innovative firms may become inclined to reduce their innovation activity in Texas to lessen the chances of being litigated in its district courts. This incentive, in turn, can lead to downsizing and closure of their Texas-located research centers. To address this question, we use United States Patent and Trademark Office (USPTO) dataset of inventors' address to establish the location of the research hub where a patent was developed. Our results demonstrate that following the Supreme Court's decision, firms' patent production has dropped in Texas compared to their innovation activity in other states. These findings are consistent with the idea that in order to reduce their chances of litigation in Texas, innovative firms have chosen

[3]Cohen et al. (2019) also find that probability of a lawsuit is higher for cash-rich firms. However, this characteristic has an ambiguous effect on innovation activity. On the one side, cash rich firms face higher expected costs of litigation, which potentially reduce the value of innovation. On the other side, cash reserves help alleviate financial constraints and mitigate underinvestment problems, spurring innovation.

to reallocate a portion of their innovation activity outside of Texas. These results provide further evidence of real economic implications that district court biases can generate. We also examine whether firms are less likely to choose Texas as their incorporation, headquarter, or main business activity state, but do not find significant effects. It is possible that cost-benefit analysis is at play: Relocating a research hub is less costly than relocating headquarters or major business activity center.

Finally, throughout our empirical analysis we do not detect a consistent and statistically significant effect of the ruling on innovation activity of Delaware firms. If Delaware court is unlikely to favor one party over the other, the transition of patent litigation cases from Texas to Delaware district courts did not necessarily reduce expected litigation costs of Delaware firms compared to firms on other non-Texas states.

This paper contributes to several strands of literature. First of all, it builds on the work of Mezzanotti (2021), Appel et al. (2019), Cohen et al. (2019), and Lee et al. (2019). These papers show that stronger legal protection of defendants in patent lawsuits increases innovation, whereas high costs of patent litigation, whether expected or realized, impede innovation activity of targeted firms. Some of these papers also exploit legal changes in enforcement at national and state levels: Mezzanotti (2021) uses Supreme Court ruling that reduces the costs of patent litigation for all U.S. defendants; Appel et al. (2019) focus on state laws that limit the ability of patent trolls to make bad-faith patent infringement claims; and Lee et al. (2019) use the passage of the Trade Related Aspects of Intellectual Property agreement that increases U.S. firms' patent litigation risk. Our paper adds to this work by showing that an important entity within patent enforcement system – U.S. district courts – play a role in shaping the innovative landscape. Our work demonstrates that a plaintiff-friendly attitude of district courts can increase the expected costs of patent litigation and create a barrier to innovation.

More broadly, this paper is also related to a strand of literature that studies the impact of corporate, rather than patent, litigation threats on innovation. Thus, Kempf and Spalt (2021) find that low quality class action lawsuits target firms that hold valuable innovation. Lin, Liu, and Manso (2021) establish that when the threat of shareholder lawsuit is lower, firms invest more in research and development and produce more patents. Our work contributes to these papers by identifying a new layer of frictions within the patent litigation system, and showing that the attitude of district courts can be a significant factor in firms' decision making with regards to innovation.

Finally, this paper attempts to resolve a long-standing argument in the law literature regarding the existence of a bias in district courts patent litigation decisions. As mentioned before, one strand of papers claims that Texas district courts are systematically pro-plaintiff, which helps attract a disproportionally large number of litigation cases (Frye and Ryan (2017)). Yet, another strand of papers disputes this view and argues that Texas popularity stems from its expertise and efficiency in handling patent cases in general, rather than from preferential attitude towards some parties (Nguyen (2008)). Finally, a third group of papers rejects all claims arguing that Texas courts are different, and posits that the views above are merely based on anecdotes (e.g., Iancu and Chung (2011)). Our paper contributes to this debate by establishing an economically meaningful heterogeneity in attitudes of district courts that, in turn, impacts innovation.

1.2 Legal Background

1.2.1 Patent Infringement Enforcement: The Structure of the Legal System

Patent rights in the United States are defined at a general federal level through the U.S. Congress. The Congress is responsible for federal patent legislation, including the legal procedures that mitigate frivolous patent lawsuits. Patent legislation process is overall similar to other types of legislation. The laws first need to be drafted as a bill, gain enough support from lawmakers and then pass the Congress.

Besides the federal legislative body, district courts are another level of the U.S. judicial system. District courts are responsible for trying all patent lawsuits on an individual case basis and for making the final calls for plaintiffs to assert their ownership over their patents. District courts have the power to impose additional court level rules, if necessary, and also have the authority to interpret the federal patent rules that are defined too generally. Due to the flexibility in interpretation of general rules and additional judicial freedom, district courts can set up procedures that might be systematically in favor of one of the parties.

1.2.2 Pre-ruling and Texas Dominance

Prior to the TC Heartland ruling of 2017, plaintiffs could file a lawsuit potentially in any district court. This practice originated in 1990 when the U.S. Court of Appeals for the Federal Circuit defined its interpretation of the patent venue statue.

Specifically, the patent venue statue has provided that "any civil action for patent infringement may be brought in the judicial district where the defendant resides, or where the defendant has committed acts of infringement and has a regular and established place of business (28 U.S.C.§1400)." Firms are generally subject to corporate jurisdiction, which limits the number of courts they can be litigated in. However, the U.S. Court of Appeals' interpretation of the statue was that the term "reside" can be translated as any jurisdiction where the defendant is subject to personal jurisdiction. This exclusive switch to personal jurisdiction for patent litigation cases has allowed plaintiffs to file a case in virtually any district court. Since the Court of Appeals has the highest authority in the judicial hierarchy, district courts were obliged to follow its rulings.

The 1990 ruling has prepared the ground for the popularity of the Eastern District of Texas (EDT). The first step in the formation of EDT friendly attitude was a series of Texas Instruments (TI) cases, filed in the early 1990s. These cases, mainly targeting Asian companies that allegedly infringed on TI's computer technology patents, were intended to force the defendants to pay patent licensing fees. The district court has ruled in favor of the plaintiff, and the news of a successful outcome, which has provided TI with generous license fees, motivated other corporations to file their patent litigation cases in EDT (Nguyen (2008)). As the the number of patent trolls - entities that acquire large portfolios of patents with the sole intention of generating revenues from patent licensing fees - has increased substantially over the past new decades, the popularity of EDT courts has increased even further.

In the past decade, the alleged plaintiff friendly bias of the EDT court has stemmed from several sources. First and foremost, cultural conditions, such as favoring personal property protection rights, generate jurors' sympathies towards plaintiffs. For the same reasons the jurors are also extremely sensitive towards any property violation allegations (Frye and Ryan (2017)). Those sympathies are further amplified in cases where a small firm litigates a large corporation.[4] Second, a set of local plaintiff-friendly rules that are put forward by EDT judges are another potential reason behind the venue's popularity among patentees. One example of such ruling is the discovery period documentation requirement, aimed to establish the process and timeline of the patent discovery and development: On a short deadline (around 3 months) the defendant is required to submit vast and comprehensive amounts of information regarding its operations, products and R&D activity to the court. To meet this requirement, the defendant often needs to assemble a team of legal, IT and technical staff to gather the requested documents in time. The costs of discovery period rule to the defendant can easily amount to hundreds of thousands of dollars (Eldar and Sukhatme (2018)). These factors, in turn, often motivate many defendants to settle cases outside the court at unfavorable terms to avoid such hectic processes.

To establish the popularity of Texas district courts empirically, we use Stanford's Non-Practicing Entity (NPE) Litigation Database. This database reports basic details of all patent litigation cases filed in U.S. district courts. Using 2016 as the last pre-ruling year, we calculate the relative share of each state's district courts out of the total nationwide litigation activity over the same period, and present the share of the three most popular for litigation states, which are Texas, Delaware, and California. Following Miller (2018b), we further classify plaitiffs, or patent asserters, as patent trolls (asserter category 1, 4, or 5) or practicing entities (all other categories). Remarkably, in 2016 around 62 percent of all patent troll cases were filed in Texas (Panel A). That is considerably higher

[4] Cohen et al (2019) show that patent trolls target defendants that have high market value and are rich in cash.

than only 7 percent of cases filed in Texas during the same period by practicing entities (Panel B). The differences confirm the popularity of Texas courts among patent trolls. More importantly, the high presence of entities whose primary source of revenues stems from patent lawsuits further supports the argument that Texas courts are likely to have a plaintiff-friendly attitude. The high popularity of Texas courts is still present when examining the fraction of cases across both categories of patent asserters, as 35 percent of total litigation cases were filed in Texas (Panel C).

[Insert Figure 1.1 here]

One may wonder whether the high fraction of Texas filings is a reflection of high innovative activity in Texas. To examine if this is the case, we calculate the fraction of patent applications filed in Texas out of the total U.S. patent applications filed. Figure 1.2 presents the relative share of the three most innovative states, as well as the share of Texas, in year 2016. In this case, Texas ranks fourth and contributes only 7 percent to the number of patent applications filed nation-wide during that period. This is a much lower fraction than the fraction of all patent litigation cases filed in Texas. This analysis, along with anecdotes of Texas pro-plaintiff attitude, supports the claim that Texas district courts employ certain practices to attract a disproportionately large number of patent litigation cases.

[Figure 1.2]

1.2.3 TC Heartland *Supreme Court Ruling*

The Supreme Court's decision in the case TC Heartland v. Kraft Foods has transformed the landscape of patent litigation. Kraft Foods has filed a patent litigation case against TC Heartland in the district court of Delaware, although TC Heartland was both headquartered and incorporated in the state of Indiana. TC Heartland requested that the venue to be moved to the district court of Indiana on the grounds that its business was concentrated in that state. The district court of Delaware rejected the motion. TC Heartland's subsequent appeal to the U.S. Court of Appeals to change the venue was also rejected. Finally, in September 2016 TC Heartland filed an appeal to the Supreme Court requesting a change of venue.

The Supreme Court issued its decision on May 22, 2017, ruling unanimously that the term "reside" in first clause of the 28 U.S.C. § 1400 is defined as the state of incorporation. However, the second clause of the 28 U.S.C. § 1400 gives the plaintiffs the right to file a patent lawsuit in districts where defendant "has an established place of business". As a result, TC Heartland outcome has made the venue selection in patent litigation cases more similar to that used in corporate litigation and bankruptcy cases (Eldar and Sukhatme (2018)). As the main consequence of *TC Heartland*, plaintiffs lost the flexibility to choose its venue.

1.2.4 Implications of the TC Heartland *Ruling*

As most plaintiffs became limited in their choice of venue, the load of patent cases has shifted from EDT and other Texas district courts to the districts where firms were incorporated or focused their main economic activity.

To demonstrate the drop in the number of cases filed in Texas in the aftermath of *TC Heartland*, we again use Stanford Non-Practicing Entity (NPE) Litigation Database to track changes in the number of new patent cases in each district court before and after the ruling. To calculate the relative share of each state, every month we divide the total number of new patent cases registered in all district courts of this state by the number of all patent cases registered in the same month nationwide. We then plot the the time-series of the share of new patent cases for Texas in Figure 1.3.

[insert Figure 1.3]

Figure 1.3 Panel A demonstrates that Texas courts suffered a drop in the number of new patent cases. Within a few months after the ruling the number of newly filed patent cases in Texas has dropped from 35 percent to 20 percent. This pattern further validates the idea that Texas was, indeed, a preferred choice of litigation venue for plaintiffs and that following the ruling, plaintiffs' ability to file their cases there has substantially declined. Note that the change was particularly pronounced among litigations initiated by patent trolls, supporting the notion that the ruling has reduced the probability of practicing firms to be litigated by non-practicing ones in plaintiff-friendly Texas courts.

In Figure 1.3 Panel B we also plot the relative share of the district court of Delaware over time. Delaware is of particular interest for this analysis because around 70% of the U.S. public firms are incorporated there. Since *TC Heartland* requires that plaintiffs litigate defendants in the district where the defendant is incorporated, Delaware should become a more popular venue for patent lawsuits after *TC Heartland*. The figure is consistent with this argument. Although the popularity of Delaware has been increasing prior to the ruling, there is a considerable spike after *TC Heartland*. Taken together, the findings of the two figures suggest that a large portion of cases that were filed in Texas prior to the ruling, now have likely moved to Delaware.

Although Delaware is unlikely to be as pro-plaintiff as Texas district courts, it is still unclear whether district court of Delaware is more pro-plaintiff compared to non-Texas district courts. On the one side, district court of Delaware inherits the rich legacy of being known as the hub of all corporate litigations. On the other side, Delaware courts are also known for their impartial and thorough analysis of cases and are unlikely to favor one party over the other (Eldar and Sukhatme (2018)). As a result, it is impossible to predict whether Delaware-exposed firms face higher expected costs of litigation after the ruling. Nevertheless, these firms are economically important, as the majority of our sample firms are incorporated in Delaware and consequently are eligible to be litigated there. The high level of exposure to Delaware courts motivates us to use the likelihood of litigation in Delaware as an additional control variable in the regression analysis. Its inclusion also addresses possible concerns that the impact of the ruling on non-Texas firms was driven by increased exposure to Delaware, rather than by lower exposure to Texas.

1.3 Empirical Approach

This section outlines the empirical settings of our study. The structure of the sections is as follows. First, we explain the construction of the main independent variables that capture the likelihood of being litigated in a certain state's district court (subsection 3.1). We then define the main dependent variables that capture innovation activity in subsection 3.2, and provide the details of the control variables in subsections 3.3. Finally, subsection 3.4 describes the final sample.

1.3.1 State Exposure Variables

Following the 2017 ruling, the Supreme Court started to mandate that only firms with an economic link to a region can be litigated in district courts of that region. Yet, measuring economic links between firms and states can present a challenge, as (1) the ruling has not provided a clear definition of which cases would constitute "an established place of business"; and (2) the commonly used practice of using headquarter state or state of incorporation as centers of a firm's economic activity may not capture economic links in a precise manner, since some firms operate in several geographic regions, and some firms conduct their main operations outside of the headquarter/incorporation state. To address these issues, we employ the broadest possible definition of an economic link, and consider states of incorporation, headquarter states, and states of economic exposure as states of "an established place of business".

Specifically, we identify firms with exposure to state s as firms that are incorporated or headquartered in that state. This information is extracted from the Notre Dame Software Repository for Accounting and Finance (SRAF) website.[5] We also capture exposure by looking at economic links that extend beyond the state of incorporation and headquarter. To this end, we follow Garcia and Norli (2012) and use the information in 10-k reports. We download 10-k documents from EDGAR and then scrape the files to count the number of times each state is mentioned in sections "Item 1: Business", "Item 2: Properties", "Item 6: Consolidated Financial Data", and "Item 7:

[5] https://sraf.nd.edu/data/augmented-10-x-header-data/

Management's Discussion and Analysis". Using this information, for each firm i, state s, and year t, we define a variable $Dependency_{i,s,t}$, which is equal to the number of times state s is mentioned in firm i's 10-k report in year t. For every firm in Compustat universe, we compute the extent of its economic relationship with every state based on its 2016 report (since *TC Heartland* event takes place in 2017, we want to use the most recent pre-ruling data point). We find that the 2016 10-k report is missing in about 10% of our sample firms. In this case, we fill in the information based on the 2015 filing. If 2015 is also missing, we use 2014.

Note that reliance on financial reports to measure geographic dispersion of firms' activities has been exploited in several studies (e.g., Cohen et al. (2012), Mukherjee et al. (2017), Addoum et al. (2020)). For example, Addoum et al. (2020) show that *Dependency* has correlation of 0.67 with firms' distribution of subsidiaries across states. The presence of a subsidiary in a region typically implies that a firm has significant economic activity in that region, and therefore *Dependency* should be a good measure to capture economic relation between each firm and state. As further validation of the approach, we find that the state with maximal $Dependency_{ist}$ is the headquarter state in 70% of the cases (unreported for the sake of brevity).

As a result of the previous discussion, we define the indicator variable $TexasExposed_{it}$ equal to one if at least one of the following conditions holds:

1. Firm i is incorporated in Texas in 2016;
2. Firm i is headquartered in Texas in 2016
3. The variable $Dependency_{ist}$ is the highest for state s=Texas in 2016.

If neither of the 1-3 conditions is satisfied, the indicator equals zero. We construct the variable $DelawareExposed_{it}$ in the same fashion as $TexasExposed_{it}$. Note that according to our definition, it is possible that some firms are exposed to both Texas and Delaware. For instance, Texas Instruments is incorporated in Delaware and headquartered in Texas. In this case, Texas Instruments is exposed to both states' district courts, as, following the ruling, a prospective plaintiff can make a reasonably valid claim of litigating Texas Instruments in either of the two states. It is also noteworthy to mention that this measure is time-invariant.

After constructing the state exposure variables, we also ensure that this methodology captures the research and development activities of firms. To this end, we look at the technology hubs and check to what extent the states of technology hub operations overlap with the headquarter state or state with maximal *Dependency*. We use the address of the first inventor to identify location of technology hubs. Specifically, for each firm we define the state that hosts the main technology hub as the state with the highest number of patents the firm has applied for in the last five years. The results show that in over 70% of the cases headquarter states and main technology hub states are the same, while in over 50% of the cases the main technology hub state is the same as the state with the highest *Dependency* value (unreported to the sake of brevity).

Admittedly, the 10-k based measure of economic activity, and even the lab location measure, may still provide noisy proxies compared to what could be obtained using databases that measure the precise location and the scope of production of each facility (e.g., the Annual Survey of Manufacturers, managed by the Census Bureau of Economics; or National Establishment Time-Series (NETS)). Note, though, that this noise biases the coefficients of the variables of interest downward and reduces the chances of finding a significant effect. If anything, an imprecise classification of Texas versus non-Texas firms reduces the chances of finding a significant result between the two groups.

Finally, there may be a concern that firms exposed to Texas might reduce their economic activity there after *TC Heartland* in order to reduce their legal exposure to Texas. We believe that this channel does not affect the interpretation of our results. First, a firm's decision to have an economic presence in Texas may be driven by other potential benefits, which might still outweigh the higher costs of litigation and therefore still prompt these firms to stay in Texas. Second, even if firm relocation is significant, it will likely bias the results downwards: If firms with economic ties to Texas reduce their exposure after *TC Heartland*, they are still considered exposed to Texas in our analysis, reducing the difference between Texas-exposed and control firms in the post-event window. We directly examine the argument of geographic relocation in section 7 of the paper. Another concern is that a firm may change its state exposure between 2015 and 2016. To ensure robustness, we re-define state exposure as of 2014 - the last year before the beginning of our sample period. The results, unreported for the sake of brevity, remain consistent.

1.3.2 Innovation Variables

To measure the innovation output of firms, we use Kogan, Papanikolaou, Seru and Stoffman (2017) (KPSS) patent data. The data contains information regarding the application and grant date of patents. KPSS is especially useful in studying corporate innovation because it employs a disambiguation method that matches patents to their owners through PERMCO identifier. As a result, the link between patents and public firms that have developed them is straightforward. KPSS patent data includes information on all the patents applied for between 1926 and 2019. This gives us two years of patenting data after the TC Heartland v Kraft Foods Supreme Court ruling.

Perhaps the most significant contribution of the KPSS database is in providing a method to estimate a dollar value of each patent by measuring market response to the news of a patent grant.[6] The idea is that the event of a patent grant by USPTO releases information about the patent to the market, which is immediately incorporated in stock prices. To extract the dollar value of a patent, KPSS calculate the cumulative abnormal return during a three-day announcement window (t to t+2 where t is the day that the patent is granted), and multiply it by the total value of the firm. To validate their measure, KPSS show that their estimate of patent dollar value is a strong predictor of patent scientific value, as well as firm growth in capital, employment and profit.

Based on the KPSS information, we construct our main independent variables. We start by

[6]Since KPSS measures market reaction around the date of patent grant, and some patents take 2-3 years to get approved, one may raise a concern that market reaction information suffers from truncation bias, as there may still be limited market reaction data on patents applied for in the later years of the sample. Fortunately, since its initial release, the KPSS database has been updated several times, and market reaction to patents applied for prior to (in) 2019 and granted after 2019 has been populated. As a result, our market reaction variable is to a large extent immune to the problems of truncation. In this work we use the version of the database posted in June 2021.

counting the number of patents a firm applied for in a given year, $\#Patents_{it}$. Next, we convert this variable into natural logarithm (a value of one is added before the conversion) and denote it $log(\#Patents_{it})$. We use patent dollar value to construct an alternative measure of innovation. $log(ValueTotal_{it})$ is the natural logarithm of one plus the total dollar value of all patents that firm i has applied for in year t. It captures the total dollar increase in firm value created by all patented innovation produced in a given year.

Using Compustat, we also capture investment in innovation by looking at the value of research and development from Compustat scaled by the value of assets (missing values are replaced with zero).

1.3.3 Control Variables

We also use Compustat to construct conventional control variables used in innovation literature. Specifically, we add the natural logarithm of assets to control for firm size. We also control for firm profitability using return on assets (ROA) and cash scaled by assets ($Cash$). Profitability determines internal financing available to finance innovation (Brown et al. (2013)), whereas high levels of cash reserves relax financial constraints and stimulate innovation. We also control for leverage measured as long-term debt over assets as another proxy of financial constraints (Brown et al (2013)). We further augment our regression by adding Tobin's Q as a measure of investment opportunities, as well and property, plant and equipment scaled by assets (PPE) to proxy for the share of tangible assets.

1.3.4 Final Sample

To construct the final sample, we start with the entire CRSP-Compustat universe and limit the sample to firms that have at least one million dollars worth of total assets. We also remove firms in financial industry (SIC industry codes between 6000 and 6999) and utilities (SIC industry codes between 4900 and 4949). Our sample period spans 2015 through 2019, which is the last year of the

KPSS database with a reliable number of observations. We exclude 2017 because the ruling has taken place in the middle of the year - May 2017, so it is unclear whether 2017 should be classified as part of the pre- or post-ruling period. This procedure gives us four years of data in total: two years before and two years after the event. Since some firms never patent, we further limit the sample to firms that applied for at least one patent in the five-year period between 2012 and 2016. As a result, our final sample, which is limited to innovative firms only, consists of 3,694 unique firm-year observations.

Table 1.1 describes the summary statistics for the main variables of interest. The first two rows present the distribution of the main independent variables. $TexasExposed$ variable indicates that more than 9% of the firm-year observations are exposed to the state of Texas. The average value is much higher for $DelawareExposed$: around 70% of the sample firms are exposed to Delaware in a given year. Recall that such a high fraction is driven by the prevalence of Delaware-incorporated firms. Both variables $\#Patents_{it}$ and $TotalValue_{it}$ are quite dispersed and positively skewed, justifying employing a logarithm transformation in the regression setting. When we consider the sub-sample of patent-active years only (Panel B), we find that the median value for $\#Patents_{it}$ is 6 while the P75 threshold is 22. The median value for $TotalValue_{it}$ is around $43 million, while P75 threshold is more than $303 million.

[Table 1.1]

1.4 Main Results

1.4.1 Total Innovation Output

In this section we examine how the Supreme Court ruling affects patenting activity using a difference-in-differences framework. Specifically, we ask whether following the ruling, innovation activity of Texas-exposed firms has declined compared to the rest of the sample. To this end, we define an indicator variable $Post_t$ that takes on a value of one for years 2018–2019, and zero otherwise. This variable captures the overall impact of the ruling. Our main variable of interest is the interaction of $Post$ with Texas exposure variable, $TexasExposed$.

If the ruling has reduced Texas firms' incentive to innovate, we should observe a reduction in their number of patent applications relative to the rest of the sample. Our main difference-in-differences regression is at a firm-year level and it takes on the following form:

$$y_{i,j,t} = \beta_1 * Post_t * TexasExposed_i + \beta_2 * Post_t * DelawareExposed_i + \gamma * X_{i,t-1} + \alpha_i + \alpha_{j,t} + \epsilon_{i,t},$$

where i refers to a firm, t indexes time and j denotes two-digit SIC industry classification. $y_{i,j,t}$ is the dependent variable that measures different aspects of innovation for firm i in year t. We add firm-fixed effects (α_i) to control for time-invariant heterogeneity in innovation across firms and industry-year fixed effects ($\alpha_{j,t}$) to control for time-varying industry shocks that could impact innovation. We further augment the empirical specification with a time-varying vector of lagged controls ($X_{i,t-1}$). Standard errors are clustered at a state dependency level (variable *Dependency*).

For robustness, we also use three additional specifications with fewer controls: One model without the vector of time-varying firm characteristics ($X_{i,t-1}$) to ensure that the results are not sensitive to the choice of control variables; another model in which industry-year fixed effects are replaced with year-fixed effects (α_t); and the most parsimonious model that only includes the exposure variables, as well as firm- and year-fixed effects.

[Table 1.2]

We first examine the effect of *TC Heartland* on firm innovation output. The dependent variable is the natural logarithm of one plus the number of new patent applications filed. The results are reported in Table 1.2 Panel A. All the specifications indicate a negative and significant effect of *TC Heartland* on the innovation output of firms related to Texas. The magnitude of the ruling effect ranges from -0.134 in the first column to -0.178 in the fourth column. Since the average log innovation is 3.4 , these magnitudes translate to a 13% to 17% decrease relative to the average level of innovation. Thus, the results show that *TC Heartland* has both statistically and economically significant effect on the innovation output of affected firms.

Since the previous approach focuses on the effect of *TC Heartland* on the overall innovation output, we also estimate the overall effect of *TC Heartland* within the subset of firms that did not stop innovating following the ruling. To gauge the effect of the ruling on the intensive margins, we limit our sample to only firm-year observations with at least one patent applied for, and re-estimate the main regression. The results are reported in Panel B of Table 1.2. The coefficient estimates are negative and significant, and their magnitudes are similar to the results of Panel A: Coefficient estimates for $TexasExposed$ vary from -0.14 in column (8) to -0.23 in column (5). Thus, the results of both panels show a significant drop in the innovation output of Texas firms after the ruling.

After establishing the baseline results using patent count as the proxy for innovation, we repeat the analysis using the dollar value of patents. It is possible that the quality of the patents or the incentives of firms to patent new inventions have changed following the ruling. Since there is a substantial heterogeneity in the ability of patents to enhance firm growth and profitability, the number of patents may not precisely capture the economic change in the innovation output.

To this end, we use the natural logarithm of the total dollar value of all patents $log(TotalValue_{i,t})$. If the scope of innovation output of Texas-exposed firms has become economically smaller, we should be able to establish the negative effect of the ruling on the total dollar value of innovation.

[Table 1.3]

Panel A reports the results for the overall sample of firms. In line with the expectations, the dollar value of the portfolio of newly applied patents dramatically drops among Texas-exposed firms compared to their non-Texas peers. Estimated coefficients for $TexasExposed$ range from -0.162 to -0.201 across specifications and all are negative and statistically significant. Based on the specification with most extensive vector of control variables and fixed effects (column 4), *TC Heartland* ruling has led to a 18 percent drop in the dollar value of innovation compared to the average for firms related to Texas. Panel B is limited to a subsample of firms that produce at least one patent in a given year. We find that the impact is similar in magnitude and statistically significant when focusing on intensive margins. Taken together, the comparison between the two panels suggests that firms where patenting play a central role cannot easily refocus their business activity in non-innovative areas, thereby fully bearing the brunt of the ruling.

Interestingly, we do not find a consistent and statistically significant impact of the ruling on innovation activity of Delaware-exposed firms in either of the two tables. This non-result is in line with the strand of arguments in the legal literature that posit that the relative strength of Delaware court lies in its objective and unbiased attitudes in litigation process. In this case, Delaware firms did not become better off following the ruling compared to the rest of non-Texas firms.

1.4.2 Average Patent Quality

In this subsection we extend the previous analysis by examining the change in the average dollar value of innovation following the Supreme Court ruling.

We argue that in this case, the effect is a-priory unclear, as the average value is affected by two forces working in the opposite directions. The first force is based on our central argument that, keeping all else constant, an increase in expected litigation costs should reduce the dollar value of a patent. Yet, Texas firms can also change the nature of their R&D projects. Therefore, the second force is based on the idea that in response to an increase in expected litigation costs, projects of low financial value that were value-enhancing prior to the ruling may no longer be profitable and will be abandoned. According to Cohen et al. (2019), projects of lower quality are also subject to a higher risk of litigation compared to projects of higher quality. Therefore, if Texas firms become more selective following the ruling and increase the average quality of their patents improves, this channel will mitigate the impact of the first one.

[Table 1.4]

The results, presented in Table 1.4, show that the effect of the ruling on Texas firms' average patent value is overall negative. In the most restrictive specification, the estimated coefficient for $TexasExposed$ is around -0.03. However, most of the coefficients are statistically insignificant.

The results are in line with the argument that in order to mitigate the impact of the ruling, firms drop the least profitable projects. Combined with the previous findings, these results suggest that following the ruling, Texas firms reduce the overall scope of innovation by dropping the least profitable projects.

We note that another commonly used method to measure patent quality is the number of citations a patent has received. However, this method is not suitable in our empirical setting. Although patents can be cited by others already at the patent application stage, they typically receive a more significant scientific exposure after being granted. Since our sample period ends in 2019 and an average patent is granted 2-3 years after the original filing, many of the patents filed during the post-ruling period were granted in 2020-2021, and have not been around long enough to accumulate citations in a way that could be considered reliable. We therefore limit the analysis of this subsection to the dollar value of patents.

The truncation issue, mentioned above, may also raise concerns that our baseline results where we estimate the number and value of new patents also suffer from truncation issues. Since KPSS database only includes patents after they were granted, some of the patent applications filed during the post-ruling period may not yet appear in the database. Note that this issue is largely mitigated by our difference-in-differences setting, where the impact of the ruling on Texas firms is evaluated relative to its impact on the rest of the sample. To the extent that the patent truncation problem affects Texas and non-Texas firms similarly, our empirical results have no bias. The remaining concern is that Texas firms could potentially specialize in a different type of patented innovation from the rest of the sample, and it may take these patents longer to get approved, even after controlling for industry-fixed effects. To address this possibility, we limit our entire sample only to patents that were granted within a two-year period from the filing date, and replicate the analysis, reported in Tables 1.2 and 1.3. The results are presented in Appendix. Although the statistical significance is somewhat weaker, we still find a statistically significant impact of the ruling on the scope and value of innovation activity. Note also the the measure of innovation input - R&D - is obtained from Compustat and is free of truncation biases. We focus on its analysis in the next subsection.

Finally, we show that our results are robust to alternative definitions of patent count. Cohn et al (2022) show that log plus one transformation produces estimates with no natural interpretation that can have the wrong sign in expectation. Using Poisson estimates suggested by Cohn etl al (2022), we show that results are robust when we replace $log(\#patents + 1)$ with direct values of $\#patents$.

1.4.3 Research and Development

After establishing the effect of the ruling on innovation output, we turn to examining innovation input and ask whether the investment in innovation activity has declined. If Texas courts impose higher costs of litigation on firms under their jurisdiction, new developments may become less profitable. As a result, Texas firms will initiate fewer innovation projects, which will require less investment in research and development. To examine the change in innovation input, we look at the research and development expenses of firms, using the *R&D* variable.

[Table 1.5]

The results of the regression are consistent with the expectations. In all specifications, except column (2), the coefficient estimates of $TexasExposed$ are negative and statistically and economically significant. For instance, based on the specification in column (4), *TC Heartland* leads to a 10% drop (-0.013/0.13) in R&D investments compared to the average levels of R&D. Overall, we find that after *TC Heartland* Texas firms do not invest in innovation as much as did before the ruling, which, in turn, is consistent with a drop in their innovation output.

1.5 Alternative Explanations and Endogeneity

In this section we address alternative explanations and endogeneity concerns. Perhaps the most serious critic of our empirical setting is that the ruling has affected only Texas firms but not other states. As a result, lower innovation activity of Texas firms could be driven by some Texas-specific events that have taken place around the ruling. To address this argument, as well as a number of other endogeneity concerns, in this section we perform a number of additional tests.

1.5.1 Patent Value in the Narrow Window Around the Ruling

Our first method to validate the impact of the ruling on the expected cost of litigation as the central mechanism at work is patent-level analysis. To this end, we compare the dollar value of patents that were granted a few months after as opposed to a few months before the ruling. The idea is as follows. On average, it takes a filed patent 2-3 years to get approved. Therefore, the patents approved around the Supreme Court ruling were developed long before this event, and the investment in its development could not be affected by the ruling or even the anticipation of its outcome. Moreover, patents approved by the USPTO office around the same time were likely developed by the firm at the same time, and therefore, should not systematically differ in their scientific quality and other characteristics. As a result, any differential effect on patent value right after as opposed to right before of the ruling's passage can be attributed to a change in the expected litigation costs following *TC Heartland*. To establish the litigation channel as the key mechanism at work, we test whether investors discount the newly granted patents of Texas firms relative to patents of non-Texas firms.

The methodology is as follows. We start by identifying all patents within our sample that were granted m months before or m months after the ruling. For robustness, we consider different window widths, so that $m \in \{3, 4, 6\}$. We further exclude patents that were granted one week before or after the ruling to ensure that the stock market response to the news of patent approval are not driven by abnormal stock price fluctuations in response to the *TC Heartland* announcement. We then estimate the dollar value of each patent as a function of $TexasExposed * Post$ variable, as well as firm-fixed effects.[7] To further control for the nature of the firm's patents approved right before

[7] Since the event window falls within the same calendar year in all the definitions of window width, we do not include time-varying control variables and time-fixed effects.

versus right after the ruling, in an alternative specification we also include technological class-fixed effects. To this end, we use USPTO's Cooperative Patent Classification (CPC) and assign a dummy variable to each class, as defined by the first letter of classification scheme. There are nine distinct first-letter classes in total, but they are not mutually exclusive, and patents can belong to several technology classes at the same time. Therefore, technology-fixed effects also capture the breadth of the patented invention, absorbing heterogeneity where some patents can span several classes, whereas others are narrowly focused in one category. In this setting, a change in the patent value following the ruling can be solely attributed to changes in the expected cost of litigation that the newly patented product may trigger in the future.

[Table 1.6]

Table 1.6 summarizes the results. We find a significant drop in the market value of the patents developed by Texas firms right after the ruling's passage. This finding is consistent with the prediction that the value of intellectual property of Texas firms has deteriorated after the ruling, as now it is less costly to litigate Texas firms relative to the rest of the sample. We also find that the value of patents that belong to Delaware firms has increased, but the result is statistically insignificant.

Note that focusing on a narrow window of several months around the May 2017 Supreme Court decision reduces the probability of other Texas-specific events happening around the same time. Even if such event were to occur within this narrow time window, it has to be related to innovation. Moreover, it must affect the value of previously developed innovation, and not the scope of current research and development.

Lerner and Seru (2022) point out that differences in patent composition across states can introduce biases in innovation studies. In particular, they show that researchers need to account for differences in propensity across technology classes, states and industries to produce patents. Imagine this scenario, after the ruling

Finally, the narrow window test also helps reinforce the conclusion we drew in section 4.2 of the paper, where we analyzed the average value of patents over our full sample period. Recall that in the full-sample-period test we found a negative, but insignificant result of the ruling, as potential changes in product mix towards higher quality patents could have mitigated the litigation channel. The empirical setting of this section allows us to separate these two forces and establish the impact of expected cost of litigation on the dollar value of patents in a precise manner.

1.5.2 CAR Analysis

Our second method of establishing the impact of *TC Heartland* as the mechanism at work in a plausibly causal way is based on event study methodology. In this subsection, we look at stock return of Texas versus non-Texas firms using cumulative abnormal return (CAR) analysis. If the ruling reduces the dollar value of future innovation for Texas firms, market participants should immediately incorporate this effect in stock prices, which will lead to a negative cumulative return of Texas firms following the announcement.

We look at the 6-day event window starting from 22 May 2017 (the day of the ruling announcement) and calculate CAR of each firm. As an additional hypothesis, we want to ensure that the impact of the ruling was largely pronounced among innovative firms. Therefore, in this test we do not limit the sample to firms with prior innovation activity, and consider the entire CRSP-Compustat universe, as described in Section 3.4.

[Table 1.7]

The first column of Table 1.7 shows the baseline specification, which includes only exposure to Texas and Delaware as explanatory variables. Texas firms experience negative abnormal return of 3.2% following *TC Heartland* announcement, and the coefficient is statistically significant. At the same time, we do not find a significant impact of the ruling on the rest of the sample firms, as indicated by a small and insignificant intercept. We also do not find a significant stock market reaction among Delaware firms. In specification presented in column 2, we enrich the regression with industry-fixed effects, which absorb some of the negative response by Texas firms. Yet, the impact of the ruling is still negative and statistically significant. In the last two columns of the table we include additional control variables and also examine a potentially differential response by innovating versus non-innovating firms, as proxied by R&D and patenting activity prior to the ruling. The results of column 3 show that the negative impact of the ruling is particularly pronounced among Texas firms with high levels of research and development. We obtain similar, albeit less statistically significant, magnitudes when classifying innovating firms based on a dummy variable that takes on a value of one for firms that have applied for at least one patent in the 5-year window prior to the ruling (column 4)[3].

Figure 1.4 further focuses on the sample of patenting firms and illustrates the evolution of cumulative abnormal return for Texas and non-Texas firms in a univariate setting. The figure shows that the cumulative abnormal return of Texas firms has been gradually declining during the six-day period. At the same time, the trend for non-Texas firms has essentially remained flat.

[Figure 1.4]

Taken together, the results of the CAR analysis confirm that the ruling has mainly affected the market value of Texas firms, and the impact was especially strong among firms-innovators. These results are also aligned with our previous findings, which point to a reduction in the overall value of innovation following the ruling as the channel at work. Furthermore, the CAR analysis helps alleviate concerns that the *TC Heartland* ruling could be anticipated by market participants ahead of time.

1.5.3 Parallel Trends

Finally, we perform a parallel trend analysis to further ensure that the ruling was not anticipated by market participants. To this end, we go back to our baseline panel data regressions and study the time-series dynamics of the effect of *TC Heartland*. In particular, we repeat the estimation of innovation input and output after replacing the variable *Post* with a vector of indicator variables

[3] We get similar results when we use abnormal return based on Market model instead of market adjusted return.

for each calendar year within our sample window. We exclude y_{2015} so that it serves as the reference point for other year coefficients.

[insert Table 1.8 here]

The results are reported in Table 1.8. There is an insignificant relationship between *TC Heartland* and indicator variable y_{2016} in all specifications except column 4. This pattern is consistent with the assumption of parallel trends, and suggests that the variable *Post* is unlikely to capture another trend that might have started among Texas firms prior to the ruling. We also find that the joint impact of the 2018 dummy variable and $TexasExposure$ is negative and statistically significant in all specifications. This lends additional support to the argument that the ruling had an economically significant effect on various aspects of firm innovation. Finally, the coefficients on 2019 are statistically significant and similar in magnitude to those obtained for 2018 within the overall sample of firms. At the same time, the effect of 2019 within the sample of patent-producing firms (intensive margins) is lower in magnitude and statistical insignificant (columns 2 and 4). This result could be potentially attributed to a decision of firms with high innovation intensity to reduce their exposure to Texas by moving the hubs outside of Texas (we explore this channel in more details in section 7). As a result, these firms may be more affected by the ruling in the short run (year 2018), but mitigate some of this effect by relocating their R&D activity away from Texas in the following year. At the same time, the best response of firms with low levels innovation activity could be to stop patenting altogether. Therefore, the effect of the ruling on innovation of these firms could be smaller in magnitude, but more time-persistent. Similar to the baseline analysis, we do not find a significant impact of ruling on Delaware firms.

1.6 Cross-Sectional Analysis

1.6.1 The Impact of TC Heartland *on Prospective Defendants*

The previous section has focused on ruling out alternative explanations. In this subsection, we try to strengthen our main argument by gauging the expected litigation costs channel as the underlying mechanism at work. To this end, we exploit cross-sectional variation in our panel data. We zoom in on the sub-sample of firms that are likely to become targets of patent troll lawsuits. The ruling has limited the ability of trolls to file opportunistic lawsuits against non-Texas firms, and has potentially shifted their attention to Texas firms. As a result, we should find that the expected costs of litigation have increased the most among those Texas firms that are more likely to become prospective targets of troll lawsuits. Consequently, the scope of innovation activity of Texas prospective defendants should also be more sensitive to the ruling.

To define the sub-sample of prospective defendants, we rely on Cohen et al. (2019), who identify firm characteristics that significantly affect the probability to be litigated by practicing and non-practicing entities. Since the impact of the ruling has largely affected the litigation patterns of patent trolls (Figure 1.3), we focus on the determinants of litigation by non-practicing entities. Cohen et al. (2019) show that this probability is higher among innovation-intensive firms, as well as among firms who are less able to defend themselves against possible lawsuits. Finally, the authors find that patent trolls behave opportunistically by targeting cash-rich firms, from whom they can extract higher rents following a lawsuit.

To perform the cross-sectional analysis, for each litigation-related characteristic, we calculate its median value in our main sample as of 2016 and split the sample into two groups: low (the characteristic value is smaller than median) and high value (higher or equal median). To capture innovation intensity, we use the ratio of R&D to assets. To capture firms' ability to defend itself, we look at size, as measured by asset value, as well as Tobin's Q. Cohen et al. (2019) find that smaller firms, as well as firms with high growth opportunities are more likely to be targeted by patent trolls. We also split the sample based on the level of cash reserves (cash scaled by assets). Note, however, that cash has an ambiguous effect on innovation activity in our setting. On the one side, cash rich firms face higher expected costs of litigation, which potentially reduce the value of innovation. On the other side, cash reserves help alleviate financial constraints and mitigate underinvestment problems, spurring innovation.

[Table 1.9]

The results are presented in Table 1.9. The first panel shows that the impact of $TexasExposed * Post$ interaction variable on the scope of innovation is negative and statistically significant among firms with high levels of R&D activity (Panel A). The results are even stronger and more consistent when measuring litigation vulnerability based on Tobin's Q: We find that Texas firms with high growth opportunities reduce the scope of innovation the most following the ruling. Tobin's Q could also be viewed as another proxy for the scope of innovation, as relatively high ratio of market to book assets can be explained by intellectual property presenting a sizeable component of the firm's overall assets. In Panel C we also split the sample based on asset size with the idea that smaller, and therefore, potentially weaker, firms are less able to weather increased litigation costs. The results support this notion. We find that small Texas firms are more likely to reduce their R&D investment following the ruling, and the total dollar value of their patents drops by more. Finally, we split the sample based on cash reserves, but find mixed pattern (Panel D). As indicated earlier, it is possible that high levels of cash do not only attract patent trolls, but also help firms better resist their attacks, and also relax financial constraints.

To summarize, the cross-sectional analyses support the notion that Texas firms that are most likely to become potential litigation targets are the ones that have reduced their innovation the most.

1.6.2 The Impact of TC Heartland *on Prospective Plaintiffs*

In this subsection, we discuss the impact of the ruling on plaintiffs. Since *TC Heartland* has strengthened the rights of prospective defendants, it could have simultaneously weakened the rights of prospective plaintiffs. If patent litigation system helps efficiently solve patent-related disputes and protect patent owner from the loss of economic value due to possible infringement, one may argue that plaintiff-friendly attitudes are the ones that promote innovation. We address this argument in several ways.

First, we refer back to Mezzanotti (2021), who uses a 2006 Supreme Court decision that has ended an automatic grant of injunction forcing the defendants to immediately shut down any operation related to the alleged violation. This decision has reduced litigation costs for prospective defendants, but at the same time, made plaintiffs worse off. After the ruling, they lost their strong bargaining position and could not immediately capture the defendant's market share of the products based on the infringed technology. Yet, Mezzanotti (2021) finds that the net impact of the ruling on patenting activity was unambiguously positive. Thus, the results of his work contradict the assumption that stricter patent litigation system helps resolve patent disputes more effectively. Moreover, his findings essentially indicate that the opposite is correct: Strict patent system gives rise to distortions that stifle innovation and prevent knowledge dissemination.

Second, we note that *TC Heartland* decision has primarily affected plaintiffs that are patent trolls - entities that do not develop their own innovation. At the same time, we do not find that the share of cases submitted in Texas courts by practicing entities has changed.

Third, we address the concern that the ruling could have heterogeneous impact on Texas versus non-Texas plaintiffs of practicing entity type, which, in turn, could serve as an alternative explanation of our results. Existing literature demonstrates that prior to the ruling, practicing entities typically filed patent litigation cases in their home state district courts. Following the ruling, they had to rely on district courts of the defendant's home state, so that the loss of convenience of proximity, as well as of familiarity with the home state's legal system and personal connections that could be helpful in the litigation process, could increase plaintiffs' litigation costs. As a result, all plaintiffs have become worse off following the ruling. However, the impact was stronger among Texas plaintiffs, who, in addition to the loss of geographic proximity and connections, also lost the benefit of litigating in plaintiff-friendly courts. If strict patent enforcement is the channel that promotes innovation, it can lead to a drop in innovation activity of Texas firms compared to non-Texas ones.

Although this channel is consistent with our baseline findings, it is an unlikely driver of all of our results. Consider, for example, market response to the ruling announcement. If plaintiff-based channel were the dominant one, we would expect to find a negative and significant returns among all patenting firms, with an additional negative effect among Texas firms. Yet, Figure 1.4 demonstrates that market reaction of non-Texas firms was essentially flat. The cross-sectional results, discussed in the previous subsection, also cast doubt on this explanation. We find that the impact of the ruling on Texas firms' innovation is primarily pronounced when these firms are likely to be defendants. Finally, the effect of the ruling on the geography of innovation, presented in the next subsection, further supports the defendant- rather than plaintiff-based mechanism. We find that following the ruling, firms are more likely to reduce their innovation activity in Texas. Note that the plaintiff-based channel does not predict any changes in the geographic boundaries of the firms. Thus, this result provides further evidence in support of the defendant-based channel: Firms protect themselves from possible litigation lawsuits by relocating research hubs outside of Texas.

1.7 The Geography of Innovation

In this section we extend the analysis of district court attitudes beyond the scope and value of innovation and ask whether these attitudes also shape geographic boundaries of firms.

If Texas-based business activity leads to a litigation-driven disadvantage following *TC Heartland*, it is possible that some firms will move their operations out of Texas in order to minimize it. Note that the second provision of the Supreme Court's decision allows a patentee to sue a defendant in a jurisdiction "where the defendant committed an act of infringement and has a regular and established place of business". As a result, both Texas and non-Texas exposed firms may choose to relocate some of their Texas facilities (e.g., plants, stores, research centers). We focus our analysis on research hubs. If a firm uses its Texas lab to develop an invention that infringes on patent rights of another entity, that entity is still able to sue in Texas, even if the alleged infringer does not have other significant economic exposure to Texas.

We rely on USPTO dataset of inventors' address to establish the location of the research hub in which the patented inventions were developed. Firms provide the address of inventors as part of their patent application filing, and we use the address of the first inventor. Note that the methodology of using inventors' location as the patented invention's place of origin is widely accepted in the literature (see, for example, Moretti (2021), Carlino et al. (2012), Autor et al. (2020)). For every firm in a given year we divide the number of Texas-originated patents by the total number of patents it has applied for and denote the resulting ratio *TexasShare*. We then estimate the fraction of Texas-originated patents as a function of *Post* indicator variable and the same vector of control variables as in the baseline regressions (note that since we do not interact *Post* with state dummies, as in baseline regression, we cannot use time fixed effects in this specification). Our results, presented in columns 1 and 2 of Table 1.10, demonstrate that following the Supreme Court's decision, firms' innovation drops in Texas compared to their innovation activity in other states. The results are not economically sizable, as the ruling leads to relocation of about 1.5% of the firms' overall patent volume. Nevertheless, these findings are consistent with the idea that in order to reduce their chances of litigation in Texas, innovative firms choose to relocate a portion of their research activity outside of Texas.

[Table 1.10]

To further examine whether firms redraw their geographic boundaries in order to reduce the chances of being litigated in Texas, we ask if, following the ruling, firms are less likely to choose Texas as their main business activity state. Specifically, we estimate a regression whether the dependent variable takes on a value of one if a firm is Texas-dependent in year t, and zero otherwise (see Section 3.1 for the details of *Dependency* construction). We also examine whether a firm is likely to be headquartered or incorporated in Texas in a given year. Since we include firm-fixed effects in all regressions, we estimate the regressions using the OLS model.

The results, presented in columns 3-4 of Table 1.10, do not point to a significant impact of the ruling on the choice of state served as the center of major economic activity. It is possible that cost-benefit analysis is at play: Relocating a research hub is likely less costly than moving a major business activity center, which location is dictated by other considerations, such as proximity to customers or suppliers, availability of skilled human capital, etc. We also do not find that firms are more likely to relocate their headquarters or states of incorporation following the ruling. Similar cost-benefits considerations may be at play here. Note also that headquarters and states of incorporation are the most time-persistent variables overall in this analysis. High values of adjusted R-square along with statistical insignificance of all time-variant independent variables indicate that the variation in the dependent variable is almost entirely absorbed by firm-fixed effects. This is another evidence that supports the idea that relocation of headquarters and states of incorporation is very costly, and is therefore rarely implemented.

Taken together, the results of this section provide further evidence of real economic implications that district court biases can generate. Although firms do not make drastic changes in their geographic boundaries following the ruling, we find a detectable impact on the geography of innovation by demonstrating that firms downsize or close their Texas research hubs following the ruling.

1.8 Conclusion

District courts have seen a large rise in the number of exploitative patent litigation cases. Legislators made certain attempts to curb the problem of meritless patent lawsuits, however, legal scholars point out that district courts might be another source of the problem. Some U.S. courts host a disproportionately large number of patent cases, and are also known for their pro-plaintiff jury and court laws. It has long been suspected by academics, practitioners, and lawmakers that Texas district courts are famous for such practices. Eastern and Western Districts are two most famous ones, hosting a considerable number of patent cases.

On May 22, 2017 the Supreme Court issued its ruling on the *TC Heartland v Kraft Foods* case stating that plaintiffs can only file a patent litigation case in the district of a state where the defendant has a regular and established business. Exploiting this ruling, we study whether district courts' heterogeneous attitude towards defendants and plaintiffs can influence corporate innovation. Overall, the ruling has increased the expected litigation costs for Texas firms, which have become more profitable lawsuit targets for patent trolls relative to non-Texas firms. At the same time, the expected costs of litigation for non-Texas firms have decreased, as, following the ruling, patent trolls could no longer litigate alleged patent infringers in plaintiff-friendly Texas courts. Consequently, we hypothesize that the ruling reduces the scope and value of innovation of Texas firms relative to non-Texas ones.

Our results support this hypothesis. After the ruling, firms with economic exposure to Texas reduce the scope of their innovation output, as measured by the number of patents they produce, compared to non-Texas firms. The total dollar value of innovation also declined. Further, we document a sizable drop in investment in research and development. Finally, we detect a statistically significant stock market reaction to the ruling event: Texas-exposed firms experience a negative and statistically significant abnormal return of 1%–3% in response to the outcome announcement, and the negative effect is particularly pronounced among firms with high innovation activity.

After documenting the overall effect of the ruling on innovation, we perform additional tests to establish expected costs of litigation as the channel at work. To this end, we show that the impact of the ruling is primarily pronounced among firms that are more likely to become litigation targets in patent troll lawsuits. We also find that controlling for firm and patent characteristics, patents that were granted right after the ruling have received lower market value compared to patents granted right before the ruling. This finding helps address a number of endogeneity concerns and also points to expected costs of litigation as the underlying mechanism.

Our results contribute new evidence to the debate on the impartiality and the effect of legal systems on firm activities and innovation in particular. This paper supports the view that certain characteristics of the legal system may deter innovation activity.

Figure 1.1: Patent Litigation Cases Filed, by State

The figure depicts the share of patent litigation cases filed in Texas [Delaware; California] district courts out of all U.S. patent litigation cases filed in year 2016, by plaintiff type. The filing date, the location of litigation case venue, and the type of patent plaintiff are obtained from Stanford Non-Practicing Entity (NPE) Litigation Database. Patent trolls (Patent Assertion Entities) are classified as plaintiffs that fall into Category 1 (acquired patents), Category 4 (corporate heritage), or Category 5 (individual-inventor started company). Plaintiffs in all other categories are classified as practicing entities.

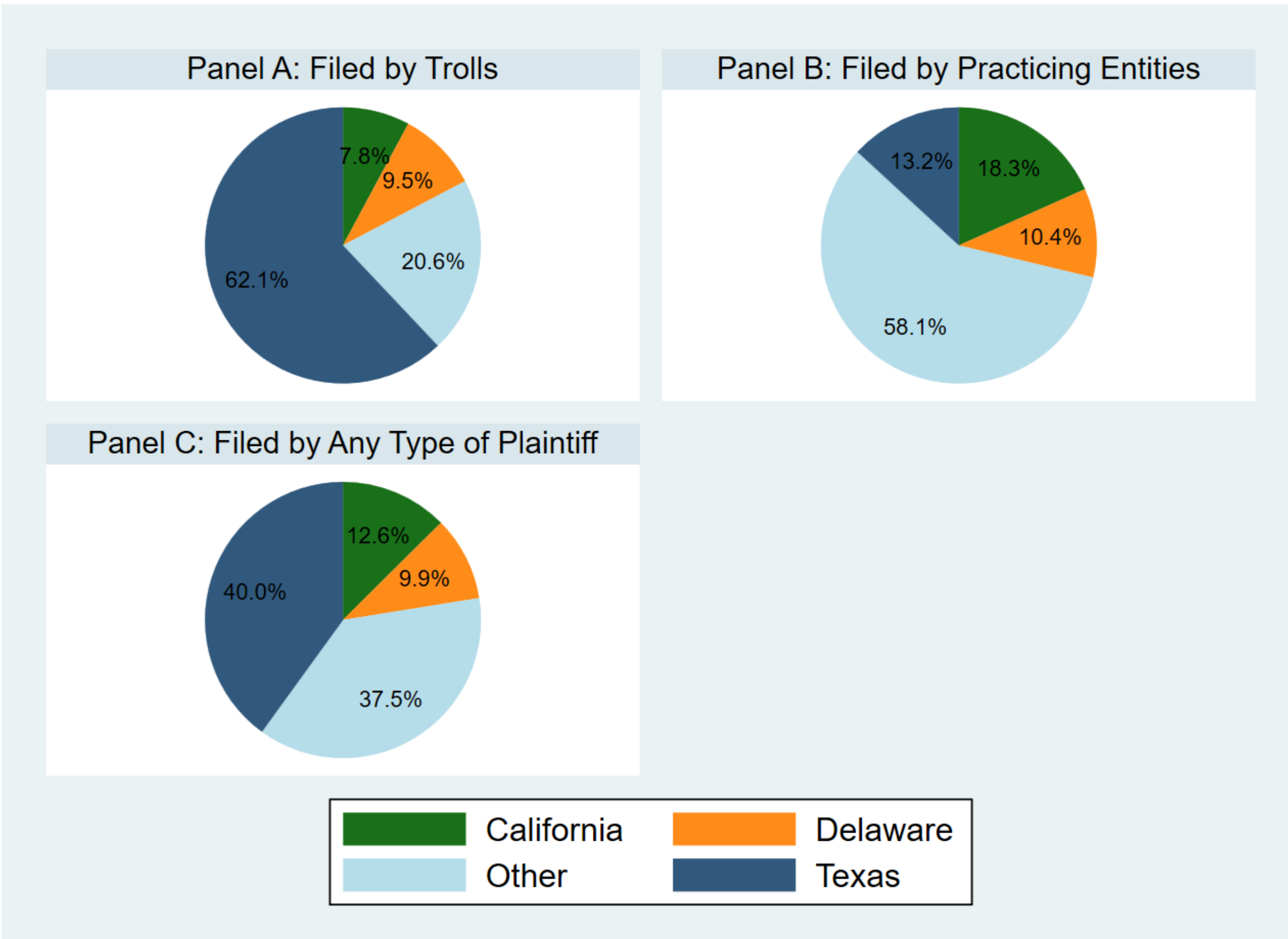


Figure 1.2: Patent Applications, by Patent's State of Origin

This pie figure depicts the distribution of patent application filings by patent state of origin, as of 2016. Patent information is extracted from Kogan et al. (2017) and matched to inventors' location using USPTO's recorded residential address of inventors. Patent's state of origin is defined as the state where the first inventor resides. We calculate the share of patents filed in each state out of all U.S. patents filed in 2016, and rank all states in descending order from the most to the least innovative. The figure depicts the relative share of the four most innovative states.

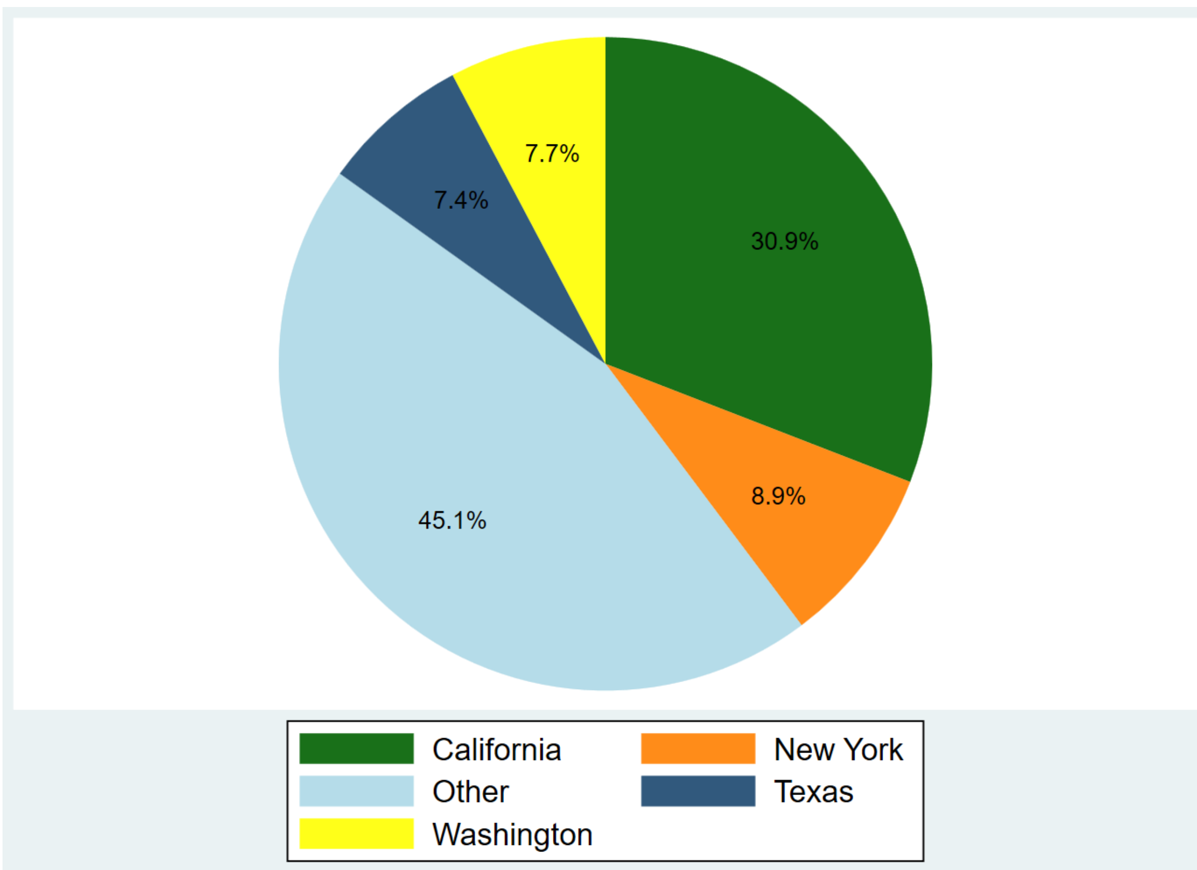
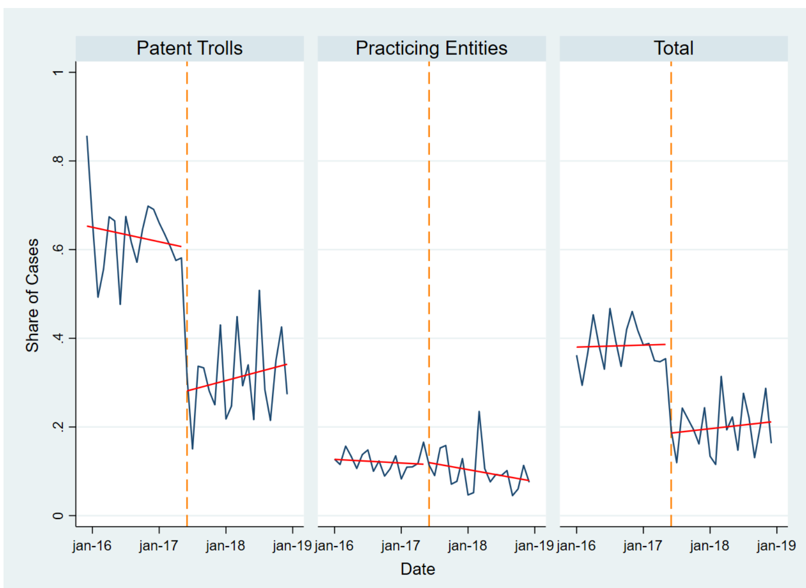


Figure 1.3: The Share of Patent Litigation Cases Filed in Texas and Delaware District Courts around *TC Heartland*

This figure depicts monthly time-series of the share of patent litigation cases filed in Texas (Panel A) and Delaware (Panel B) district courts out of the number of cases filed nationwide, by plaintiff type. The filing date and the location of each litigation case venue are obtained from Stanford Non-Practicing Entity (NPE) Litigation Database. Patent trolls (Patent Assertion Entities) are classified as plaintiffs that fall into Category 1 (acquired patents), Category 4 (corporate heritage), or Category 5 (individual-inventor started company). Plaintiffs in all other categories are classified as practicing entities. The first graph in each panel represents the evolution of patent cases filed by practicing entities. The second graph represents patent cases filed by trolls, and the last graph represents patent cases filed by any type of plaintiff. The time window spans the period from January 2016 to January 2019. The timing of the May 2017 Supreme Court ruling is marked with a vertical yellow dotted line.

Panel A: Texas



Panel B: Delaware

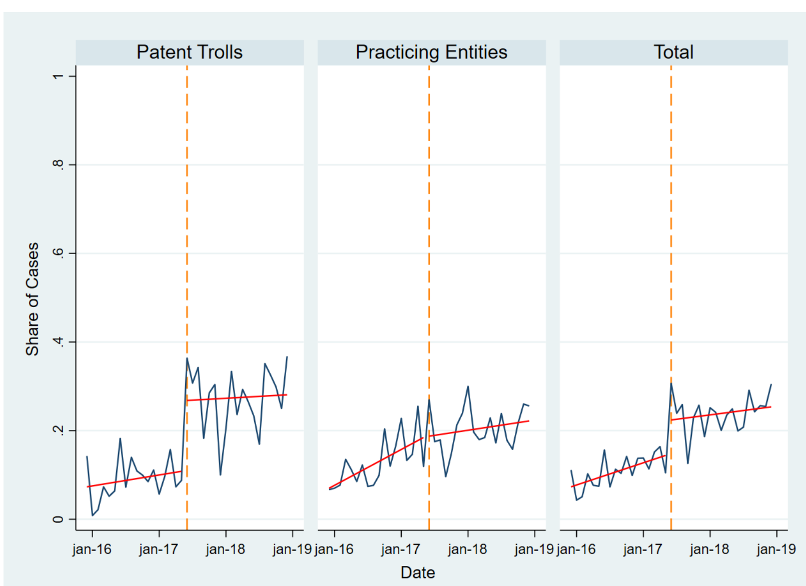


Figure 1.4: The Effect of *TC Heartland* on the Cumulative Abnormal Return of Innovative Firms

This figure presents the cumulative abnormal return of $TexasExposed$ and non-$TexasExposed$ firms (denoted as $Texas$ and $non-Texas$ firms at the bottom of the figure) within a six-day window starting from May 22, 2017 (day 0). See Table 1.1 and Section 3.1 for the definition of $TexasExposed$ and $DelawareExposed$. The sample includes all publicly-traded patenting firms (see Section 3.4 for further details). CAR is cumulative abnormal return, measured as daily stock return net of CRSP value-weighted market returns.

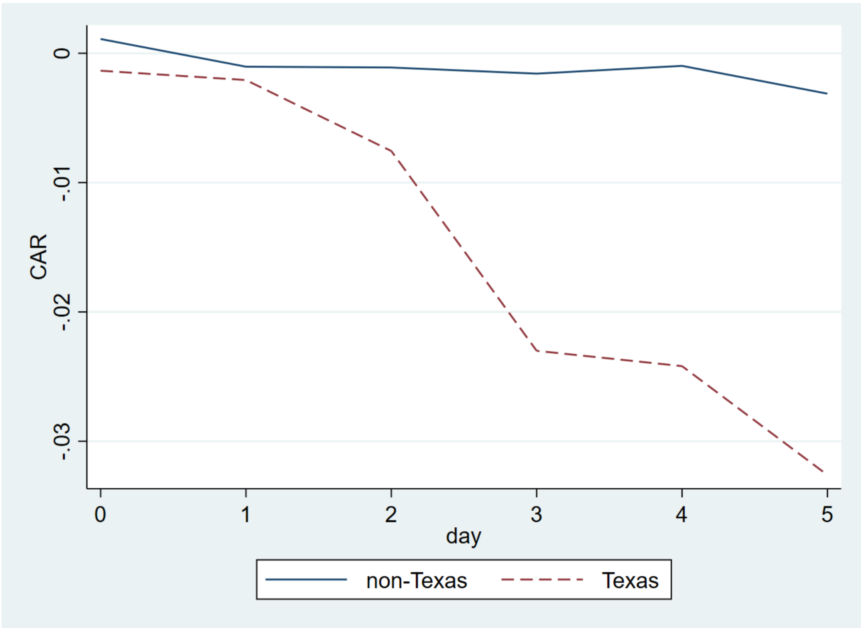


Table 1.1: Summary Statistics

This table reports summary statistics of the variables used in our empirical analysis. The sample period is 2015-2019 (excluding 2017). The sample in Panel A includes all publicly-traded patenting firms (see Section 3.4 for further details). The sample in Panel B is further limited to years in which firms have applied for at least one patent. *TexasExposed* (*DelawareExposed*) is a time-invariant indicator variable that takes a value of one if one of the following is true: the firm is incorporated in Texas (Delaware) in 2016; the firm is headquartered in Texas (Delaware) in 2016; the number of times Texas (Delaware) is mentioned in the firm's 10-k filings for 2016 is the highest among all U.S states. *R&D* is investment in research and development scaled by total value of assets. Missing values are replaced by zero. *Capex* is capital expenditures scaled by total assets. *Log*(*Assets*) is the natural logarithm of total assets. *ROA* is defined as EBITDA over total assets. *Cash* is cash and marketable securities over total assets. *Leverage* is long-term debt plus debt in current liabilities, all divided by total assets. *Tobin's Q* is the ratio of (book value of assets minus common equity plus market value of equity minus deferred taxes and investment tax) over (book value of assets minus 0.1*common equity plus 0.1*market value of equity). *PPE* is property, plant and equipment scaled by total assets. *#Patents* is the number of new patent applications filed by a firm in a given year. If a firm has not applied for a patent in year t, the value is zero. *TotalValue* is the total dollar value, in million, of all new patents that a firm applied for in a given year (extracted from Kogan et al. (2017)). If a firm has not applied for a patent in year t, *TotalValue* is zero. *AverageValue* is the average dollar value, in millions, of all new patents that a firm applied for in a given year, also from Kogan et al. (2017).

	N	Mean	Std Dev	p25	p50	p75
	Panel A: All Patenting Firms					
TexasExposed	3694	0.09	0.28	0.00	0.00	0.00
DelawareExposed	3694	0.70	0.46	0.00	1.00	1.00
R&D	3692	0.13	0.19	0.01	0.06	0.15
Capex	3690	0.03	0.03	0.01	0.02	0.04
Log(Assets)	3692	6.77	2.32	5.09	6.74	8.40
ROA	3690	-0.04	0.35	-0.06	0.09	0.15
Cash	3692	0.29	0.27	0.08	0.20	0.45
Leverage	3692	0.24	0.24	0.02	0.22	0.37
Tobin's Q	3461	2.04	1.03	1.29	1.76	2.54
PPE	3691	0.17	0.17	0.05	0.11	0.22
#Patents	3694	30.24	110.88	0.00	2.00	10.00
TotalValue	3694	560.45	2029.78	0.00	4.71	100.88
	Panel B: All Patenting Firms With at Least One Patent in Year t					
#Patents	2387	50.52	158.81	2.00	6.00	22.00
TotalValue	2387	922.92	2804.17	5.97	42.66	303.93
AverageValue	2387	18.02	32.21	2.06	6.08	18.61

Table 1.2: The Effect of *TC Heartland* on Innovation Output

This table estimates the effect of Supreme Court's TC Heartland vs Kraft Foods decision on innovation output, as measured by the total number of patents. The sample period is 2015–2019 (excluding 2017). The sample in Panel A includes all publicly-traded patenting firms (see Section 3.4 for further details). The sample in Panel B is further limited to years in which firms have applied for at least one patent. The variable *Post* is equal to one for years 2018 and 2019 and zero otherwise. See Table 1.1 and Section 1.3.1 for the definition of $TexasExposed$ and $DelawareExposed$. The dependent variable, $Log(\#patents_{it})$, is the natural logarithm of one plus the number of patent applications filed by firm i in year t. Firm-year observations with no patent applications receive a value of zero in Panel A, and are dropped from the sample in Panel B. See Table 1.1 and Section 1.3.3 for the description of control variables. The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the state level (defined as the state with the maximal *Dependency* value (See Section 3.1 for the definition)). The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Overall Sample				Panel B: Intensive Margin			
TexasExposed*Post	-0.152**	-0.134*	-0.178***	-0.158***	-0.230***	-0.190**	-0.170**	-0.141*
	(0.057)	(0.068)	(0.053)	(0.057)	(0.063)	(0.075)	(0.073)	(0.072)
DelawareExposed*Post	-0.052	-0.030	-0.060	-0.041	0.041	0.068	0.015	0.038
	(0.038)	(0.046)	(0.045)	(0.053)	(0.053)	(0.055)	(0.055)	(0.061)
Log(Assets)		-0.013		-0.007		0.118**		0.113**
		(0.036)		(0.041)		(0.046)		(0.053)
ROA		0.071		0.076		-0.035		-0.035
		(0.076)		(0.085)		(0.109)		(0.090)
Cash		0.162		0.155		0.224		0.198
		(0.208)		(0.217)		(0.359)		(0.357)
Capex		0.076		0.328		-0.401		-1.042
		(0.591)		(0.600)		(0.647)		(0.838)
Leverage		-0.045		-0.050		-0.150		-0.150
		(0.109)		(0.112)		(0.095)		(0.105)
Tobin's Q		-0.000		0.015		-0.022		-0.008
		(0.019)		(0.022)		(0.029)		(0.028)
PPE		0.273		0.254		0.544		0.796*
		(0.305)		(0.302)		(0.409)		(0.442)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	No	No	Yes	Yes	No	No	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
N	3694	3433	3694	3433	2387	2200	2387	2200
adj.R2	0.89	0.89	0.89	0.89	0.90	0.90	0.90	0.90

Table 1.3: The Effect of *TC Heartland* on the Total Dollar Value of Innovation

This table estimates the effect of Supreme Court's TC Heartland vs Kraft Foods decision on the monetary value of innovation output, as measured by the total dollar value of patents. The sample period is 2015-2019 (excluding 2017). The sample in Panel A includes all publicly-traded patenting firms (see Section 3.4 for further details). The sample in Panel B is limited to years in which firms have applied for at least one patent. The variable *Post* is equal to one for years 2018 and 2019 and zero otherwise. See Table 1.1 and Section 1.3.1 for the definition of $TexasExposed$ and $DelawareExposed$. The dependent variable, $Log(\#TotalValue_{it})$, is one plus the total dollar value of all new patents that a firm applied for in a given year (extracted from Kogan et al. (2017)), in natural logs. Firm-year observations with no patent applications receive a value of zero in Panel A, and are dropped from the sample in Panel B. See Table 1.1 and Section 3.3 for the description of control variables. The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the state level (defined as the state with the maximal *Dependency* value (See Section 3.1 for the definition)). The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Overall Sample				Panel B: Intensive Margin			
TexasExposed*Post	-0.201***	-0.190***	-0.173**	-0.162**	-0.277***	-0.225***	-0.193**	-0.169**
	(0.053)	(0.063)	(0.069)	(0.075)	(0.065)	(0.071)	(0.085)	(0.084)
DelawareExposed*Post	-0.085	-0.053	-0.118	-0.078	0.067	0.112	0.032	0.084
	(0.076)	(0.090)	(0.084)	(0.103)	(0.065)	(0.068)	(0.065)	(0.068)
Log(Assets)		-0.090		-0.076		0.151**		0.138**
		(0.067)		(0.074)		(0.061)		(0.064)
ROA		0.239		0.197		0.138		0.127
		(0.145)		(0.163)		(0.133)		(0.125)
Cash		-0.040		-0.024		0.231		0.211
		(0.214)		(0.216)		(0.437)		(0.434)
Capex		0.414		0.494		-0.349		-1.219
		(1.082)		(1.164)		(1.197)		(1.283)
Leverage		-0.018		-0.029		0.133		0.142
		(0.180)		(0.192)		(0.184)		(0.189)
Tobin's Q		0.009		0.007		0.031		0.031
		(0.051)		(0.055)		(0.049)		(0.052)
PPE		-0.029		-0.109		0.683		0.898*
		(0.401)		(0.365)		(0.462)		(0.459)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	No	No	Yes	Yes	No	No	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
N	3694	3433	3694	3433	2387	2200	2387	2200
adj.R2	0.88	0.88	0.88	0.88	0.94	0.94	0.94	0.94

Table 1.4: The Effect of *TC Heartland* on the Average Dollar Value of Patents

This table estimates the effect of TC Heartland vs Kraft Foods Supreme Court's decision on the average dollar value of patents. The sample period is 2015-2019 (excluding 2017). The sample includes all publicly-traded patenting firms (see Section 3.4 for further details), and is further limited to include only years in which firms have applied for at least one patent. The variable *Post* is equal to one for years 2018 and 2019 and zero otherwise. See Table 1.1 and Section 1.3.1 for the definition of $TexasExposed$ and $DelawareExposed$. The dependent variable, $Log(\#AverageValue_{it})$, is the average dollar value of all new patents that a firm applied for in a given year (extracted from Kogan et al. (2017)), in natural logs. See Table 1.1 and Section 1.3.3 for the description of control variables. The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the state level (defined as the state with the maximal *Dependency* value (See Section 3.1 for the definition)). The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
TexasExposed*Post	-0.075***	-0.048	-0.041	-0.034
	(0.023)	(0.033)	(0.041)	(0.041)
DelawareExposed*Post	0.004	0.017	-0.009	0.011
	(0.039)	(0.037)	(0.040)	(0.034)
Log(Assets)		0.068***		0.066***
		(0.023)		(0.023)
ROA		0.069		0.059
		(0.059)		(0.059)
Cash		0.070		0.074
		(0.081)		(0.078)
Capex		0.162		0.005
		(0.774)		(0.722)
Leverage		0.302**		0.315**
		(0.130)		(0.128)
Tobin's Q		0.056**		0.047*
		(0.023)		(0.025)
PPE		0.131		0.090
		(0.288)		(0.292)
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	No	No	Yes	Yes
Year FE	Yes	Yes	No	No
N	2387	2200	2387	2200
adj.R2	0.94	0.94	0.94	0.94

Table 1.5: The Effect of *TC Heartland* on Research and Development

This table estimates the effect of Supreme Court's TC Heartland vs Kraft Foods decision on investment in research and development. The sample period is 2015-2019 (excluding 2017). The sample includes all publicly-traded patenting firms (see Section 3.4 for further details). The variable *Post* is equal to one for years 2018 and 2019 and zero otherwise. See Table 1.1 and Section 1.3.1 for the definition of *TexasExposed* and *DelawareExposed*. The dependent variable is investment in research and development scaled by total value of assets. Missing values are replaced by zero. See Table 1.1 and Section 1.3.3 for the description of control variables. The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the state level (defined as the state with the maximal *Dependency* value (See Section 3.1 for the definition)). The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
TexasExposed*Post	-0.009*	-0.004	-0.015***	-0.013*
	(0.004)	(0.007)	(0.004)	(0.006)
DelawareExposed*Post	0.002	0.003	0.005	0.007
	(0.008)	(0.008)	(0.009)	(0.008)
Log(Assets)		0.028**		0.027**
		(0.011)		(0.012)
ROA		-0.162***		-0.166***
		(0.026)		(0.027)
Cash		0.104***		0.106**
		(0.037)		(0.040)
Capex		0.159		0.177
		(0.106)		(0.124)
Leverage		-0.023		-0.022
		(0.029)		(0.031)
Tobin's Q		-0.008*		-0.010**
		(0.004)		(0.005)
PPE		-0.023		-0.027
		(0.077)		(0.081)
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	No	No	Yes	Yes
Year FE	Yes	Yes	No	No
N	3692	3433	3692	3433
adj.R2	0.76	0.77	0.75	0.76

Table 1.6: The Effect of *TC Heartland* on the Dollar Value of Patents Within a Narrow Window Around the Ruling

This table estimates the effect of Supreme Court's TC Heartland vs Kraft Foods decision on the dollar value of patents granted within a narrow window of m months before or m months after the ruling, where $m \in \{3, 4, 6\}$. The sample includes all patents that were granted to publicly-traded patenting firms (see Section 3.4 for further details) during the time period defined above. The variable *Post* is equal to one if a patent's grant date is May 22, 2017 or later, and zero otherwise. See Table 1.1 and Section 1.3.1 for the definition of $TexasExposed$ and $DelawareExposed$. We use USPTO's CPC technology classification (at the first letter level) to construct technology class-fixed effects. The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the state level (defined as the state with the maximal *Dependency* value (See Section 3.1 for the definition)). The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) 3 months	(2) 3 months	(3) 4 months	(4) 4 months	(5) 6 months	(6) 6 months
TexasExposed*Post	-1.523***	-1.491***	-1.565***	-1.550***	-1.931*	-1.927*
	(0.447)	(0.431)	(0.446)	(0.432)	(0.973)	(0.973)
DelawareExposed*Post	0.726	0.733	0.830	0.832	0.407	0.412
	(0.862)	(0.848)	(0.740)	(0.730)	(0.757)	(0.751)
Post	0.688	0.685	0.301	0.298	0.224	0.217
	(0.601)	(0.596)	(0.533)	(0.528)	(0.595)	(0.591)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Technology Class FE	No	Yes	No	Yes	No	Yes
N	23326	23326	33864	33864	50693	50693
adj.R2	0.750	0.750	0.740	0.740	0.718	0.718

Table 1.7: The Effect of *TC Heartland* on CAR around the Announcement

This table employs an event-study methodology to examine the cumulative abnormal stock returns of publicly-traded firms around the Supreme Court's TC Heartland vs Kraft Foods decision. The sample is constructed as described in Section 3.4, but includes both patenting and non-patenting firms ($Patenting \in \{0, 1\}$). *Patenting* is an indicator variable that is equal to one if a firm has at least one patent application during the five-year period before the ruling, and zero otherwise. CAR is computed by summing up daily abnormal returns over a [0,+5] day window following the Supreme Court ruling announcement of May 22, 2017. Abnormal returns are returns net of value-weighted market index, obtained from CRSP. See Table 1.1 and Section 1.3.1 for the definition of $TexasExposed$ and $DelawareExposed$. See Table 1.1 and Section 1.3.3 for the description of control variables. The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the state level (defined as the state with the maximal *Dependency* value). The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
TexasExposed	-0.032***	-0.011***	-0.010***	-0.012***
	(0.003)	(0.003)	(0.003)	(0.004)
DelawareExposed	-0.001	0.002	0.002	0.004
	(0.003)	(0.002)	(0.003)	(0.004)
TexasExposed*R&D			-0.048***	
			(0.013)	
DelawareExposed*R&D			0.013	
			(0.027)	
R&D			-0.043*	
			(0.023)	
Log(Assets)			-0.000	0.001
			(0.001)	(0.001)
Tobin's Q			0.000	-0.001
			(0.002)	(0.002)
TexasExposed*Patenting				-0.003
				(0.006)
DelawareExposed*Patenting				-0.005
				(0.006)
Patenting				-0.000
				(0.004)
Intercept	-0.003			
	(0.002)			
Industry FE	No	Yes	Yes	Yes
N	1967	1967	1867	1867
adj.R2	0.03	0.11	0.12	0.11

Table 1.8: Dynamic Effect of *TC Heartland* on Innovation

This table estimates the dynamic effect of Supreme Court's TC Heartland vs Kraft Foods decision on proxies of innovation input and output. The sample period is 2015-2019 (excluding 2017). The sample in columns 1, 3, and 5 includes all publicly-traded patenting firms (see Section 3.4 for further details). The sample in columns 2 and 4 is limited to years in which firms have applied for at least one patent, and firm-year observations with no patent applications are dropped. y_{2016} is an indicator variable that equals one for firm-year observations in year 2016, and zero otherwise. We follow the same methodology to construct indicators y_{2018} and y_{2019}. See Table 1.1 and Section 1.3.1 for the definition of $TexasExposed$ and $DelawareExposed$. See Table 1.1 and Section 1.3.2 for the description of dependent variables. All specifications use the same vector of control variables as the baseline specification (see even-numbered columns in Table 1.2). The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the state level (defined as the state with the maximal *Dependency* value (See Section 3.1 for the definition)). The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	$Log(\#Patents)$		$Log(TotalValue)$		R&D
	Overall Sample	Intensive Margin	Overall Sample	Intensive Margin	
TexasExposed* y2016	-0.099	-0.074	-0.125	-0.129**	-0.003
	(0.065)	(0.059)	(0.087)	(0.053)	(0.005)
TexasExposed*y2018	-0.193***	-0.249***	-0.174*	-0.310***	-0.034***
	(0.057)	(0.086)	(0.090)	(0.094)	(0.007)
TexasExposed*y2019	-0.225***	-0.052	-0.282**	-0.097	0.007
	(0.077)	(0.102)	(0.112)	(0.122)	(0.012)
DelawareExposed*y2016	-0.003	-0.073	0.152*	-0.011	0.014
	(0.048)	(0.052)	(0.084)	(0.064)	(0.010)
DelawareExposed*y2018	-0.031	0.002	0.041	0.083	0.013
	(0.048)	(0.058)	(0.092)	(0.076)	(0.011)
DelawareExposed*y2019	-0.055	-0.005	-0.045	0.066	0.016
	(0.070)	(0.105)	(0.138)	(0.122)	(0.009)
Controls	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
N	3433	2200	3433	2200	3433
adj.R2	0.89	0.90	0.88	0.94	0.76

Table 1.9: The Effect of *TC Heartland* on Innovation Among Prospective Defendants: Cross-sectional Analysis

This table employs cross-sectional methodology to estimate the effect of Supreme Court's TC Heartland vs Kraft Foods decision on proxies of innovation input and output. The sample period is 2015-2019 (excluding 2017). To perform the cross-sectional analysis, for each litigation-related characteristic, we calculate its median value in our main sample as of 2016 and split the sample into two groups: low (the characteristic value is smaller than median) and high value (higher or equal median). The characteristics are: the ratio of R&D to assets in Panel A; Tobin's Q in Panel B; total book assets in Panel C, and the ratio of cash to assets in Panel D. The sample in columns 1-4 and 7-8 includes all publicly-traded patenting firms (see Section 3.4 for further details). The sample in columns 5-6 and 9-10 is limited to years in which firms have applied for at least one patent, and firm-year observations with no patent applications are dropped. See Table 1.1 and Section 1.3.1 for the definition of $TexasExposed$ and $DelawareExposed$. See Table 1.1 and Section 1.3.2 for the description of dependent variables. All specifications use the same vector of control variables as the baseline specification (see even-numbered columns in Table 1.2). The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the state level (defined as the state with the maximal *Dependency* value (See Section 3.1 for the definition)). The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Split Based on R&D										
	R&D		$Log(\#Patents)$				$Log(TotalValue)$			
			Overall Sample		Intensive Margin		Overall Sample		Intensive Margin	
	Low	High	Low	High	Low	High	Low	High	Low	High
TexasExposed*Post	-0.001	-0.016*	0.072	-0.221***	0.114	-0.163**	0.186	-0.340***	0.302	-0.262**
	(0.002)	(0.008)	(0.074)	(0.058)	(0.192)	(0.077)	(0.234)	(0.088)	(0.188)	(0.121)
DelawareExposed*Post	-0.001	0.007	-0.136	-0.034	0.294	0.039	-0.404	-0.024	0.342	0.089
	(0.003)	(0.010)	(0.115)	(0.059)	(0.228)	(0.070)	(0.317)	(0.091)	(0.234)	(0.080)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	628	2750	628	2750	276	1896	628	2750	276	1896
adj.R2	-0.007	0.742	0.866	0.892	0.893	0.901	0.810	0.883	0.955	0.937

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel B: Split Based on Tobin's Q										
	R&D		*Log(#Patents)*				*Log(TotalValue)*			
			Overall Sample		Intensive Margin		Overall Sample		Intensive Margin	
	Low	High	Low	High	Low	High	Low	High	Low	High
TexasExposed*Post	0.015**	-0.048***	-0.129	-0.193***	-0.187	-0.148**	0.005	-0.378***	-0.219	-0.195**
	(0.007)	(0.014)	(0.086)	(0.059)	(0.158)	(0.067)	(0.126)	(0.080)	(0.185)	(0.089)
DelawareExposed*Post	0.004	0.012	0.031	-0.052	0.051	0.028	0.025	-0.106	0.065	0.065
	(0.009)	(0.012)	(0.080)	(0.075)	(0.130)	(0.088)	(0.122)	(0.132)	(0.118)	(0.098)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1237	2107	1237	2107	679	1467	1237	2107	679	1467
adj.R2	0.752	0.745	0.883	0.887	0.896	0.898	0.889	0.863	0.943	0.933
Panel C: Split Based on Assets										
	R&D		*Log(#Patents)*				*Log(TotalValue)*			
			Overall Sample		Intensive Margin		Overall Sample		Intensive Margin	
	Low	High	Low	High	Low	High	Low	High	Low	High
TexasExposed*Post	-0.032*	0.002	-0.115*	-0.137*	-0.056	-0.164*	-0.221**	-0.109	-0.366*	-0.160
	(0.018)	(0.004)	(0.060)	(0.075)	(0.102)	(0.083)	(0.092)	(0.144)	(0.185)	(0.106)
DelawareExposed*Post	0.024	0.003	-0.137**	-0.035	-0.171	0.063	-0.124	-0.132	-0.013	0.090
	(0.021)	(0.005)	(0.065)	(0.051)	(0.117)	(0.052)	(0.095)	(0.108)	(0.144)	(0.077)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1372	2006	1372	2006	729	1443	1372	2006	729	1443
adj.R2	0.687	0.795	0.692	0.911	0.644	0.913	0.710	0.863	0.792	0.928

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel D: Split Based on Cash										
	R&D		*Log(#Patents)*				*Log(TotalValue)*			
			Overall Sample		Intensive Margin		Overall Sample		Intensive Margin	
	Low	High	Low	High	Low	High	Low	High	Low	High
TexasExposed*Post	0.002	-0.024**	-0.230***	-0.026	-0.210*	-0.047	-0.135	-0.072	-0.158	-0.201
	(0.001)	(0.012)	(0.082)	(0.048)	(0.117)	(0.077)	(0.165)	(0.102)	(0.144)	(0.135)
DelawareExposed*Post	0.001	0.013	-0.025	-0.064	-0.025	0.070	-0.172	-0.059	0.030	0.087
	(0.002)	(0.014)	(0.100)	(0.072)	(0.112)	(0.090)	(0.189)	(0.130)	(0.114)	(0.110)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1207	2171	1207	2171	724	1448	1207	2171	724	1448
adj.R2	0.860	0.715	0.883	0.891	0.892	0.899	0.855	0.888	0.946	0.933

Table 1.10: The Effect of *TC Heartland* on Firms' Economic Activity in Texas

This table shows the results of estimating the scope of corporate economic activity in the state of Texas as a function post-*TC Heartland* time indicator variable ($Post$). The sample is based on publicly traded firms over 2015-2019 period (excluding 2017). The sample in columns 1-2 is based on patenting firms, whereas the sample in columns 3-8 includes both patenting and non-patenting firms (see Section 3.4 for further details). $TexasShare$ is the fraction of Texas-originated patents out of all the patents applied for by the firm in a given year (see Section 7 for more details). $TexasDep$ is an indicator value that takes on a value of one if $Dependency_{i,s,t}$ is the highest for state s=Texas in year t (see Section 3.1 for more details). $TexasHQ_{i,t}$ is an indicator variable that equals to one if the firm is headquartered in Texas in year t. $TexasInc_{i,t}$ is an indicator variable that is equal to one if the firm is incorporated in Texas in year t. The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the state level (defined as the state with the maximal *Dependency* value).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$TexasShare$		$TexasDep$		$TexasHQ$		$TexasInc$	
Post	-0.014*	-0.016*	0.003	0.004	0.005*	0.004	0.000	-0.000
	(0.008)	(0.009)	(0.005)	(0.005)	(0.003)	(0.003)	(0.000)	(0.000)
Log(Assets)		0.019*		0.002		0.002		0.000
		(0.011)		(0.001)		(0.001)		(0.000)
ROA		0.003		-0.003*		0.003*		-0.000
		(0.009)		(0.002)		(0.001)		(0.000)
Cash		0.025*		-0.030		-0.002		-0.002
		(0.014)		(0.024)		(0.003)		(0.002)
Capex		0.319		0.027		0.062		0.001
		(0.214)		(0.045)		(0.050)		(0.001)
Leverage		0.011		0.006		-0.001		-0.000
		(0.015)		(0.005)		(0.002)		(0.000)
Tobin's Q		0.003		0.000		0.002		-0.000
		(0.004)		(0.001)		(0.001)		(0.000)
PPE		-0.084*		-0.021		0.021		-0.002
		(0.049)		(0.031)		(0.019)		(0.002)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3694	3433	12686	9961	10581	8295	10581	8295
adj.R2	0.586	0.587	0.697	0.698	0.961	0.962	0.968	0.980

2 Labor Market Concentration and Productivity: Evidence from Star Inventors

2.1 Introduction

Over the past several decades tech clustering - the tendency of firms specializing in the same area of technology development to reside in the same geographic location – has grown in size and economic importance. Over 2015-2018 period 15 metropolitan areas in the US accounted for 57% of total patents produced, 56% of overall high-skilled labor employed, but only 31% of total population (Kerr and Robert-Nicoud (2021)). The most salient benefits of tech clusters include efficient sharing of ideas, inputs, and human capital, as well as knowledge spillovers that ultimately boost inventor productivity (Moretti, 2021). These features, in turn, are important in corporate decisions. For example, Kerr (2018) indicates dynamic environment and proximity to young talent as the key reason behind General Electric's decision to move its headquarters from Connecticut to Boston, Massachusetts in 2016.

However, geographic clusters vary not only in their overall size, but also in the size diversity of individual employers within the cluster. Keeping the cluster size constant, some geographic areas are concentrated more than others. For example, in 2002 Seattle and DC-Baltimore had roughly the same number of inventors specializing in the field of semi-conductors. Yet, employer concentration, as measured by the HHI index of concentration, was almost fifty percent higher in DC-Baltimore than in Seattle. The extent of employer concentration within the local labor markets can also change over time. For example, Azar, Marinescu and Steinbaum (2022) and Benmelech, Bergman, and Kim (2022) document that US labor markets are currently characterized by high levels of labor market concentration.

In this paper, we examine how labor distribution across business entities that operate in the same cluster affects innovation output. We focus on employment concentration in local labor markets and ask whether inventors that reside in clusters that experience an increase in concentration change the scope and quality of innovation they produce. Although innovators can be more productive in large clusters, it is not clear whether the dominance of large employers within a region also plays a role. On the one side, concentrated labor markets can lead to lower innovation. Firms in concentrated labor markets benefit from monopsonic power and often restrain employment (Manning 2010, 2021). Moreover, individuals facing concentrated labor markets may choose not to invest in acquisition of new skills, as the additional productivity gains will ultimately be captured by the employer. As a result, concentrated labor markets may be characterized by underemployment of skilled labor, which, in turn, limits the spillover effects and inventor productivity. On the other side, there may be disadvantages to competitive labor markets. For example, competitive labor markets can reduce employers' incentives to invest in employees' human capital, as the risk of losing workers to a peer firm is higher (Acemoglu and Pischke, 1999). Competitive labor markets can also give rise to distorted compensation schemes, or "bonus culture", which shifts the focus of employees away from long-term projects, such as innovation (Benabou and Tirole (2016)).

We address our research question by examining how the structure of local labor markets affects innovation produced by star inventors, as measured by their patenting output. To this end, we use COMETS database to extract patent-related information for the period of 1977-2003. This database reports generic features of a patent including filing date, the name of the firm that owns the patent, and the names and addresses of all inventors who participated in patent development. COMETS also provides a technology classification system, which divides patents into one of the five major fields. We use inventor address to assign them to geographic regions based on MSA grouping system. Throughout our paper a cluster is defined as the unique combination of MSA and technological field. We next limit our COMETS sample to a subset of most active innovators, whom we denote star-inventors. Following Moretti (2021), we construct the distribution of all inventors based on the overall number of patents each of them produced over our sample period and denote star inventors as those that fall in the top 10%. Although this cutoff is somewhat arbitrary, it allows to construct a relatively long time-series of innovators over time. We use the number of patents that each inventor files as the proxy of inventor output. To measure patent quality, we use patent citations and the patent dollar value, as measured by Kogan, Papanikolaou, Seru and Stoffman (2017), hereafter KPSS, (the latter measure is only available for publicly-traded firms).

The primary empirical challenge in identifying the impact of labor market concentration on innovation output is endogeneity concerns. Favorable regional economic climate may spur both employee productivity and the entry of new employers. Opening of the product markets to foreign competition may lead to the survival of the fittest, giving rise to "superstar firms" that dominate both product and labor markets (Autor et al. (2020)). Reverse causality can also be of concern. For example, an exogenous increase in inventor productivity may encourage star inventors to create spin-off companies, thereby reducing labor market concentration in the area.

To address this challenge, we employ a difference-in-differences (DiD) setting, and exploit mergers of publicly traded firms as an exogenous shock that increases labor market concentration. To ensure that our empirical design is clean of confounding factors, we compare successful mergers (treated group) with failed mergers with similar characteristics (control group). We also remove inventors that belong to the target and the acquiring firms so that our test does not capture changes in productivity that result from reorganization of the merging entities. This setting allows us to examine innovation output of an inventor who, following the merger event, is facing a more concentrated labor market, but is not directly affected by the merger through employment in either the target or the acquiring firm. Finally, we overcome the econometrical problems of staggered DiD design by using a "stacked-cohort" approach, where we assign a suitable control event (a failed merger) to every shock in our sample (a successful merger that increases local labor concentration) and include a separate fixed-effect for each such pair to absorb possible heterogeneity of the merger effect across sup-experiment groups.

Our results indicate that an exogenous increase in labor market concentration boosts innovation output as measured by the total number of patents produced. The results are statistically significant, and economically sizeable. Across all specifications, an exogenous increase in labor market concentration is associated with a 5%-8% increase in innovation output at an individual inventor level. We also find that the increase in quantity comes at the expense of patent quality. Following the event, the average value of patents declines. Finally, we find that there is no meaningful impact of the merger even on overall dollar value of innovation output at an individual inventor level, so that increase in quantity is offset by a decline in quality. Taken together, the results indicate that although the overall scope of innovation output does not change significantly following the merger event, the type of innovation projects changes: Inventors in the treatment clusters work on a larger number of smaller tasks compared to inventors in control clusters, producing more patents of a lower average value.

Our difference-in-differences setting based on comparison of successful and failed mergers in the local labor markets allows us to draw causal inferences about the impact of increased labor market concentration on innovation output. However, the merger setting is limited to a relatively small number of clusters and narrow time periods. Next, we generalize these results by performing a panel data analysis across all geographical clusters in the US. To measure the level of labor market concentration, we use the Herfindahl-Hirschmann Index (HHI), defined as the sum of squared shares of each firm in the cluster. The share of each firm in a cluster is obtained by dividing the number of active inventors a firm employs in a given locality-field by the total number active inventors in that cluster. Our rich set of fixed effects enables us to reject the majority of alternative explanations. For instance, the interaction of city-year fixed effects in our settings controls for productivity shocks in the locality, whereas firm and inventor fixed-effects account for unobservable inventor talent and firm quality and also rule out explanations related to assortative matching across certain types of inventors and firms. Our results are in line with the DiD analysis. We find that inventors that reside in clusters that experience an increase in labor market concentration increase their innovation output and register a higher number of patents. At the same time, the average quality of the patents deteriorates, and each patent is associated with lower dollar value.

In the last part of the paper, we turn to exploring economic channels that may be responsible for these results. Overall, we consider two broad explanations: product-market driven, and labor-market driven.

The key idea behind the first explanation is that product market concentration is the driver of labor market concentration. Firms that become successful and crowd out competitors in the product market space may eventually become dominant in labor market space as well, thereby reducing both product market and labor market competition (CEA 2016). Since concentrated product markets are also characterized by more differentiated products (Hoberg and Phillips (2016)), it is possible that firm's strategy towards more differentiated output affects innovation output by changing the nature of innovators' tasks. Inventors in more concentrated markets are assigned to more specific projects that are tailored to the firm's unique needs. This specialization, in turn, may lead to a shift towards a larger number of smaller projects, as our main results suggest.

To examine whether product market concentration and the associated firm specialization are the mechanisms behind our findings, we perform two tests. First, for every publicly-traded firm we calculate the product market concentration index of its respective industry, as measured by SIC 3-digit code. We then include the measure of concentration in the main regressions, along with its interaction with labor market HHI, as existing literature shows that cases of simultaneous monopoly and monopsony make firms particularly powerful. We find that the impact of labor market concentration on output remains positive and significant. In a second test we capture changes in product market competition more flexibly by reestimating our main regressions after including firm-year fixed effects. If changes in the nature of a firm's overall output are the channel at work, we should find that the inclusion of firm-year dummy will absorb the impact of labor market concentration on inventor output. We find that this is not the case and the impact of labor market concentration on innovative output remains positive and significant.

We next turn to a labor-market driven explanations. The idea is based on Eisfeldt and Papanikolaou (2014), who show that cash flows from intangible capital are divided between a firm (financier) and its key talent (employees) according to the sharing rule that depends on outside options of the employees. As a result, a firm maximizes its ownership of intangible cash flows when it has high bargaining power, whereas employees – weak. According to Eisfeldt and Papanikolaou (2014), lower outside options weaken the bargaining power of key talent. Therefore, firms in concentrated labor markets can strengthen their relative bargaining position by reducing outside options of their star inventors by assigning them to firm-specific and non-redeployable tasks. This argument is also consistent with labor market literature that shows that employers in concentrated labor markets extract additional rents from employees by exploiting their limited mobility and outside employment opportunities (e.g., Benmelech et al. (2022), Prager and Schmidtt (2021), Manning (2021)). At the same time, firms that have to compete for talent often advertise themselves to potential recruits by emphasizing how the experience they receive at the firm can help their subsequent career development and employability (Bar-Isaac and Levy (2022)). To summarize, firms in concentrated industries can enhance their bargaining power by changing the nature of inventors' tasks from transferable towards firm-specific ones.

To the extent that patents of smaller scope capture firm-specific innovation, our main results are consistent with this idea: firms in concentrated industries induce inventors to switch from general projects based on portable knowledge to niche and firm-specific ones. To test the validity of this channel more directly, we examine additional patent characteristics and find that patents become less explorative and more exploitative in concentrated labor environment. Further consistent with the labor market channel, we find that inventors in concentrated labor clusters become more likely to be assigned into teams, especially with other star inventors. Since interdependences developed during teamwork can make it difficult to exploit the knowledge held by an individual inventor outside this firm, teamwork organization also reduce labor mobility of inventors (Palomeras and Melero (2010)). Finally, we find that inventors are more likely to be asked to work on projects outside of their key expertise area. Increasing the workload of projects in which the inventor does not have high proficiency can further reduce their bargaining power and allow the firm to claim a higher ownership over the resulting intangible assets.

This paper contributes to several strands of academic research work. First, it adds to the literature on agglomeration and innovation (see Carlino and Kerr (2014) and Kerr and Robert-

Nicoud (2021) for literature review) by expanding our understanding of how firm organization within a local labor market affects innovation output. In addressing this question, existing literature has largely focused on the spillover effects that "anchor firms" can generate by transmitting their knowledge to small start-up firms (Markusen (1996); Agrawal, Cockburn, Galasso, and Oettls (2014)). In contrast, our paper examines a broader spectrum of size diversity within a cluster. It also established the role of employee bargaining power, as opposed to knowledge spillover, in innovation output.

Second, our paper expands the literature on labor markets, and the role of monopsonic power in particular. Over the past years policy makers around the world have become increasingly concerned with rise in profit share in the economy and the decline in labor market share. An increase in monopsonic power has been offered as one potential mechanism behind these trends (CEA 2016, CEA 2022). Although our data, which ends in 2007, does not allow to confirm the existence of a similar trend in the labor market for inventors, we offer a possible mechanism at work that supports the link between monopsonic power and labor share. Specifically, we show that firms with high levels of bargaining power can further increase their profit share and reduce the labor market share by changing the nature of tasks assigned to key talent employees.

Finally, our paper is related to the literature on mergers. Our work adds to the papers by Arnold (2021) and Azar et al (2022), who show that local mergers depress labor wages. We show how local mergers, and the resulting increase in employer bargaining power, can change the innovative productivity among local firms. Finally, our paper is complementary to the work of Li and Wang (2020), who study changes in inventor productivity of target and acquiring firms following merger events. We expand their work by focusing on local spillover effects of mergers and examining how mergers affect productivity of inventors that did not belong to the merging firms.

The rest of the paper proceeds as follows. In section 2, we describe our data and variable construction method. In section 3, we explain our DiD methodology behind successful-failed merger analysis, and present the main results. In section 4, we expand our analysis to panel-data that exploits the entire dataset if inventor-years. In section 5, we list mechanisms that drive the results. Finally, we conclude the findings in section 6.

2.2 Data and Variable Construction

Following Moretti and Wilson (2017) and Moretti (2021), we use COMETS database on patents. This comprehensive dataset contains the patent number, the name of a firm that owns the patent and the names of all inventors that appear in the patent filing. COMETS also assigns each patent to one of the five research fields. These fields are: "semiconductors, integrated circuits, and superconductors"; "computing and information technology" "biology, chemistry, and medicine"; "other engineering"; and "other science". Each research field is further divided into numerous sub-categories called research classes.

We follow Moretti (2021) to match the location of inventors to a commute area. The address of each inventor involved in a patent's development is stated in the patent application. Moretti (2021) uses Bureau of Economic Analysis definition of economic areas. In most cases, Economic Areas are the same as Metropolitan Statistical Areas (MSAs). Take the Dallas-Fort Worth MSA as an example. This MSA includes city of Dallas, Fort Worth, Arlington, Plano, Garland and Irving and Frisco (all with populations well above 100,000). The cities that are assigned to the same MSA are typically within reasonably commutable distance from each other. For example, it takes an individual around 30 minutes to arrive from Arlington, TX to downtown Dallas by car. That in turn, allows residents of Arlington to commute to Dallas on a daily basis. The mobility of labor across such cities, motivates people with the same skill set to compete for the same job market. As a result, we choose to use MSA definition of locality as opposed to granular definitions such as county based measures.

2.2.1 Tracking Inventors

We do not have direct access to each inventor's employment records. To overcome this challenge, we use patents' information as a reference to follow inventors across firms and cities. The procedure is as follows.

First, we assume a star inventor is active in the skilled labor market in year t if they have at least one patent filed in that year.[4] We then identify each star inventor's employer, field of research and residing city using patent applications that they file during that year. An inventor is employed by the firm j in year t if firm j is the assignee (owner) of inventor's patent applications during that year. We also assume that an inventor specializes in research field s if the majority of the inventor's patent applications in year t are assigned to research field s by COMETS. Finally, we assume inventor i resides in city c during year t if patent applications that she files during that year declare city c as her place of residence.

In cases where there are patent applications assigned to inventor i with different firm assignees within year t, we use the focal firm as the employer during that year. We follow the same methodology to resolve the issue when an inventor has patents with different research fields or city addresses in a given year.[5]

Since majority of inventors only have few patents, we cannot track their employment and location during consecutive years. That is why in the final step we only keep star inventors, i.e. inventors with patent productivity above 90 percent among all inventors.

2.2.2 Measures of Labor Market Concentration

The Herfindahl-Hirschmann Index (HHI) is a widely used measure of firm market power in the literature. Another field of study in Economics that uses HHI is the labor market. For example,

[4] Using years with at least one patent application in that year might remove some information, since inventors might be employed by a firm in a given year but not apply for any patent. We address this issue in our robustness analysis in section 5

[5] These events are generally rare in our dataset.

Arnold (2021), Azar et al. (2022) and Benmelech et al. (2022) use HHI based on employment shares to capture labor market concentration in a locality.

We construct HHI assuming that inventors compete for employment in their research field of expertise and in the city they reside.

We define our HHI measure as:

$$HHI_{cst} = \sum_j (s_j)^2 \tag{2.1}$$

where s_j is the share of firm j from city c's inventor labor market that specializes in research field s. The measure HHI_{cst} potentially suffers from skewness. To address this issue, we use the logarithmic transformation of HHI_{cst}.

2.2.3 Measures of Productivity

Our main measure of inventor productivity is the number of patents each inventor i files during year t, converted into natural logs and denoted as y_{icst}. We aggregate the patent-level data at an inventor-year level by summing up the number of patents filed by an inventor in a year. For multi-author patents, we divide each patent by the number of authors and assign the resulting fraction to each inventor. For instance, suppose an inventor i files for two patents during year 1990, one with 4 other inventors and another sole-authored. Then, the productivity of the inventor i is $(1/5) + (1/1) = (1.2)$.

Overall, $log(y_{icst})$ has one distinctive advantage compared to other measures of productivity. The number of patents is the measurement with the least information asymmetry between inventors and managers. Although it is hard for managers to monitor the quality of each innovation, it is rather easy for them to track the quantity of inventor output namely the number of patents.

2.2.4 Control Variables

Moretti (2021) shows that geographic proximity of inventors results in significant productivity gains. To control for knowledge spillover of inventors in a city, we construct the variable $ClusterSize_{cst}$ which is the number of inventors in the same research field-city pair scaled by the total number of inventors in research field c We use log transformation of the variable $ClusterSize_{cst}$ as a measure to address the mentioned issues. Specifically we follow Moretti (2021) and define $ClusterSize_{cst}$ as:

$$ClusterSize_{cst} = \frac{\#inv_{cst} - 1}{total_{st} - 1} \tag{2.2}$$

In which $\#inv_{cst}$ is the number of inventors who are active in the city c, research field s during year t. We also scale this variable by $total_{st}$ which is the total number of inventors active in research field s across the US during year t. One is deducted from the nominator and denominator to exclude the focal inventor effect.

2.2.5 Final Sample

Our sample consists of 932630 inventor-year observations in our baseline regressions. These observations span across 179 cities, 5 research fields and 37 years between 1971 and 2007. The sample includes both public and private firms and also other public institutions such as government entities and universities. When we limit our analysis to public firms, our sample includes 510227 inventor-year observations. The summary statistics are presented in table 2.1.

[Insert table 2.1]

The difference between mean and median in HHI_{cst} and $ClusterSize_{cst}$ shows that both variables are heavily skewed. Looking at these variables post-log transform, the distribution becomes more symmetric. We further see that the median number of employees that a firm hires is 18 inventors. Meanwhile, the median number of inventors in a cluster is 10. Both of these variables also show considerable level of dispersion across firms.

We also illustrate the evolution of the number of inventors, number of firms and labor market HHI across different research fields over time. The graphs span the period from 1977 to 2003. To generate a value for each research field-year pair, we average the variable of interest across all cities. The results are illustrated in figures 1-3. We can see that the average number of active inventors has seen an upward surge through time, specially in the field of semiconductors. Moreover, we see the same pattern in the number of firms. There has also been a considerable year-by-year increase in the average number of active firms in ”semiconductor” field.

2.3 The Impact of Labor Concentration on Innovation: Successful versus Failed Mergers

In this section, we examine the impact of labor market concentration on innovation output. To this end, we local merger events as exogenous shocks to labor market concentration. This helps us mitigate any confounding factor with respect to local labor organization and local innovation output.

Specifically, we employ a difference-in-differences (DiD) setting, and exploit mergers that increase local labor concentration as an exogenous shock to HHI. To this end, we identify mergers between two firms that both have a research lab in the same city and same research field. When these firms merge, the labor market concentration increases for all inventors working in that city and research field. Importantly, following the merger, a local inventor that does not belong to the merging firms will still face a more concentrated labor market with fewer firms to work for.

To ensure that our empirical design is clean of confounding factors, we construct our analysis in the following way. First, we remove inventors that belong to the target and the acquiring firms to make sure our results are not affected by merger productivity shocks to these firms. For example, Li and Bena (2014) show that after merger completion, acquirer firm’s inventors increase their patenting performance, while target firm’s inventors become less productive.

The second concern with using mergers as shocks to local labor market concentration is that affected localities may be different from non-affected localities and these differences stimulate both higher levels of merger deals and higher levels of productivity. To address this concern, we use failed merger events as our control group. If mergers are driven by some omitted factors that are also linked to inventor productivity, these factor should be similar for both successful and failed mergers. Merger proposals can fail for numerous reasons, including but not limited to regulatory limitations, rejection of offer from major investors or demand for further due diligence from one of the parties.

A third potential concern of our methodology stems from recent advances in econometrics, which reveal that there are certain problems related to staggered DiD design. Overall, the staggered DiD analysis is based on the idea that a plausibly exogenous treatment affects different groups within the sample (e.g. counties, states, countries) at different points of time. However, this analysis is based on two critical assumptions: first, that the treatment does not change over time, and second, that there is no partial adjustment of the treated group to the shock, and the full effect of the treatment is realised immediately. Baker et al. (2022) show that when treatment effects change over time and across treatment groups, staggered DiD treatment effect estimates can actually obtain the opposite sign of the true estimates, even if the researcher were able to randomize treatment assignment.

Since our merger shocks happen across different localities and years, we need to use a DiD design that alleviates such concerns. To control for potentially dynamic effect of the treatments, we use a ”stacked-cohort” approach and construct sub-experiments by assigning a suitable control event (a failed merger) to every shock in our sample (a successful merger that increases local labor concentration).

We also make sure that all the treatment and control groups are included in the experiment for the same period of time. We use merger proposition date as the date of the shock, and examine the time window of three years before and three years after the proposed merger date to avoid possible truncation problems that might bias the true impact of the treatment. We also remove the inventor-year observations in the year of merger proposal. In addition, we ensure that there is no overlap of pre- and post-merger periods between two separate mergers. Specifically, we require that the time gap between the two shocks in the same cluster is more than seven years. For instance, if a merger shock happens in San Diego-Semiconductors cluster in year 1993, the next earliest year where we consider valid for this cluster is 2000.

Finally, we stack all sub-experiments, and use a fixed effect for each successful-failed merger pair to account for possible heterogeneity across sub-experiments.

2.3.1 Selecting Merger Events

To construct the universe of affected inventors, we first identify the mergers that affect local inventor labor market. To this end, we start by obtaining information on merger events from SDC Platinum. A merger between firms A and T in year t is considered a valid shock to labor concentration in city c and research field s if:

1. Both A and T are publicly traded firms.
2. Neither A nor T belong to the utility or financial sectors.
3. The value of merger transaction is above one million US dollars.
4. The date of merger announcement is between 1981 and 2003. Since our inventor sample ends in year 2007, using 2003 as the last merger year gives enough time to local inventors to respond to the merger shock and helps avoid truncation problems.
5. There is least one active inventor in the city c who specializes in research field s and works in firm A(T) during year $t-1$.
6. The expected change in the concentration variable as a result of the merger is economically sizable. To this end, we calculate $ExpChangeHHI_{cst}$ (described in more details below) for the universe of mergers, passing the filters 1-5, and rank these mergers from the smallest to

largest based on the values of $ExpChangeHHI_{cst}$. We then drop the mergers that fall below the 25% threshold of the resulting distribution.

Condition 6 above helps us eliminate economically inconsequential mergers and only focus on merger events that considerably change the concentration level in a locality. We define $ExpChangeHHI$ in the following way:

$$ExpChangeHHI_{cst} = \frac{E(HHI_{c,s,t+1}|Mergershock_{year=t}) - (HHI_{c,s,t-1})}{(HHI_{c,s,t-1})} \tag{2.3}$$

where t is the year of merger and c and s refer to city and research field, respectively.

Also:

$$E(HHI_{c,s,t+1}|Mergershock_{year=t}) = \sum_{j \neq acq,tar} (s_j)^2 + (s_{acq} + s_{tar})^2 \tag{2.4}$$

where s_{acq} [s_{tar}] is the number of acquirer's [target's] inventors in city c's in research field s, scaled by the total number of inventors in that cluster. The formula is based on the idea that after the merger, the inventors of the acquirer and target firms' labs in city c operate as part of the same corporate entity.

Since from equation 2.1 we know that

$$HHI_{c,s,t-1} = \sum_j (s_j)^2 = \sum_{j \neq acq,tar} (s_j)^2 + (s_{acq})^2 + (s_{tar})^2 \tag{2.5}$$

,

the expected change in HHI can then be expressed in the following way:

$$ExpChangeHHI_{cst} = \frac{2 s_{acq} s_{tar}}{HHI_{c,s,t-1}} \tag{2.6}$$

.

This identity confirms that, ceteris paribus, all merger shocks are expected to lead to a higher level of local concentration.

We also apply conditions 1-6 to our failed merger sample to identify the subset of potential control events. In addition, we remove failed mergers that took place within a three-year period before or after a successful merger in the same locality. These failed mergers produce potentially contaminated control events because the inventors of that locality could be affected by a successful merger shock that took place a few years earlier/later.

After we apply all conditions, we are left with 51 successful merger events and 24 control ones.

As the second step of our analysis, we match each treated cluster to a control cluster (with replacement) based on the following conditions:

1. Both treated and control localities belong to the same research field.

2. The time gap between successful merger in the treated cluster and failed merger in the control cluster does not exceed ten years.

3. Treated and control merger pairs exhibit similar characteristics prior to the merger. To ensure that, we calculate the absolute value of the differences between treated and control localities one year prior to the merger for each of the following variables: $ClusterSize_{c,s,t-1}$, $HHI_{c,s,t-1}$ or $ExpChangeHHI_{c,s,t-1}$. For every variable, we then rank the matched pairs from the lowest to the highest value of the absolute distance, and keep only those pairs in which the distance between treated and control localities does not exceed 25th percentile of the distribution.

4. In cases where we are left with several control candidates for one treated locality, we only keep one control locality. The proposed treated-control pair has the lowest distance in terms of $HHI_{c,s,t-1}$ between treated and control locality pairs .

As a result of this step, we end up with 25 pairs of treated and control merger events. The comparative statistics of the two groups is summarized in panel B of table 2.2. It is clear that after the matching process, the characteristics of treated and control localities become closer and any difference becomes insignificant. For instance, the gap between $Log(HHI_{cst})$ between treated and control groups drops from 0.34 to 0.09. The gap in variables $ClusterSize_{cst}$ remains insignificant, while the gap in terms of $ExpChangeHHI_{cst}$ between those two groups drops to 0.3 percent, about one fourth of its value before matching.

[Insert Table 2.2 Here]

2.3.2 Final Inventor Sample and Regression analysis

As the last step of our empirical design, we identify all inventors that reside in the affected localities. To this end, for each treated (control) merger event that passed all our filters, we use all inventors that reside in the locality during a three-year period before and three-year period after the successful (failed) mergers and work in the same research field as the merging firms. We then remove all inventors that were employed either by acquirer or target in a successful (failed) merger event in the treated (control) cluster at any time during our event window.

That leaves us with 18,942 unique treated inventors and 11,282 control inventors (note that since a failed merger can be used as a match to more than one successful merger, we end up with more treated inventors). Our final merger sample has 68,397 inventor-year observations out of which 33,482 belong to our control group and 34,915 belong to treated group. Among 5 research fields, the group biology (biology, chemistry, and medicine) contains the highest number of treated and control observations. Details of the distribution of our sample based on research fields are listed in table 2.3.

[Insert Table 2.3 Here]

After identifying both treated and control inventors, we estimate the following regression: $\log(\mathrm{y}_{ijcst}) = \delta_1 treated_{cst} * post_{cst} + \gamma_2 Log(ClusterSize_{cst}) + treated_{cst} + post_{cst} + events_{cst} + \lambda_{ijcst} + \epsilon_{ijcst}$

where $treated_{cst}$ is an indicator variable that is equal to one for a treated group inventor, and zero otherwise. Variable *post* takes the value of one starting one year after the merger and onward, and zero otherwise. λ_{ijcst} is a vector of fixed effects.

The rich set of fixed effects and controls address numerous endogeneity concerns that arise in these settings. First, we employ basic controls to mitigate endogeneity concerns. The variable $log(ClusterSize_{cst})$ captures spillover effects that emerge from the innovation activity of other inventors or firms in a city. Moreover, our year fixed effects control for any macroeconomic effect at national level that might bias the results. We also employ city and research field fixed effects to control for stationary effects related to a particular city or research field.

We also use research field-year fixed effects to address endogeneity issues that might arise. Consider a research field such as semiconductors which became extensively popular year by year. Consequently, many firms entered the market to seize profit share. At the same time, increasing attractiveness of such research field might motivate researchers to be more enthusiastic about their research and exert more effort. The outcome of these simultaneous phenomena is a downward bias. Our field-year fixed effect controls for such biases.

In addition to λ_{ijcst}, we employ another level of fixed effects namely *event* which assigns an identifier to each sub-experiment i.e. inventors who reside in a treated locality and its control counterpart during the shock. This helps us compare each treated locality only with its control counterpart.

Results are listed in table 2.4. First, panel A presents results of regressions where number of patents is the variable of interest. Specification (6) employs the most restrictive set of fixed effects. The estimated value is 0.067 with p-value below 5%. In contrast to the results for the number of patents, results for other panels looks insignificant. When we use number of citations (panel b), average citations (panel c) or total value of patents (panel d) we see insignificant results in all specifications. Moreover, results for average dollar value of patents (panel e) even show negative effects in less restrictive specifications. The results show that while labor market concentration leads to higher level of inventor quantitative output, the qualitative output doesn't change. Next, we study whether this effect is limited to local mergers or is in fact common across all localities.

2.4 Baseline Regressions

We also look at the relationship between labor market productivity and innovation output in a panel setting. Our main baseline regression has the following form:

$$log(y_{ijcst}) = \beta_1 log(HHI_{cst}) + \beta_2 log(ClusterSize_{cst}) + \eta_{ijcst} + \epsilon_{ijcst} \tag{2.7}$$

where y_{ijcst} is the measure of innovation productivity. In addition to control variables, we employ a rich set of fixed effects that further address endogeneity issues. η_{ijcst} is a vector of fixed effects and ϵ_{ijcst} is the idiosyncratic component of inventors' productivity. We cluster our standard errors at the inventor level.

Our fixed effects identical to the fixed effects we use in our merger regressions, with two exceptions. First, we also use city-year fixed effects in some of the specifications. Our city-year fixed effects control for possible social and economic changes at metropolitan level that affect labor productivity. Second, we add research class fixed effect and its interaction with city and year to augment our results further.

The results of this regression are presented in table 5.

[Insert Table 2.5 Here]

Panel A reports results for the number of patents as the dependent variable. We see that in all specifications the relationship between labor market concentration and the number of patents produced is positive and significant. Note that the number of observations is considerably larger than in our merger sample, as we now include all cluster-years of our sample.

Results for panels B-E are consistent with our previous results. While we see that the citation related measures don't respond to local mergers, the results for dollar value related measures of output are mostly significant. In particular, we see a significant drop in the average dollar value of patents produced by inventors in concentrated markets. The number of observations in dollar related measures are considerably lower that observations for panels a-c, since we are limited to public firms when we study the dollar related measures of output.

2.5 Channels at Work

2.5.1 Product Market Channel

We introduce several variables to study channels at work. To study product market rivalry, we use the following definition of industry level competition:

$$Competition_{ind,t} = \frac{1}{\sum_{j \in ind}(s_j)^2} \tag{2.8}$$

in which s_j is the annual sale value of firm j at year t scaled by the sales value of the industry ind that firm j belongs to. ind is defined as the 3-digit SIC industry. Next, we interact this variable with $log(HHI_{cst})$ and run the following regression:

$\log(\mathrm{y}_{ijcst}) = \beta 1 Competition_{ind,t} + \beta_2 log(HHI_{cst}) * Competition_{ind,t} + \beta_3 log(HHI_{cst}) + \beta_4 log(ClusterSize_{cst}) + \eta_{ijcst} + \epsilon_{ijcst}$

The results are presented in table 2.6

[Insert Table 2.6 Here]

Results show that product market rivalry cannot explain all the productivity gain from concentration. Consequently, we turn our attention to channels which directly stem from labor market concentration.

Labor Market Channel In this part, we identify several channels through which firms manage the inventors. First, we study whether patents' explorative(exploitative) nature is related to concentration level of labor market. Explorative patents are patents that explore outside firm's existing knowledge. On the other hand Exploitative patents are built upon existing knowledge of firm.

To define explorative(exploitative) measure at the inventor-year level, we first find out whether each patent is explorative(exploitative). Patent p is explorative(exploitative) if more than %80(%20) of all the patents that it cites are not related to firm j's existing knowledge i.e. patents firm j owns and patents it cites before. We then aggregate this value at the inventor-year level. We run the same regression using the dependant variable $Exploitative_{ijcst}$.

Next, we intend to study the effect of concentration on average team size. We also use the natural log transformation to curtail asymmetries and outliers present in this variable.

Finally, we study the effect of concentration on the scope of inventor innovation. To this end, we define our dependant variable as the number of patents the inventor i produces in her class of specialization in year t scaled by the number of all patents she produces in that year. Class of specialization in year t is defined as the technology class with the highest number of patent filings for inventor i up to year t.

Results for each channel is listed in table 2.7. Results in panel a and panel b show that, in a concentrated local labor market, inventors produce fewer explorative patents while producing more firm dependent and exploitative patents. Panel c demonstrates that team size gets bigger when a labor market is more concentrated. Finally, regression results in panel d tell us that a concentrated labor market leads to inventors producing more patents outside their specialized scope.

[Insert Table 2.7 Here]

2.6 Robustness

2.6.1 Merger Shocks and Parallel Trend Assumption

For our control group to be a valid counterfactual, the dynamics of productivity should be parallel among treated and control groups.

Our parallel trend regression is similar to our merger shock regression model **2.3**.2 with one exception. We replace variable $post_{cst}$ with indicator variables $dist_k$ that indicate the distance of each year of analysis . Specifically, for each locality c, research field s that is part of treatment (control) group, we define $dist_k$ as a vector of indicator variables where $k \in [T-3, +3+T]$ and T is the year when successful (failed) merger shock happened.

$$log(y_{ijcst}) = \delta_1 treated_{cst} * dist_k + \gamma_2 Log(ClusterSize_{cst}) + \lambda_{ijcst} + treated_{cst} + dist_k + \epsilon_{ijcst} \quad (2.9)$$

Results are reported as graphs in figure 2.4. Looking at coefficient estimates of regression 2.9,we can infer that, before merger events, any departure between treated and control groups is marginally weak. However, after the year of treatment, the difference becomes strong and significant. Overall, results show that any pre-trends before shocks are non-existent or weak.

2.6.2 Poisson Regression Results

Cohn et al (2022) show that model choice can significantly change the size and sign of estimand inpanel regressions. Specifically, they show when dealing with count data and skewed distribution, Poisson regression produces unbiased and consistent estimates conditional on standard exogeneity conditions. However, estimands in comparative models such as models that use logarithmic transformations suffer from possible biases and lack economic meaning. Consequently, we use Poisson regression model to check the validity of our results. Here, we use direct values of patent productivity instead of its log transformed versions. We keep the same type of fixed effects we used in our log based regressions. Results for merger regressions are listed in the appendix for this chapter.

Moreover, we replace $log(y_{ijcst})$ in our baseline regressions and redo panel (a) of table 2.5. Results are still significant for both baseline and merger regressions even when we use Poisson model.

2.7 Conclusion

We uncover large persistent effect of local mergers on local skilled labor's output. Our results indicate that local mergers enhance inventors' output through labor market power, since local mergers reduce the number of potential employers in city.

Our attempts to identify channels show that labor market concentration affects inventor output through changing the characteristics of innovation. inventors in consolidated labor markets tend to do less grounding innovation and instead build on their employers' existing knowledge. We also see that the inventor team-size is positively related to the level of labor market concentration. Results indicate that firm manages its skilled workers in a way that increases their dependency to the firm.

While we can't rule out that our estimates are biased by unobserved shocks to local labor output, our restrictive set of fixed effects and novel shock mitigate such concerns to a great extent. Further, we address any bias that arise through regression specification problems such as using logarithmic transformation or staggered difference in differences.

This paper sheds light on the relationship between skilled labor and firms and how this relationship is affected through regional labor market activities.

Figure 2.1: Number of Active Inventors Over Time

This graph shows changes in the number of active inventors in localities from 1977 to 2003. Localities are city-research field pairs. Cities are defined as Metropolitan Statistical Areas by Bureau of Economic Analysis. Research fields are technology classes defined by COMETS database. For more information regarding our sample, refer to section 2.

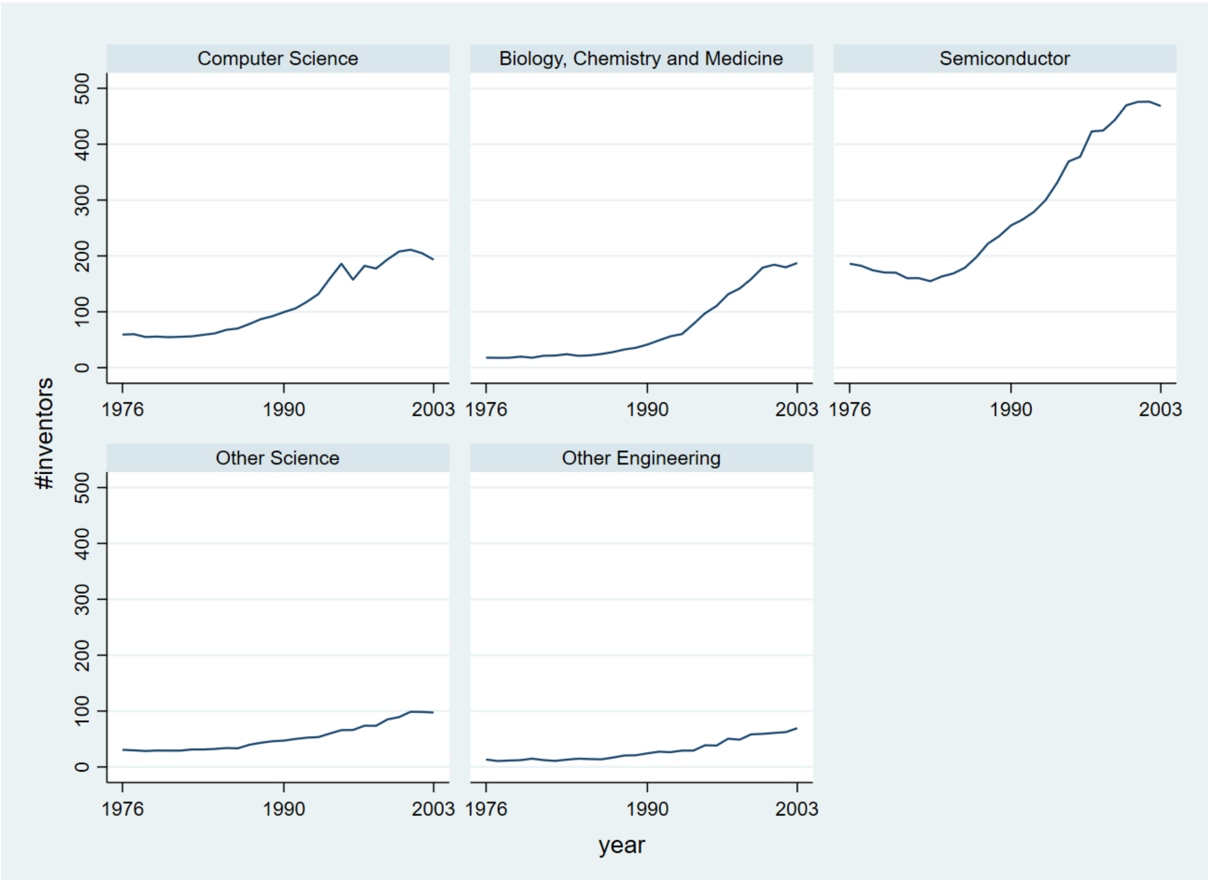

Figure 2.2: Labor Market Concentration Over Time, by Research Field

This graph shows evolution of concentration level of labor market for active star inventors from 1977 to 2003. Concentration is measured by Herfindahl-Hirschman Index, and averaged across all clusters in a given year. Clusters are city-research field pairs, where cities are defined as Metropolitan Statistical Areas by Bureau of Economic Analysis. Research fields are technology classes defined by COMETS database. For more information regarding our sample, refer to section 2.

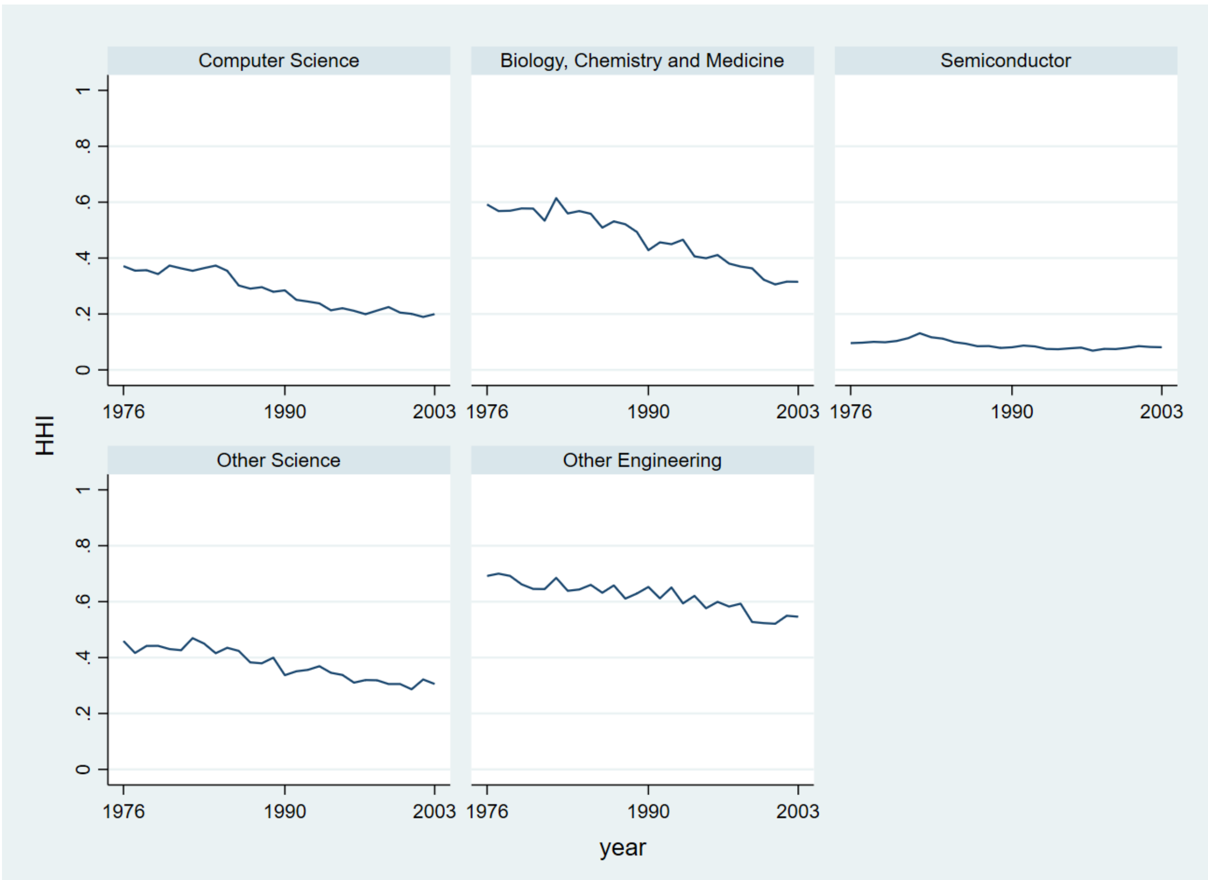

Figure 2.3: Number of Firms Over Time

This graph shows changes in the number of active firms in localities from 1977 to 2003. A firm is considered active in a locality if it employs at least one inventor in that locality. Localities are city-research field pairs. Cities are defined as Metropolitan Statistical Areas by Bureau of Economic Analysis. Research fields are technology classes defined by COMETS database. For more information regarding our sample, refer to section 2.

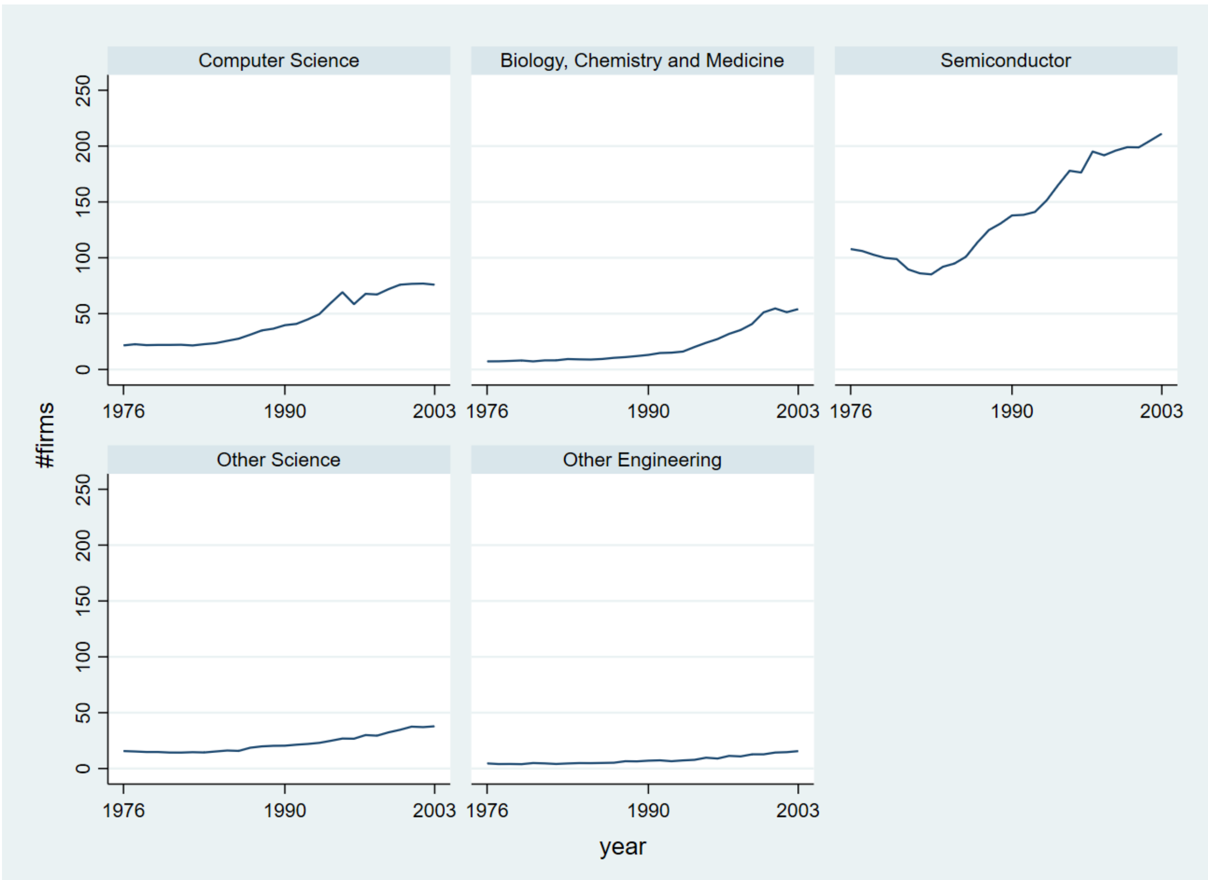

Figure 2.4: Merge Shocks: Parallel Trend

This table represents graphs that correspond to regression 2.9 using different sets of fixed effects. In panel a we use year, city and research field fixed effects. In panel b, we employ the interaction of city and research field fixed effects in addition to panel a. In panel c, we also add field-year interaction to the fixed effects used in panel b. Finally in panel d, we add firm and inventor fixed effects to the fixed effects employed in panel c.

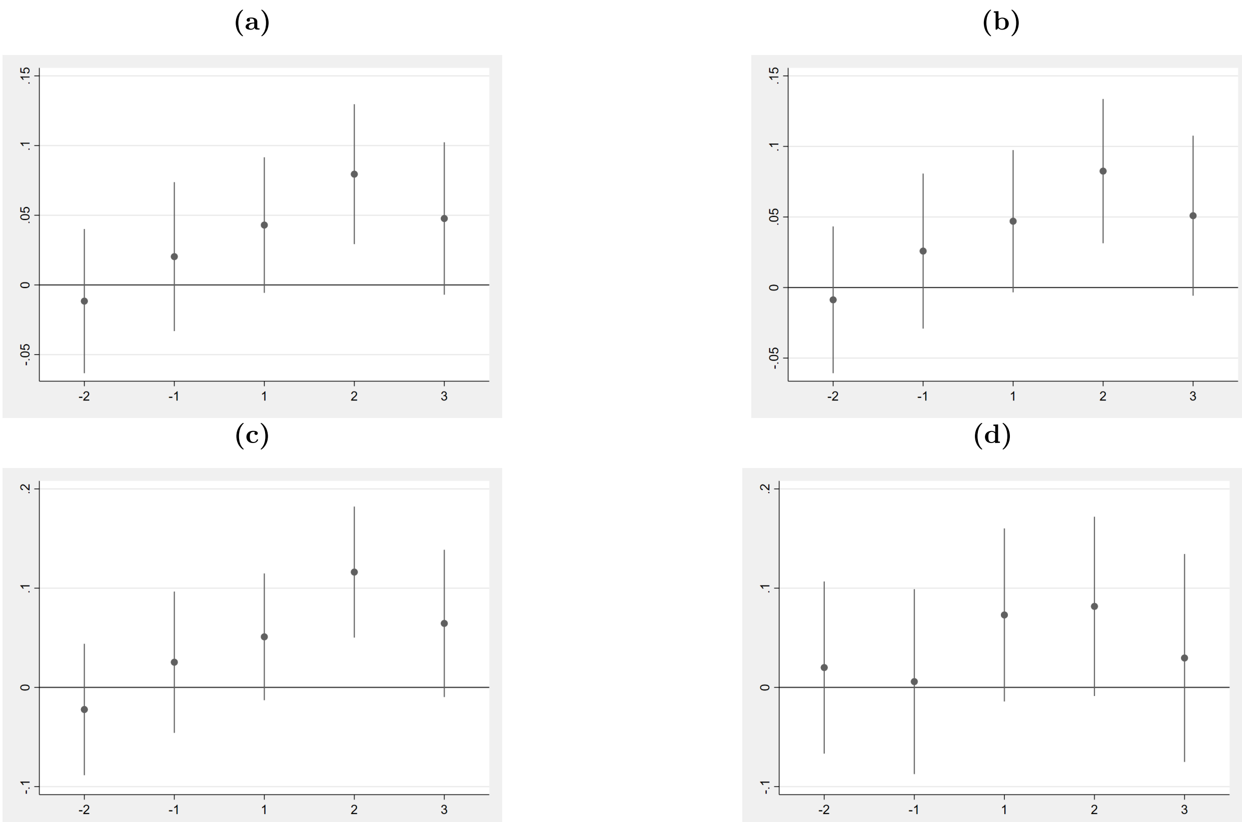

Table 2.1: Summary Statistics

This table reports summary statistics of the variables used in our empirical analysis. The sample period is 1977-2007 and the unit of observation is at inventor-year level. See section 2 to find out how we construct the sample. For detailed description of variable construction please refer to Appendix table A.

	count	mean	sd	p1	p25	p50	p75	p99
HHI_{cst}	932630	0.08	0.14	0.00	0.01	0.03	0.08	0.72
$ClusterSize_{cst}$	932630	0.05	0.05	0.01	0.03	0.06	0.22	
$log(HHI)_{cst}$	932630	-3.39	1.26	-5.61	-4.35	-3.55	-2.59	-0.33
$log(ClusterSize_{cst})$	932026	-3.74	1.34	-7.50	-4.50	-3.52	-2.76	-1.51
$log(\#firms_{cst})$	931122	-3.84	1.27	-7.16	-4.71	-3.74	-2.74	-1.69
$FirmSize_{jcst}$	932658	60.15	177.91	0.00	1.00	8.00	45.00	648.00
$LabSize_{ijcst}$	932658	40.96	93.77	1.00	1.00	4.00	30.00	509.00
$log(\#Patents_{ijcst})$	932658	-0.25	0.80	-2.08	-0.69	0.00	0.18	1.67
$log(TotalCite_{ijcst})$	932658	0.18	4.52	-11.51	0.02	1.61	2.64	4.85
$log(AverageCite_{ijcst})$	932658	0.43	4.36	-11.81	0.69	1.79	2.71	4.68
$log(TotalValue_{ijcst})$	510227	1.46	1.64	-2.87	0.46	1.51	2.53	5.17
$log(AverageValue_{ijcst})$	510227	1.72	1.48	-2.50	0.90	1.79	2.64	5.06
$General_{ijcst}$	435755	0.35	0.27	0.00	0.00	0.38	0.56	0.83
$Original_{ijcst}$	528699	0.40	0.26	0.00	0.19	0.44	0.61	0.84
$explore_{ijcst}$	571978	0.42	0.46	0.00	0.00	0.08	1.00	1.00
$exploit_{ijcst}$	571978	0.25	0.39	0.00	0.00	0.00	0.50	1.00

Table 2.2: Treated and Control Localities

This table presents the characteristics of treated and control localities. A locality is considered treated if it hosts a successful merger that is likely to increase HHI. A control locality is a locality that experiences a failed merger and is similar in several characteristics to its matched treated locality. For detailed explanation of how we match treated and control localities refer to section 3.1.

	Treated Locality	Control Locality	Comparison Test
Panel A: Before Matching Process			
$log(HHI_{cst})$	-3.07	-2.73	0.34*
			(1.90)
$log(ClusterSize_{cst})$	-3.28	-3.61	-0.33
			(-1.50)
$ExpChangeHHI_{cst}$	0.035	0.023	-0.012
			(-0,70)
Panel B: After Matching Process			
$log(HHI_{cst})$	-3.00	-2.90	0.09
			(0.70)
$log(ClusterSize_{cst})$	-3.43	-3.75	-0.32
			(-1.53)
$ExpChangeHHI_{cst}$	0.007	0.01	0.003
			(1.03)

Table 2.3: Number of Treated and Control Inventors

This table includes number of inventors in each research field. A locality is considered treated if it hosts a successful merger that is likely to increase HHI. A control locality is a locality that experiences a failed merger and is similar in several characteristics to its matched treated locality. For detailed explanation of how we match treated and control localities refer to section 3.1. To find more information regarding our inventor sample construction refer to section 2.

	Semiconductors	Computing	Biology	Other Eng	Total
Control	7,573	462	25,251	444	33,730
Treated	3,055	1,738	30,411	378	35,582
Total	10,628	2,200	55,662	822	69,312

Table 2.4: Mergers as Shocks

This table shows how labor market concentration affects inventor output. Each panel represents results for a measure of productivity. To address endogeneity issues, we use local merger events in a difference in differences setting. For further information regarding our identification strategy refer to section 3. To find more information regarding our inventor sample construction refer to section 2. Refer to Appendix table A1 for detailed information about how we construct our independent and dependant variables. The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the inventor level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
panel a:$log(\#Patents)$						
$treated_{cst} * post_{cst}$	0.056***	0.050**	0.056***	0.051**	0.077***	0.067**
	(0.017)	(0.024)	(0.018)	(0.025)	(0.024)	(0.033)
$log(ClusterSize_{cst})$	0.201***	0.161***	0.231***	0.190**	0.303***	0.257***
	(0.040)	(0.062)	(0.057)	(0.081)	(0.066)	(0.093)
obs	69312	69233	69312	69233	69312	69233
adj.R2	0.023	0.394	0.023	0.394	0.024	0.395
year	x	x	x	x	x	x
city	x	x	x	x	x	x
field	x	x	x	x	x	x
city*field			x	x	x	x
field*year					x	x
inventor		x		x		x
firm		x		x		x

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	(1)	(2)	(3)	(4)	(5)	(6)
panel b: $log(TotalCite)$						
$treated_{cst} * post_{cst}$	0.031	0.015	0.050	0.037	0.124	0.121
	(0.062)	(0.091)	(0.065)	(0.095)	(0.088)	(0.123)
$log(ClusterSize_{cst})$	0.448***	0.119	0.356*	0.301	0.572**	0.532
	(0.147)	(0.240)	(0.215)	(0.324)	(0.266)	(0.404)
obs	69312	69233	69312	69233	69312	69233
adj.R2	0.114	0.416	0.114	0.416	0.115	0.416
year	x	x	x	x	x	x
city	x	x	x	x	x	x
field	x	x	x	x	x	x
city*field			x	x	x	x
field*year					x	x
inventor		x		x		x
firm		x		x		x
panel c: $log(AverageCite)$						
$treated_{cst} * post_{cst}$	-0.025	-0.035	-0.006	-0.014	0.047	0.054
	(0.058)	(0.085)	(0.061)	(0.089)	(0.083)	(0.115)
$log(ClusterSize_{cst})$	0.247*	-0.042	0.126	0.111	0.269	0.275
	(0.138)	(0.226)	(0.202)	(0.305)	(0.250)	(0.383)
obs	69312	69233	69312	69233	69312	69233
adj.R2	0.126	0.427	0.127	0.427	0.128	0.428
year	x	x	x	x	x	x
city	x	x	x	x	x	x
field	x	x	x	x	x	x
city*field			x	x	x	x
field*year					x	x
inventor		x		x		x
firm		x		x		x

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	(1)	(2)	(3)	(4)	(5)	(6)
panel d: $log(TotalValue)$						
$treated_{cst} * post_{cst}$	-0.036	-0.002	-0.040	-0.012	0.009	0.034
	(0.035)	(0.035)	(0.036)	(0.035)	(0.049)	(0.047)
$log(ClusterSize_{cst})$	0.154*	-0.103	0.169	-0.203*	0.337**	-0.094
	(0.083)	(0.086)	(0.114)	(0.113)	(0.136)	(0.132)
obs	69312	69233	69312	69233	69312	69233
adj.R2	0.080	0.722	0.080	0.722	0.081	0.722
year	x	x	x	x	x	x
city	x	x	x	x	x	x
field	x	x	x	x	x	x
city*field			x	x	x	x
field*year					x	x
inventor		x		x		x
firm		x		x		x
panel e: $log(AverageValue)$						
$treated_{cst} * post_{cst}$	-0.092***	-0.052**	-0.096***	-0.063**	-0.068	-0.033
	(0.031)	(0.024)	(0.032)	(0.025)	(0.044)	(0.033)
$log(ClusterSize_{cst})$	-0.048	-0.264***	-0.062	-0.394***	0.034	-0.351***
	(0.074)	(0.061)	(0.100)	(0.077)	(0.121)	(0.093)
obs	69312	69233	69312	69233	69312	69233
adj.R2	0.098	0.834	0.099	0.834	0.099	0.834
year	x	x	x	x	x	x
city	x	x	x	x	x	x
field	x	x	x	x	x	x
city*field			x	x	x	x
field*year					x	x
inventor		x		x		x
firm		x		x		x

Table 2.5: Panel Regressions

This table shows how labor market concentration affects inventor output in a panel regression setting. To find more information regarding our inventor sample construction refer to section 2. Refer to Appendix table A1 for detailed information about variable construction. To find more information regarding our inventor sample construction refer to section 2. Refer to Appendix table A1 for detailed information about how we construct our independent and dependant variables. The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the inventor level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
panel a: log(#Patents)								
$log(HHI_{cst})$	0.010***	0.028***	0.026***	0.025***	0.031***	0.041***	0.040***	0.040***
	(-0.003)	(-0.004)	(-0.004)	(-0.004)	(-0.004)	(-0.004)	(-0.005)	(-0.006)
$log(ClusterSize_{cst})$	0.053***	0.080***	0.091***	0.094***	0.072***	0.096***	0.050***	0.062***
	(-0.003)	(-0.004)	(-0.005)	(-0.005)	(-0.005)	(-0.005)	(-0.008)	(-0.009)
obs	932026	932026	932026	932026	932026	932026	932026	823375
adj.R2	0.042	0.044	0.061	0.064	0.071	0.225	0.227	0.242
year	x	x	x	x	x	x	x	x
city	x	x	x	x	x	x	x	x
field	x	x	x	x	x	x	x	x
class	x	x	x	x	x	x	x	x
city*field		x	x	x	x	x	x	x
city*class			x	x	x	x	x	x
field*year				x	x	x	x	x
class*year					x	x	x	x
inventor						x	x	x
city*year							x	x
firm								x

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>*panel b:* $log(TotalCite)$</u>								
$log(HHI_{cst})$	-0.009	0.00	-0.018	-0.041***	0.005	0.043**	0.026	0.037
	(-0.009)	(-0.014)	(-0.015)	(-0.015)	(-0.015)	(-0.017)	(-0.023)	(-0.026)
$log(ClusterSize_{cst})$	0.121***	0.172***	0.196***	0.203***	0.232***	0.215***	0.169***	0.157***
	(-0.011)	(-0.017)	(-0.018)	(-0.018)	(-0.018)	(-0.021)	(-0.032)	(-0.036)
obs	932026	932026	932026	932026	932026	932026	932026	823375
adj.R2	0.424	0.425	0.435	0.439	0.454	0.504	0.505	0.502
year	x	x	x	x	x	x	x	x
city	x	x	x	x	x	x	x	x
field	x	x	x	x	x	x	x	x
class	x	x	x	x	x	x	x	x
city*field		x	x	x	x	x	x	x
city*class			x	x	x	x	x	x
field*year				x	x	x	x	x
class*year					x	x	x	x
inventor						x	x	x
city*year							x	x
firm								x

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
panel c: log(AverageCite)								
$log(HHI_{cst})$	-0.019**	-0.028**	-0.044***	-0.066***	-0.026*	0.002	-0.014	-0.004
	(-0.009)	(-0.014)	(-0.014)	(-0.014)	(-0.014)	(-0.016)	(-0.022)	(-0.025)
$log(ClusterSize_{cst})$	0.068***	0.092***	0.105***	0.109***	0.160***	0.119***	0.119***	0.095***
	(-0.01)	(-0.017)	(-0.017)	(-0.017)	(-0.017)	(-0.02)	(-0.031)	(-0.035)
obs	932026	932026	932026	932026	932026	932026	932026	823375
adj.R2	0.437	0.438	0.449	0.452	0.468	0.514	0.515	0.514
year	x	x	x	x	x	x	x	x
city	x	x	x	x	x	x	x	x
field	x	x	x	x	x	x	x	x
class	x	x	x	x	x	x	x	x
city*field		x	x	x	x	x	x	x
city*class			x	x	x	x	x	x
field*year				x	x	x	x	x
class*year					x	x	x	x
inventor						x	x	x
city*year							x	x
firm								x

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
panel d: $log(TotalValue)$								
$log(HHI_{cst})$	0.112***	0.061***	0.029***	-0.019*	-0.011	0.00	0.043***	0.020*
	(-0.007)	(-0.01)	(-0.01)	(-0.01)	(-0.011)	(-0.01)	(-0.013)	(-0.012)
$log(ClusterSize_{cst})$	0.084***	-0.008	-0.015	-0.028**	-0.019	-0.027**	-0.014	-0.01
	(-0.008)	(-0.013)	(-0.013)	(-0.013)	(-0.013)	(-0.013)	(-0.018)	(-0.016)
obs	509984	509984	509984	509984	509984	509984	509984	506539
adj.R2	0.113	0.129	0.201	0.205	0.224	0.532	0.539	0.626
year	x	x	x	x	x	x	x	x
city	x	x	x	x	x	x	x	x
field	x	x	x	x	x	x	x	x
class	x	x	x	x	x	x	x	x
city*field		x	x	x	x	x	x	x
city*class			x	x	x	x	x	x
field*year				x	x	x	x	x
class*year					x	x	x	x
inventor						x	x	x
city*year							x	x
firm								x

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>*panel e:* $log(AverageValue)$</u>								
$log(HHI_{cst})$	0.109***	0.033***	-0.002	-0.051***	-0.050***	-0.060***	0.005	-0.023***
	(-0.007)	(-0.009)	(-0.009)	(-0.009)	(-0.009)	(-0.008)	(-0.01)	(-0.008)
$log(ClusterSize_{cst})$	0.006	-0.147***	-0.166***	-0.177***	-0.142***	-0.178***	-0.094***	-0.093***
	(-0.007)	(-0.012)	(-0.012)	(-0.012)	(-0.012)	(-0.011)	(-0.015)	(-0.012)
obs	509984	509984	509984	509984	509984	509984	509984	506539
adj.R2	0.15	0.17	0.26	0.268	0.291	0.65	0.66	0.779
year	x	x	x	x	x	x	x	x
city	x	x	x	x	x	x	x	x
field	x	x	x	x	x	x	x	x
class	x	x	x	x	x	x	x	x
city*field		x	x	x	x	x	x	x
city*class			x	x	x	x	x	x
field*year				x	x	x	x	x
class*year					x	x	x	x
inventor						x	x	x
city*year							x	x
firm								x

Table 2.6: Product Market Channel

This table studies the effect of product market competitions on inventor output. Each column represents one dependant variable. For each regression, we use the most restrictive vector of fixed effects. $Competition_{cst}$ is defined as the inverse of HHI index for sales share of each firm in a 3-digit SIC industry. You can find the definition of the rest of the variables in Appendix table A1. The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the inventor level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	$log(\#Patents)$	$log(TotalCite)$	$log(AverageCite)$	$log(TotalValue)$	$log(AverageValue)$
$Competition_{cst}$	0.008***	0.006	-0.002	0.001	-0.007***
	(0.001)	(0.006)	(0.006)	(0.002)	(0.002)
$log(HHI_{cst}) * Competition_{cst}$	0.001**	-0.001	-0.002	-0.001	-0.001***
	(0.0003)	(0.0015)	(0.0014)	(0.0005)	(0.0004)
$log(HHI_{cst})$	0.030***	0.064*	0.033	-0.0108	-0.041***
	(0.008)	(0.034)	(0.032)	(0.011)	(0.008)
$log(ClusterSize_{cst})$	0.078***	0.155***	0.076	0.011	-0.068***
	(0.012)	(0.049)	(0.046)	(0.016)	(0.012)
year	x	x	x	x	x
city	x	x	x	x	x
field	x	x	x	x	x
class	x	x	x	x	x
city*field	x	x	x	x	x
city*class	x	x	x	x	x
field*year	x	x	x	x	x
class*year	x	x	x	x	x
inventor	x	x	x	x	x
city*year	x	x	x	x	x
firm	x	x	x	x	x
obs	483078	483078	483078	483078	483078
adj.R2	0.250	0.500	0.513	0.628	0.789

Table 2.7: Labor Market Channel

This table studies the effect of different labor market related factors on the number of patents an inventor produces each year. Each panel represents the effect of one channel on productivity of inventors. You can find the definition of the variables in Appendix table A1. The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the inventor level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel a: Exploratory Patents								
HHI_{cst}	-0.032***	-0.022***	-0.014***	-0.024***	-0.023***	-0.018***	-0.021***	-0.010***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
$ClusterSize_{cst}$	-0.056***	-0.085***	-0.085***	-0.083***	-0.074***	-0.074***	-0.030***	-0.027***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.006)	(0.006)
obs	571674	571674	571674	571674	571674	571674	571674	571674
adj.R2	0.073	0.077	0.118	0.118	0.129	0.228	0.232	0.271
year	x	x	x	x	x	x	x	x
city	x	x	x	x	x	x	x	x
field	x	x	x	x	x	x	x	x
class	x	x	x	x	x	x	x	x
city*field		x	x	x	x	x	x	x
city*class			x	x	x	x	x	x
field*year				x	x	x	x	x
class*year					x	x	x	x
inventor						x	x	x
city*year							x	x
firm								x

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel b: Exploitative Patents								
HHI_{cst}	0.007***	0.001	-0.001	0.010***	0.009***	0.008***	0.004	-0.001
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
$ClusterSize_{cst}$	0.032***	0.062***	0.060***	0.060***	0.051***	0.052***	0.016***	0.014**
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.006)
obs	571674	571674	571674	571674	571674	571674	571674	571674
adj.R2	0.064	0.066	0.091	0.092	0.105	0.166	0.171	0.178
year	x	x	x	x	x	x	x	x
city	x	x	x	x	x	x	x	x
field	x	x	x	x	x	x	x	x
class	x	x	x	x	x	x	x	x
city*field		x	x	x	x	x	x	x
city*class			x	x	x	x	x	x
field*year				x	x	x	x	x
class*year					x	x	x	x
inventor						x	x	x
city*year							x	x
firm								x

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>*Panel c: Team Size*</u>								
HHI_{cst}	0.027***	0.019***	0.010***	0.019***	0.018***	0.007***	0.006**	0.008**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
$ClusterSize_{cst}$	0.040***	0.060***	0.046***	0.047***	0.052***	0.039***	0.066***	0.063***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)
obs	932026	932026	932026	932026	932026	932026	932026	932026
adj.R2	0.180	0.185	0.229	0.231	0.238	0.478	0.479	0.478
year	x	x	x	x	x	x	x	x
city	x	x	x	x	x	x	x	x
field	x	x	x	x	x	x	x	x
class	x	x	x	x	x	x	x	x
city*field		x	x	x	x	x	x	x
city*class			x	x	x	x	x	x
field*year				x	x	x	x	x
class*year					x	x	x	x
inventor						x	x	x
city*year							x	x
firm								x

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel d: share of the top class from all filed patents								
HHI_{cst}	-0.003***	-0.005***	-0.005***	-0.006***	-0.006***	-0.006***	-0.005***	-0.006***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$ClusterSize_{cst}$	-0.012***	-0.023***	-0.025***	-0.025***	-0.023***	-0.023***	-0.016***	-0.017***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
obs	932026	932026	932026	932026	932026	932026	932026	932026
adj.R2	0.092	0.094	0.107	0.108	0.112	0.205	0.206	0.209
year	x	x	x	x	x	x	x	x
city	x	x	x	x	x	x	x	x
field	x	x	x	x	x	x	x	x
class	x	x	x	x	x	x	x	x
city*field		x	x	x	x	x	x	x
city*class			x	x	x	x	x	x
field*year				x	x	x	x	x
class*year					x	x	x	x
inventor						x	x	x
city*year							x	x
firm								x

3 Do Managers Time Patent Filings?

3.1 Introduction

Starting from Schumpeter's creative destruction theory, a large body of economic literature, posits that innovation plays a key role for growth and survival of firms. Firms try to guard their intellectual resources by getting protection in the form of patent grants. As a result, it is natural to assume that firms should try to obtain this protection at the earliest, and therefore apply for a patent right upon completion of their innovation projects. Therefore, we would expect to see a uniform distribution of patent applications over a calendar year. However, we find that this is not the case, and 40% of overall applications are filed in the last quarter of the year.

We argue that there is a strategic element in the timing of patent applications by firms, which we denote as patent window dressing. Although existing finance literature has largely focused on window dressing by pension and mutual fund managers (Lakonishok et al (1991), Agarwal et al (2014)), there is evidence that corporate managers engage in window dressing, too. For example, Allen and Saunders (1992) find evidence of a systematic upward window dressing adjustment in bank assets on the last day of each quarter. Chen, Cohen, and Lou (2016) demonstrate that managers of multi-segment conglomerates position their firms as part of a certain industry when that industry is more favorable than the others. Extensive evidence of earnings management is further consistent with the idea that firms window dress their accounting performance. If managers try to window dress their accounting statements, it is also possible that they will try to window dress firms' intangible assets that are relevant to market participants but are not captured by accounting statements.

If patent clustering is truly an attempt to window dress performance, there should be a reward for managers. Existing studies show that investors do pay attention to patent applications. Kogan et al (2017) for instance, claim that firms publicize their patent application even before patent grant and investors price this news. Consequently, managers who are motivated to increase the value of their firms during certain periods, are motivated to window dress their patent applications.

We start our analysis by disentangling the window dressing explanation, according to which firms strategically time the filing of patent applications by postponing the filing of completed applications and rushing the filing of uncompleted ones, from alternative explanations. For example, firms may routinely use calendar end-year as project deadlines (which, in turn, could be related to Christmas bonuses, work slowdowns during to winter holidays, or psychological desire to finish old projects before "new beginnings" of the new year). As a result, clustering of patent application in the last quarter may reflect some general features of organizational work planning, and indicate completion of existing R&D projects that is not driven by timing.

To establish window dressing as the underlying mechanism, we perform several tests. First, we look at the quality of patents that are filed during each fiscal quarter. If firms use patent applications to window dress their R&D performance, they may choose to rush some projects instead of spending more time on polishing the application and improve its quality. Therefore, we expect the quality of fourth quarter patents to be considerably lower than patents that are filed during the other quarters. Hence, we introduce three variables that capture quality. First, the adjusted citation count (Hall, Jaffe & Trajtenberg, 2001) measures the number of citations received by a patent after correcting for truncation lag. We see there is a significant drop in the citations received by patents filed for in the 4th quarter. Next, we look at a measure of breakthrough innovation, Top10, (Byun, et. al, 2020). We select these measures as they capture the relative importance of the patents in their given cohort. This indicator variable is equal to 1 if the citations received by the patent falls in the top 10% of the citations received by all patents filed each year. Both of these citation measures are all corrected for the truncation lag based on the fixed-effect approach by Hall, Jaffe and Trajtenberg, 2001. Given these measures, we look at the probability that a given patent is a Top10 patent. The result shows a clear trend that, patents filed in the last quarter are less likely to be breakthrough innovations. The higher than usual patent applications in the 4th quarter, coupled with a significant drop in patent quality, are consistent with window dressing explanation.

Second, we exploit a quasi-natural experiment that has exogenously increased the costs of window dressing. Before March 2013, the strategy of postponing patent filing was a low risk one. [6] Under the "first to invent" legal principle, the patent was granted to the firm that could show it was the first to conceive and develop a prototype, and not to the firm that was the first to file the patent application. In 2013 the US patent system switched over to a "first to file" system following Leahy–Smith America Invents Act (AIA for short), so that in the case of multiple applications of the same innovation the patent was granted the entity that has filed the application first. This legal change has increased the costs of window dressing. When firms try to file for patent applications that are not fully baked, the chances of rejection are high. The time gap between which a firm is informed of patent rejection and when it revises the patent application for the next filing gives rivals an opportunity to file for the same patent application and exploit "first to file" status. This in turn, induces a huge cost on firms that apply for a patent that is not fully ready. Consequently, AIA implementation offers a quasi-natural experiment to test whether the clustering of patent applications in the 4th quarter is a deliberate attempt on the part of firms to window dress their innovation portfolio. We find that post-AIA there has been a decrease in patent applications in the last quarter. We also find that the quality of last quarter patents has improved.

After establishing the window-dressing incentives behind 4th year patent clustering, we dive into understanding its details. First, firms are motivated to end their fiscal year on a positive note. We call this a fiscal year hypothesis. Second, firms also try to show improvements in performance before the calendar year-end. This leads to higher stock valuation for the firm and gives their institutional shareholders better performance at the end of a calendar year. We call this calendar year hypothesis. When we look at the public firms with fiscal year-end other than December, any high level of patenting at fourth fiscal quarter could only stem from the second hypothesis i.e. the calendar year effect. Our results show the presence of patent window dressing but at lower significance. It means that compared to other firms, firms with December fiscal year-end are much

[6] It is imperative to note that firms cannot window dress their patent grant date. A patent is granted by the United States Patent and Trademark Office (USPTO) at the end of patent examination process that can take between 1.5-3 years on average.

more motivated to window dress their patenting behavior as it kills two birds with one stone.

Next, we ask what types of firms engage in window dressing their innovation. If patent application clustering is truly a window dressing behavior, we should see that the extent of this behavior is negatively affected by external monitoring and positively- by information asymmetry (Degeorge et al (1999)). One of the key players monitoring firms and generating information are analysts. Irani and Oesch (2016) show that reduction in analyst coverage leads to lower quality of financial reporting and higher usage of discretionary earnings. Building upon prior literature, we hypothesize that if firms actively manipulate patent filings, stronger external monitoring should reduce this behavior. Indeed, we find that there is a reduction in the 4th quarter patent applications when more analysts track the firm. Using the number of analysts that follow a firm as a source of external monitoring and a rich set of firm level control variables, we see that firms reduce their patent clustering during their last fiscal quarter significantly when they are followed by more analysts.

Finally, we look at another powerful external monitor - Institutional investors are more sophisticated than retail investors and monitor firms more extensively. Specific to our question, existing evidence demonstrates that institutional ownership is associated with higher-quality reporting, as evident in better earnings quality (Velury and Jenkins (2006) and lower fraud probabilities Sharma (2004); Chen et al (2006)). Following the literature, we hypothesize that there is a negative correlation between firms' institutional ownership and patent clustering in the 4th quarter. We construct our independent variable as the sum of all shares owned by institutions divided by the overall shares of a firm to capture the intensity of institutional ownership. Using panel data with a rich set of fixed effects, we see that institutional investment reduces patent clustering in the fourth quarter of each year.

This paper adds to several strands of literature. First, we identify another dimension of window dressing. Several papers show that fund managers manipulate and alter their portfolios to attract more investors by selling losing shares and buying winning stocks at the end of each reporting period (Lakonishok et al (1991)). This is common across different fund types and asset classes (He, Ng and Wang (2004); Morey and O'Neal (2006)). Window dressing, however, is not limited to funds. Firms can also window dress by real earnings management through manipulating R&D (Baber et al (1991), Dechow and Sloan (1991)). Burgstahler and Dichev (1997) and Degeorge, et.al (1999) show that firms actively manage their earnings to pass certain thresholds. Overall, these papers show that window dressing is also practiced at the firm level to manipulate investor sentiment. Although previous papers have identified firm managers' patterns of earnings management and its relationship with other firm related factors, little has been done with respect to market manipulation through patent filing. Second, we add to innovation literature by identifying firms' pattern of timing the market through patent filing. Innovation literature relies heavily on the assumption that date of patent application is the date when firms achieved an innovation. In this paper, we show that patent filling date is also a function of market considerations.

The rest of the paper is as follows: in section 3.2 we describe our data and variables, in section 3.3, we explain our empirical methodology. After that, we present our findings in section 3.4. In section 3.5, we list our robustness methods and results. Finally, we conclude our paper and summarize our contribution and findings in section 3.6.

3.2 Data and Variables

Our sample is based on the publicly available patents data from USPTO and spans the period of 1976 – 2020. USPTO's PatentsView project provides data on all granted patents including the applications' filing date. We use this data to compute the measures of patent quality. Our first measure is the adjusted citation counts received by the patent. To mitigate the problem of citation truncation we follow the fixed-effect procedure by Hall, Jaffe and Trajtenberg, 2001. Our other measure of patent quality is an indicator variable for breakthrough innovation (Byun, et. al 2020). We identify a patent as a breakthrough, or Top 10 patent, if the citations received by that patent is in the top 10% of all citations received by all patents in that year. By focusing on these measures, we can identify patents which allow more future innovation and hence have a higher technological value. The citation distribution is corrected using the fixed effect approach as before. There are still possible truncation issues within a year. This means that patents that are applied for in first months of a calendar year have more time to get cited compared to the patents filed during the last months of the same year. To address this issue, we use a modified year-quarter fixed effect approach as well where we deflate the citation by the year-quarter average. The results are not materially different.

We use the dataset available from Kogan, Papanikolaou, Seru and Stoffman (2017) (hereafter KPSS) to match patents with their public firm owners. KPSS employs a disambiguation algorithm to find PERMNO of each public patent owner using the name of the patent owners and matching them with the names of the firms in CRSP. We can then match our patent dataset with Compustat firms and get the firm level accounting performance data, as well as the fiscal year-ends. After matching the KPSS data with USPTO, we are left with 1,676,630 patents for which we have identified a firm. We next aggregate the patent data at the firm level. We end up with 92,764 year-quarter observations for firms. We list the summary statistics at patent level and aggregate firm level in table 3.1 Panels A and B, respectively. [insert table 3.1 here] On average, each firm in our sample applies for 15.206 patents in each quarter with a median value of 3 patents. Patents in our sample receive on average 11 citations. Similarly, firms have on average 2.3 Top 10 patents. That indicates that about 15.1% (2.296/15.206) of patents applied by public firms during that period are breakthrough innovation based on the Top 10 measure. We also calculate the innovation intensity for a given firm in each quarter by calculating the fraction of patents applied as follows:

$$TotalFraction = \frac{Total\ Patents\ applied\ for\ in\ the\ Quarter}{Total\ Patents\ applied\ for\ in\ the\ Year} \tag{3.10}$$

Summary statistics for Total Fraction indicate that around 40% of all patents are filed during the 4th quarter. The fact that this value deviates so much from 25% indicates that patents are not distributed equally across quarters. To calculate the innovation quality, we calculate similar fractions for the breakthrough measures as follows:

$$Top\ 10\ Fraction = \frac{Total\ Top\ 10\ Patents\ in\ the\ Quarter}{Total\ Patents\ applied\ for\ in\ the\ Quarter} \tag{3.11}$$

To study the motivations behind the window dressing effect, we make use of data on analysts and institutional ownership. First, we use IBES analyst data to study how many analysts follow firm i at each particular time t (Harris and Marston (1992), Frnakel, Kothari and Weber (2006) and Mohanram et al (2020) are a few examples of papers that use this database). Second, we also use Thomson Reuters' institutional investor 13F data to examine the fraction of institutional ownership for each firm.

3.3 Empirical Methodology

To study the patent application pattern, we start with the individual granted patents in our sample. To establish the patent clustering phenomenon econometrically, we aggregate the total patents applied for in each quarter and run a regression on quarter dummies with the following specification:

$$Total\ Patents_q = \alpha + \sum_{k=2,3,4} \beta_k \times QtrDummy_{k,q} + \varepsilon_q \tag{3.12}$$

Here q represents the year-quarter observation based on calendar quarter. $Total\ Patents_q$ are the total patents applied for in the given quarter, q. $QtrDummy_{k,q}$ represents the k quarter dummy (k = 2, 3 & 4) for the q quarterly observation and is equal to 1 if the given q quarter observation is the k calendar quarter. β_k represents the coefficient of the quarter dummy. We add the application year fixed effect to address the concern that the overall number of patents produced has been gradually increasing over time, so that an indicator variable for the fourth quarter may capture this pattern.

Next, we analyze the quality of these patents by looking at the adjusted citations received by the patent. Since the adjusted citation measure is available at the patent level, we don't aggregate the citations and can run the following regression:

$$Adjusted\ Citations_i = \alpha + \sum_{k=2,3,4} \beta_k \times QtrDummy_{k,i} + \varepsilon_i \quad (4) \tag{3.13}$$

Here i represents the i patent. $QtrDummy_{k,i}$ represents the k quarter dummy (k = 2, 3 & 4) for the i patent and is equal to 1 if the given patent was applied for in the k calendar quarter. β_k represents the coefficient of the quarter dummy. Application year fixed-effect is included to control for year-to-year differences in patent quality. As there can be fundamental time-invariant differences in the firms applying for these patents, we include the firm fixed-effect and cluster the standard errors at the firm level.

To further highlight that these patents are truly different in quality, we look at an alternate measure of quality, the Top 10 indicator variable. Top 10 is equal to 1 for a given patent, if the patent has received enough citations to be in the top 10 percentile of all patent citations applied for in the given year. These measures allow us to discern the relative quality of the patents being applied for. We run a logistic regression for these measures, specified as follows:

$$Top\ 10\ dummy_i = \alpha + \sum_{k=2,3,4} \beta_k \times QtrDummy_{k,i} + \varepsilon_i \tag{3.14}$$

As before, i refers to the patent i. $QtrDummy_{k,i}$ represents the k quarter dummy (k = 2, 3 & 4) for the patent i and is equal to 1 if the given patent was applied for in the k calendar quarter. β_k represents the coefficient of the quarter dummy.

To study the firm's behavior, we aggregate the patent level measures at the firm level. Importantly, we now change the definition of a quarter from calendar year to that of fiscal year for a firm. That is because different firms have different fiscal year-ends. By looking at the quarterly data based on a given firm's fiscal year-end, we can separate the calendar year and fiscal year effects. We look at the relative distribution of patent applications of a firm in a given fiscal year by using the following regression:

$$Total\ Fraction_{i,q} = \alpha + \sum_{k=2,3,4} \beta_k \times QtrDummy_{k,i,q} + Controls_{i,q-1} + \varepsilon_{i,q} \tag{3.15}$$

Here i refers to the ith firm. q is the qth fiscal quarter observation for the firm. $Total\ Fraction_{i,q}$ is the sum of patents applied for by a firm in a given fiscal quarter scaled by the sum of all patents applied for by the firm in the same fiscal year. $QtrDummy_{k,i,q}$ represents the quarter dummy k (k = 2, 3 4) for firm i and is equal to 1 if the given firm quarter observation is the fiscal quarter k. β_k represents the coefficient of the quarter dummy. $Controls_{i,q-1}$ are the set of standard control variables for the i firm for the fiscal quarter $q-1$ observation. We define similar regression models for the patent quality measures.

To understand this phenomenon more and identify possible channels, we look at several factors. Specifically, we study how patent clustering during fourth quarter is affected by vigilante external monitoring, namely institutional shareholders, and analysts' coverage. The regression below represents our approach.

$$4Qtr\ Fraction_{i,t} = \ \beta_1 \times Analyst_{i,t-1} + Controls_{i,t-1} + \alpha_i + \alpha_{j,t} + \varepsilon_{i,t} \tag{3.16}$$

We define 4Qtr Fraction as the number of patents applied in the last fiscal quarter of each fiscal year t divided by the number of patents filed during that fiscal year. $Analyst_{i,t-1}$ is the number of analysts that follow firm i in fiscal year t-1. We augment our regressions by adding firm and industry year fixed effects. We also use firm level control variables lagged one year. You can find the construction method of these variables in Appendix. We also try other specifications by replacing industry-year fixed effects with year fixed effects and/or removing control variables.

We follow the same procedure to study the effect of institutional ownership on patent clustering.

$$4Qtr\ Fraction_{i,t} = \beta_1 \times Institute_{i,t-1} + Controls_{i,t-1} + \alpha_i + \alpha_{j,t} + \varepsilon_{i,t} \tag{3.17}$$

The specification description remains the same as the model before with the exception that the main independent variable has changed. $Institute_{i,t-1}$, our main independent variable is the number of shares institutional shareholders hold scaled by the overall number of shares both calculated at the end of fiscal year t-1.

3.4 Empirical Findings

The signs of patent clustering are first evident when we look at the individual patent application behavior. Table 3.2 shows the results at the patent level.

[table 3.2 here]

Column 1 shows the results of quarterly patent applications regressed on the quarter dummies, adjusted for application year fixed effect for the 179 quarters in our sample period. By controlling for the application year, we remove the year-to-year changes in application behavior as well as demean the observations by the quarterly average over the calendar year. We see that for the 4th quarter, there are on average 894 more patent applications over the 1st quarter which is both larger than any other quarter coefficient and statistically significant. The next largest quarter is the 2nd quarter with 580 more patent applications over the 1st quarter. The 1st and 3rd quarter are not statistically different. Thus, the 4th quarter establishes itself as the dominant quarter for patent applications being almost 55% (894/580) larger than the next highest quarter.

Next, we look at the quality of these patents. Column 2 shows the citations received by an individual patent adjusted for truncation lag. We find that the increased patenting activity is associated with a significant drop in quality. 4th quarter patents received 0.1159 fewer citations than patents in the 1st quarter. The 2nd and 3rd quarters experienced similar drops in citations received but, the drop was largest for the 4th quarter being almost 54% (-0.1159/-0.075) larger than the next largest drop in citations which was for the 3rd quarter. As the patents in our sample represent those which were identified to a firm, we also repeat the regression with controls for firm fixed effect and cluster the standard errors at the firm level. This helps us control for any firm specific characteristics which might be driving these results. We also add the application year fixed effect. The results are in column 3, which are nearly identical in nature. The 4th quarter still has the largest drop in citations being almost 44% (-0.1393/-0.0969) larger than the next largest drop.

We repeat the same steps for our other 2 measures of patent quality, namely Top 10 patent dummies. The results of the logistic regressions further cement the trend we saw earlier. The probability for a patent to be a top 10 patent is negatively affected the most for the 4th quarter being approximately 14.4%. The next lowest quarter is the 3rd quarter with a probability of 16.1%. That is a 1.7% drop in probability in moving from the 3rd to the 4th quarter. Hence, we see that firm patents were disproportionately applied for in the last quarter with an associated drop in patent quality.

But, to truly see the firm patenting behavior and study fiscal behavior, we need to aggregate these patents measures at the firm level and analyze if these trends continue. The result of this analysis is presented in table 3.3.

[insert table 3.3 here]

Column 1 shows the fraction of patent applications, regressed on the quarter dummies based on fiscal year ends. We see that the 4th fiscal quarter patent application fraction is larger and statistically significant with a value of 0.4051, a 2.3% (0.009/0.3961) increase over the 1st quarter value. This means that for firms with 4th quarter patent applications in a fiscal year (23,190 fiscal quarter observations in our sample), the 4th quarter patents represent over 40% of the yearly applications on average when present for the firm. The 2nd quarter patent application is statistically not different from the 1st quarter, while there is a drop in the fraction value for the 3rd quarter. The 4th quarter effect is present even when we control for firm and fiscal year fixed effects. The results are in column 2, which show that the 4th quarter coefficient is positive and statistically significant with a value of 0.0085.

The other columns in the table show the quality of these patent applications in the respective quarters. The average adjusted citations received by the patents in each quarter for the firm is given in column 3. We see that unlike quarter 1, 2 and 3, there is a significant drop in the citations received by the 4th quarter patents. The coefficient for the 4th quarter is negative and statistically significant with a value of -0.0602. This represents a 5% (0.0602/1.2525) reduction in citations received on average by patents filed by firms in the 4th quarter. Even when we control for firm fixed effects and fiscal year fixed effects, column 4, the 4th quarter coefficient is negative, statistically significant, and more than twice as large as the next coefficient showing a significant drop in citations received by 4th quarter patent applications.

A similar story plays out when we look at the measure of breakthrough innovation at the firm level. Column 5 shows that on average considerably lower number of patents applied for by a firm in the 4th quarter end up being a top 10 patent, that is almost 7.9% (0.0164/0.2088) lower than the 1st quarter value. Controlling for firm and fiscal year fixed effect does not remove this trend, with a negative coefficient value of -0.006 which is statistically significant for the 4th quarter.

We now turn our focus onto the external factors which contribute to the different levels of patent window dressing among firms. Given that we see firms have a large negative relation between high patent filing in the last quarter and the quality of the patents, we hypothesize that increased external monitoring should have an adverse impact on the intensity of this patent clustering phenomenon. We first look at analyst coverage that firms receive as a source of external monitoring. As the number of analysts that follow a firm increase, the fraction of patents applied by the firm in the last quarter should decrease. We test this hypothesis in a regression setting and the results are presented in table 4.

[insert table 3.4 here]

From column 1, we see that there is a strong negative relationship between the number of analysts that follow a firm and the fraction of the patents applied by the firm in the 4th quarter. The coefficient for the independent variable Analysts is -0.006. From table 3.1 we know the average firm in our sample has about 4.64 analysts following the firm, which means there is a drop of 0.0278 (4.64 x 0.006) in the fraction of applied patents in the 4th quarter. We run the same regression with different specifications for fixed effects. The results are shown in columns 2, 3 and 4. The coefficient for the number of analysts remains negative and statistically significant. Thus, we see that with increased external monitoring, firms tend to reduce their 4th quarter applications.

We follow a similar strategy to see if internal monitoring has the same perceived effect on patent clustering as external monitoring. To this effect, we use the number of institutional shareholders as a proxy for the level of internal monitoring. The results are presented in table 3.5.

[insert table 3.5 here]

From column 1, we see that there is a strong negative effect of institutional ownership on patent clustering by firms. The coefficient for the independent variable Institute, which is the number of shares held by institutional shareholders scaled by the total common shares outstanding, is negative at -0.103 and statistically significant. The average firm in our sample has an institutional shareholding of 0.48. That translates to a drop in 4th quarter patent application fraction of 0.049. We run the same regression with different specifications for fixed effects. The results are shown in columns 2, 3 and 4. The coefficient for the institutional ownership variable remains negative and statistically significant. Thus, we see that with increased investor monitoring, firms tend to reduce their 4th quarter applications.

3.5 Further Discussions

In the previous section, we see that patent applications in the last quarter represents a major proportion of the overall patent applications for firms and these patents have lower citations received and are less likely to be breakthrough innovations. Here, we intend to see whether window dressing exists for firms with non-December fiscal year-end. To check this, we use a sub-sample of our firms which have non-December fiscal year end. The results are presented in table 3.6.

[insert table 3.6 here]

From column 1, we see that for firms with fiscal years not ending in December, the 4th quarter is no longer significantly different from the 1st quarter. The 4th quarter fractional patent application turns out to 0.4168 which is higher than the value for all firms, 0.4051. The 2nd and 3rd quarters receive lower patent applications overall. Column 2 shows that when we control for firm and fiscal year fixed effects the 4th quarter is positive and slightly statistically significant. The coefficient for the 4th quarter is 0.0062, compared to 0.0085 for all firms. Columns 3-8 represent our analysis of patent quality. Columns 3 and 4 show that the average quality of the patents based on citations is not significantly different among the 4 quarters. Column 5 shows the same result for top 10 breakthrough patents with not significant differences between the quarters. There is an increase in top 10 patents when we account for firm and fiscal year fixed effects, from column 6, for quarters 3 and 4. So, with these set of results we can attribute the patent clustering phenomenon mostly to the calendar year effect. That suggests that firms strategically time patent applications when investors need better performance from them.

The window dressing hypothesis assumes that there are no or small costs associated with showing this behavior. If costs of window dressing go beyond a certain threshold, managers might stop doing window dressing. To check the existence of costs in patent window dressing, we make use of a quasi-natural experiment. In March 2013, the America Invents Act (AIA) came into full effect. With the switch from first-to-invent to first-to-file system, firms now faced a costly choice whether to time their patents. If prior to AIA, firms strategically delayed their lower quality patents to wait until the year-end to pad their innovation portfolio, after AIA we expect the last quarter to no longer have the same fraction of patent applications. To test this out, we use the following model specification:

$$\begin{aligned} Total\ Fraction_{i,q} = \ & AIA_q + \sum_{k=2,3,4} \beta_k \times QtrDummy_{k,i,q} + \\ & \sum_{l=2,3,4} \gamma_l \times AIA_q \times QtrDummy_{l,i,q} + Controls_{i,q-1} + \varepsilon_{i,q} \end{aligned} \tag{3.18}$$

Here i refers to the ith firm. q is the qth fiscal quarter observation for the firm. $Total\ Fraction_{i,q}$

is the sum of patents applied for by a firm in a given fiscal quarter scaled by the sum of all patents applied for by the firm in the same fiscal year. AIAq is a dummy variable which is equal to 1 if the current quarter is after March 2013. $QtrDummy_{k,i,q}$ represents the kth quarter dummy (k = 2, 3 & 4) for the ith firm and is equal to 1 if the given firm quarter observation is the kth fiscal quarter. β_k represents the coefficient of the quarter dummy. l is the coefficient for the interaction term between the AIA dummy and the lth quarter dummy. $Controls_{i,q-1}$ are the set of standard control variables for the ith firm for the q-1th fiscal quarter observation. We define similar regression models for the patent quality measures. The results for these regressions are presented in table 3.7.

[insert table 7 here]

Column 1 shows the pre and post AIA fractional patent applications for the quarters. We see pre-AIA the 4th quarter had 0.4162 of all patent applications, whereas post-AIA this fractional amount drops to 0.344. This represents a 17.35% (0.0722/0.4162) drop. This clearly shows that once the cost of timing patent applications is introduced to managers objective function, they can no longer inflate applications during the fourth quarter. Column 2 shows that pre-AIA the 4th quarter contributed 0.0190 towards the fractional patent applications, while post-AIA this went down to -0.0546. Next, we look at the quality of these patents. From column 3 we can see that pre-AIA 4th quarter patents received on average 1.2749 citations, while post-AIA this value is 0.7739. However, it is difficult to say clearly that this trend is purely a quarter effect as one possible reason can be that the fixed effect approach to citation adjustment hasn't completely corrected for truncation lag. This was not an issue in the earlier regressions as we weren't comparing two sub-samples directly, as we do here with the pre and post AIA periods. Hence, we must couple the citation result with other measures of quality. That is exactly what we do with the top 10 breakthrough measure. Column 5 shows that pre-AIA 0.1614 or about 16.14% of patents filed in the 4th quarter make it to the top 10 percentile of citations received in a year, whereas post-AIA 0.3454 or about 34.45% of patents are top 10 patents. This shows a reverse trend where the application in the 4th quarter is going down post-AIA while the quality goes up. As such we can conclude that firms have strategically applied for patents towards the end of the calendar year in a bid to window-dress their intellectual capital and future potential when they had the opportunity of making this choice with lower costs.

3.6 Conclusions

In this paper we show the window dressing effect in the innovation context for firms. We first look at the overall patent filings for firms and show that there is an increase in patents applications in the last quarter of the calendar year. This increase in patents is accompanied by a drop in patent quality. The last quarter patents receive fewer citations and are less likely to be breakthrough innovations. We also study the patent portfolio at the firm level. Again, we find that firms make patent applications in the last quarter which represents a large portion of their yearly patent applications. This ties in with our previous observation of more patents being filed in the last quarter. On average 4th quarter patents represent almost 40% of the total yearly patent filings. However, the quality of these patents deviates from the patents filed in other quarters. These last quarter patents receive fewer citations and are less frequently breakthrough patents.

We provide further evidence that this behavior is linked to window dressing on the part of firms by examining the effect of increased monitoring. When there is more monitoring proxied by higher presence of institutional shareholders there is a drop in the fraction of patents applied for in the 4th quarter. A similar result holds when we look at increased external monitoring as proxied by the number of analysts following the firm. This behavior suggests firms may be exploiting the information asymmetry to appear more innovative than they are.

Finally, we provide robustness tests to show that the window dressing behavior is indeed present. First, we look at firms with non-December fiscal year ends and check whether they show a similar behavior. We do not find any evidence that these firms have the same higher 4th quarter patent filings with an associated drop in quality. Next, we look at the patent application behavior in a quasi-natural experiment setting. Specifically, we take the adoption of the America Invents Act

(AIA) in March 2013 as an external shock which increases the costs of holding out on patents for strategic timing benefits. We find that post AIA firms see a decrease in the 4th quarter patent filings and an increase in the quality of these patents overall. This strongly suggests that patent applications were not a random phenomenon. Instead, there had been a strategic aspect to when to file a given patent.

Our findings add to the current literature by showing an additional dimension to window dressing by firms. Firms exploit information asymmetry and pass off being more innovative, with a larger intellectual asset base than time would suggest. They try to pass off more quantity as a substitute for quality right when more eyes are on them. Investors and other stakeholders should consider the possibility of window dressing by firms in the innovation context and integrate this possibility when making decisions accordingly.

Table 3.1: Descriptive Statistics

This table reports summary statistics of measures for all granted patents at the patent and firm level respectively. Panel 1A summarizes the characteristics of patents granted from 1976-2020. Citations is the total number of citations received by a patent. Panel 1B lists summary stats for public and innovative firms with at least one patent granted from 1976-2020. All firm level variables are observed quarterly with quarters defined by the fiscal year. Panel 1C lists summary stats for public and innovative firms with at least one patent granted from 1976-2020. All firm level variables are observed at fiscal year. Please refer to Appendix to find variable description. All continuous variables are winsorized at 1% and 99%.

	Mean	Median	1st Qu.	3rd Qu.	SD	Obs
Panel 1A – Patent Level Variables – Granted Patents						
Citations	11.228	3	0	11	23.510	1,676,630
Adjusted Citations	0.943	0.304	0	0.940	1.883	1,676,630
Top 10	0.166	0	0	0	0.372	1,676,630
Panel 1B – Firm Level Variables – Quarterly Level Data						
Average Citations	1.230	0.716	0.322	1.412	1.710	92,997
Total Patents	15.206	3	1	9	42.150	92,997
Top 10 Patents	2.296	0	0	2	6.183	92,997
Interest Burden	0.263	0.174	0.055	0.384	0.275	92,764
Book-To-Market	0.629	0.610	0.396	0.840	0.299	92,764
R&D/Sales	0.465	0.032	0	0.138	2.257	92,764
LTDI	0.795	1	1	1	0.404	92,764
Log Sales	5.170	5.208	3.585	6.786	2.365	92,764
Total Fraction	0.396	0.302	0.213	0.500	0.270	92,997
Top 10 Fraction	0.201	0	0	0.250	0.327	92,997
AIA Dummy	0.167	0	0	0	0.373	92,764
Panel 1C-Firm Level Variables-Annual Level Data						
4Qtr Fraction	0.400	0.325	0.222	0.500	0.270	16,545
Analysts	4.640	3.000	0.000	7.000	5.380	16,545
Institute	0.480	0.547	0.159	0.764	0.320	16,545
Cash	0.260	0.181	0.062	0.400	0.250	13,782
ROA	0.080	0.122	0.053	0.179	0.200	13,749
Leverage	0.180	0.150	0.010	0.285	0.180	13,887
Tobin's Q	2.030	1.719	1.275	2.478	1.050	12,170
Log size	7.470	7.352	5.932	9.023	2.140	13,281
R&D	0.100	0.055	0.017	0.122	0.130	13,955
Capex	0.050	0.036	0.020	0.064	0.040	13,673
PPE	0.200	0.160	0.078	0.284	0.170	13,933

Table 3.2: Patent Applications and Quality of Patents

This table reports the regression results of Total Patents, Adjusted citations, Top 10 Patents on the calendar quarter that a patent is applied in. Sample period is from 1976-2020 that includes USPTO's whole universe. column 1 observations are at the calendar quarter level while observations for column 2-5 are at the patent level. Columns 4 and 5 are logistic regressions. Variable descriptions are listed in Appendix. All continuous variables are winsorized at 1% and 99%. Numbers in parenthesis are t-statistics and ***, ** and * denote 1%, 5% and 10% statistical significance, respectively.

	Total Patents	Adjusted Citations		Top 10	
	(1)	(2)	(3)	(4)	(5)
Qtr2	580.2222**	-0.0173***	-0.0436***	-0.072***	-0.056***
	(2.4244)	(-3.9993)	(-6.0547)	(-12.650)	(-5.477)
Qtr3	363.7333*	-0.0752***	-0.0969***	-0.175***	-0.128***
	(1.7529)	(-17.6667)	(-11.6306)	(-29.810)	(-13.153)
Qtr4	893.9549***	-0.1159***	-0.1393***	-0.309***	-0.205***
	(3.7402)	(-28.0545)	(-11.7505)	(-52.207)	(-15.179)
Observations	179	1,676,630	1,676,630	1,676,630	1,676,630
Application Year FE	YES		YES		YES
Firm FE			YES		YES
Clus. SE			FIRM		FIRM
Adjusted R2	0.9747	0.0006	0.1166		
Log Likelihood				-752463	
Deviance				1504926	1132556

Table 3.3: Public Firms' Innovation Activity and Quality

This table reports the regression results of firm level patenting activity and quality measures applied for by a firm in the fiscal quarter. Sample period is from 1976-2020. The sample covers the universe of public firms from Kogan, Papanikolaou, Seru, Stoffman (2017). Total Fraction is the total patents applied in a quarter scaled by the total patents applied for by the firm in the fiscal year. Average Citations is the average of adjusted citations received by all patents applied for by the firm in each quarter. Top 10 Fraction is the total patents which are above the 90th percentile of the citation distribution of granted patents in the calendar year, in a quarter scaled by the total patents applied for by the firm in the given quarter. Qtr2, Qtr3 and Qtr4 are dummy variables which are 1 if the fiscal quarter is the 2nd, 3rd and 4th quarter of the fiscal year respectively. All continuous variables are winsorized at 1% and 99%. We use a vector of control variables for specifications 2,4 and 6. These variables are Interest Burden, R&D/Sales, Book-to-market ratio, Long-term Debt Indicator and Log Sales. Details of control construction are in the Appendix. Numbers in parenthesis are t-statistics and ***, ** and * denote 1%, 5% and 10% statistical significance, respectively.

	independent variable					
	Total Fraction		Average Citations		Top 10 Fraction	
	(1)	(2)	(3)	(4)	(5)	(6)
Qtr 2	0.0012	-0.0012	-0.0108	-0.0350***	-0.0036	-0.0013
	(0.4933)	(-0.5943)	(-0.6759)	(-2.7334)	(-1.1633)	(-0.5585)
Qtr 3	-0.0108***	-0.0103***	-0.0203	-0.0468***	-0.0092***	-0.0018
	(-4.2831)	(-5.3244)	(-1.2605)	(-3.8131)	(-3.0062)	(-0.8440)
Qtr 4	0.0090***	0.0085***	-0.0602***	-0.0980***	-0.0164***	-0.0060***
	(3.5402)	(3.8693)	(-3.8160)	(-7.9868)	(-5.4279)	(-2.6202)
Observations	92,997	92,659	92,997	92,659	92,997	92,659
Controls		YES		YES		YES
Fiscal Year FE		YES		YES		YES
Firm FE		YES		YES		YES
Clus. SE		FIRM		FIRM		FIRM
Adjusted R2	0.0007	0.4372	0.0001	0.3928	0.0003	0.4411

Table 3.4: Analyst Coverage and Patent Window Dressing

This table reports the regression results of the effect of the number of analysts on the 4th quarter patent application as a fraction of total patent applications in a fiscal year. The sample is based on publicly traded patenting firms over 1976-2015 period. The dependent variable 4Qtr Fraction is the number of applied patents in the fourth quarter divided by the number of patents applied for throughout the fiscal year. The variable Analysts is the number of analysts that follow firm i in year t and is constructed using IBES analysts' data. We use a vector of control variables in specifications 2 and 4. These control variables are Cash, ROA, Leverage, Tobin's Q, Log Size, RD, Capex and PPE. See the Appendix for the description of control variables. We augment the regressions with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the firm level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Analysts	-0.006***	-0.006***	-0.005***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	16,537	16,545	12,011	12,007
Controls		YES		YES
Firm FE	YES	YES	YES	YES
Year FE	YES		YES	
SIC * Year FE		YES		YES
Adjusted R2	0.372	0.361	0.374	0.378

Table 3.5: Institutional Shareholders and Patent Window Dressing

This table reports the regression results of the effect of the number of institutional shareholders on the 4th quarter patent application as a fraction of total patent applications in a fiscal year. The sample is based on publicly traded patenting firms over 1976-2015 period. The dependent variable 4Qtr Fraction is the number of applied patents in the fourth quarter divided by the number of applied patents applied for throughout the fiscal year. The variable institute is the number of shares institutional shareholders hold scaled by the overall number of shares both calculated at the end of fiscal year t-1. Control variables are Cash, ROA, Leverage, Tobin's Q, Log Size, RD, Capex and PPE. See the Appendix for the description of control variables. We augment the regressions with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the firm level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Institute	-0.103***	-0.105***	-0.027**	-0.021*
	(0.012)	(0.011)	(0.012)	(0.013)
Observations	16,537	16,545	12,011	12,007
Controls		YES		YES
Firm FE	YES	YES	YES	YES
Year FE	YES		YES	
SIC * Year FE		YES		YES
Adjusted R2	0.374	0.362	0.371	0.376

Table 3.6: Effect of Lower Exposure to Innovation Information

This table reports the regression results of firm level patenting activity and quality measures applied for by a firm in the fiscal quarter for firm with non-December fiscal year ending. Sample period is from 1976-2020. Total Fraction is the total patents applied in a quarter scaled by the total patents applied for by the firm in the fiscal year. Average Citations is the average of adjusted citations received by all patents applied for by the firm in each quarter. Top 10 Fraction is the total patents which are above the 90th percentile of the citation distribution of granted patents in the calendar year, in a quarter scaled by the total patents applied for by the firm in the given quarter. Qtr2, Qtr3 and Qtr4 are indicator variables which are 1 if the fiscal quarter is the 2nd, 3rd and 4th quarter of the fiscal year respectively. All continuous variables are winsorized at 1% and 99%. We use a vector of control variables for specifications 2,4 and 6. These variables are Interest Burden, RD/Sales, Book-to-market ratio, Long-term Debt Indicator and Log Sales. Details of control construction are in the Appendix. Numbers in parenthesis are t-statistics and ***, ** and * denote 1%, 5% and 10% statistical significance, respectively.

	Dependent Variable					
	Total Fraction		Adjusted Citations		Top 10 Fraction	
	(1)	(2)	(3)	(4)	(5)	(6)
Qtr 2	-0.0118***	-0.0106***	0.0007	-0.0159	-0.0001	0.0028
	(-2.6295)	(-3.0693)	(0.0253)	(-0.6650)	(-0.0168)	(0.6408)
Qtr 3	-0.0136***	-0.0119***	0.0069	-0.0006	0.0011	0.0083**
	(-3.0183)	(-3.3761)	(0.2469)	(-0.0270)	(0.2099)	(2.0927)
Qtr 4	0.0044	0.0062*	-0.0058	-0.0342	0.0045	0.0134***
	(0.9743)	(1.6942)	(-0.2072)	(-1.6092)	(0.8644)	(3.1187)
Observations	31,149	31,077	31,149	31,077	31,149	31,077
Control		YES		YES		YES
Fiscal Year FE		YES		YES		YES
Firm FE		YES		YES		YES
Clus. SE		FIRM		FIRM		FIRM
Adjusted R2	0.0007	0.4410	-0.0001	0.3374	-0.0001	0.3776

Table 3.7: America Invents Act and Corporate Patenting Activity

This table reports the regression results of the effects of the America Invents Act on corporate patenting activity and quality. Sample period is from 1976-2020. Total Fraction is the total patents applied in a quarter scaled by the total patents applied for by the firm in the fiscal year. Average Citations is the average of adjusted citations received by all patents applied for by the firm in each quarter. Top 10 Fraction is the total patents which are above the 90th percentile of the citation distribution of granted patents in the calendar year, in a quarter scaled by the total patents applied for by the firm in the given quarter. Qtr2, Qtr3 and Qtr4 are dummy variables which are 1 if the fiscal quarter is the 2nd, 3rd and 4th quarter of the fiscal year respectively. AIA Dummy is a dummy variable which is 1 for firm quarters after March 2013. All continuous variables are winsorized at 1% and 99%. We use a vector of control variables for specifications 2,4 and 6. These variables are Interest Burden, RD/Sales, Book-to-market ratio, Long-term Debt Indicator and Log Sales. Details of control construction are in the Appendix. Numbers in parenthesis are t-statistics and ***, ** and * denote 1%, 5% and 10% statistical significance, respectively.

	Dependent Variable:					
	Total Fraction		Adjusted Citations		Top 10 Fraction	
	(1)	(2)	(3)	(4)	(5)	(6)
AIA * Qtr 2	-0.0525***	-0.0405***	-0.0229	-0.0840**	-0.0431***	0.0078
	(-8.1819)	(-8.1597)	(-0.5194)	(-2.4325)	(-4.2805)	(1.3748)
AIA * Qtr 3	-0.0655***	-0.0519***	-0.0989**	-0.1866***	-0.0718***	0.0036
	(-10.3081)	(-10.7243)	(-2.2502)	(-5.1821)	(-7.1384)	(0.6006)
AIA * Qtr 4	-0.0762***	-0.0551***	-0.1565***	-0.2661***	-0.0927***	0.0019
	(-11.7756)	(-10.2693)	(-3.7021)	(-7.2025)	(-9.2340)	(0.3069)
AIA	0.0040	-0.0185**	-0.3445***	0.0018	0.2767***	-0.0264**
	(0.8195)	(-2.0026)	(-10.6097)	(0.0294)	(37.7400)	(-2.4249)
Qtr 2	0.0100***	0.0067***	-0.0030	-0.0192	0.0001	-0.0021
	(3.5914)	(3.1112)	(-0.1745)	(-1.3585)	(0.0502)	(-0.8272)
Qtr 3	-0.0001	-0.0004	0.0011	-0.0132	-0.00005	-0.0018
	(-0.0246)	(-0.1704)	(0.0602)	(-1.0040)	(-0.0168)	(-0.7664)
Qtr 4	0.0210***	0.0190***	-0.0304*	-0.0509***	-0.0032	-0.0057**
	(7.4414)	(7.6728)	(-1.7615)	(-3.9185)	(-1.0943)	(-2.3267)
Observations	92,764	92,657	92,764	92,657	92,764	92,657
Fiscal Year FE		YES		YES		YES
Firm FE		YES		YES		YES
Clus. SE		FIRM		FIRM		FIRM
Adjusted R2	0.0061	0.4384	0.0085	0.3933	0.0674	0.4411

Appendix

Chapter 1

A.1.1 Variable Description

Variable	Description
Independent Variables	
TexasExposed (DelawareExposed)	An indicator variable that takes the value of one for firm i if at least one of the following is true: (a) firm i is incorporated in Texas (Delaware) in 2016; (b) firm i is headquartered in Texas (Delaware) in 2016; (c) the number of times Texas (Delaware) is mentioned in firm i's 10-k filings for year 2016 is the highest among all U.S. states. If neither (a)-(c) holds, the value is zero.
Capex	Capital expenditures scaled by total book assets.
Log(Assets)	Natural logarithm of assets at the end of the fiscal year.
ROA	EBITDA over total book assets.
Cash	Cash and receivables, scaled by total book assets.
Leverage	Long term debt plus the current part of the long term debt, all scaled by total book assets.
Tobin's Q	(Book value of assets - common equity + market value of equity - deferred taxes and investment tax) divided by (book value of assets - 0.1 * common equity + 0.1 * market value of equity)
PPE	Property, Plant and Equipment scaled by total book assets.
Dependent Variables	
Log(#Patents)	Natural logarithm of one plus the number of patents applied for by firm i in year t, obtained from Kogan, Papanikolaou, Seru and Stoffman (2017). If the firm has not applied for patents in year t, the value is zero.
R&D	Research and development expenditures scaled by total book assets. If research and development expenditures are missing, the value is set as zero.
CAR	CAR is computed by summing up daily abnormal returns over a [0,+5] day window following the Supreme Court ruling announcement of May 22, 2017. Abnormal returns are returns net of value-weighted market index, obtained from CRSP.

A.1.2 The Effect of *TC Heartland* on Innovation: Truncation-Adjusted Sample

This table estimates the effect of Supreme Court's decision on TC Heartland vs Kraft Foods on innovation output. The sample is based on publicly-traded patenting firms over 2015-2019 period (excluding 2017). See Section 3.4 for more details on sample construction. We further limit the sample only to patents that were granted within a two-year period from the filing date. The variable *Post* is equal to one for years 2018 and 2019 and zero otherwise. See section 3.1 for the definition of $TexasExposed$ and $DelawareExposed$. $Log(\#Patents_{it})$ is the natural logarithm of one plus the number of patent applications filed by firm i in year t. $Log(TotalValue_{it})$ is the natural logarithm of one plus the total dollar value, in million, of all new patents that a firm applied for in a given year (extracted from Kogan et al. (2017)). Firm-year observations with no patent applications are replaced with zero in specifications 1 and 3, and dropped from the sample in specifications 2 and 4. See Table 1.1 and Section 1.3.3 for the description of control variables. All the regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the state level (defined as the state with the maximal *Dependency* value). The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	$Log(\#Patents)$		$Log(\#TotalValue)$	
	Overall Sample	Intensive Margin	Overall Sample	Intensive Margin
TexasExposed*Post	-0.064*	-0.100	-0.182**	-0.225*
	(0.038)	(0.079)	(0.084)	(0.124)
DelawareExposed*Post	-0.058	0.004	-0.027	0.058
	(0.042)	(0.075)	(0.102)	(0.084)
Log(Assets)	0.026	0.148**	0.037	0.251***
	(0.038)	(0.063)	(0.083)	(0.074)
ROA	0.014	-0.212	0.090	0.133
	(0.070)	(0.145)	(0.123)	(0.216)
Cash	0.117	0.091	-0.185	-0.086
	(0.163)	(0.354)	(0.167)	(0.330)
Capex	-0.492	-1.794	-0.656	-3.374
	(0.522)	(1.113)	(1.341)	(2.103)
Leverage	-0.055	-0.106	-0.043	0.388**
	(0.098)	(0.122)	(0.159)	(0.178)
Tobin's Q	0.017	0.013	0.039	0.110
	(0.018)	(0.033)	(0.059)	(0.072)
PPE	0.226	0.506	-0.234	0.929
	(0.264)	(0.634)	(0.383)	(0.571)
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
N	3433	2043	3433	2043
adj.R2	0.89	0.88	0.87	0.92

A.1.3 The Effect of *TC Heartland* on Innovation: PPMLE estimates

This table estimates the effect of Supreme Court's decision on TC Heartland vs Kraft Foods on innovation output. The sample is based on publicly-traded patenting firms over 2015-2019 period (excluding 2017). See Section 3.4 for more details on sample construction. Here, we use Poisson regression estimates instead of panel OLS. The variable $Post$ is equal to one for years 2018 and 2019 and zero otherwise. See section 3.1 for the definition of $TexasExposed$ and $DelawareExposed$. $\#Patents_{it}$ is the number of patent applications filed by firm i in year t. $TotalValue_{it}$ is the total dollar value, in million, of all new patents that a firm applied for in a given year (extracted from Kogan et al. (2017)). Firm-year observations with no patent applications are replaced with zero in specifications 1 and 3, and dropped from the sample in specifications 2 and 4. See Table 1.1 and Section 1.3.3 for the description of control variables. All the regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the state level (defined as the state with the maximal $Dependency$ value). The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	#Patents				Total Value			
	Overall Sample		Intensive Margin		Overall Sample		Intensive Margin	
TexasExposed*post	-0.258*	-0.152	-0.234**	-0.163*	-0.445***	-0.303***	-0.374***	-0.273***
	(0.134)	(0.129)	(0.104)	(0.096)	(0.107)	(0.071)	(0.086)	(0.055)
DelawareExposed*post	-0.054	-0.080	-0.079	-0.103*	0.072	-0.035	-0.003	-0.091
	(0.072)	(0.056)	(0.068)	(0.062)	(0.079)	(0.087)	(0.089)	(0.096)
Log(Assets)		0.234***		0.162***		0.395***		0.306***
		(0.069)		(0.062)		(0.074)		(0.068)
ROA		-0.417**		-0.277*		0.729**		0.714**
		(0.163)		(0.158)		(0.369)		(0.309)
Cash		0.764***		0.878***		1.181***		1.085***
		(0.153)		(0.173)		(0.215)		(0.206)
Capex		-3.550**		-2.989***		-4.369**		-2.556
		(1.423)		(0.900)		(1.920)		(1.684)
Leverage		-0.269		-0.171		0.016		-0.058
		(0.305)		(0.352)		(0.339)		(0.308)
TobinQ		-0.009		-0.002		0.038**		0.013
		(0.022)		(0.022)		(0.017)		(0.015)
PPE		2.581***		2.200***		3.304***		2.689***
		(0.859)		(0.524)		(0.707)		(0.676)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	No	No	No	No
N	2387	2384	3694	3685	2387	2384	3694	3685
Log Likelihood	-7989.74	-7743.43	-8895.88	-8701.87	-79956.21	-72154.07	-92936.51	-87098.72

Chapter 2

A.2.1 Variable Description and definitions

Variable	Description
Independent Variables	
$log(HHI)_{cst}$	The logarithm of the the Herfindahl-Hirschmann Index based on labor market power specifically, we have $HHI_{cst} = \Sigma_j (s_j)^2$ where s_j is the share of firm j from city C labor market that specialize in technology class s.
$ClusterSize_{cst}$	The logarithm of the number of active inventors in city C who specialize in technology s during year t
$Competition_{ind,t}$	One over HHI for sales value in 3-digit SIC industry ind which firm j belongs to.
Dependent Variables	
$log(\#Patents_{ijcst})$	Logarithm of one plus the scaled number of patents applied for by inventor i working in city C and specializing in technology class s during year t and employed by firm j. Each patent application is scaled by the number of co-inventors.
$log(TotalCite_{ijcst})$	Logarithm of the total number of citations that inventor i receives from patent application that are filed during year t. Each patent citation is scaled by the number of co-inventors
$log(AverageCite_{ijcst})$	Logarithm of the average citation per patent that are filed by inventor i in year t.
$log(TotalValue_{ijcst})$	Logarithm of the total value of all patents inventor i has applied for in year t where value of each patent is divided among inventors. The value of patents is obtained from Kogan, Papanikolaou, Seru and Stoffman (2017).
$log(AverageValue_{ijcst})$	Logarithm of the average dollar value of patents filed by inventor i in year t.
$Explorative_{ijcst}$	Aggregated number of Explorative patents scaled by the number of patents.
$Exploitative_{ijcst}$	Aggregated number of Exploitative patents scaled by the number of patents.
$TeamSize_{ijcst}$	Average number of team size across all patent projects that inventor i works in during year t

A.2.2 Parallel Trend Assumption

This table reports results for regression 2.9. The dependant variable in all columns is $log(\#Patents_{ijcst})$. Only coefficient results for variables of interest are reported for brevity. You can find the definition of the variables in Appendix table A1. The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the inventor level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
$treated * y_3$	0.048	0.048	0.051	0.044	0.064	0.030
	(0.033)	(0.045)	(0.034)	(0.047)	(0.045)	(0.064)
$treated * y_2$	0.079***	0.067*	0.082***	0.063	0.116***	0.082
	(0.031)	(0.040)	(0.031)	(0.041)	(0.040)	(0.055)
$treated * y_1$	0.043	0.066*	0.047	0.062	0.051	0.073
	(0.030)	(0.039)	(0.031)	(0.041)	(0.039)	(0.053)
$treated * y_{-1}$	0.020	0.025	0.026	0.026	0.025	0.006
	(0.032)	(0.042)	(0.033)	(0.043)	(0.043)	(0.057)
$treated * y_{-2}$	-0.012	0.014	-0.009	0.012	-0.022	0.020
	(0.031)	(0.040)	(0.032)	(0.040)	(0.040)	(0.053)
year	x	x	x	x	x	x
city	x	x	x	x	x	x
field	x	x	x	x	x	x
city*field			x	x	x	x
field*year					x	x
inventor		x		x		x
firm		x		x		x
obs	69312	69233	69312	69233	69312	69233
adj.R2	0.023	0.394	0.023	0.394	0.024	0.395

A.2.3 Mergers as Shocks: Poisson Estimates

This table shows how labor market concentration affects inventor output. The regression specifications are similar to **2.3**.2 with the differences. First, we use direct values of HHI_{cst} and do not use logarithmic transformation. Second, we use pseudo Poisson maximum likelihood estimators instead of OLS estimations. For more information regarding why we use PPMLE refer to section 6. For further information regarding our identification strategy refer to section 3. To find more information regarding our inventor sample construction refer to section 2. Refer to Appendix table A1 for detailed information about how we construct our independent and dependant variables. The regressions are augmented with fixed effects, as indicated at the bottom of each column. Standard errors are clustered at the inventor level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
$treated_{cst} * post_{cst}$	0.086***	0.051*	0.083***	0.052*	0.114***	0.066*
	(0.022)	(0.029)	(0.023)	(0.030)	(0.029)	(0.037)
$ClusterSize_{cst}$	0.293***	0.144	0.305***	0.151	0.417***	0.279**
	(0.078)	(0.104)	(0.101)	(0.122)	(0.112)	(0.137)
year	x	x	x	x	x	x
city	x	x	x	x	x	x
field	x	x	x	x	x	x
city*field			x	x	x	x
field*year					x	x
inventor		x		x		x
firm		x		x		x
obs	69312	69233	69312	69233	69312	69233
log_likelihood	-88674	-60280	-88670	-60279	-88634	-60255

A.2.4 Panel Regression: Poisson

This table shows how labor market concentration affects inventor output. The regression specifications are similar to 2.7 with some differences. First, we do not use logarithmic transformation. Second, we use pseudo Poisson maximum likelihood estimates instead of OLS estimations. For more information regarding why we use PPMLE refer to section 6. For further information regarding panel regressions refer to section 4. To find more information regarding our inventor sample construction refer to section 2. Refer to Appendix table A1 to find out how we construct our independent and dependant variables.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HHI_{cst}	0.040***	0.068***	0.062***	0.044***	0.050***	0.061***	0.048***	0.041***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.009)
$ClusterSize_{cst}$	0.056***	0.097***	0.110***	0.136***	0.111***	0.154***	0.115***	0.137***
	(0.008)	(0.009)	(0.009)	(0.010)	(0.009)	(0.011)	(0.014)	(0.016)
year	x	x	x	x	x	x	x	x
city	x	x	x	x	x	x	x	x
field	x	x	x	x	x	x	x	x
class	x	x	x	x	x	x	x	x
city*field		x	x	x	x	x	x	x
city*class			x	x	x	x	x	x
field*year			x	x	x	x	x	x
class*year					x	x	x	x
inventor						x	x	x
city*year							x	x
firm								x
obs	930891	930891	930891	930891	930891	930891	930891	822436
Log likelihood	-1218883	-1217282	-1195777	-1194263	-1185295	-1063299	-1060781	-919730

Chapter 3

A.3.1 Variable Description and definitions

Name	Definition
Adjusted Citations	This is the citation count adjusted for truncation lag using the fixed-effect approach as per Hall, Jaffe and Trajtenberg (2005).
Top 10 Dummy	This is a dummy variable which is 1 if the patent is above the 90th percentile of citation distribution of all granted patents in the year.
Average Citations	This is the average of adjusted citations received by all patents applied for by the firm in each fiscal quarter.
Total Fraction	This is the total patents applied for in a fiscal quarter scaled by the total patents applied for by the firm in the fiscal year.
Top 10 Fraction	This is the sum of all patents which are above the 90th percentile of the citation distribution of granted patents in the calendar year, in a fiscal quarter scaled by the sum of patents applied for by the firm in the same fiscal quarter.
Interest Burden	The three-year average of interest expense scaled by operating income before depreciation.
Book-To-Market	The three-year average ratio of book value of assets over the sum of book value of liabilities and market value of equity.
R&D/Sales	The three-year average of research and development expense scaled by sales.
LTDI	An indicator variable for long-term debt, which is equal to 1 if the firm has long-term debt outstanding and 0 otherwise.
Log Sales	Log of the total sales each fiscal quarter.
AIA Dummy	This is a dummy variable which is 1 for firm fiscal quarters after March 2013.
Firm level variables	
4Qtr Fraction	Fraction of granted patents granted to a firm in the last quarter of a fiscal year scaled by the number of all granted patents to that firm for that fiscal year
Analysts	Number of analysts following a firm during year t
Institute	the number of shares of a firm institutional shareholders hold scaled by the overall number of shares both calculated at the end of fiscal year t-1
Cash	The value of cash holdings over assets both measured at fiscal year-end t-1
ROA	EBITDA over assets both measured at fiscal year-end t-1
Leverage	Long-term debt over assets both measured at fiscal year-end t-1
Tobin's Q	(Book value of assets - common equity + market value of equity - deferred taxes and investment tax) divided by (book value of assets -0.1 * common equity + 0.1 * market value of equity)
Log size	Natural log of one plus the market value of firm both measured at fiscal year-end t-1
R&D	Research and development over assets both measured at fiscal year-end t-1
Capex	Capital expenditures scaled by assets both measured at fiscal year-end t-1
PPE	Property plant and equipment scaled by assets both measured at fiscal year-end t-1

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