SHO time for innovation: The real effects of short sellers

Jie (Jack) He
Terry College of Business
University of Georgia
jiehe@uga.edu
(706) 542-9076

Xuan Tian
Kelley School of Business
Indiana University
tianx@indiana.edu
(812) 855-3420

This version: December, 2015

* We thank Russell Investments for providing us the list of Russell 3000 index used in this paper. We are grateful for helpful comments from Utpal Bhattacharya, Thomas Chemmanur, Cissy Chen, Alex Edmans, Vivian Fang, Juanita Gonzalez-Uribe, Todd Gormley, Itay Goldstein, Xiaoxia Lou, Liang Ma, Farzad Saidi, Anna Scherbina, Henri Servaes, Merih Sevilir, Tao Shu, Vikrant Vig, Julie Wu, Youchang Wu, Fei Xie, Moqi Xu, Liyan Yang, Gokhan Yilmaz, seminar participants at Clemson University, and conference participants at the 2014 LBS Summer Finance Symposium, the 2014 Entrepreneurial Finance and Innovation Conference, the 2014 China International Conference in Finance, and the 2014 Financial Management Association Meetings. We also thank Boya Chen, Tong Liu, Haotian Xu, Zhe Wang, Aner Zhou and Haoxuan Zou for their excellent research assistance. We remain responsible for any remaining errors or omissions.

SHO time for innovation: The real effects of short sellers

Abstract

We examine the effect of short sellers on innovation. Using exogenous variation in short-selling costs generated by a quasi-natural experiment, Regulation SHO, which randomly assigns a subsample of Russell-3000 firms into a pilot program and removes the tick restriction on their stocks, we show that short sellers have a positive, causal effect on the quality, efficiency, and originality of corporate innovation. The exposure to patenting-related litigations initiated by short sellers is a plausible mechanism through which short sellers affect firms' innovative activities. Our paper provides new insights into an unintended real effect of short sellers – their improvement of technological innovation.

Key words: Innovation; Short selling; Regulation SHO; Litigation risk

JEL number: G14; G18; O31; O32

1. INTRODUCTION

Do financial markets have real effects on economic activities or are they just a side show? Although conventional wisdom believes that financial market security prices merely reflect expectations about future cash flows but do not affect them, a fast growing strand of literature in financial economics challenges this traditional view and argues for the real effects of secondary stock markets on corporate decision making (e.g., Grossman, 1976; Hellwig, 1980; Subrahmanyam and Titman, 1999; Goldstein and Guembel, 2008).[1] In this paper, we complement the above literature by exploring the real effect of one key ingredient of financial markets, namely, short sellers, whose economic impact has been intensively debated among academics, practitioners, and policy makers in the past few decades.

Critics claim that short sellers play a detrimental role to the society by adversely affecting security prices, creating high market volatility, and undermining investors' confidence in the real sector of the economy because of panic selling. However, advocates of short selling take an opposite stand and argue that short sellers could help improve market efficiency, facilitate price discovery, and prevent financial misconducts due to their active information production and intensive disciplining of the corporate management.[2] While there might be some elements of truth in both sides of these arguments, in practice it is hard to identify the causal effect of short sellers on the real economy due to the endogenous nature of short sales: short selling activities could give rise to or result from the underlying characteristics of the corporate sector in the real economy.[3]

To tackle the endogeneity problem, we exploit a quasi-natural experiment, Regulation SHO, which removed short selling constraints for a randomly selected group of stocks. We provide the first empirical study that examines the causal effect of short sellers on corporate innovation. A deeper understanding of this issue is of particular interest to policy makers and

[1] Bond, Edmans, and Goldstein (2012) provide an excellent survey on theoretical and empirical studies that examine the effects of financial markets on the real economy.

[2] In a highly publicized case, short seller Muddy Waters LLC discovered that Sino-Forest, a Canadian company that had its operations in China, exaggerated the level of its principal assets (i.e., trees) that were not even owned by the company. In contrast, Sino-Forest's auditor, Ernst & Young, failed to detect the accounting fraud but still claimed "we are confident that Ernst & Young Canada's work… met all professional standards. … Ernst & Young Canada did extensive audit work to verify ownership and existence of Sino-Forest's timber assets." (New York Times, December 6, 2012)

[3] For instance, a drop in stock prices following a period of active short sales may imply that short sellers depress the price level via their trading, but it could also reflect the fact that short sellers are able to predict an upcoming decreasing trend in the stock market and thus trade on their expectations.

firm stakeholders not only because innovation is a crucial driver of a nation's economic growth (Solow, 1957) and competitive advantage (Porter, 1992), but also because short selling activities in the U.S. are highly regulated and can be altered by security laws and regulations over time.

We are not the first to explore the effect of short sellers on corporate investment and financing activities. For example, Gilchrist, Himmelberg, and Huberman (2005) and Grullon, Michenaud, and Weston (2015) find that short selling constraints as well as the removal of these constraints alter a firm's conventional investment activities (such as ordinary capital expenditures) and financing decisions. However, our focus on technological innovation (as opposed to investments in routine tasks) allows us to provide a number of new insights beyond those offered by existing studies.

First, innovation has many unique features that are distinct from conventional investment. As Holmstrom (1989) points out, innovation is a long-term, risky, and idiosyncratic investment in intangible assets that requires much exploration of unknown approaches, whereas conventional investment is the exploitation of well-known methods. Hence, relative to conventional investment such as ordinary capital expenditures, corporate innovation entails a heavier use of various intangible assets (such as human capital, nonmonetary incentives, and organizational support), requiring more managerial discretion and making it harder for outsiders to evaluate and monitor the whole process. As a result, innovation activities are more susceptible to capital market frictions (e.g., adverse selection and moral hazard) and thus more likely to be influenced by market ingredients that affect such frictions, such as short sellers. For example, there has been an emerging literature showing that several economic factors affect conventional investment and innovation in substantially different ways.[4] Therefore, while some concurrent studies (e.g., Grullon, Michenaud, and Weston, 2015) have shown that the removal of short selling constraints due to Regulation SHO leads to a cut in ordinary capital expenditures for small, financially constrained firms, it is unclear *ex ante* how short sellers affect a firm's innovation activities.

[4] For instance, while the traditional IPO literature documents that going public allows firms to raise capital and increase capital expenditures, Lerner, Sorensen, and Stromberg (2011) and Bernstein (2015) find that private ownership, rather than public ownership, promotes innovation because the former allows more failure tolerance from investors (Manso, 2011) than the latter does. A second example is with respect to the effect of financial analysts. Some studies argue that financial analysts reduce information asymmetry and the cost of capital, which in turn increase ordinary capital expenditures (e.g., Derrien and Kecskes, 2013). However, recent studies such as Benner and Ranganathan (2012) and He and Tian (2013) find that analysts actually hinder corporate innovation by imposing excessive pressure on managers to meet short-term earnings targets.

Second, our use of patenting data, which has become standard in the literature to capture firm innovation, allows us to observe multiple dimensions of a firm's innovation output including the number of patents a firm generates, the number of citations these patents receive in subsequent years, the innovation output per dollar of research and development (R&D) expenses, and the originality and generality of the patent portfolio a firm has. Hence, we are able to explore the effect of short sellers on not only the quantity but also the quality, efficiency, and fundamental nature of the innovation output by a firm. This unique feature makes technological innovation an outcome variable that is superior to those input-based measures examined in previous studies, because one cannot easily measure the change in the quality, efficiency, and fundamental nature of capital expenditures (or other conventional investment inputs) and financing activities, despite the change in their quantities.

We develop two competing hypotheses based on existing theories and the prevailing views of short selling. Our first hypothesis conjectures that short sellers help improve the inherent quality and fundamental nature of corporate innovation because of the disciplining role they play. Moral hazard models such as Grossman and Hart (1988) and Harris and Raviv (1988) argue that, to enjoy private benefits such as "quiet life" (Bertrand and Mullainathan, 2003), managers who are not properly monitored will shirk or tend to invest more in unchallenging routine tasks. Even if they invest in innovative projects, managers pursuing their self interests rather than those of the shareholders may have incentives to push up the quantity of their innovation output at the expense of the innovation quality out of grandstanding or entrenchment concerns (Gompers, 1996; Scharfstein and Stein, 2002). Value-destroying sub-optimal investment in innovative projects due to agency problems could be mitigated by the threat of depressing stock prices from short sellers because managers' job security and compensation are contingent on the firm's stock price. Whenever short sellers detect weak signs of a firm's performance due to managerial agency, such as shirking or sub-optimal investment in long-term innovative projects, they could immediately short sell the company's stock, initiating or speeding up the price tumbling process, which in turn leads to quick negative market reactions (sometimes even causing "overreactions" from certain traders) and potential disciplinary actions against the managers, including reduced bonuses and even forced managerial turnover.[5] In anticipation of

[5] There are many real-world examples about how short sellers pay close attention to firms' innovation output and file lawsuits against firms with weak-quality patents to generate trading profits. See the detailed example of Hayman

this adverse "snowball" effect, managers would discipline themselves *ex ante* when making innovation decisions. Hence, the mere presence of short sellers and the *threat* of disciplining by them align managerial incentives with shareholders and motivate managers to maximize firm value by making value-enhancing investment in innovative projects. As a result, firms with a higher exposure to short selling threat enhance their innovation efficiency and generate higher-quality innovation output.[6] We term this view the *disciplining hypothesis*.

An alternative hypothesis predicts the opposite. Short sellers are often accused of creating tremendous price pressure on a firm's stock (e.g., Mitchell, Pulvino, and Stafford, 2004), which leads to excessive pressure on managers to focus on short-term activities, exacerbating the managerial myopia problem. Indeed, Graham, Harvey, and Rajgopal (2005) find that 78% of executives would sacrifice long-term value to meet short-term targets in a survey of 401 U.S. CFOs. Manso (2011) theoretically shows that tolerance for failure is necessary for effectively motivating and nurturing innovation due to the long-term, risky, idiosyncratic, and unpredictable nature of technological innovation.[7] However, short sellers have an innate distaste for tolerance towards short-term failures, because their main objective is to identify underperforming firms that are likely overvalued, sell short these stocks which reflects their unfavorable information, and make trading profits. As a consequence, firm managers who care more about short-term stock prices and operating performance may sacrifice long-term firm value by cutting necessary inputs and organizational support for long-run, risky, but innovative projects to keep their current stock prices high in the presence of short selling pressure. Therefore, our second hypothesis, the *pressure hypothesis*, argues that short sellers, by imposing short-term pressure on managers, reduce innovation quality and efficiency.[8]

As we argued before, identifying the causal effect of short sellers on firm innovation is challenging because of the endogenous nature of short selling activities. Therefore, in this paper,

---

Capital below. For another example, a New York hedge fund, Ferrum Ferro Capital LLC, filed a petition with the U.S. Patent and Trademark Office's Patent Trial and Appeal Board to invalidate an Allergan patent in March 2015.

[6] To some extent, the role of short sellers in promoting innovation is similar to that of hostile takeover bidders or buyout funds in improving internal corporate governance, both serving as a form of "external governance" for firm managers.

[7] Recent empirical papers such as Acharya et al, (2013, 2014), Ederer and Manso (2013), and Tian and Wang (2014) all find supporting evidence for the implications of the failure tolerance theory.

[8] Note that the *pressure hypothesis* also predicts that short sellers should have a negative impact on the quantity of innovation output (i.e., the number of patents generated), whereas the *disciplining hypothesis* has no clear implications for the quantity of innovation output: firms could either cut down their inefficient innovative projects or improve both the quantity and quality of their innovation investment when facing more disciplining from short sellers.

we use a quasi-natural experiment, Regulation SHO, to identify the causal effect of short sellers on firm innovation. Short selling activities in the U.S. have been largely constrained historically. For example, the uptick rule, which was established in 1935, prohibits short sales when stock prices are declining, imposing significant costs on short sellers. In July 2004, the Security and Exchange Commission (SEC) announced a new regulation on short-selling activities in the U.S. equity market, Regulation SHO, which removed the uptick rule restriction for an ex-ante *randomly* selected pilot group of firms (about one third of the Russell 3000 firms listed on NYSE, NASDAQ, and AMEX). Meanwhile, the uptick rule remained in effect for the non-pilot Russell 3000 firms (i.e., the rest two thirds of the Index). This sudden regulatory change, by significantly reducing the costs of short selling only for pilot firms but not for non-pilot firms, provides us a nice quasi-laboratory setting to observe the causal effect of short sellers on firm innovation, as it was not initiated to alter firms' investment behavior in anyway. Another crucial advantage of this experiment is that it does not require pilot firms to experience an *actual* increase in short selling activities (and the corresponding price pressure) after the regulatory shock. The mere *threat* (or possibility) of becoming more likely to be shorted will influence managerial behavior and affect their incentives to implement innovative projects. We adopt a difference-in-differences (DiD) method to analyze how the quality, efficiency, and nature of a firm's innovation output are affected by this exogenous shock to short-selling constraints.

We extract innovation output data from the Google Patent Database, which is compiled from the United States Patent and Trademark Office (USPTO) database. In addition to the number of patent applications filed in a given year that are eventually granted, which proxies for the quantity of a firm's innovation output and is widely used in existing literature (e.g., Aghion et al., 2005, Seru, 2014, Chemmanur et al., 2014), we construct four more measures to capture a firm's innovation quality, efficiency, and fundamental nature. The first measure counts the total number of non-self citations each patent receives in subsequent years, which captures the significance and quality of a firm's innovation output. The second measure is the total number of non-self citations made by a firm divided by the firm's one-year-lagged R&D expenses, which gauges the firm's innovation efficiency. The third and fourth measures capture the fundamental nature of a firm's innovation output: the originality and generality scores of the generated patents. Patents that cite a wider array of technology classes of existing patents are considered as having greater originality; patents that are cited by a wider array of technology classes of subsequent patents are viewed as having greater generality.

After performing various diagnostic tests to ensure that the parallel trend assumption, the key identifying assumption of the DiD test, is satisfied, we show a positive, causal effect of short sellers on the quality, efficiency, and originality of firm innovation. According to our multivariate DiD analysis, a reduction in short selling costs due to Regulation SHO leads to a 5.1% larger increase in non-self citations per patent, a 22.7% larger increase in innovation efficiency, and a 68.8% increase in patent originality scores for the treatment (pilot) group compared to the control (non-pilot) group. We do not find evidence that Regulation SHO has a significant effect on the number of patents (i.e., the quantity of innovation output) or the patent generality scores. Further, we find a stronger positive effect of short sellers on the quality, efficiency, and originality of innovation for a subsample of firms that generate at least one patent in our sample period. These baseline results are consistent with the implication of the *disciplining hypothesis*.[9]

We next perform two robustness tests for the baseline DiD analysis. First, to address the concern that our DiD results could have been driven by chance, we run simulations that randomize the inclusion of pilot firms in our analysis, and find that the DiD estimators obtained from this randomization test are on average close to zero. Second, to address the concern that unobservable shocks which are unrelated to Regulation SHO could have driven the results, we conduct a placebo test by artificially picking a "pseudo-event" year when we assume a regulatory shock reduced short selling costs for the pilot firms. We do not find significant difference in the quality, efficiency and nature of innovation activities between pilot and non-pilot firms around such "pseudo-event" years.

We then attempt to explore possible underlying mechanisms through which short sellers help enhance the quality, efficiency, and originality of firm innovation. One tool commonly used by short sellers to attack innovative firms and make profits out of it is to file lawsuits against the latter, accusing their patents of being either spurious or of little value, while in the meantime betting against their share prices. Ample anecdotal evidence suggests that this is a powerful and

[9] The pilot program ended on August 6, 2007 when the tick restriction was removed for all stocks. This feature of the experiment provides us a nice opportunity to check whether the pattern of innovation outputs for pilot and non-pilot firms reversed after the pilot program ended. Consistent with our conjecture, we find that the non-pilot firms indeed experienced a significantly larger increase in innovation quality and originality after their short selling constraints are removed in August 2007 than did the pilot firms whose costs of short selling remain unchanged.

profitable strategy used by short sellers.[10] A seminal paper by Cohen, Gurun, and Kominers (2015) formally documents the existence and patterns of patenting-related lawsuits and further shows that such lawsuits impose a credible and real litigation risk to innovative firms. Hence, we propose that patenting-related litigation risk, namely, the risk of being sued by short sellers against a firm's patent portfolio, is a plausible mechanism through which the increase in short selling threat disciplines firm management to improve the firm's innovation quality, efficiency, and originality.

To explore this possible economic mechanism, we perform three tests. First, we examine whether a reduction in short selling costs due to Regulation SHO indeed increases a firm's patenting-related litigation risk. Adopting the same DiD framework as in our main analyses, we show that the exposure to patenting-related litigations by potential short sellers increases significantly more for treatment firms compared to control firms. Second, we partition the sample based on a firm's one-year-lagged exposure to patenting-related litigations by potential short sellers and find that the positive effect of short sellers on the quality, efficiency, and originality of innovation is more pronounced when a firm's ex-ante litigation risk is higher, but is absent when a firm's ex-ante litigation risk is lower. Third, if litigation risk is indeed an important mechanism through which short sellers affect innovation, our DiD estimates should become weaker once we directly control for the change in litigation risk before and after Regulation SHO. In other words, there should be a much smaller "residual" effect of Regulation SHO (the treatment) on firms' innovation activities once the litigation risk channel has been taken into account. To test this conjecture, we directly include a firm's exposure to patenting-related litigation in our baseline DiD regressions. We find that the positive effect of short sellers on the quality, efficiency, and originality of innovation substantially weakens or becomes insignificant, and that the explanatory power of the independent variables (i.e., the R^2) increases significantly after we include the new mechanism variable in the regressions. Overall, the evidence suggests that litigation risk is a plausible economic mechanism through which short

[10] For example, in February 2015, Mr. Kyle Bass, head of Hayman Capital Management LP (a hedge fund), challenged two of the five patents covering multiple sclerosis drug Ampyra, which accounts for 91% of the revenue of Acorda Therapeutics Inc. Mr. Bass previously challenged patents held by Shire PLC and later challenged patents held by Jazz in April 2015. His main accusation was that "a small minority of drug companies are abusing the patent system to sustain invalid patents that contain no meaningful innovations but serve to maintain their anti-competitive high-price monopoly." As a result, stock prices of the target firms fell significantly after these litigation filings. Meanwhile, he short sold the shares of these firms and made profits out of the trading. (Bloomberg News, June 26, 2015).

sellers help improve innovation.

One caveat of our study is that we are not claiming that short sellers, who typically have a limited investment horizon and seek profits from stock price drops, intentionally promote firm innovation through their intervention activities.[11] Short sellers are active market players and short-term speculators. Even though they file lawsuits, accusing firms of generating spurious or little-value patents, their intention is to sell short the latter's stocks and make trading profits. Hence, they are unlikely to truly care about a firm's innovation unless the investment in these projects has immediate implications for the firm's market value. What we document in this paper could simply be an *unintended* consequence of active short sellers due to the disciplining role they play. In this sense, short sellers act like hostile takeover bidders or professional buyout funds whose ultimate goal is to identify and purchase undervalued assets but at the time unintentionally stimulate their target firms to improve their corporate governance. Nevertheless, even if the positive effect on corporate innovation by short sellers is unintended, our findings still offer new insights and have important policy implications, especially given the considerable wide variation in short selling rules and practices around the world and the fact that short selling activities in the U.S. have been heavily regulated.

The rest of the paper is organized as follows. Section 2 discusses the related literature. Section 3 describes sample selection and reports summary statistics. Section 4 presents the main results. Section 5 discusses possible mechanisms. Section 6 discusses alternative explanations and Section 7 concludes.

2. RELATION TO THE EXISTING LITERATURE

Our paper mainly contributes to two strands of literature. First, it is broadly related to the growing literature arguing for the real effects of financial markets. Starting from Hayek (1945), who argues that prices are a useful source of information, researchers (e.g., Grossman (1976) and Hellwig (1980)) realize that financial markets aggregate the information of many market participants who, though individually less informed, are collectively more informed than corporate decision makers. Dow and Gorton (1997), Subrahmanyam and Titman (1999), and

[11] That being said, there is a wide variation in the length of investment horizons by short sellers. For example, Bill Ackman's Pershing Square Capital, a top performing hedge fund of 2014, held a short position against the nutritional drink company, Herbalife, for more than two years. To aid its shorting strategy, Ackman even started a site called *factsaboutherbalife.com* accusing Herbalife of running an illegal pyramid scheme. (Bloomberg News, June 1, 2015).

Goldstein and Guembel (2008) show that decision makers use the new information learned from financial market prices to guide their real decisions.[12]

Regarding the role played by short sellers, although there is a large strand of literature that debates the effects of short-selling constraints on asset prices (e.g., Miller, 1977; Harrison and Kreps, 1978; Chen, Hong, and Stein, 2002; Battalio and Schultz, 2006; Diether, Lee, and Werner, 2009; Beber and Pagano, 2013; Boehmer, Jones, and Zhang, 2008, 2013; Boehmer and Wu, 2013), empirical studies that examine the effect of short sellers or short-selling constraints on corporate decisions are quite limited. Gilchrist, Himmelberg, and Huberman (2005) show that short-selling constraints distort firm investment and financial decisions. Karpoff and Lou (2010) and Hirshleifer, Teoh, and Yu (2011) find that short sellers are able to detect corporate financial misconduct and dump on suspicious firms. Massa, Zhang, and Zhang (2015) show that short sellers reduce earnings management in a cross-country setting.

Our paper also contributes to the literature on finance and innovation. Holmstrom (1989) shows that innovation activities are inherently different from and may not mix well with routine tasks in an organization. Manso (2011) demonstrates that managerial contracts that tolerate failure in the short run and reward success in the long run are best suited to motivate managers to engage in innovation activities. Empirical evidence shows that various firm characteristics and economic forces affect managerial incentives of investing in innovation. For example, a larger institutional ownership (Aghion, Van Reenen, and Zingales, 2013), corporate instead of independent venture capitalists (Chemmanur et al., 2014), debtor- rather than creditor-friendly bankruptcy laws (Acharya and Subramanian, 2009), and private instead of public equity ownership (Lerner, Sorensen, and Stromberg, 2011) all enhance managerial and employees' incentives to innovate.[13] However, existing literature has been silent on how short sellers, an important group of active market players and speculators, affect firms' innovation activities. Our paper contributes to this line of research by filling in the gap.

A few recent papers use the same quasi-natural experiment as ours to examine the real effect of short sellers on corporate finance activities. Grullon, Michenaud, and Weston (2015)

---

[12] Bond, Goldstein, and Prescott (2010) further argue that while it is important for managers to learn information from stock prices, they need to have some independent informational sources to achieve the desirable outcome.

[13] Other studies have examined the effects of product market competition, general market conditions, firm boundaries, CEO overconfidence, banking competition, and failure tolerance on corporate innovation (e.g., Aghion et al., 2005; Nanda and Rhodes-Kropf, 2013; Hirshleifer et al., 2012; Cornaggia et al., 2014; Seru, 2014; Tian and Wang, 2014).

show that an exogenous change in short-selling constraints causes stock prices to fall and financially constrained firms respond to the drop in prices by reducing equity issues and investment.[14] Fang, Huang, and Karpoff (2015) find that an exogenous decrease in short-selling costs reduces pilot firms' propensity to engage in earnings management and that this pattern reverses after the SHO program ends. Li and Zhang (2015) find that managers reduce the readability of bad news forecast in response to the increased short selling pressure, and De Angelis, Grullon, and Michenaud (2015) examine how Regulation SHO affects managerial compensation. Different from the above studies, our paper focuses on the causal effect of the removal of short-selling constraints on firm innovation, which has many unique features and is critical for long-term economic growth. We further identify a new mechanism, the patenting-related litigation risk, through which short sellers discipline corporate managers and enhance the quality and efficiency of firm innovation. Our paper thus provides the first empirical analysis that sheds light on this important research question.

3. SAMPLE SELECTION AND SUMMARY STATISTICS

3.1 Sample Selection

Our sample construction starts with the Russell 3000 index in June 2004. Following the SEC's first pilot order issued on July 28, 2004 (Securities Exchange Act Release No. 50104), which describes in detail how the pilot and non-pilot stocks in the Regulation SHO program were chosen, we exclude stocks that were not listed on the NYSE, AMEX, or NASDAQ NM, and stocks that went public or had spin-offs after April 30, 2004. Out of the remaining 2,952 stocks, we identify 986 pilot stocks according to the published list of the SEC's pilot order and the rest 1,966 stocks comprise the initial non-pilot sample. The exchange distribution of these stocks shows that they are very representative of the Russell 3000 Index. For example, around 50% of the pilot stocks are listed on the NYSE, 48% on the NASDAQ NM, and 2% on the AMEX. The exchange distribution of the non-pilot stocks is almost the same.

[14] Besides the difference between ordinary capital expenditures and innovation discussed in the introduction, our paper uses patenting as the innovation output measure, which encompasses the successful usage of all (both observable and unobservable) innovation inputs and is most likely to be influenced by the disciplining activities of short sellers. Therefore, our use of patents and citations (as opposed to ordinary capital expenditures or R&D spending that is just one observable innovation input) as the main outcome variables helps explain why we observe a different (and probably an even more important) effect of short sellers on firms' investment behavior from that reported in Grullon, Michenaud, and Weston (2015).

To examine the dynamics of innovation output around the implementation of Regulation SHO in July 2004, we extract firm characteristics from various data sources two years before and after the event year (i.e., 2004).[15] Specifically, we examine innovation outcomes of firms whose fiscal year ending dates are either between July 1, 2002 and June 30, 2004 for the pre-event period (which covers firms whose majority of investment activities take place during the calendar year period of 2002 to 2003), or between July 1, 2005 and June 30, 2007 for the post-event period (which covers firms whose majority of investment activities take place during the calendar year period of 2005 to 2006). We further require all firms to have non-missing Compustat records to calculate firm characteristics across the above sample period. The resulting final sample consists of 748 pilot firms and 1,486 control firms.[16] We collect firm-year patent and citation information from the Google USPTO Bulk Downloads, which is available at http://www.google.com/googlebooks/uspto.html. This database provides rich information on all patents filed and granted by the USPTO between 1976 and 2014, including patent application and grant date, patent assignee name (the entity that owns the patent), the technology class of the patent, and detailed information on subsequent patents that cite the patent.

To calculate the control variables used in our study, we collect financial statement information from Compustat, stock price information from CRSP, institutional holdings data from Thomson's CDA/Spectrum database (form 13F), and patenting-related litigation data from the Audit Analytics Litigation Database (augmented with manually reading and processing the litigation case information from Factiva as well as Google search).

3.2 Variable Measurement

3.2.1 Measuring Innovation

We construct four measures to gauge the quality, efficiency, and fundamental nature of a firm's innovation output. The first measure is non-self citations per patent, defined as the total number of non-self citations received by all the patents generated in a given year divided by the

[15] The choice of a window of two years before and two years after the event year reflects a trade-off between the accurate measurement of innovation outcomes and noise (i.e., relevance of the event). While a longer period may capture innovation outcome more accurately, it could also introduce more noise, which makes it harder to attribute any changes in innovation output only to the event (Regulation SHO).

[16] If we relax this requirement and only retain firms with non-missing Compustat records in any year during our sample period, the resulting full sample contains 908 pilot firms and 1,832 control firms in the year immediately before the announcement of the pilot program (i.e., 2003). Although all results reported in the paper are based on the restricted sample, they are very similar if the analyses are carried out on the full sample.

total number of patents filed (and eventually granted) in that year. This measure captures the quality (impact) of innovation. We use the application year instead of the grant year to determine a firm's innovation output (i.e., patents) in a given year because the patent application year better aligns with the actual time when the innovation activities take place (Griliches, Pakes, and Hall, 1988).[17] In addition, given that Hall, Griliches, and Hausman (1986) show that the average lag between R&D investment and patent application is within one year (6-12 months), our use of patent application year is reasonably close to when the innovation is being done.

Nevertheless, both the numerator and denominator of the above innovation quality measure are subject to truncation problems. Because we only observe patents that are eventually granted by the end of 2014, patents filed in the last few years of our sample period may still be under review and not granted by 2014. Similarly, patents tend to receive citations over a long period after its grant date, but we observe at best the citations received up to 2014. To deal with these truncation problems, we adjust the patent and citation data by using the "weight factors" first developed by Hall, Jaffe, and Trajtenberg (2001, 2005) and estimating the shape of the application-grant distribution and the citation-lag distribution, respectively.

The patent databases used in our study are unlikely to be affected by survivorship bias. As long as a patent application is eventually granted, it is attributed to the applying firm at the time of application even if the firm later gets acquired or goes bankrupt. Moreover, since patent citations are attributed to the patent rather than the applying firm, the patent granted to a firm that later gets acquired or goes bankrupt can still keep receiving citations long after the firm ceases to exist.

The second measure captures a firm's innovation efficiency, defined as the number of total non-self citations received by the patents generated in a year divided by the R&D expenditures in the previous year. This variable measures the quality of innovation output per dollar of R&D expenses invested. The last two measures capture the fundamental nature of a patent: its originality and generality scores. Patents that cite a wider array of technology classes of existing patents are perceived as having greater originality, while patents that are cited by a wider array of technology classes of subsequent patents are viewed as having greater generality. Specifically, we define a patent's originality score as one minus the Herfindahl index of the

[17] Hall, Jaffe, and Trajtenberg (2001) find that there is an average lag of two to three years between patent application year and grant year, though there is significant variation in the approval time.

three-digit technology class distribution of all the existing patents it cites, and a patent's generality score as one minus the Herfindahl index of the three-digit technology class distribution of all the subsequent patents that cite it.[18]

We merge the patent data with the Russell 3000 index sample. Following the innovation literature, we set the patent and citation counts to zero for Russell-3000 firms not matched to the patent database, because our patent sample covers the entire universe of publicly-traded firms that have filed with the U.S. Patent Office. The distributions of patent grants and non-self citations in our final sample are right skewed, with their medians at zero. Due to the right skewness of non-self citations per patent, we winsorize it at the 99th percentile and then use the natural logarithm of one plus the number of non-self citations per patent (*LnNSCitePat)* as the main innovation quality measure in our analysis. We label the other three innovation output measures, all of which have been winsorized at the 99th percentiles, as *NSCiteLagRD*, *Originality*, and *Generality*, respectively.

3.2.2 Measuring Control Variables

Following the innovation literature, we control for a vector of firm and industry characteristics that may affect a firm's innovation output in our analysis. We compute all variables for firm *i* over its fiscal year *t*. Our control variables include firm size (the natural logarithm of book value assets), firm age (the natural logarithm of a firm's age since its IPO year), profitability (ROA), investments in intangible assets (R&D expenditures over total assets), asset tangibility (net PPE scaled by total assets), leverage, capital expenditures, growth opportunities (Tobin's *Q*), financial constraints (the Kaplan and Zingales (1997) five-variable KZ index), industry concentration (the Herfindahl index based on sales), and institutional ownership. To control for non-linear effects of product market competition on innovation outputs (Aghion et al., 2005), we also include the squared Herfindahl index in our regressions. We provide detailed variable definitions in the Appendix.

3.3 Summary Statistics

[18] Note that due to truncation problems, a patent's originality score (which only relies on past information) is more accurately measured than its generality score, which can only capture the technology classes of subsequent patents up to 2014.

To minimize the effect of outliers, we winsorize all control variables at the 1st and 99th percentiles. Table 1 provides summary statistics of the variables. On average, a firm in our sample has about 0.8 (=$e^{0.606}$-1) non-self citations per patent. One million dollars of R&D expenditures in the previous year will on average generate 1.6 non-self citations in the current year. The average originality and generality scores are 0.137 and 0.113, respectively. Regarding control variables, an average firm has a book value asset of $5.48 billion, ROA of 9.2%, PPE-to-assets ratio of 47.4%, leverage of 17.0%, capital expenditure ratio of 4.5%, Tobin's *Q* of 1.9, and is 21.3 years old since its IPO date.

4. EMPIRICAL RESULTS

4.1 Baseline Difference-in-differences Results

In our baseline analysis, we use a quasi-natural experiment, Regulation SHO, to identify the causal effect of short sellers on firm innovation. Before July 2004, short selling activities in the U.S. equity market were constrained by a regulation commonly referred to as the "uptick rule", which prohibited short sales when stock prices were declining. On July 28, 2004, however, the SEC announced a new policy experiment, Regulation SHO, to remove all short sale restrictions for a randomly selected group of firms (the pilot group), which include 968 stocks. The selection of pilot firms followed a Rule 202T program, which first ranked all Russell 3000 stocks listed on NYSE, NASDAQ, and AMEX according to their average trading volume, and then picked every third stock within each of the three exchanges starting with the second one. The pilot stocks were exempted from the short-sale price tests (including the bid test for NASDAQ National Market stocks and the tick test for exchange-listed stocks) after the implementation of Regulation SHO, which significantly reduced the costs of short selling these stocks during the period. Meanwhile, non-pilot stocks in the SHO program, however, were still subject to the short-sale price tests.

When selecting the pilot firms, the SEC was mainly concerned with the equal representation of the three stock exchanges in the list and the average trading volumes of such stocks, because the objective of the policy experiment was to test the effect of short selling restrictions on market volatility, stock liquidity, and price efficiency. Therefore, the pilot study was not initiated due to any specific corporate events. Nor did it aim to influence firms' investment behavior (especially their innovation activities) in any significant way.

Regulation SHO provides a nice quasi-natural experiment to examine the causal effects of short sellers on innovation: the assignment of pilot firms was random and unexpected in the sense that there were no signs of lobbying and individual firms could not predict ex-ante whether they would be included into the pilot program. Further, the costs of selling short were significantly reduced for pilot firms (the treatment group) compared to non-pilot firms in the Russell 3000 Index (the control group) because of the elimination of price tests. Therefore, it allows us to adopt a difference-in-differences (DiD) framework to study the effect of short sellers on firm innovation.

Before conducting our DiD analysis, we first verify the premise that the selection of pilot firms was a random draw from the Russell 3000 index. Following the previous literature, we compare the characteristics of pilot and control firms at their fiscal year ends immediately before the announcement month of the pilot program (July, 2004). We report the results in Table 2. In the top four rows, we compare the four outcome variables, *LnNSCitePat, NSCiteLagRD, Originality,* and *Generality*, between treatment and control groups. While the treatment firms appear slightly less innovative than the control firms along these quality and efficiency dimensions, the differences in both mean and median are not statistically significant.

Next, we compare other characteristics across these two groups of firms and observe similar mean values of firm assets, R&D expenditure ratios, asset tangibility, leverage, capital expenditure ratios, Tobin's Q, KZ index, Herfindahl index, and institutional ownership. It appears that treatment firms are slightly older and more profitable than control firms, though the magnitude in the differences is small.

Finally, we check whether the parallel trend assumption (which is the key identifying assumption) of the DiD approach holds in our sample of treatment (pilot) and control (non-pilot) firms. The parallel trend assumption states that, in the absence of treatment (Regulation SHO in our setting), the observed DiD estimator is zero. To be more precise, the parallel trend assumption does not require the *level* of innovation variables to be identical between the treatment and control firms over the two-year periods before the event because these distinctions are differenced out in the estimation. Instead, this assumption requires similar pre-event *trends* in innovation variables for both the treatment and control groups. Hence, before we carry out the DiD estimation, we perform two diagnostic tests and present corresponding evidence to show that the parallel trend assumption is not violated.

The first piece of evidence is reported in the last eight rows of Table 2. Specifically, we calculate one-year and two-year growth rates of the four innovation output measures before the event (Regulation SHO). The univariate comparisons suggest that there are no statistically significant differences in innovation growth rates between treatment and control firms before the event, suggesting that the parallel trend assumption is likely to hold. The second piece of evidence supporting the satisfaction of the identifying assumption is reported in Figure 1. Panel A depicts the mean of *LnNSCitePat* for the treatment group (net of the control group) over a five-year event window surrounding the passage of Regulation SHO (excluding the event year itself). It shows that the number of non-self citations per patent is trending closely in parallel for the two groups in the two years leading up to the event. Panel B reports a similar pattern for the mean difference of *NSCiteLagRD* between both groups of firms. Likewise, Panel C and Panel D reveal that there does not exist a pre-event trend for the difference between the two groups of firms regarding the originality and generality of innovation.

Next, we perform the DiD tests in a multivariate regression framework. Following Fang, Huang, and Karpoff (2015), we estimate various forms of the following model:

$$Innovation_{i,j,t} = \alpha + \beta Pilot_i * Post_t + \eta Pilot_i + \gamma Z_{i,t} + Industry_j + Year_t + \varepsilon_{i,j,t} \quad (1)$$

where i indexes firm, j indexes industry, and t indexes time. $Innovation_{i,j,t}$ is one of the four innovation output measures. $Pilot_i$ is a dummy variable that equals one for treatment firms and zero for control firms. $Post_t$ is a dummy variable that equals one if the fiscal year ending date is after July 1, 2005 but on or before June 30, 2007, and equals zero if the fiscal year ending date is after July 1, 2002 but on or before June 30, 2004, which ensures that the innovation outputs of a firm capture all of its activities over an entire fiscal year either before or after the exogenous shock (Regulation SHO).[19] Z is a vector of firm and industry characteristics that may affect a firm's innovation productivity as we discussed in Section 3.2.2. *Industry* and *Year* capture industry (2-digit SIC level) fixed effects and fiscal year fixed effects, respectively. The coefficient estimate on *Pilot*Post* is the DiD estimate that captures the causal effect of short sellers on firm innovation. Note that *Post* itself is dropped in the specification because it is perfectly correlated with (and thus fully absorbed by) the year fixed effects. To address possible correlations among residuals both within firm and across time, we cluster standard errors by both firm and year (i.e., adopting a two-way clustering method).

[19] This specification effectively removes the event year from our analysis.

Table 3 Panel A reports the regression results estimating equation (1). The dependent variable is *LnNSCitePat* in columns (1) and (2). In column (1), we present a parsimonious specification without including any control variables (other than industry and year fixed effects). The DiD estimator, which is the coefficient estimate on *Pilot*Post*, is 0.027 and significant at the 5% level, suggesting that pilot firms whose exposure to short selling goes up due to Regulation SHO experience an increase of *LnNSCitePat* that is 0.027 higher (which also means an increase of non-self citations per patent that is 2.7% higher) than that of control firms over a five-year period around the event. This difference is economically sizeable, as it represents approximately 10.3% of the average change of *LnNSCitePat* for the control firms in our sample (-0.261). In column (2), we include a battery of control variables. The coefficient estimate on *Pilot*Post* continues to be positive and significant at the 5% level. The magnitude of the DiD estimator suggests that a reduction in short selling costs due to Regulation SHO leads to an increase of 4.5% in non-self citations per patent for the treatment group compared to the control group, which is also economically significant.

We replace the dependent variable with *NSCiteLagRD* in columns (3) and (4), and continue to observe positive and significant DiD estimators. The DiD estimator for *NSCiteLagRD* in column (4) is 0.262 and significant at the 1% level. Given that the average change of *NSCiteLagRD* for the control firms in our sample is -1.163, this represents an approximately 22.5% of the change, which is also economically sizable. Similarly, columns (5) and (6) find that the exogenous reduction in short selling costs leads to a positive and significant increase in *Originality* for the treatment group relative to the control group. The magnitude of the DiD estimator in column (6) suggests that Regulation SHO leads to an increase of 0.011 in the patent originality score for the treatment group compared to the control group, about 68.8% of the average change of *Originality* for the control firms (-0.016). However, we do not find any significant change in *Generality* of the patents generated by the treatment group relative to the control group in columns (7) and (8). Overall, the evidence from the baseline DiD tests is consistent with the *disciplining hypothesis*.

One concern of our baseline DiD analysis is that many firms in the sample do not generate patents at all, which may bias our results. To address this concern, we re-estimate equation (1) based on a sample of firms that generate at least one patent in our sample period and report the results in Panel B of Table 3. The coefficient estimates on *Pilot*Post* are positive and significant at the 5% or 1% level for *LnNSCitePat* and *NSCiteLagRD* and at the 10% level for *Originality* in column (6), which is broadly consistent with the results reported in Panel A. In addition, the magnitudes of the DiD estimators in Panel B are larger than those in Panel A because this sample contains more innovation-relevant firms. The evidence presented in Panel B shows that our main results are not driven by the large number of firm-year observations with zero innovation output.

One unique feature of the SHO experiment is that the tick restriction was officially removed for all stocks on August, 2007, which provides us an opportunity to check whether the pattern of innovation output for pilot and non-pilot firms reversed after the pilot program ended.[20] Hence, we carry out a DiD test for the "reversal" of the SHO experiment using the same set of pilot and control firms but focusing on their innovation activities around August, 2007. Specifically, we redefine *Post* to be zero if the fiscal year ending date is after July 1, 2005 but on or before June 30, 2007, and to be one if the fiscal year ending date is after July 1, 2008 but on or before June 30, 2010. Since the pilot firms experienced no changes in their exposure to short sellers whereas the control group became more exposed to short selling pressure after the pilot program ended in August 2007, we expect the DiD estimators for our innovation measures to be negative. Panel C of Table 3 reports the DiD test results. Broadly consistent with our conjecture, the coefficient estimates on *Pilot*Post* are negative and significant at the 5% or 1% level for *LnNSCitePat* and *Originality*, suggesting that the control firms indeed experienced a greater increase in innovation quality and originality after their short selling constraints are removed than did the pilot firms whose costs of short selling remain unchanged. However, we find no evidence of reversal for *NSCiteLagRD*, which could be due to the partially anticipated nature of this second experiment of Regulation SHO.

Finally, for completeness, we also examine the quantity of innovation output, i.e., the natural logarithm of one plus the total number of patents a firm generates, in our DiD framework. This analysis can also shed light on the implications of the *pressure hypothesis*, which posits that an increase in short selling pressure due to Regulation SHO should decrease the quantity of innovation output for pilot firms by a larger extent than for non-pilot firms. Panel D reports the results. While the DiD estimator is negative, it is statistically insignificant, which seems

[20] However, this second experiment offered by the Regulation SHO program is not as unexpected or exogenous as the first one in 2004 because the pilot and non-pilot firms were already chosen during the first experiment and they might anticipate (or even lobby for) the reversal of the SHO experiment as time goes by.

inconsistent with the implications of the *pressure hypothesis*.

Overall, our identification tests reported in this subsection suggest a positive, causal effect of short sellers on the quality, efficiency, and originality of technological innovation, consistent with the *disciplining hypothesis*. At the same time, short sellers appear not to have a significant impact on the quantity of innovation output.

4.2 Robustness tests

In this subsection, we perform two robustness tests for our DiD analysis reported in Section 4.1 to strengthen our causal argument.

First, to address the concern that our DiD results could have been driven by chance, we run simulations that randomize the inclusion of pilot firms in our analysis. For each simulation, we draw a random sample of 748 “pilot” firms from the pool of actual pilot and non-pilot firms in the event year (2004), and then treat the rest of the pool (the remaining 1,486 firms) as “non-pilot” firms. We perform the DiD test on this simulated sample following the model specifications in Table 3 Panel A and repeat this procedure 5,000 times. We then summarize the regression results from this bootstrapped sample, and report the distribution (i.e., mean, 25^{th} percentile, median, 75^{th} percentile, and standard deviation) of the DiD estimates, namely, the coefficient estimates on *Pilot*Post*, as well as their corresponding t-statistics in Panel A of Table 4. The DiD estimators based on the randomized sample have negative signs across all eight model specifications, with magnitudes close to zero. In addition, the distribution of the t-statistics indicates that none of these DiD estimators is statistically significant on average. Therefore, we cannot reject the null hypothesis that the DiD estimates obtained from this randomization test are zero.

Second, we address the concern that our identification tests mainly rely on one regulatory change (i.e., Regulation SHO) that took place in 2004. Specifically, unobservable shocks which occurred prior to 2004 but are unrelated to Regulation SHO could have driven both the inclusion into the pilot list and firm innovation, undermining the causal inference we draw from the experiment. Although this is unlikely, since the choice of the pilot stocks by SEC only depends on the ranking of Russell 3000 stocks’ trading volume on an exogenously given date, which is highly random, we still perform a formal test to address this concern. To that end, we conduct a placebo test by taking the true set of pilot and non-pilot firms identified by Regulation SHO but artificially picking a "pseudo-event" year when we assume a regulatory shock reduced short selling costs. Panel B of Table 4 reports the DiD estimation results using 2001 (which is three years before the actual event year) as the pseudo-event year. To save space, we suppress the coefficients of control variables. The coefficient estimates on *Pilot*Post*, while positive, are much smaller than our main DiD estimators and statistically insignificant.[21]

In summary, the above robustness test results suggest that the identified positive effect of short sellers on the quality, efficiency, and originality of firm innovation, using plausibly exogenous variation generated by Regulation SHO, is unlikely to be driven by chance or by other unobservable shocks. Therefore, the effect of short sellers on firm innovation appears causal.

5. UNDERLYING MECHANISM: PATENTING-RELATED LITIGATION RISK

Having established a positive, causal link between short sellers and the quality, efficiency, and originality of firm innovation, in this section, we aim to further understand the underlying mechanisms through which the exposure to short selling activities enhances firm innovation.

Our *disciplining hypothesis* predicts that short sellers promote firm innovation through their threat of selling short when they detect the inferior quality of the patents generated by the firm. As previous literature shows, short sellers are one of the most important groups of investors that can affect stock prices due to their active information production and flexible trading strategies. Whenever they expect any upcoming adverse events for the firms or detect any misconducts of the management, they would short sell the shares, initiating or speeding up the price decline and triggering disciplinary actions against the managers due to poor stock performance.[22]

One particular strategy adopted by short sellers to attack innovative firms (and make profits from it) is to file lawsuits against the latter, accusing their patents of being either spurious or of little value, while in the meantime betting against their share prices. There are plenty of real-world examples demonstrating the existence and prevalence of this powerful and profitable strategy used by short sellers. For example, in early 2015, Hayman Capital challenged two of the five patents covering multiple sclerosis drug Ampyra, which accounts for 91% of the revenue of

[21] Using other "pseudo-event" years such as 1999 or 2000 yields qualitatively similar results.
[22] Furthermore, existing literature has shown that short sellers play an active monitoring role in the sense that they reduce earnings management (Fang, Huang, and Karpoff, 2015), improve corporate governance (Massa, Zhang, and Zhang, 2015), and detect financial frauds (Karpoff and Lou, 2010).

Acorda Therapeutics Inc. For another example, Ferrum Ferro Capital filed a petition with the USPTO's Patent Trial and Appeal Board to invalidate an Allergan patent in March 2015. Cohen, Gurun, and Kominers (2015) formally document the existence and patterns of patenting-related lawsuits and further show that such lawsuits impose a credible and real litigation risk to innovative firms. Hence, the patenting-related litigation risk, namely, the risk of being sued by short sellers against a firm's patent portfolio, is a plausible mechanism through which the increase in short selling threat disciplines firm management to improve the firm's innovation quality, efficiency, and originality.

To explore this possible economic channel, we perform three tests. First, we examine whether a reduction in short selling costs due to Regulation SHO indeed increases a firm's patenting-related litigation risk. For each year in our sample period, we collect the information about all U.S. patenting-related lawsuits from the Audit Analytics Litigation Database. Then, we manually distinguish between the types of the plaintiffs and defendants of each lawsuit. We focus on the lawsuits in which the defendant is an industrial company while the plaintiffs are financial institutions, law companies, patent companies, or individuals other than the current or previous employees of the defendant. These lawsuits are likely to be initiated by either potential short sellers or their affiliates. If, in a year, a U.S. public firm experiences any patenting-related lawsuits as defined above, we set the dummy variable, *PatentCase*, to be one for that year, and zero otherwise.

Next, we follow Cohen, Gurun, and Kominers (2015) to estimate the probability that a given firm will face a patenting-related lawsuit in any year by regressing *PatentCase* on a firm's one-year lagged size (measured by the logarithm of market capital), Tobin's Q, cash ratio (cash over assets), stock price run-up in the previous twelve months, and the size of its patent portfolio (the logarithm of the total number of patents generated by the firm in the previous three years), as well as firm and year fixed effects. We obtain the fitted value of *PatentCase* from this linear probability model, *LitigationRisk_NonInd*, and treat it as a firm's patenting-related litigation risk from potential short sellers. We also estimate a firm's patenting-related litigation risk from all types of plaintiffs, *LitigationRisk_All*, by examining lawsuits with all kinds of plaintiffs (i.e., including industrial companies and former/current employees of the firm) and adopting a similar methodology.[23]

Tests reported in Table 5 adopt the same DiD framework as in our main analyses and examine how the patenting-related litigation risk changes differently for pilot and non-pilot firms around the implementation of Regulation SHO. As we can see, the DiD estimators for both *LitigationRisk_NonInd* and *LitigationRisk_All* are positive and significant at either the 1% or 5% level. In terms of economic significance, the coefficient estimate of 0.018 before *Pilot*Post* in Column (2) indicates that the exposure to patenting-related litigations by potential short sellers increases by 0.018 percentage points more for pilot (treatment) firms compared to non-pilot (control) firms. This difference is nontrivial, as it represents approximately 4% of the average level of *LitigationRisk_NonInd* for our sample firms before the regulatory shock. This result demonstrates that a reduction in short selling costs due to Regulation SHO indeed increases a firm's patenting-related litigation risk from potential short sellers. If we examine the litigation risk from all types of plaintiffs (as short sellers, rather than suing the company directly, may also bring the attention to the patenting issues from other industrial companies who then launch the lawsuits), the results still hold, as shown in Columns (3) and (4).

Second, we partition our sample based on a firm's ex-ante exposure to patenting-related litigation risk and examine the cross-sectional variation in our DiD estimators. In particular, we partition our sample into two subsamples based on whether a firm's one-year-lagged exposure to patenting-related litigations by potential short sellers (*LitigationRisk_NonInd*) is above (below) the top (bottom) tercile of our sample. We then perform the same DiD test as in our main analyses on the two subsamples separately and report the results in Table 6.

The first two columns report the results with innovation quality (non-self citations per patent) as the dependent variable. The DiD estimator, namely, the coefficient estimate on *Pilot*Post* in column (1), in which firms face a higher degree of patenting-related litigation risk, is positive and significant at the 1% level. In contrast, the positive effect of short sellers on innovation quality is absent in column (2) which examines firms facing lower ex-ante litigation risk. Further, a Chi-squared test of the difference between high and low groups using a seemingly unrelated regression technique is significant at the 1% level. We observe similar patterns for innovation efficiency and originality (in columns (3) to (6)), though we do not find

[23] Since the unconditional probability of having a patenting-related lawsuit is small in the population, we multiply both measures by 100 to express them in terms of percentage points.

significantly differential treatment effects for a firm's innovation generality (columns (7) and (8)). These findings confirm our conjecture that the positive effect of short sellers on innovation is more pronounced for firms with greater ex-ante patenting-related litigation risk.

Third, if patenting-related litigation risk is indeed an important mechanism through which short sellers affect innovation, our DiD estimates should become weaker once we directly control for the change in such litigation risk before and after Regulation SHO. In other words, there should be a much weaker "residual" effect of Regulation SHO (i.e., our main treatment) on firms' innovation activities once the litigation risk channel has been explicitly controlled for.

To test this conjecture, we directly include a firm's exposure to patenting-related litigation in our baseline DiD regressions, and report the results in Table 7. We find that the positive effect of short sellers on the quality, efficiency, and originality of innovation substantially weakens or becomes insignificant, and that the total explanatory power of the independent variables (i.e., the R^2) increases significantly after we directly include the new mechanism variable in the regressions. For example, if we compare columns (1) and (2), the coefficient estimate on *Pilot*Post* is 0.04 and significant at the 1% without controlling for the change in litigation risk, whereas it becomes 0.02 and statistically insignificant after this mechanism variable is controlled for. In the meantime, the R^2 increases by about 55% (from 0.324 to 0.502) across the two models, showing the large incremental explanatory power offered by the additional independent variable.[24]

Overall, the evidence in this section suggests that patenting-related litigation risk is a plausible economic channel through which short sellers helps discipline corporate managers and improves the quality, efficiency, and originality of innovation.

6. ALTERNATIVE INTERPRETATIONS

In this section, we discuss a few alternative interpretations for our main results.

First of all, a potential explanation for the positive effect of short sellers on innovation is managerial learning from more informative prices. Specifically, the removal of short selling constraints by Regulation SHO allows short sellers to better incorporate their private information about pilot firms into the latter's stock prices, making the prices more informative. Due to the

[24] Note that in order to properly compare the models with and without the patenting-related litigation risk variable, we need to require that the two samples on which we perform estimation are identical. That's why the baseline DiD results reported in this table may differ slightly from those reported in Table 3 Panel A: We require firms to have non-missing litigation risk variable here.

feedback effect of secondary market trading on firms' investment decisions, as documented by recent studies such as Chen, Goldstein, and Jiang (2007), managers of pilot firms would then learn better from their more informative stock prices and improve their innovation strategies, which leads to an enhancement in the quality, efficiency, and originality of their generated patents.

To investigate this alternative explanation for our main findings, we follow the literature to construct two measures of stock price informativeness and examine their changes surrounding the policy experiment of Regulation SHO. The first measure is the probability of informed trades (PIN), first developed in Easley, Kiefer, and O'Hara (1996, 1997a, b) and modified extensively by many follow-up studies. The idea behind this measure is to use a structural market microstructure model to directly capture the probability of informed trading in a stock via analysis of buy and sell orders. Trades for stocks with a high PIN are more likely to convey information coming from private sources than from public sources. The PIN measure we use in this paper is developed in Brown and Hillegeist (2007) and generously provided on the website of Stephen Brown (http://scholar.rhsmith.umd.edu/sbrown/pin-data). The second measure is price nonsynchronicity, which is first proposed by Roll (1988) and computed based on the correlation between a firm's stock return and the return of its industry and the overall market. Following Chen, Goldstein, and Jiang (2006), we first regress a firm's daily stock returns in year *t* on a constant, the CRSP value-weighted market return, and the return of the 3-digit SIC industry portfolio, and then use one minus the R^2 from the above regression as a proxy for the price nonsynchronicity for this firm-year. A higher value of price nonsynchronicity indicates that the firm's stock price reflects more firm-specific information (which is useful for managerial investment decisions) because its stock return is less correlated with the market and the industry returns. As argued by various previous studies, this measure has little correlation with public news and thus mostly captures private information contained in the stock price.

In untabulated analysis, we do not find these two stock price informativeness measures to increase more significantly for pilot firms compared to those for non-pilot firms in our DiD setting. Hence, the results are inconsistent with the conjecture that the removal of the uptick rule by Regulation SHO improves stock price informativeness, which does not support managerial learning as an alternative explanation for our main results.[25]

[25] In a contemporaneous study, De Angelis, Grullon, and Michenaud (2015) find a similar result to ours.

Second, a change in the CEO compensation structure due to the increased short selling threat may potentially explain our main results. De Angelis, Grullon, and Michenaud (2015) argue that Regulation SHO, by relaxing the short selling constraints for pilot firms, increases their downside risk, which might reduce managerial effort and discourage firm managers from taking risks. In order to combat the negative effect of the short selling pressure on managerial incentives, the boards of pilot firms would grant relatively more stock options (rather than restricted stocks) to their executives and adopt new anti-takeover provisions. To the extent that more stock options and a larger number of anti-takeover provisions increase firm innovation (Francis, Hasan, and Sharma, 2013; Chemmanur and Tian, 2014), short sellers might improve the quality, efficiency, and originality of innovation through an executive compensation channel.

However, if these new changes to managerial compensation are aimed at increasing the lowered risk-taking incentive of managers due to Regulation SHO, we should not expect a systematic increase in risk taking (e.g., the improvement of innovation quality) by pilot managers unless we assume that pilot boards on average "overshoot" in designing these new compensation measures to encourage risk taking. In other words, if pilot firms on average do not over-invest or under-invest in innovation prior to Regulation SHO, then we would not observe a relative increase in innovation for this group of firms because the new compensation measures would just offset the negative (and suboptimal) effect of the increased short selling pressure on managers' innovation incentives. Thus, the fact that we observe a positive and significant effect of Regulation SHO on the quality, efficiency, and originality of innovation suggests that the management compensation channel cannot fully explain our results (unless these new compensation packages are suboptimal and encourage excessive risk taking).

Moreover, if the executive compensation channel drives our main results, we should be able to observe an increase in both the input of innovation (i.e., R&D expenses) and the quantity of it (i.e., patent counts), in addition to the quality, efficiency, and originality of it. As we reported earlier, we do not observe a significantly larger increase in patent counts for pilot firms than for non-pilot firms. In untabulated analysis, we do not find a significant larger increase in R&D expenditures for pilot firms either. These findings suggest that the executive compensation channel discovered by De Angelis, Grullon, and Michenaud (2015) is unlikely to be the main driver of our results.

7. CONCLUSION

In this paper, we examine the causal effect of short sellers on the real economy in the case of innovation. To establish causality, we use exogenous variation in short-selling costs generated by a quasi-natural experiment, Regulation SHO, which randomly assigns a subsample of the Russell 3000 index firms into a pilot program and eliminates all their short selling restrictions. We show that short sellers appear to have a positive, causal effect on the quality, efficiency, and originality of innovation. An exogenous reduction in short selling costs due to Regulation SHO leads to a larger increase in patent citations, innovation output per R&D dollar, and patent originality scores for pilot firms compared to non-pilot firms in the same Russell 3000 index, and this pattern is partially reversed when Regulation SHO officially ends (i.e., short selling constraints removed for all firms) in 2007. The exposure to patenting-related litigations initiated by short sellers appears to be a plausible underlying mechanism through which short sellers affect firm innovation. Our paper provides new insights into an unintended real effect of short sellers – their enhancement on innovation.

REFERENCES

Acharya, V. and K. Subramanian, 2009. Bankruptcy codes and innovation. *Review of Financial Studies* 22, 4949-4988.

Acharya, V., R. Baghai, and K. Subramanian, 2013. Labor laws and innovation. *Journal of Law and Economics* 56, 997-1037.

Acharya, V., R. Baghai, and K. Subramanian, 2014. Wrongful discharge laws and innovation. *Review of Financial Studies* 27, 301-346.

Aghion, P., N. Bloom, R. Blundell, R. Griffith, and P. Howitt, 2005. Competition and innovation: An inverted-U relationship. *Quarterly Journal of Economics* 120, 701-728.

Aghion, P., J. Van Reenen, and L. Zingales, 2013. Innovation and institutional ownership. *American Economics Review* 103, 277-304.

Battalio, R. and P. Schultz, 2006. Options and the bubble. *Journal of Finance* 61, 2071-2012.

Beber, A. and M. Pagano, 2013. Short-selling bans around the world: Evidence from the 2007-2009 crisis. *Journal of Finance* 68, 343-381.

Benner, M. J. and R. Ranganathan, 2012. Offsetting illegitimacy? How pressures from securities analysts influence incumbents in the face of new technologies. *Academy of Management Journal* 55, 213-233.

Bernstein, S., 2015. Does going public affect innovation? *Journal of Finance* 70, 1365-1403.

Bertrand, M. and S. Mullainathan, 2003. Enjoying the quiet life? Corporate governance and managerial performance. *Journal of Political Economy* 111, 1043-1075.

Boehmer, E., C. Jones, and X. Zhang, 2008. Which shorts are informed? *Journal of Finance* 63, 491-527.

Boehmer, E., C. Jones, and X. Zhang, 2013. Shackling short sellers: The 2008 shorting ban. *Review of Financial Studies* 26, 1363-1400.

Boehmer, E. and J. Wu, 2013. Short selling and the price discovery process. *Review of Financial Studies* 26, 287-322.

Bond, P. E., A. Edmans and I. Goldstein, 2012. The real effects of financial markets. *Annual Review of Financial Economics* 4, 339-360.

Bond, P., I. Goldstein and E. S. Prescott, 2010. Market-Based corrective actions. *Review of Financial Studies* 23, 781-820.

Brown, S., and S. Hillegeist, 2007. How disclosure quality affects the level of information asymmetry. *Review of Accounting Studies* 12, 443-477.

Chemmanur, T., E. Loutskina, and X. Tian, 2014. Corporate venture capital, value creation, and innovation. *Review of Financial Studies* 27, 2434-2473.

Chemmanur, T., and X. Tian, 2014. Anti-takeover provisions, innovation, and firm value: A regression discontinuity analysis. *Unpublished working paper*.

Chen, J., H. Hong, and J. Stein, 2002. Breadth of ownership and stock returns, *Journal of Financial Economics* 66, 171-205.

Cohen, L. H., U. Gurun, and S. Kominers, 2015. Patent trolls: Evidence from targeted firms. *Unpublished working paper*.

Cornaggia, J., Y. Mao, X. Tian, and B. Wolfe, 2014. Does banking competition affect innovation? *Journal of Financial Economics*, forthcoming.

De Angeles, D., G., Grullon, and S. Michenaud, 2015. The effects of short-selling threats on incentive contracts: Evidence from a natural experiment. *Unpublished working paper*.

Derrien, F. and A. Kecskes, 2013. The real effects of financial shocks: Evidence from exogenous changes in analyst coverage. *Journal of Finance* 68, 1407-1440.

Diether, M., J. Lee, and J. Werner, 2009. It's SHO time! Short-sales price tests and market quality. *Journal of Finance* 64, 37-73.

Dow, J. and G. Gorton, 1997. Stock market efficiency and economic efficiency: Is there a connection? *Journal of Finance* 52, 1087-1129.

Easley, D., N. Kiefer, and M. O'Hara, 1996. 'Cream-skimming or profit-sharing? The curious role of purchased order flow. *Journal of Finance* 51, 811–833.

Easley, D., N. Kiefer, and M. O'Hara, 1997a. The information content of the trading process. *Journal of Empirical Finance* 4, 159–186.

Easley, D., N. Kiefer, and M. O'Hara, 1997b. One day in the life of a very common stock. *Review of Financial Studies* 10, 805–835.

Ederer, F. and G. Manso, 2013. Is pay-for-performance detrimental to innovation? *Management Science* 59, 1496-1513.

Fang, V., A. Huang, and J.M. Karpoff, 2015. Short selling and earnings management: A controlled experiment. *Journal of Finance,* forthcoming.

Francis, B., I. Hasan, and Z. Sharma, 2009. Do incentives create innovation? Evidence from CEO compensation contracts. Working Paper, Rensselaer Polytechnic Institute.

Goldstein, I. and A. Guembel, 2008. Manipulation and the allocational role of prices. *Review of Economic Studies* 75, 133-164.

Graham, J. R., C. R. Harvey and S. Rajgopal, 2005. The economic implications of corporate financial reporting. *Journal of Accounting and Economics* 40, 3-73.

Griliches, Z., A. Pakes, and B. Hall, 1988. The value of patents as indicators of inventive activity. *Unpublished working paper.*

Gilchrist, S., C. Himmelberg, and G. Huberman, 2005. Do stock price bubbles influence corporate investment? *Journal of Monetary Economics* 52, 805-827.

Gompers, P, 1996. Grandstanding in the venture industry. *Journal of Financial Economics* 42, 133-156.

Grullon, G., S. Michenaud, and J. Weston, 2015. The real effects of short-selling constraints. *Review of Financial Studies* 28, 1737-1767.

Grossman, S., 1976. On the efficiency of competitive stock markets where trades have diverse information. *Journal of Finance* 31, 573-585.

Grossman, S. and O. Hart, 1988. One share-one vote and the market for corporate control. *Journal of Financial Economics* 20, 175-202.

Hall, B., A. Griliches, and J. Hausman, 1986. Patents and R&D: Is there a lag? *International Economic Review* 27, 265-283.

Hall, B., A. Jaffe, and M. Trajtenberg, 2001. The NBER patent citation data file: lessons, insights and methodological tools. *Unpublished working paper.*

Hall, B., A. Jaffe, and M. Trajtenberg, 2005. Market value and patent citations, *RAND Journal of Economics* 36, 16-38.

Harris, M. and A. Raviv, 1988. Corporate governance: Voting rights and majority rules. *Journal of Financial Economics* 20, 203-235.

Harrison, J. and D. Kreps, 1978. Speculative investor behavior in a stock market with heterogeneous expectations. *Quarterly Journal of Economics* 92, 323-336.

Hayek, F. A., 1945. The use of knowledge in society. *American Economic Review* 35, 519-530.

He, J. and X. Tian, 2013. The dark side of analyst coverage: The case of innovation. *Journal of Financial Economics* 109, 856-878.

Hellwig, M. F., 1980. On the aggregation of information in competitive markets. *Journal of Economic Theory* 22, 477-498.

Hirshleifer, D., A. Low, and S.H. Teoh, 2012. Are overconfident CEOs better innovators? *Journal of Finance* 67, 1457-1498.

Hirshleifer, D., S.H. Teoh, and J. Yu, 2011. Short arbitrage, return asymmetry and the accrual anomaly. *Review of Financial Studies* 24, 2429-2461.

Holmstrom, B., 1989. Agency costs and innovation. *Journal of Economic Behavior and Organization* 12, 305-327.

Li, Y., and L. Zhang, 2015. Short selling pressure, stock price behavior, and management forecast precision: Evidence from a natural experiment. *Journal of Accounting Research*, 53, 79-117.

Kaplan, S. and L. Zingales, 1997. Do financial constraints explain why investment is correlated with cash flow? *Quarterly Journal of Economics* 112, 169-215.

Karpoff, J. M. and X. Lou, 2010. Short sellers and financial misconduct. *Journal of Finance* 65, 1879-1913.

Lerner, J., M. Sorensen, and P. Stromberg, 2011. Private equity and long-run investment: The case of innovation. *Journal of Finance* 66, 445-477.

Manso, G., 2011. Motivating innovation. *Journal of Finance* 66, 1823-1860.

Massa, M., B. Zhang, and H. Zhang, 2015. Invisible hand of short selling: Does short-selling discipline earnings management? *Review of Financial Studies* 28, 1701-1736.

Miller, E., 1977. Risk, uncertainty, and divergence of opinion. *Journal of Finance* 32, 1151-1168.

Mitchell, M., T. Pulvino, and E. Stafford, 2004. Price pressure around mergers. *Journal of Finance* 59, 31-63.

Nanda, R. and M. Rhodes-Kropf, 2013. Investment cycles and startup innovation. *Journal of Financial Economics* 110, 403-418.

Nagel, S., 2005. Short sales, institutional investors and the cross-section of stock returns. *Journal of Financial Economics* 78, 277-309.

Porter, M., 1992. Capital disadvantage: America's failing capital investment system. *Harvard Business Review* 70, 65-82.

Roll, R., 1988. R2. *Journal of Finance* 43, 541–566.

Scharfstein, D. and J. Stein, 2002. The dark side of internal capital markets: Divisional rent-seeking and inefficient investment, *Journal of Finance* 55, 2537–2564.

Seru, A., 2014. Firm boundaries matter: Evidence from conglomerates and R&D activity. *Journal of Financial Economics* 111, 381-405.

Solow, R., 1957. Technological change and the aggregate production function. *Review of Economics and Statistics* 39, 312-320.

Stein, J., 1988. Takeover threats and managerial myopia. *Journal of Political Economy* 96, 61-80.

Subrahmanyam, A. and S. Titman, 1999. The going-public decision and the development of financial markets. *Journal of Finance* 54, 1045-1082.

Tian, X. and T. Wang, 2014. Tolerance for failure and corporate innovation. *Review of Financial Studies* 27, 211-255.

Appendix: Definition of variables

Variable	Definition
Measures of innovation	
$LnPatent_t$	Natural logarithm of one plus firm *i*'s total number of patents filed (and eventually granted) in year *t*;
$LnNonCitePat_t$	Natural logarithm of one plus firm *i*'s total number of non-self citations received on the firm's patents filed (and eventually granted), scaled by the number of the patents filed (and eventually granted) in year *t*;
$NSCiteLagRD_t$	Firm *i*'s total number of non-self citations received by the patents generated in year *t* divided by the R&D expenditures (#46) in year *t-1*;
$Originality_t$	Firm *i*'s average originality score across all patents it generates in year *t*, where the score for each patent is defined as one minus the Herfindahl index of the three-digit technology class distribution of all the previous patents it cites;
$Generality_t$	Firm *i*'s average generality score across all patents it generates in year *t*, where the score for each patent is defined as one minus the Herfindahl index of the three-digit technology class distribution of all the subsequent patents that cite it;
Measures of control variables	
$Assets_t$	Book value of total assets (#6) measured at the end of fiscal year *t*;
$R\&DAssets_t$	Research and development expenditures (#46) divided by book value of total assets (#6) measured at the end of fiscal year *t*, set to 0 if missing;
Age_t	Firm *i*'s age, approximated by the number of years the firm has been listed on Compustat;
ROA_t	Return-on-assets ratio defined as operating income before depreciation (#13) divided by book value of total assets (#6), measured at the end of fiscal year *t*;
$PPEAssets_t$	Property, Plant & Equipment (net, #8) divided by book value of total assets (#6) measured at the end of fiscal year *t*;
$Leverage_t$	Firm *i*'s leverage ratio, defined as book value of debt (#9 + #34) divided by book value of total assets (#6) measured at the end of fiscal year *t*;
$CapexAssets_t$	Capital expenditure (#128) scaled by book value of total assets (#6) measured at the end of fiscal year *t*;
$TobinQ_t$	Firm *i*'s market-to-book ratio during fiscal year *t*, calculated as [market value of equity (#199 × #25) plus book value of assets (#6) minus book value of equity (#60) minus balance sheet deferred taxes (#74, set to 0 if missing)] divided by book value of assets (#6);

$KZindex_t$	Firm *i*'s KZ index measured at the end of fiscal year *t*, calculated as -1.002 × Cash Flow ((#18+#14)/#8) plus 0.283 × Q ((#6+#199×#25-#60-#74)/#6) plus 3.189 × Leverage ((#9+#34)/(#9+#34+#216)) minus 39.368 × Dividends ((#21+#19)/#8) minus 1.315 × Cash holdings(#1/#8), where #8 is lagged;
$Hindex_t$	Herfindahl index of 4-digit SIC industry *j* where firm *i* belongs, measured at the end of fiscal year *t*;
$InstOwn_t$	The institutional holdings (%) for firm *i* over fiscal year *t*, calculated as the arithmetic mean of the four quarterly institutional holdings reported through form 13F;
$LitigationRisk\_NonInd_t$	Firm *i*'s fitted value of *PatentCase* in year *t* from a linear probability model that regresses *PatentCase* on the firm's lagged log of market capital, Tobin's Q, cash ratio (cash over assets), stock price run-up in the previous twelve months, and its patent portfolio (the log of total number of patents generated by the firm in the previous three years), as well as firm and year fixed effects, where *PatentCase* is a dummy variable equal to 1 if firm *i* is the defendant of a patenting-related lawsuit whose plaintiffs are financial institutions, law companies, patent companies, or individuals other than the current or previous employees of the defendant;
$LitigationRisk\_All_t$	Firm *i*'s fitted value of *PatentCaseAll* in year *t* from a linear probability model that regresses *PatentCaseAll* on the firm's lagged log of market capital, Tobin's Q, cash ratio (cash over assets), stock price run-up in the previous twelve months, and its patent portfolio (the log of total number of patents generated by the firm in the previous three years), as well as firm and year fixed effects, where *PatentCaseAll* is a dummy variable equal to 1 if firm *i* is the defendant of a patenting-related lawsuit whose plaintiffs can be any types of institutions or individuals.

Table 1: Summary statistics

This table reports the summary statistics for variables constructed based on the difference-in-differences (DiD) estimation sample of Russell 3000 index firms. Our sample construction starts with the Russell 3000 index in June 2004. Following the SEC's first pilot order issued on July 28, 2004 (Securities Exchange Act Release No. 50104), which describes in detail how the pilot and non-pilot stocks in the Regulation SHO program were chosen, we exclude stocks that were not listed on the NYSE, AMEX, or NASDAQ NM, and stocks that went public or had spin-offs after April 30, 2004. Out of the remaining stocks, we identify pilot stocks according to the published list of the SEC's pilot order and the rest of the Russell 3000 stocks comprise the initial non-pilot sample. We examine fiscal years whose ending dates are between July 1, 2002 and June 30, 2007, and further require all firms to have non-missing Compustat records to calculate firm characteristics across the sample period. Definitions of variables are listed in the Appendix.

Variable	Mean	P25	Median	P75	S.D.	N
LnNSCitePat	0.606	0.000	0.000	1.227	1.029	8,942
NSCiteLagRD	1.594	0.000	0.000	0.142	6.236	8,575
Originality	0.137	0.000	0.000	0.000	0.276	8,942
Generality	0.113	0.000	0.000	0.000	0.253	8,942
Assets	5.481	0.376	1.130	3.722	12.173	8,940
Age	21.336	9.000	15.000	32.000	15.641	8,942
LnAssets	7.152	5.930	7.030	8.222	1.704	8,940
LnAge	2.860	2.303	2.773	3.497	0.713	8,942
ROA	0.092	0.037	0.104	0.164	0.147	8,901
R&DAssets	0.038	0.000	0.000	0.040	0.081	8,942
PPEAssets	0.474	0.175	0.377	0.699	0.371	7,915
Leverage	0.170	0.009	0.123	0.271	0.178	8,920
CapexAssets	0.045	0.013	0.030	0.057	0.051	8,500
TobinQ	1.935	1.122	1.487	2.209	1.300	8,937
KZindex	-9.719	-9.762	-2.260	0.395	22.718	8,563
HIndex	0.314	0.110	0.220	0.432	0.269	8,942
InstOwn	0.628	0.456	0.668	0.826	0.244	8,936

Table 2: Characteristics immediately prior to the SHO program

This table compares the characteristics of treatment (pilot) and control (non-pilot) firms at their fiscal year ends immediately before the announcement month of the Regulation SHO pilot program (July, 2004). Definitions of variables are listed in the Appendix. The last two columns report the two-sample t-test and the Wilcoxon Ranksum test for the difference between pilot and control firms, respectively. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

	Pilot			Control			Difference	
	Mean	Median	S.D.	Mean	Median	S.D.	T-stat	Wilcoxon
LnNSCitePat	0.685	0.000	1.101	0.700	0.000	1.128	-0.312	0.191
NSCiteLagRD	2.045	0.000	7.801	2.170	0.000	7.220	-0.365	0.717
Originality	0.131	0.000	0.272	0.144	0.000	0.281	-1.106	1.043
Generality	0.119	0.000	0.261	0.127	0.000	0.266	-0.663	0.964
LnAssets	7.006	6.807	1.676	7.057	6.954	1.731	-0.661	0.546
LnAge	2.836	2.833	0.742	2.768	2.639	0.738	2.041**	2.062**
ROA	0.100	0.108	0.127	0.088	0.098	0.143	2.005**	2.168**
R&DAssets	0.035	0.000	0.072	0.038	0.000	0.080	-1.044	0.924
PPEAssets	0.494	0.409	0.365	0.473	0.385	0.375	1.196	1.668
Leverage	0.179	0.145	0.179	0.169	0.119	0.176	1.224	1.353
CapexAssets	0.045	0.031	0.049	0.042	0.028	0.047	1.568	2.043**
TobinQ	2.151	1.573	1.611	2.074	1.546	1.454	1.138	0.665
KZindex	-8.315	-1.633	20.741	-9.115	-2.045	21.399	0.830	1.277
HIndex	0.317	0.210	0.272	0.302	0.204	0.261	1.193	1.020
InstOwn	0.583	0.613	0.235	0.584	0.615	0.243	-0.119	0.294
LnNSCitePatGrow	-0.071	0.000	0.736	-0.102	0.000	0.693	0.979	1.549
NSCiteLagRDGrow	-0.348	0.000	5.214	-0.527	0.000	4.688	0.790	0.619
OriginalityGrow	-0.017	0.000	0.177	-0.005	0.000	0.184	-1.450	0.949
GeneralityGrow	-0.022	0.000	0.186	-0.018	0.000	0.179	-0.465	0.057
LnNSCitePatGrow2Yr	-0.128	0.000	0.824	-0.145	0.000	0.753	0.486	0.310
NSCiteLagRDGrow2Yr	-0.613	0.000	6.363	-0.940	0.000	6.592	1.059	0.614
OriginalityGrow2Yr	-0.025	0.000	0.174	-0.013	0.000	0.185	-1.422	0.202
GeneralityGrow2Yr	-0.023	0.000	0.188	-0.031	0.000	0.199	0.801	1.666

Table 3: Difference-in-differences (DiD) test

This table reports the results of the difference-in-differences (DiD) test on how the exogenous shock to short selling costs, Regulation SHO, affects firm innovation. Definitions of variables are listed in the Appendix. Panel A reports results regarding the quality, efficiency, originality, and generality of innovation using the full DiD sample. Panel B reports results using firms that generate at least one patent over the sample period. Panel C reports the DiD test results for the "reversal" of the SHO experiment (August 2007) by using fiscal years whose ending dates are between July 1, 2005 and June 30, 2010. Panel D reports results on the quantity of innovation output. Control variables are lagged by one year. Each regression includes a separate intercept as well as year and industry fixed effects. Standard errors clustered by both firm and year (i.e., two-way clustering) are displayed in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Panel A: Full sample

Dep. Var.	*LnNSCitePat*$_{t+1}$		*NSCiteLagRD*$_{t+1}$		*Originality*$_{t+1}$		*Generality*$_{t+1}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
*Pilot*Post*	0.027**	0.045**	0.198***	0.262***	0.009**	0.011**	-0.004	-0.004
	(0.012)	(0.020)	(0.065)	(0.090)	(0.005)	(0.005)	(0.004)	(0.005)
Pilot	-0.020	-0.019	-0.251	-0.295	-0.007	-0.007	-0.004	-0.004
	(0.033)	(0.035)	(0.171)	(0.190)	(0.009)	(0.009)	(0.008)	(0.007)
LnAssets		0.130***		-0.041		0.064***		0.062***
		(0.013)		(0.069)		(0.004)		(0.004)
LnAge		0.045		0.026		0.026***		0.023***
		(0.027)		(0.129)		(0.007)		(0.007)
ROA		0.147		-0.607		0.104***		0.083***
		(0.140)		(0.712)		(0.032)		(0.025)
R&DAssets		2.661***		-1.831		0.738***		0.653***
		(0.267)		(1.340)		(0.070)		(0.062)
PPEAssets		0.068		0.193		0.023		0.017
		(0.060)		(0.299)		(0.017)		(0.017)
Leverage		-0.297**		-0.570		-0.082***		-0.067***
		(0.131)		(0.644)		(0.023)		(0.022)
CapexAssets		-0.086		0.690		-0.032		0.053
		(0.248)		(1.785)		(0.060)		(0.051)
TobinQ		0.060***		0.390***		0.012***		0.013***
		(0.015)		(0.086)		(0.003)		(0.003)
KZindex		0.000		-0.001		0.000		0.000
		(0.001)		(0.004)		(0.000)		(0.000)
HIndex		-0.603**		-3.051**		-0.041		-0.034
		(0.269)		(1.482)		(0.062)		(0.061)
HIndex^2		0.572**		2.281*		0.072		0.053
		(0.225)		(1.170)		(0.051)		(0.056)
InstOwn		0.047		0.134		-0.052***		-0.057***
		(0.072)		(0.376)		(0.018)		(0.015)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,942	7,798	8,575	7,455	8,942	7,798	8,942	7,798
R-squared	0.292	0.326	0.149	0.156	0.249	0.339	0.225	0.327

Panel B: Positive patent sample

Dep. Var.	*LnNSCitePat$_{t+1}$*		*NSCiteLagRD $_{t+1}$*		*Originality $_{t+1}$*		*Generality $_{t+1}$*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
*Pilot*Post*	0.068**	0.073**	0.470***	0.429**	0.022	0.023*	-0.009	-0.009
	(0.033)	(0.031)	(0.152)	(0.188)	(0.014)	(0.014)	(0.011)	(0.012)
Pilot	-0.015	-0.017	-0.468	-0.543	-0.007	-0.011	-0.004	-0.006
	(0.058)	(0.055)	(0.425)	(0.405)	(0.020)	(0.018)	(0.017)	(0.014)
LnAssets		0.145***		-0.301*		0.099***		0.100***
		(0.020)		(0.167)		(0.006)		(0.006)
LnAge		-0.109**		-0.415		-0.004		0.001
		(0.049)		(0.288)		(0.013)		(0.013)
ROA		0.273		0.223		0.084*		0.053
		(0.166)		(0.982)		(0.043)		(0.042)
R&DAssets		1.288***		-10.293***		0.631***		0.607***
		(0.288)		(2.505)		(0.094)		(0.080)
PPEAssets		0.117		0.462		0.057*		0.041
		(0.099)		(0.630)		(0.030)		(0.034)
Leverage		-0.113		0.284		-0.077**		-0.065*
		(0.202)		(1.124)		(0.037)		(0.035)
CapexAssets		0.012		4.584		-0.173		0.010
		(0.508)		(4.490)		(0.164)		(0.148)
TobinQ		0.066***		0.615***		0.013***		0.014***
		(0.015)		(0.109)		(0.004)		(0.005)
KZindex		0.001		-0.007		-0.000		-0.000
		(0.002)		(0.010)		(0.000)		(0.000)
HIndex		-0.886**		-4.650*		-0.126		-0.122
		(0.370)		(2.727)		(0.088)		(0.093)
HIndex^2		0.823***		3.253		0.158**		0.133
		(0.315)		(2.172)		(0.073)		(0.090)
InstOwn		-0.052		-1.251		-0.072**		-0.085***
		(0.123)		(0.991)		(0.031)		(0.027)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,909	3,848	3,542	3,505	3,909	3,848	3,909	3,848
R-squared	0.187	0.228	0.137	0.171	0.115	0.276	0.123	0.303

Panel C: Reversal of SHO

Dep. Var.	*LnNSCitePat* $_{t+1}$		*NSCiteLagRD* $_{t+1}$		*Originality* $_{t+1}$		*Generality* $_{t+1}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
*Pilot*Post*	-0.027***	-0.024***	0.005	0.010	-0.009***	-0.008**	0.007	0.009
	(0.007)	(0.008)	(0.020)	(0.022)	(0.003)	(0.003)	(0.007)	(0.007)
Pilot	0.011	0.015	-0.018	-0.020	0.001	0.002	-0.009	-0.008
	(0.024)	(0.022)	(0.043)	(0.043)	(0.009)	(0.007)	(0.008)	(0.007)
LnAssets		0.094***		0.045**		0.060***		0.053***
		(0.011)		(0.021)		(0.004)		(0.004)
LnAge		0.039*		0.028		0.031***		0.020***
		(0.020)		(0.033)		(0.008)		(0.006)
ROA		0.167		-0.272*		0.117***		0.073*
		(0.114)		(0.158)		(0.044)		(0.042)
R&DAssets		1.795***		-0.011		0.750***		0.516***
		(0.353)		(0.443)		(0.072)		(0.088)
PPEAssets		0.004		0.050		0.007		0.008
		(0.042)		(0.079)		(0.017)		(0.018)
Leverage		-0.156**		-0.138		-0.071***		-0.054***
		(0.063)		(0.111)		(0.025)		(0.020)
CapexAssets		-0.179		0.270		-0.058		-0.015
		(0.262)		(0.325)		(0.084)		(0.077)
TobinQ		0.042***		0.113***		0.016***		0.014***
		(0.012)		(0.030)		(0.004)		(0.003)
KZindex		0.001*		0.000		0.000		0.000
		(0.000)		(0.001)		(0.000)		(0.000)
HIndex		-0.254*		-0.445		0.053		0.039
		(0.142)		(0.274)		(0.054)		(0.046)
HIndex^2		0.308**		0.467*		0.002		-0.004
		(0.128)		(0.263)		(0.047)		(0.042)
InstOwn		0.076*		0.168*		-0.036*		-0.041***
		(0.045)		(0.096)		(0.019)		(0.015)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,697	7,430	7,489	7,226	7,697	7,430	7,697	7,430
R-squared	0.268	0.318	0.198	0.210	0.243	0.358	0.189	0.311

Panel D: Quantity of innovation output (total number of patents)

Dep. Var.	$LnPatent_{t+1}$	
	(1)	(2)
*Pilot*Post*	-0.008	-0.015
	(0.010)	(0.011)
Pilot	-0.028	-0.018
	(0.044)	(0.042)
LnAssets		0.423***
		(0.023)
LnAge		0.143***
		(0.040)
ROA		0.711***
		(0.159)
R&DAssets		4.866***
		(0.441)
PPEAssets		0.088
		(0.093)
Leverage		-0.537***
		(0.133)
CapexAssets		0.448
		(0.308)
TobinQ		0.102***
		(0.017)
KZindex		0.001
		(0.001)
HIndex		-0.598*
		(0.328)
$HIndex^2$		0.708**
		(0.284)
InstOwn		-0.395***
		(0.105)
Industry FE	Yes	Yes
Year FE	Yes	Yes
Observations	8,942	7,798
R-squared	0.294	0.443

Table 4: Robustness tests for the DiD analysis

This table reports robustness test results for the difference-in-differences (DiD) analysis. Definitions of variables are listed in the Appendix. Panel A reports results for randomization tests based on 5000 simulated samples. For each simulation, we draw a random sample of 748 "pilot" firms from the pool of actual pilot and non-pilot firms in the event year (2004), and then treat the rest of the pool (1,486 of them) as "non-pilot" firms. We then perform the DiD tests as in Table 3 Panel A on this simulated sample. We repeat the simulation process 5000 times and summarize the distributions of the coefficients and t-stats for the main variable of interest, *Pilot**Post*. Panel B reports results for Placebo tests using 2001 as the "pseudo-event" year. Specifically, we take the set of actual pilot and non-pilot firms and perform the DiD analysis on their innovation activities in the five-year period before and after the "event" year. Control variables are lagged by one year. Each regression includes a separate intercept as well as year and industry fixed effects. Standard errors clustered by both firm and year (i.e., two-way clustering) are displayed in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Panel A: Randomization tests based on 5000 simulated samples

Variable	Mean	P25	Median	P75	S.D.	N
Model (1) of Table 3 Panel A						
Coefficient before *Pilot*Post*	-0.001	-0.018	-0.001	0.016	0.026	5,000
T-stat for *Pilot*Post*	-0.119	-1.206	-0.080	1.056	2.051	5,000
Model (2) of Table 3 Panel A						
Coefficient before *Pilot*Post*	-0.002	-0.022	0.001	0.018	0.030	5,000
T-stat for *Pilot*Post*	-0.113	-1.219	0.024	1.053	1.910	5,000
Model (3) of Table 3 Panel A						
Coefficient before *Pilot*Post*	-0.006	-0.116	-0.013	0.109	0.165	5,000
T-stat for *Pilot*Post*	-0.080	-1.351	-0.151	1.206	2.207	5,000
Model (4) of Table 3 Panel A						
Coefficient before *Pilot*Post*	-0.008	-0.132	-0.007	0.131	0.192	5,000
T-stat for *Pilot*Post*	-0.084	-1.306	-0.078	1.173	2.023	5,000
Model (5) of Table 3 Panel A						
Coefficient before *Pilot*Post*	-0.000	-0.005	0.000	0.004	0.007	5,000
T-stat for *Pilot*Post*	-0.051	-1.090	-0.050	0.945	1.754	5,000
Model (6) of Table 3 Panel A						
Coefficient before *Pilot*Post*	-0.000	-0.005	0.000	0.006	0.008	5,000
T-stat for *Pilot*Post*	-0.041	-1.021	-0.060	0.968	1.700	5,000
Model (7) of Table 3 Panel A						
Coefficient before *Pilot*Post*	-0.000	-0.004	0.000	0.004	0.006	5,000
T-stat for *Pilot*Post*	-0.055	-1.068	0.045	1.090	1.972	5,000
Model (8) of Table 3 Panel A						
Coefficient before *Pilot*Post*	-0.000	-0.005	-0.000	0.005	0.008	5,000
T-stat for *Pilot*Post*	-0.055	-1.034	-0.004	1.024	1.897	5,000

Panel B: Placebo tests using 2001 as the "event" year

Dep. Var.	*LnNSCitePat* $_{t+1}$		*NSCiteLagRD* $_{t+1}$		*Originality* $_{t+1}$		*Generality* $_{t+1}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
*Pilot*Post*	0.032	0.058	0.076	0.153	0.008	0.011	0.003	0.008
	(0.031)	(0.037)	(0.122)	(0.156)	(0.007)	(0.010)	(0.007)	(0.008)
Pilot	-0.084**	-0.107**	-0.312*	-0.415**	-0.010	-0.014	-0.016*	-0.021*
	(0.042)	(0.048)	(0.173)	(0.210)	(0.011)	(0.013)	(0.009)	(0.011)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,577	7,241	8,120	6,837	8,577	7,241	8,577	7,241
R-squared	0.324	0.372	0.246	0.258	0.295	0.383	0.294	0.382

Table 5: DiD tests for the patenting-related litigation risk

This table reports the results of the difference-in-differences (DiD) tests on a firm's patenting-related litigation risk. Columns (1) and (2) examine the litigation risk from potential short sellers (non-industrial companies and non-employees). Columns (3) and (4) examine the litigation risk from all types of plaintiffs. Definitions of variables are listed in the Appendix. Control variables are lagged by one year. Each regression includes a separate intercept as well as year and industry fixed effects. Standard errors clustered by both firm and year (i.e., two-way clustering) are displayed in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Dep. Var.	*LitigationRisk_NonInd* $_{t+1}$		*LitigationRisk_All* $_{t+1}$	
	(1)	(2)	(3)	(4)
*Pilot*Post*	0.014**	0.018***	0.040**	0.045***
	(0.006)	(0.006)	(0.018)	(0.014)
Pilot	-0.022	-0.018	-0.104	-0.073
	(0.015)	(0.011)	(0.071)	(0.052)
LnAssets		0.192***		0.947***
		(0.018)		(0.030)
LnAge		0.019*		0.068
		(0.010)		(0.049)
ROA		0.162**		0.704***
		(0.066)		(0.254)
R&DAssets		1.582***		8.267***
		(0.150)		(0.695)
PPEAssets		0.017		0.025
		(0.023)		(0.120)
Leverage		-0.245***		-1.355***
		(0.048)		(0.187)
CapexAssets		0.020		0.198
		(0.108)		(0.507)
TobinQ		0.005		0.162***
		(0.005)		(0.020)
KZindex		0.001**		0.002
		(0.000)		(0.001)
HIndex		-0.224***		-1.251***
		(0.079)		(0.391)
HIndex Squared		0.232***		1.252***
		(0.077)		(0.374)
InstOwn		-0.020		-0.149
		(0.038)		(0.119)
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	8,754	7,612	8,754	7,612
R-squared	0.705	0.830	0.434	0.692

Table 6: Cross-sectional tests for the patenting-related litigation risk

This table reports the results of the difference-in-differences (DiD) tests using top and bottom terciles of the sample partitioned on a firm's ex-ante exposure to patenting-related litigation risk from potential short sellers (non-industrial companies and non-employees). Definitions of variables are listed in the Appendix. Control variables are lagged by one year. Each regression includes a separate intercept as well as year and industry fixed effects. Standard errors clustered by both firm and year (i.e., two-way clustering) are displayed in parentheses. The last two rows report the Chi-squared test statistics and the corresponding p-values for the difference in the DiD estimators between the "HighRisk" columns and the "LowRisk" columns. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Dep. Var.	*LnNSCitePat* $_{t+1}$		*NSCiteLagRD* $_{t+1}$		*Originality* $_{t+1}$		*Generality* $_{t+1}$	
	HighRisk	*LowRisk*	*HighRisk*	*LowRisk*	*HighRisk*	*LowRisk*	*HighRisk*	*LowRisk*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
*Pilot*Post*	0.133***	-0.034*	1.068***	-0.169	0.020**	0.000	-0.004	0.001
	(0.030)	(0.018)	(0.275)	(0.120)	(0.010)	(0.003)	(0.010)	(0.003)
Pilot	-0.054	0.033	-0.846*	0.036	-0.004	-0.000	-0.010	0.005
	(0.058)	(0.025)	(0.452)	(0.152)	(0.017)	(0.004)	(0.018)	(0.004)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,991	2,765	2,815	2,703	2,991	2,765	2,991	2,765
R-squared	0.371	0.142	0.211	0.137	0.336	0.059	0.357	0.050
Chi -squared Test	43.19		38.40		6.103		0.255	
P-value	<0.001		<0.001		0.014		0.613	

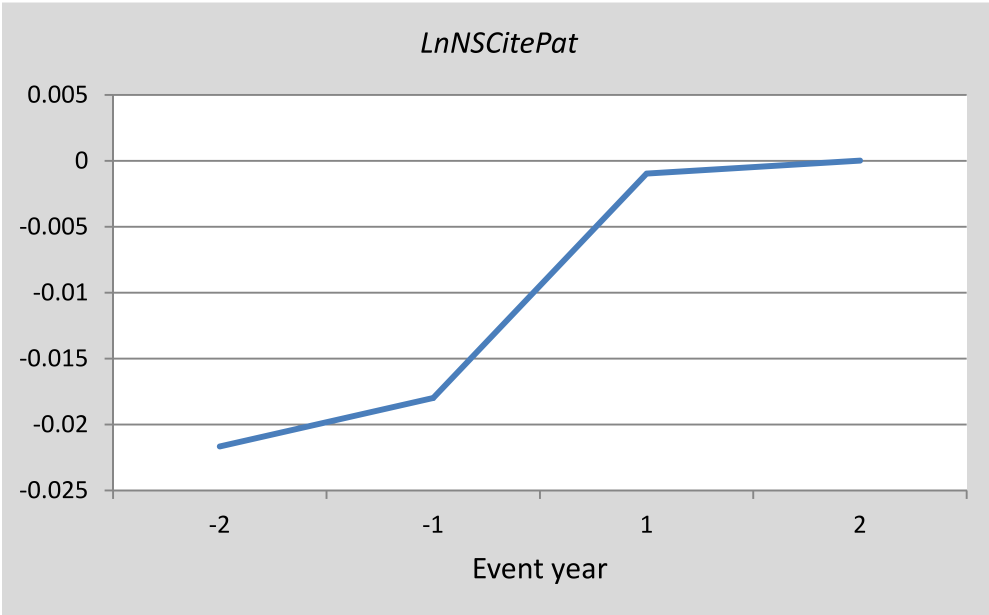
Table 7: DiD tests when controlling for the exposure to patenting-related litigation risk

This table reports the results of the difference-in-differences (DiD) tests after controlling for the exposure to patenting-related litigation risk from non-industrial companies and non-employees. Control variables are lagged by one year. Definitions of variables are listed in the Appendix. Each regression includes a separate intercept as well as year and industry fixed effects. Standard errors clustered by both firm and year (i.e., two-way clustering) are displayed in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

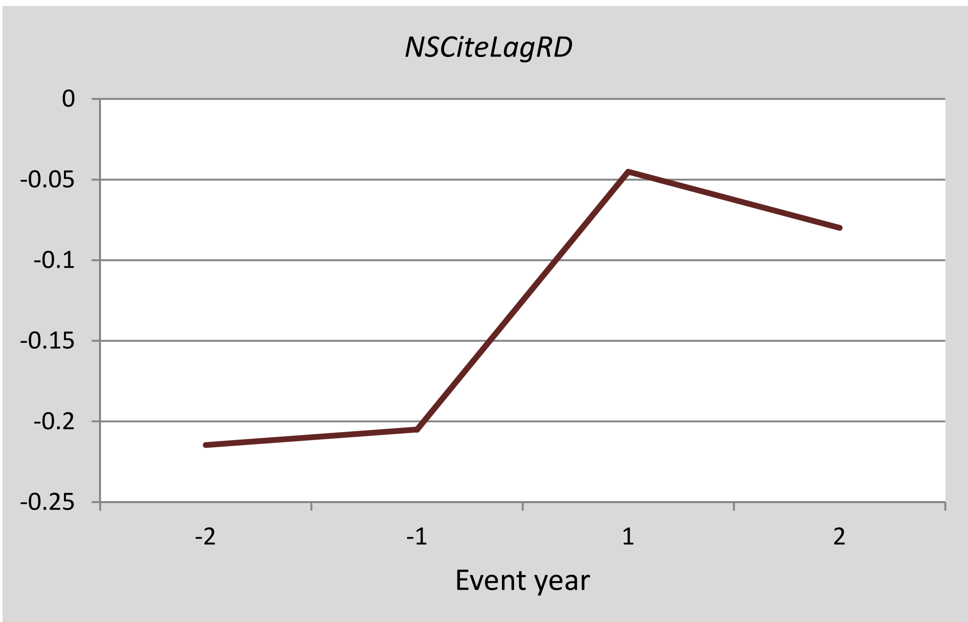
Dep. Var.	*LnNSCitePat* $_{t+1}$		*NSCiteLagRD* $_{t+1}$		*Originality* $_{t+1}$		*Generality* $_{t+1}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
*Pilot*Post*	0.040***	0.020	0.245***	0.177**	0.010*	0.004	-0.004	-0.010*
	(0.014)	(0.014)	(0.085)	(0.081)	(0.005)	(0.006)	(0.005)	(0.006)
Pilot	-0.013	0.013	-0.277	-0.183	-0.006	0.002	-0.003	0.004
	(0.032)	(0.026)	(0.188)	(0.169)	(0.009)	(0.008)	(0.007)	(0.006)
LitigationRisk		1.456***		4.999***		0.444***		0.388***
		(0.083)		(0.816)		(0.012)		(0.020)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,690	7,690	7,349	7,349	7,690	7,690	7,690	7,690
R-squared	0.324	0.502	0.154	0.248	0.339	0.556	0.328	0.525

Figure 1: Innovation characteristics in the pilot sample (net of control) surrounding Regulation SHO

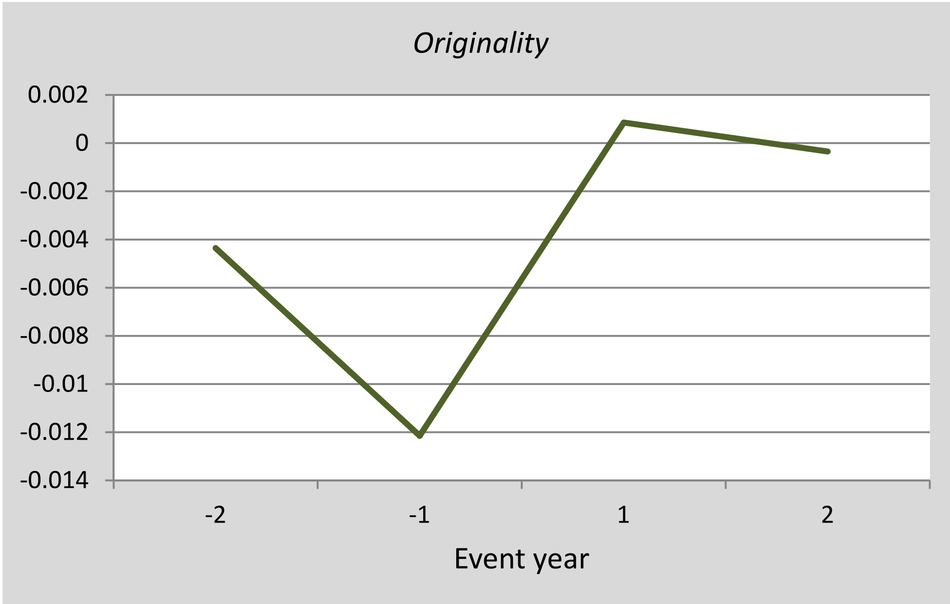
This figure shows the trend of innovation characteristics for the pilot firms net of the control (non-pilot) firms two years before and after the event (Regulation SHO) year. The sample comprises 748 pilot firms and 1,486 control firms. Panel (a) reports the mean logarithm of non-self citations per patent. Panel (b) reports the mean non-self citations per million dollars of lagged R&D expenses. Panel (c) reports the mean originality index. Panel (d) reports the mean generality index.



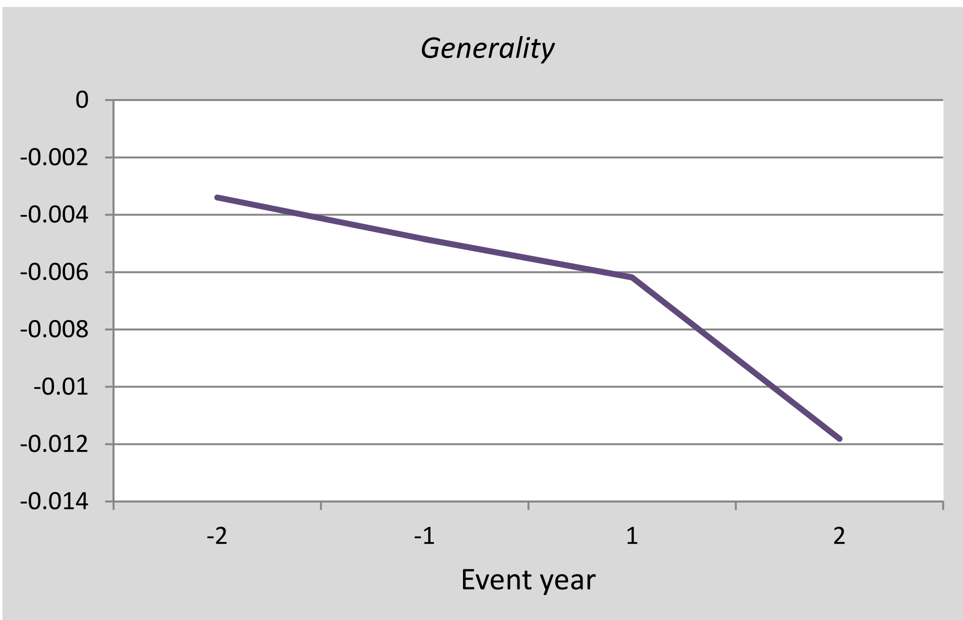
(a)



(b)


Originality
0.002
0
-0.002
-0.004
-0.006
-0.008
-0.01
-0.012
-0.014
-2
-1
1
2
Event year

(c)


Generality
0
-0.002
-0.004
-0.006
-0.008
-0.01
-0.012
-0.014
-2
-1
1
2
Event year

(d)