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: November, 2014

* 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, Craig Holden, Jiekun Huang, Itay Goldstein, Xiaoxia Lou, Liang Ma, Farzad Saidi, Anna Scherbina, Henri Servaes, Merih Sevilir, Tao Shu, Vikrant Vig, Brian Wolfe, 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 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 the Russell 3000 index firms into a pilot program and removes the tick restriction on their stocks, we show that short sellers appear to have a positive, causal effect on innovation. Managerial learning from stock prices, reduced information asymmetry, and the threat of disciplining are three plausible underlying mechanisms through which short sellers encourage innovation. Our paper provides new insights into an unintended real effect of short sellers – their encouragement for innovation.

Key words: Innovation; Short selling; Regulation SHO; Learning from stock prices; Feedback effect

JEL number: G14; G18; O31; O32

1. INTRODUCTION

Do financial markets have real effects on economic activities or are they just a side show? While 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 by both theoretically arguing for and empirically documenting evidence about the real effects of secondary stock markets on corporate decision making. Specifically, following the insightful work of Hayek (1945), which posits that prices are a useful source of information, recent theories developed by, for example, Grossman (1976), Hellwig (1980), Dow and Gorton (1997), Subrahmanyam and Titman (1999), and Goldstein and Guembel (2008) argue that while individual market participants may be less informed than corporate managers, financial markets as a whole have the ability of aggregating different pieces of information possessed by various market players and incorporating them into security prices. Consequently, firm managers who do not have perfect knowledge about every decision-relevant factor will be able to learn new information contained in secondary market prices and use this information to guide their real investment decisions.[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 regulators 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 an element of truth in both sides of these arguments, in practice it is hard to identify the causal effect of short sellers on

[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)

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, and 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 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 (2014) 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

[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.

investment and innovation in substantially different ways.[4] Therefore, while some concurrent studies (e.g., Grullon, Michenaud, and Weston, 2014) 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.

Second, our use of patenting data allows us to observe both the number of patents a firm generates and the number of citations these patents receive in the future. Hence, we are able to explore the effect of short sellers on not only the quantity but also the quality of 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 judge the change in the *quality* 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 encourage innovation. There are at least three reasons why this occurs.

First, short sellers facilitate effective learning by managers from stock prices. Various theoretical studies (e.g., Grossman, 1976, Hellwig, 1980, Dow and Gorton, 1997, and Subrahmanyam and Titman, 1999) argue that although individual speculators may possess less firm-specific information than corporate managers, they collectively could be more informed about a firm's current status and its external business environment such as the state of the economy, the demand by consumers, and the degree of industry competition. Since financial markets aggregate various pieces of information contained in the trades by these speculators and incorporate them into security prices, firm managers will be able to learn new information relevant for their decision making from secondary market prices and apply these fresh insights to their real investment activities. Due to the high costs of implementing short selling strategies, short sellers would be more prudent in their trading decisions and generally spend more

[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 (2014) 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.

resources than other market participants on active information gathering and processing, which makes stock prices more informative and efficient.[5] This improved stock price informativeness then facilitates more effective learning by managers and allows them to better use this additional firm-specific information to overcome the concern about the highly uncertain and risky nature of innovative projects, enhancing the firm's innovation output. Moreover, the new information learned from the more efficient stock prices will also help managers pursue more impactful innovation projects, leading to superior innovation quality in terms of citations of the patents.

Second, short sellers, via their active trading, are able to reduce information asymmetry between firm insiders and outsiders by facilitating information transmission from managers to investors, which overcomes underinvestment in innovation. Innovation activities involve exploring untested and unknown approaches and typically have a long and risky process. Hence, firms investing more heavily in innovative projects are subject to a larger degree of information asymmetry (Bhattacharya and Ritter, 1983) and their fundamental value is only partially reflected in their stock prices, leading up to possible undervaluation (Fishman and Hagerty, 1989) and a higher likelihood of hostile takeovers (Stein, 1988). As a result, managers tend to reduce their investment in long-term innovation projects (in many cases sub-optimally) and invest more on routine tasks that offer faster, more stable returns and whose cash flows can be better reflected in current stock prices. Short sellers help overcome underinvestment in innovation by reducing the information gap between firm insiders and the outside market, because while the actual trading actions by short sellers reveal bad news (i.e., overvaluation) about the short-sold stocks, the fact that short sellers can but don't short sell a stock after producing information about the firm's fundamentals itself conveys good news to the equity market – the so-called "the dog that did not bark" effect. In other words, the *possibility* of short selling a stock (rather than the *actual* trading of it) allows a firm's fundamentals to be better reflected in its current stock price and reduces information gap (as well as potential stock undervaluation), which in turn encourages managers to invest in long-run innovation.

The third reason relates to the disciplining role of short sellers. Moral hazard models such as Grossman and Hart (1988) and Harris and Raviv (1988) argue that managers who are not

[5] Bris, Goetzmann, and Zhu (2007) find that prices are more efficient in countries where short sales are allowed and practiced in a cross-country setting. Boehmer, Jones, and Zhang (2008, 2013) show that short sellers are important contributors to efficient stock prices. Boehmer and Wu (2013) find that stock prices are more accurate when short sellers are more active.

properly monitored will shirk or tend to invest more in unchallenging routine tasks to enjoy private benefits such as "quiet life" (Bertrand and Mullainathan, 2003). Value-destroying underinvestment 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 slack such as shirking on 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. In anticipation of this adverse "snowball" effect, managers would discipline themselves *ex ante* when making innovation decisions. As a result, 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.

Taken together, our first hypothesis argues that short sellers, by improving price informativeness, reducing information asymmetry, and disciplining managers, encourage firm innovation. We term this view the *positive-feedback 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.[6] However, short sellers have an innate distaste for tolerance towards short-term failures, because their main job 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 their investments in

[6] 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.

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, impede firm innovation.

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, 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 impact 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 innovate. We adopt a difference-in-differences (DiD) method to analyze how firms' innovation outputs are affected by this exogenous shock to short-selling constraints.

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 firm innovation. According to our multivariate DiD analysis, a reduction in short selling costs due to Regulation SHO leads to a 23% larger increase in patent counts and a 34% larger increase in patent citations for the treatment (pilot) group compared to the control (non-pilot) group. Further, we find a stronger positive effect of short sellers on 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 *positive-feedback hypothesis*.[7]

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 find no significant difference in innovation activities between pilot and non-pilot firms around such "pseudo-event" years.

We further attempt to identify three possible underlying mechanisms through which short sellers encourage firm innovation. To this end, we examine how cross-sectional variation in stock price informativeness, information asymmetry, and agency problems alters our main results. We find that the positive effect of short sellers on innovation is more pronounced when a firm's pre-event stock price contains less firm-specific information, when the firm is subject to a larger degree of information asymmetry, and when the firm is more likely to have managerial agency problems. The evidence suggests that managers' learning from stock prices, reduced information asymmetry between insiders and outsiders (i.e., "the dog that did not bark" effect), and the threat of disciplining are plausible mechanisms through which short sellers encourage firm innovation.

Lastly, we find that the positive effect of short sellers on innovation is stronger for firms without options trading, in which case investors with negative opinions about a firm can only use short sales as a trading strategy.[8] Further, the effect of short sellers on innovation is present regardless of whether the firm is likely to be scrutinized by the SEC, inconsistent with an alternative explanation for our study that increased attention and scrutiny from regulators (such as the SEC) towards pilot firms during Regulation SHO is the main driver of our baseline DiD results.

[7] 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 greater increase in innovation output after their short selling constraints are removed in August 2007 than did the pilot firms whose costs of short selling remain unchanged.

[8] This finding mitigates the concern that the Regulation SHO experiment captures changes in other determinants of innovation than the likelihood of short selling.

One caveat of our study is that we are not claiming that short sellers, typically with a limited investment horizon, intentionally promote firm innovation (a type of long-term projects) through their intervention activities. Short sellers are active market players and short-term speculators who identify overvalued firms, sell short their stocks, and make trading profits. Hence, they are unlikely to care about a firm's long-term projects such as 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 their information production and disseminating role. However, 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 offers additional evidence to supplement our main analysis. Section 7 concludes.

2. RELATION TO THE EXISTING LITERATURE

Our paper mainly contributes to two strands of literature. First, it is related to the growing literature, both theoretical and empirical, arguing for and documenting the real effect 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 Goldstein and Guembel (2008) show that decision makers use the new information learned from financial market prices to guide their real decisions. 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.

Empirical studies provide evidence consistent with the learning channel through which financial markets affect firms' investment and financing activities. For example, Giammarino, Heinkel, Hollifield, and Li (2004) find that information acquisition by the market influences managers' financial decisions in the SEO setting. Luo (2005), in the M&A setting, finds that managers learn new information from announcement returns of M&A deals and are more likely to withdraw a deal if its announcement return is lower. Kau, Linck, and Rubin (2008) further show that managers are more likely to listen to the market when more of their shares are held by large blockholders and when their CEOs have higher pay-performance sensitivities. Edmans, Goldstein, and Jiang (2012) identify a negative, causal effect of a firm's share price on its likelihood of receiving a takeover bid and argue that this effect arises from a feedback learning channel. In a more general setting, Chen, Goldstein, and Jiang (2007) find that the sensitivity of investment to stock price is stronger when there is more private information injected into the price during the trading process, suggesting that managers learn new information from the price and use it in their investment decisions. Bakke and Whited (2010) decompose stock-price movements that are relevant for investment from those that are not and confirm the findings of previous studies that managers incorporate private investor information when making investment decisions. In a related study, Durnev, Morck, and Yeung (2004) show that price informativeness is positively related to investment efficiency.

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 rather than 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.[9] 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.

[9] 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).

Two 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 (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.[10] Fang, Huang, and Karpoff (2014) find that an exogenous decrease in short-selling costs due to the Regulation SHO program reduces pilot firms' propensity to engage in earnings management and that this pattern reverses when the difference in short-selling constraints between pilot and control firms disappears after the SHO program ends. This paper provides support for the disciplining role played by short sellers. Different from the above two 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. Our paper thus provides the first empirical analysis that sheds light on this important research question.[11]

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%

---

[10] 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 information production and disciplining activities of short sellers. Therefore, our use of patenting (as opposed to ordinary capital expenditures or R&D spending that is just one observable innovation input) as the main outcome variable 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 (2014).

[11] Our paper is also broadly related to the literature on short selling constraints and asset price properties (e.g., Miller, 1977; Harrison and Kreps, 1978; Chen, Hong, and Stein, 2002; Hong and Stein, 2003; Battalio and Schultz, 2006; Diether, Lee, and Werner, 2009; Beber and Pagano, 2013). However, with a few exceptions (e.g., Hirshleifer, Teoh, and Yu, 2011; Henry, Kisgen, and Wu, 2013), the empirical literature that relates short sellers to corporate decisions is quite limited.

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.

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).[12] 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.[13] We collect firm-year patent and citation information from three sources. First, we retrieve our patent and citation data between 2001 and 2006 from the latest version of the National Bureau of Economic Research (NBER) Patent Citation database. The NBER database provides information for all utility patents granted by the US Patent and Trademark Office (USPTO) over the period of 1976-2006. Second, we obtain information on patents granted over the period of 2007-2009 that is provided by Kogan et al. (2012) (available at https://iu.box.com/patents). Third, we construct a dataset for patent citations over the period of 2007-2009 using the Harvard Business School (HBS) patent database (available at http://dvn.iq.harvard.edu/dvn/dv/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), anti-takeover provision information from the RiskMetrics database, analyst coverage data from the Institutional Brokers Estimate Systems (I/B/E/S) database, and the probability of informed trading (PIN) data from Stephen Brown's website (http://scholar.rhsmith.umd.edu/sbrown/pin-data).

[12] 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).

[13] 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.

3.2 Variable Measurement

3.2.1 Measuring Innovation

We construct two measures to gauge a firm's innovation output. The first measure is the total number of patents filed (and eventually granted) in a given year, which captures the quantity of innovation. 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. We use the application year instead of the grant year to determine a firm's innovation output 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). 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.

Despite its straightforward intuition and easy implementation, a simple measure of patent counts hardly distinguishes groundbreaking innovations from incremental technological improvements. Hence, we construct the second measure of innovation output, the total number of citations each patent receives in subsequent years, which captures the quality (impact) of innovation.

Nevertheless, both innovation measures are subject to truncation problems. Since we only observe patents that are eventually granted by the end of 2009, patents filed in the last few years of our sample period may still be under review and not granted by 2009. Similarly, patents tend to receive citations over a long period after its grant date, but we observe at best the citations received up to 2009. 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.

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 distribution of patent grants in our final sample is right skewed, with its median at zero. Due to the right skewness of patent counts and citations per patent, we winsorize these variables at the 95th percentiles and then use the natural logarithm of one plus patent counts (*LnPatent)* and the natural logarithm of one plus the number of citations per patent (*LnCitePat)* as the main innovation measures in our analysis.

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

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 5.41 granted patents per year and each patent receives 0.35 citations. Regarding other variables, an average firm has a book value asset of $5.48 billion, R&D-to-assets ratio of 3.8%, 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.[14]

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 two rows, we compare the two outcome variables, *LnPatent and LnCitePat*, between treatment and control groups. While the treatment firms appear slightly less innovative than the control firms, 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 and median 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 periods before and after 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 four rows of Table 2. Specifically, we calculate one-year and two-year growth rates of innovation variables 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

[14] See e.g., Fang, Huang, and Karpoff (2014) and Grullon, Michenaud, and Weston (2014), for discussions of more institutional details about the U.S. regulations on short sellers as well as a detailed justification for why Regulation SHO is a valid quasi-natural experiment to analyze corporate decisions.

the mean of *LnPatent* 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 patents 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 *LnCitePat* between both groups of firms.

Next, we perform the DiD tests in a multivariate regression framework. Following Fang, Huang, and Karpoff (2014), we estimate various forms of the following model:

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

where i indexes firm, j indexes industry, and t indexes time. $Pilot_i$ 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).[15] 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).

Table 3 Panel A reports the regression results estimating equation (1). The dependent variable is *LnPatent* 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 0.026 and significant at the 1% level, suggesting that pilot firms whose exposure to short selling goes up due to Regulation SHO experience an increase of *LnPatent* that is 0.026 higher than that of control firms over a five-year period around the event. This difference is economically sizeable, as it represents approximately 21% of the average change of *LnPatent* for the control firms in our sample (-

[15] This specification effectively removes the event year from our analysis, which follows the spirit of related studies such as Fang, Huang, and Karpoff (2014) and Grullon, Michenaud, and Weston (2014).

0.125). 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 0.029 in *LnPatent* for the treatment group compared to the control group, about 23% of the average change of *LnPatent* for the control firms.

We replace the dependent variable with *LnCitePat* in columns (3) and (4), and continue to observe positive and significant DiD estimators. The DiD estimator for *LnCitePat* in column (4) is 0.050 and significant at the 1% level. Given that the average change of *LnCitePat* for the control firms in our sample is -0.146, this represents an approximately 34% of the change, which is also economically sizable. Thus, the evidence from the baseline DiD tests is consistent with the *positive-feedback 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 in all four columns, 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 relevant firms. The evidence presented in Panel B implies 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. 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 one if the fiscal year ending date is after July 1, 2005 but on or before June 30, 2007, and to be zero if the fiscal year ending date is after July 1, 2008 but on or before December 31, 2009 (recall that we observe patent and citation data only up to 2009). 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. Consistent with our conjecture, the coefficient estimates on *Pilot*Post* are negative and significant at the 5% or 1% level in all four columns, suggesting that the control firms indeed experienced a greater increase in innovation activities after their short selling constraints are removed than did the pilot firms whose costs of short selling remain unchanged.

Overall, our identification tests reported in this subsection suggest a positive, causal effect of short sellers on innovation, consistent with the *positive-feedback hypothesis*.

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, 25th percentile, median, 75th 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 mixed signs across all four 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 estimators 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 (except for Column (3)), are much smaller than our main DiD estimators and statistically insignificant.[16]

In summary, the above robustness test results suggest that the identified positive effect of short sellers on 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 MECHANISMS

Having established a positive, causal link between short sellers and firm innovation, in this section, we aim to further understand the underlying mechanisms through which the exposure to short selling activities encourages firm innovation. We achieve this by exploring how short sellers affect innovation output differently in the cross section.

5.1 Learning from stock prices

We first explore managerial learning from stock prices as a potential mechanism. Theories propose that speculators who individually may be less informed than firm managers can collectively be more informed about a firm's current conditions and future prospects. Financial markets aggregate various pieces of information possessed by these individual speculators and incorporate them into security prices. Hence, corporate managers can learn new information about their firms from the trading and price of their own stock and in turn use this information to guide their real investment decisions (see, e.g., Bond, Edmans, and Goldstein, 2012). As short sellers actively gather information about the firms they intend to short, their trading activities would make stock prices contain more firm-specific information that is new to firm insiders such as managers. This improved stock price informativeness facilitates more effective learning from stock prices by managers, allowing them to better use this new information to guide their

[16] Using other "pseudo-event" years such as 1999 or 2000 yields qualitatively similar results.

innovation decisions. Unlike ordinary capital expenditures, corporate innovation involves an extremely long, opaque, and risky process, which deters most firms from fully investing in innovative projects. Hence, the new information learned from the stock prices will help managers know more about the nature, current status, and future prospects of their long-term innovative projects, encouraging them to invest more in such projects and thus overcome the underinvestment problem. Moreover, such new information will help managers improve the quality of their innovation as well.

Following the above logic, we expect that the positive effect of short sellers on innovation should be more pronounced for firms whose pre-event stock prices contain less private information so that there is bigger room for short sellers to improve their stock price informativeness.

To test the above conjecture, we examine two measures of price informativeness widely used in the recent literature. The first 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 (*Nonsync*) for this firm-year. We set *Nonsync* as missing if it is estimated with less than 30 daily observations. A higher value of *Nonsync* 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.

Our second measure for price informativeness is the probability of informed trading (PIN), first developed in Easley, Kiefer, and O'Hara (1996, 1997a,b) and modified extensively by many follow-up studies. This measure is also used in Chen, Goldstein, and Jiang (2007) to capture stock price informativeness. 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).

To analyze the learning-from-stock-prices channel for our main findings, we first partition our sample into two subsamples based on whether a firm's average price nonsynchronicity over the three years before Regulation SHO is above the sample median. We report the results in Table 5 Panel A. The first two columns report the results with patent counts as the dependent variable, and the last two columns report the results with patent citations as the dependent variable. The DiD estimator, namely, the coefficient estimates on *Pilot*Post*, for patent counts is positive and significant in column (2) that examines firms with lower pre-event price nonsynchronicity but statistically insignificant in column (1) that examines firms with higher price nonsynchronicity. Further, a Chi-squared test of the difference between high and low groups is significant at the 1% level. Although the DiD estimators for citations in columns (3) and (4) are both positive and significant, the one examining firms with lower pre-event price nonsynchronicity (in column (4)) has a much larger magnitude than the one examining firms with higher price nonsynchronicity (in column (3)), and a Chi-squared test of the difference is significant at the 10% level. This finding confirms our conjecture that the positive effect of short sellers on innovation is more pronounced for firms with less pre-event price informativeness, consistent with managerial learning from secondary stock market as an explanation for our main results.

Next, we partition our sample into two subsamples based on whether the average PIN of a firm over the three years before Regulation SHO is above the sample median, and report the results in Table 5 Panel B. The DiD estimator for patent counts is positive and significant in column (2) in which we examine firms with a smaller pre-event PIN but statistically insignificant in column (1) in which we examine firms with a larger pre-event PIN. Moreover, a Chi-squared test of the difference between high and low groups is significant at the 1% level. Although the DiD estimators for citations in columns (3) and (4) are both positive and significant, the one examining firms with a smaller pre-event PIN (in column (3)) has a larger magnitude than the one examining firms with a larger PIN (in column (4)), though a Chi-squared test of the difference is not significant. These results suggest that, consistent with our conjecture, the positive effect of short sellers on innovation is stronger for firms with less informative pre-event stock prices, which benefit the most from managerial learning from the stock market to guide their innovation activities.

Overall, in this subsection we find that the positive effect of short sellers on innovation is more pronounced when firms have a lower level of price informativeness before SHO (and hence there is larger room for short sellers to incorporate firm-specific information into prices), consistent with the feedback effect of financial markets (in particular short sellers' trading behavior) on corporate policy making.

5.2 Reducing information asymmetry

The second underlying mechanism through which short sellers encourage innovation is reduced information asymmetry between firm insiders and outsiders due to short sellers' information production activities. Since innovative firms tend to be undervalued by equity markets due to information asymmetry (which leads to greater concerns for adverse selection and higher costs of capital), managers typically underinvest in long-term, risky innovative projects. Short sellers help overcome underinvestment in innovation by reducing the information gap between firm insiders and outsiders. Because of the high costs of implementing short selling strategies and the short-lived nature of the arbitrage opportunities, short sellers have strong incentives to actively produce information about firms' fundamental values. While the actual trading actions by short sellers reveal bad news (i.e., overvaluation) about the short-sold stocks, the fact that short sellers can short sell a stock (due to SHO) but don't do so after producing information about the firm's fundamentals itself conveys good news to the equity market, allowing a firm's fundamentals to be better reflected in its current stock price. The reduced information asymmetry due to short sellers' enhanced ability to trade in turn encourages managers to invest more in long-run innovative projects. Hence, we expect that the positive effect of short sellers on innovation is more pronounced when firms are subject to a larger degree of information asymmetry as short sellers can play a bigger role in making the outside market informed about such firms. On the contrary, short sellers may not be able to reduce the information gap between insiders and outsiders (and thus encourage innovation) to the same degree for firms that are already transparent to the market (i.e. outside investors). In this subsection, we study two dimensions of cross-sectional variation in information asymmetry.

The first dimension is analyst coverage. Financial analysts actively collect information from various sources, evaluate the current performance of firms they follow, make forecasts about their future prospects, and make buy/sell/hold recommendations to current and potential investors. Firms covered by a larger number of financial analysts are generally considered to have a lower degree of information asymmetry. Therefore, to the extent that financial analysts and short sellers are alternative information producers about firms' fundamentals, we expect that the positive effect of short sellers on innovation is more pronounced when firms are followed by a smaller number of financial analysts.

To examine this conjecture, we partition our sample into two subsamples based on whether the number of financial analysts following a firm is above the sample median. We report the results in Table 6 Panel A. The first two columns report the results with patent counts as the dependent variable, and the last two columns report the results with patent citations as the dependent variable. The DiD estimator for the number of patents, namely, the coefficient estimate on *Pilot*Post* in column (2), in which firms are followed by a smaller number of analysts (and therefore are subject to a greater degree of information asymmetry), is positive and significant. In contrast, the DiD estimator is statistically insignificant in column (1) which examines firms followed by a larger number of analysts (and therefore are subject to a smaller degree of information asymmetry). While the DiD estimators for citations in columns (3) and (4) are both positive and significant, the one examining firms followed by a smaller number of analysts (in column (4)) has a much larger magnitude than the one examining firms followed by a larger number of analysts (in column (3)), and a Chi-squared test of the difference has a p-value of 0.03. These findings confirm our conjecture that the positive effect of short sellers on innovation is more pronounced for firms followed by fewer financial analysts. To the extent that these firms are subject to a greater degree of information asymmetry, short sellers play a bigger role reducing information asymmetry between insiders and outsiders, which ultimately encourages firm innovation to a larger extent.

Next, we explore how cross-sectional variation in firm size influences the positive effect of short sellers on innovation. Larger firms are typically considered to be more transparent, have higher visibility to the investing world, and are subject to a lower degree of information asymmetry. Therefore, we expect the positive effect of short sellers on innovation to be more pronounced for smaller firms. To examine this conjecture, we once again partition our sample into two subsamples based on whether the book values of firm assets are above the sample median.

We report the results in Table 6 Panel B. The DiD estimator for patent counts is positive and significant in column (2) in which smaller firms are examined but statistically insignificant in column (1) that examines larger firms. Although the DiD estimators for citations in columns (3) and (4) are both positive and significant, the one examining smaller firms (in column (4)) has a much larger magnitude than the one examining larger firms (in column (3)), and a Chi-squared test of the difference is significant at the 5% level. These results suggest that, consistent with our conjecture, the positive effect of short sellers on innovation is stronger for smaller firms, which are subject to a larger degree of information asymmetry and in more need of alternative information producers such as short sellers.

Overall, in this subsection we find that the positive effect of short sellers on innovation is more pronounced when firms suffer from a greater extent of information asymmetry, i.e., when firms are followed by a smaller number of financial analysts and when firms are smaller in size. The evidence suggests that information transmission from firm insiders to the outside market facilitated by short sellers serves as another plausible underlying mechanism through which short sellers encourage innovation.

5.3 Disciplining

The last underlying mechanism through which short sellers promote firm innovation is their (possibly unintended) disciplining effect on managerial incentives from the threat of selling short. Corporate managers generally perceive short sellers as one of the most important groups of investors that affect the stock prices of their firms. This is because whenever the short sellers 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. 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, 2014), improve corporate governance (Massa, Zhang, and Zhang, 2013), and detect financial frauds (Karpoff and Lou, 2010). Hence, the mere presence of short sellers and the threat of their trading could potentially help mitigate the shareholder-manager conflicts due to managerial incentives to either shirk or pursue private benefits during the long, risky, and opaque process of corporate innovation activities. As a result, we expect that the positive effect of short sellers on innovation is stronger when firms are more likely to suffer from agency problems, i.e., when firms have weaker existing disciplining mechanisms against the management. For such firms, the disciplining role played by short sellers is much more needed. We consider three common disciplining mechanisms for a firm.

The first disciplining mechanism we consider is institutional ownership. Existing studies show that institutional investors play an important role in disciplining managers and mitigating agency problems. For instance, Aghion, Van Reenen, and Zingales (2013) find that higher institutional ownership is associated with greater innovation output, consistent with the idea that institutional investors help monitor managers who are likely to derive private benefits from shirking or other opportunistic behavior when carrying out long-term innovation projects. If short sellers can substitute for institutional investors in disciplining managers, we would expect that the positive effect of short sellers on innovation is stronger for firms with lower institutional ownership.

To test this conjecture, we partition our sample into two subsamples based on whether or not a firm's institutional ownership (*InstOwn*) is above the sample median. Table 7 Panel A reports the results using the specification from Equation (1), which includes the full set of controls, industry fixed effects, and year fixed effects. Columns (1) and (2) report the results with patent counts as the dependent variable, and columns (3) and (4) report the results with patent citations as the dependent variable. The DiD estimators, i.e., the coefficient estimates on *Pilot*Post*, are positive and significant in columns (2) and (4) in which firms with low institutional ownership are examined. In contrast, the coefficient estimates on *Pilot*Post* are statistically insignificant for firms with high institutional ownership, as reported in columns (1) and (3). In addition, the magnitudes of the DiD estimators are much larger for firms with low institutional ownership than those with high institutional ownership, and a Chi-squared test based on seemingly unrelated regression (SUR) techniques shows that the differences in the DiD estimators are statistically significant. This evidence is consistent with our conjecture that the positive effect of short sellers on innovation is more pronounced for firms with more severe agency problems as indicated by lower institutional ownership.[17]

[17] An alternative argument is that the incremental impact of short sellers on innovation could be larger for firms with higher institutional ownership because such firms have lower short selling costs (see, e.g. Nagel, 2005; Hirshleifer,

Next, we consider how product market competition affects the positive effect of short sellers on innovation. Product market competition creates pressure for firms to invest in innovative projects so as to gain or keep competitive advantages over their rivals, because otherwise they might lose market share and operating profits in the long run, leading up to a decline in firm performance and threatening the job security of the managers. Therefore, it helps to mitigate the agency problem between shareholders and the management (see a similar argument made by Aghion et al. 2005). If this argument is true, we would expect the positive effect of short sellers on firm innovation to be stronger for firms operating in less competitive product markets in which alternative disciplining mechanisms such as short sellers are mostly needed.

Following existing literature, we capture the intensity of product market competition by using the 4-digit SIC Herfindahl index based on sales (*HIndex*). A larger value of *HIndex* indicates higher industry concentration, which means a lower level of product market competition among rival firms. To test our conjecture, we partition the DiD sample into two subsamples based on whether the industry to which a firm belongs has a Herfindahl index that is above the sample median, and report the results in Table 7 Panel B. The first two columns report the results with patent counts as the dependent variable, and the last two columns report the results with patent citations as the dependent variable.

The coefficient estimates on *Pilot*Post* are positive and significant in columns (1) and (3) which examine firms operating in less competitive product markets (i.e., industries with higher Herfindahl index). In contrast, the coefficient estimates on *Pilot*Post* are statistically insignificant in columns (2) and (4) which examine firms operating in more competitive product markets (i.e., industries with lower Herfindahl index). Further, the magnitudes of the DiD estimators are much larger for the group of firms operating in less competitive product markets (with the formal Chi-squared tests being significant at 15% and 5% for patents and citations, respectively). The evidence in this panel is consistent with our conjecture that the positive effect of short sellers on innovation is stronger for firms with less external disciplining from the product market (due to a lack of competition), in which case short sellers play a more pronounced disciplinary role and help better encourage firm innovation.

Teoh, and Yu, 2011) and thus are exposed to a greater threat from short sellers after the passage of Regulation SHO. However, this conjecture, while plausible, is inconsistent with our results.

The third cross-sectional variation in a firm's existing disciplining mechanisms (and the degree of agency problem) that we explore pertains to its corporate governance. Firms with good corporate governance discipline managers and mitigate managerial agency problems more efficiently, and thereby enjoy higher stock returns and valuation (Gompers, Ishii, and Metrick, 2003). Hence, to the extent that good corporate governance provides an alternative disciplining mechanism to that provided by short sellers, the positive effect of short sellers on firm innovation should be stronger in firms with poorer corporate governance.

While existing literature generally uses the G-index developed in Gompers, Ishii, and Metrick (2003) to measure the quality of corporate governance, Bebchuk, Cohen, and Ferrell (2009) find that six anti-takeover provisions of the corporate charter, which are part of the 24 anti-takeover provisions comprising the G-index, are most useful in capturing the effective level of managerial entrenchment and have the merit of not being vulnerable to the noise produced by the other anti-takeover provisions in the G-index.[18] Therefore, we measure corporate governance using the entrenchment index ("E-index") consisting of the six provisions discussed in Bebchuk, Cohen, and Ferrell (2009). A higher level of the E-index indicates poorer corporate governance because the managers are more entrenched. Then we partition our DiD sample into two subsamples based on whether a firm's E-index is above the sample median, and test how our baseline DiD results differ across these two subsamples.

We report the results in Table 7 Panel C. The first two columns report the results with patent counts as the dependent variable, and the last two columns report the results with patent citations as the dependent variable. The DiD estimator for patent counts, i.e., the coefficient estimate on *Pilot*Post*, is positive and significant in column (1), which examines firms with poor corporate governance (i.e., with a high E-index), but the coefficient estimate on *Pilot*Post* is statistically insignificant in column (2) which examines firms with good corporate governance (i.e., with a low E-index). While the DiD estimators for citations in columns (3) and (4) are both positive and significant, the one examining firms with a higher E-index has a much larger magnitude than the one examining firms with a lower E-index and a Chi-squared test of the difference is significant at the 15% level. This evidence is consistent with our conjecture that the positive effect of short sellers on innovation is stronger for firms with poorer corporate

[18] The six provisions are staggered boards, limits to bylaw amendments, limits to charter amendments, supermajority requirements for mergers, poison pills, and golden parachutes.

governance and thus more severe agency problems, where short sellers have the biggest disciplining effect on firm managers.

Overall, we find that the positive effect of short sellers on innovation is more pronounced when firms are likely to have more severe agency problems between shareholders and managers, i.e., when institutional ownership is lower, when product markets are less competitive, and when corporate governance is poorer. The evidence reported in this subsection thus suggests that the threat of short selling as a disciplining mechanism against firm management is likely to be a plausible underlying mechanism through which short sellers encourage innovation.

6. ADDITIONAL TESTS

In this section, we carry out several additional tests to address various concerns of our main analysis. Section 6.1 examines how the availability of option trading alters the effect of short sellers on firm innovation. Section 6.2 studies whether pilot firms increase their innovation activities more than the control group after Regulation SHO simply due to the fact that they are subject to more scrutiny from the SEC.

6.1 Availability of option trading

For investors with pessimistic views about a firm's prospects, one alternative trading strategy other than establishing a short position is trading options on the firm (i.e., selling calls or buying puts). Hence, the availability of low-cost options trading for a firm can potentially substitute for short selling in influencing its corporate decisions. If that is the case, then we would expect the positive effect of short sellers on firm innovation to be weakened when the firm has exchange-listed options available for trading because investors who wish to take a short position in the company's stock can now trade options instead of engaging in short sales.

To test this conjecture, we collect option trading data during our sample period from OptionMetrics and partition our sample into two subsamples based on whether a firm has available traded options. The dummy variable *Option* equals zero if a firm has no options traded during the entire sample period, and equals one otherwise. We report the results in Table 8 Panel A. The coefficient estimates on *Pilot*Post* are positive and significant at the 1% level in columns (2) and (4) in which we examine firms without any options trading during our sample period. In contrast, the coefficient estimates on *Pilot*Post* are statistically insignificant in columns (1) and

(3) in which we examine firms with stock options trading. Further, the magnitudes of the DiD estimators are much larger for the group of firms without option trading (with a formal Chi-squared test significant at 1% and 5% for patents and citations, respectively). The evidence in this panel is consistent with our conjecture that the positive effect of short sellers on innovation is stronger for firms without options trading, in which case investors with negative opinions about a firm can only use short sales as a trading strategy, mitigating the concern that the Regulation SHO experiment captures changes in other determinants of innovation than the likelihood of short selling.

6.2 SEC scrutiny

One potential explanation for our main results is an enhanced visibility of the pilot firms to regulators, such as the SEC, during the experiment. Specifically, the increased attention and scrutiny from the SEC towards these pilot firms during Regulation SHO may have led them to innovate more. In this case, the disciplining effect identified in this paper might not be due to short sellers per se, but rather SEC scrutiny. To address this concern, we split our sample firms into two subsamples based on their possibility of being scrutinized by the SEC. If SEC scrutiny is the main reason why pilot firms experience a higher increase in innovation than control firms around the SHO experiment, then we would expect this positive effect of Regulation SHO on innovation to be stronger for firms that are less likely to be scrutinized by the SEC to begin with. The reason is that for these less scrutinized firms, Regulation SHO will change the relative level of SEC scrutiny for pilot and control groups to a larger extent and thus lead to bigger differences in their innovation output changes. In contrast, firms already under intense scrutiny by the SEC will not experience much change in the level of attention and disciplining from the regulators simply due to the SHO experiment and thus will mitigate the treatment effect we try to identify.

Following the recent literature on SEC enforcement (see, e.g., Kedia and Rajgopal, 2014), we measure a firm's likelihood of being scrutinized by the SEC using the geographical distance between the two parties (*SECdist*). Specifically, for each firm in our sample, we calculate the distance between the county where its headquarter is located and the county where the nearest SEC regional office (including the SEC headquarters in Washington D.C.) is located by using the corresponding latitudes and longitudes. We then partition our sample into two subsamples based on whether the distance between a firm and the nearest SEC regional office is above the sample median, and report the results in Table 8 Panel B. The DiD estimator for patent counts is positive and significant in column (2) in which firms closer to the SEC (and thus already under heavy scrutiny from the SEC) are examined but statistically insignificant in column (1) that examines firms farther away from the SEC. However, the difference between the two DiD estimators in the two columns are not statistically significant (with a p-value of 0.84). Although the DiD estimators for citations in columns (3) and (4) are both positive and significant, the one examining firms closer to the SEC (in column (4)) has a slightly greater magnitude than the one examining firms farther away from the SEC (in column (3)), though a Chi-squared test of the difference is again insignificant at conventional levels.

These results show that the positive effect of short sellers on innovation is indistinguishable between firms likely to and unlikely to be scrutinized by the SEC. If anything, firms closer to the SEC (already under scrutiny from the SEC) display a slightly stronger (though statistically insignificant) treatment effect than those farther away from the SEC (with little SEC scrutiny to begin with), inconsistent with the prediction of the alternative explanation. Hence, the increased attention and scrutiny from regulators such as the SEC towards pilot firms during Regulation SHO seem unlikely to be the main driver of our baseline DiD 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 have a positive, causal effect on firm innovation in a difference-in-differences framework. An exogenous reduction in short selling costs due to Regulation SHO leads to a larger increase in patents and citations for pilot firms compared to non-pilot firms in the same Russell 3000 index, and this pattern is reversed when Regulation SHO ends (i.e., short selling constraints removed for all firms) in 2007. The positive effect of short sellers on innovation is more pronounced when firms have less informative stock prices and when they are subject to a higher degree of information asymmetry and more severe agency conflicts.

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.

Bakke, TE., and T. Whited, 2010. Which firms follow the market? An analysis of corporate investment decisions. *Review of Financial Studies* 23, 1941-1980.

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

Bebchuk, L., A. Cohen, and A. Ferrell, 2009. What matters in corporate governance? *Review of Financial Studies* 22, 783-827.

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.

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

Bhattacharya, S. and J. Ritter, 1983. Innovation and communication: Signalizing with partial disclosure. *Review of Economic Studies* 50, 331-346.

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.

Bris, A., W. N. Goetzmann and N. Zhu, 2007. Efficiency and the bear: Short sales and markets around the world. *Journal of Finance* 62, 1029-1079.

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.

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

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

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.

Durnev, A., R. Morck and B. Yeung, 2004. Value-enhancing capital budgeting and firm-specific stock return variation. *Journal of Finance* 59, 65-105.

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.

Edmans, A., I. Goldstein and W. Jiang, 2012. The real effects of financial markets: The impact of prices on takeovers. *Journal of Finance* 67, 933-971.

Fang, V., A. Huang, and J.M. Karpoff, 2014. Short selling and earnings management: A controlled experiment. *Unpublished working paper*.

Fang, V., X. Tian, and S. Tice, 2014. Does stock liquidity enhance or impede firm innovation? *Journal of Finance* 69, 2085-2125.

Fishman, M. J. and K. M. Hagerty, 1989. Disclosure decisions by firms and the competition for price efficiency. *Journal of Finance* 44, 633-646.

Giammarino, R., R. Heinkel, B. Hollifield, and K. Li, 2004. Corporate decisions, information and prices: Do managers move prices or do prices move managers? *Economic Notes* 33, 83-110.

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

Gompers, P., J. Ishii, and A. Metrick, 2003. Corporate governance and equity prices. *Quarterly Journal of Economics* 118, 107-155.

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.

Grullon, G., S. Michenaud, and J. Weston, 2014. The real effects of short-selling constraints. *Unpublished working paper.*

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.*

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.

Henry, T., D. Kisgen, and J. Wu, 2013. Equity short selling and bond rating downgrades. *Unpublished working paper*.

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.

Hong, H. and J. Stein, 2003. Differences of opinion, short-sales constraints, and market crashes. *Review of Financial Studies* 16, 487-525.

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.

Kau, J., J. Linck, and P. Rubin, 2008. Do managers Listen to the market? *Journal of Corporate Finance* 14, 347-362.

Kedia, S., and S. Rajgopal, 2014. Do the SEC's enforcement preferences affect corporate misconduct? *Journal of Accounting and Economics*, forthcoming.

Kogan, L., D. Papanikolaou, A. Seru, and N. Stoffman, 2012. Technological innovation and growth. *Unpublished working paper.*

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

Luo, Y., 2005. Do insiders learn from outsiders? Evidence from mergers and acquisitions. *Journal of Finance* 60, 1951-1982.

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

Massa, M., B. Zhang, and H. Zhang, 2013. Governance through threats: Does short-selling improve corporate governance? *Unpublished working paper.*

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.

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*;
$LnCitePat_t$	Natural logarithm of one plus firm *i*’s total number of 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*;
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;
E-index	The sum of six corporate charter anti-takeover provisions for firm *i*, where the six anti-takeover provisions are staggered boards, limits to bylaw

	amendments, limits to charter amendments, supermajority requirements for mergers, poison pills, and golden parachutes;
$Analyst_t$	The arithmetic mean of the 12 monthly numbers of earnings forecasts for firm *i* extracted from the I/B/E/S summary file over fiscal year *t*;
$Nonsync_t$	One minus the R^2 from the regression that regresses firm *i*'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;
PIN_t	A measure of price informativeness for firm *i* in year *t*, developed in Brown and Hillegeist (2007), who use a structural market microstructure model to directly capture the probability of informed trading in the firm's stock via analysis of buy and sell orders;
Option	The dummy variable that equals zero if firm *i* has no options traded during the entire sample period, and equals one otherwise;
SECDist	The geographical distance between the county where firm *i*'s headquarter is located and the county where the nearest SEC regional office (including the SEC headquarter in Washington D.C.) is located by using the corresponding latitudes and longitudes.

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
Patent	5.410	0.000	0.000	2.026	13.020	8,942
Citepat	0.347	0.000	0.000	0.043	0.902	8,942
LnPatent	0.753	0.000	0.000	1.107	1.239	8,942
LnCitePat	0.180	0.000	0.000	0.042	0.412	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 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.

	Pilot			Control			Difference	
	Mean	Median	S.D.	Mean	Median	S.D.	T-stat	Wilcoxon
LnPatent	0.782	0.000	1.258	0.808	0.000	1.274	-0.471	0.611
LnCitePat	0.216	0.000	0.442	0.235	0.000	0.465	-0.986	0.851
LnAssets	7.015	6.817	1.669	7.064	6.952	1.726	-0.656	0.558
LnAge	2.839	2.833	0.741	2.772	2.639	0.736	2.001	2.026
ROA	0.100	0.108	0.126	0.089	0.099	0.145	1.925	1.985
R&DAssets	0.034	0.000	0.072	0.038	0.000	0.080	-1.073	0.957
PPEAssets	0.493	0.409	0.365	0.473	0.384	0.376	1.139	1.623
Leverage	0.179	0.146	0.179	0.169	0.118	0.177	1.263	1.408
CapexAssets	0.045	0.031	0.049	0.041	0.028	0.047	1.623	2.144
TobinQ	2.141	1.575	1.561	2.099	1.565	1.478	0.614	0.461
KZindex	-8.370	-1.671	20.795	-9.267	-2.090	21.531	0.935	1.400
HIndex	0.317	0.210	0.271	0.302	0.204	0.261	1.189	1.076
InstOwn	0.584	0.616	0.235	0.586	0.617	0.242	-0.199	0.375
PatentGrow	-0.999	0.000	18.553	-0.310	0.000	35.725	-0.600	0.256
CitePatGrow	-0.162	0.000	6.106	-0.320	0.000	6.961	0.549	0.549
PatentGrow2yr	-0.434	0.000	36.470	0.152	0.000	47.381	-0.319	0.737
CitePatGrow2yr	-0.731	0.000	7.099	-0.584	0.000	7.925	-0.437	0.257

Table 3: Multivariate 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 activities. Definitions of variables are listed in the Appendix. Panel A reports results 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 December 31, 2009. 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.	*LnPatent*		*LnCitePat*	
	(1)	(2)	(3)	(4)
*Pilot*Post*	0.026***	0.029**	0.037***	0.050***
	(0.009)	(0.013)	(0.007)	(0.011)
Pilot	-0.033	-0.019	-0.026**	-0.029*
	(0.037)	(0.033)	(0.012)	(0.016)
LnAssets		0.372***		-0.005
		(0.016)		(0.006)
LnAge		0.048		-0.010
		(0.033)		(0.012)
ROA		0.609***		-0.093*
		(0.147)		(0.050)
R&DAssets		4.805***		0.546***
		(0.331)		(0.112)
PPEAssets		0.130*		0.007
		(0.072)		(0.031)
Leverage		-0.559***		-0.113**
		(0.096)		(0.052)
CapexAssets		-0.247		-0.047
		(0.277)		(0.096)
TobinQ		0.054***		0.007
		(0.013)		(0.004)
KZindex		0.001*		-0.000
		(0.001)		(0.000)
HIndex		-0.534*		-0.153
		(0.279)		(0.122)
HIndex Squared		0.560**		0.135
		(0.250)		(0.108)
InstOwn		-0.208***		0.085**
		(0.074)		(0.034)
Industry Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	8,942	7,757	8,942	7,757
R-squared	0.348	0.499	0.145	0.153

Panel B: Positive patent sample

Dep. Var.	*LnPatent*		*LnCitePat*	
	(1)	(2)	(3)	(4)
*Pilot*Post*	0.056**	0.042**	0.076***	0.081***
	(0.023)	(0.021)	(0.014)	(0.020)
Pilot	0.020	0.033	-0.036	-0.037
	(0.076)	(0.054)	(0.026)	(0.029)
LnAssets		0.490***		-0.060***
		(0.022)		(0.017)
LnAge		-0.011		-0.028
		(0.052)		(0.021)
ROA		0.363**		0.003
		(0.176)		(0.078)
R&DAssets		4.045***		-0.125
		(0.366)		(0.268)
PPEAssets		0.181		-0.008
		(0.124)		(0.058)
Leverage		-0.681***		-0.069
		(0.137)		(0.086)
CapexAssets		-0.815		-0.107
		(0.648)		(0.230)
TobinQ		0.063***		0.003
		(0.015)		(0.005)
KZindex		0.001		-0.001
		(0.001)		(0.001)
HIndex		-0.788**		-0.042
		(0.387)		(0.139)
HIndex Squared		0.732**		0.041
		(0.357)		(0.149)
InstOwn		0.053		0.146**
		(0.119)		(0.061)
Industry Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	4,096	3,997	4,096	3,997
R-squared	0.183	0.446	0.114	0.150

Panel C: Reversal of SHO

Dep. Var.	*LnPatent*		*LnCitePat*	
	(1)	(2)	(3)	(4)
*Pilot*Post*	-0.040***	-0.024**	-0.010**	-0.010**
	(0.014)	(0.012)	(0.004)	(0.005)
Pilot	0.012	0.026	0.010**	0.011**
	(0.039)	(0.033)	(0.004)	(0.005)
LnAssets		0.291***		0.003***
		(0.030)		(0.001)
LnAge		0.119***		-0.004
		(0.037)		(0.003)
ROA		0.206		-0.000
		(0.143)		(0.010)
R&DAssets		6.913***		0.244*
		(1.101)		(0.127)
PPEAssets		0.145**		-0.001
		(0.062)		(0.004)
Leverage		-0.303***		-0.010
		(0.110)		(0.012)
CapexAssets		-0.801**		-0.059**
		(0.394)		(0.026)
TobinQ		0.080***		0.003**
		(0.014)		(0.001)
KZindex		0.002***		0.000
		(0.001)		(0.000)
HIndex		0.305		-0.016
		(0.247)		(0.031)
HIndex Squared		-0.142		0.016
		(0.239)		(0.029)
InstOwn		-0.206***		0.022**
		(0.067)		(0.011)
Industry Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	6,340	5,426	6,340	5,426
R-squared	0.297	0.463	0.161	0.175

Table 4: Robustness tests for the multivariate DiD analysis

This table reports robustness test results for the multivariate 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. 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	-0.015	-0.001	0.013	0.022	5000
T-stat for *Pilot*Post*	-0.058	-1.180	-0.046	1.077	1.911	5000
Model (2) of Table 3 Panel A						
Coefficient before *Pilot*Post*	-0.001	-0.019	-0.001	0.018	0.027	5000
T-stat for *Pilot*Post*	-0.049	-1.235	-0.051	1.271	2.013	5000
Model (3) of Table 3 Panel A						
Coefficient before *Pilot*Post*	0.000	-0.011	-0.000	0.011	0.015	5000
T-stat for *Pilot*Post*	0.081	-1.001	-0.005	1.179	1.987	5000
Model (4) of Table 3 Panel A						
Coefficient before *Pilot*Post*	0.000	-0.012	-0.000	0.012	0.017	5000
T-stat for *Pilot*Post*	0.067	-1.068	-0.052	1.093	1.892	5000

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

Dep. Var.	*LnPatent*		*LnCitePat*	
	(1)	(2)	(3)	(4)
*Pilot*Post*	0.014	0.014	-0.010	0.008
	(0.012)	(0.010)	(0.023)	(0.024)
Pilot	-0.054	-0.054*	-0.001	-0.012
	(0.033)	(0.031)	(0.027)	(0.027)
Controls	No	Yes	No	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	9,215	7,733	9,215	7,733
R-squared	0.358	0.501	0.160	0.158

Table 5: Cross-sectional tests for the degree of price informativeness

This table reports the results of the multivariate difference-in-differences (DiD) tests using subsamples partitioned on a firm's degree of stock price informativeness. Panel A examines subsamples based on the average price nonsynchronicity over the three pre-event years. Panel B examines subsamples based on the average probability of informed trading (PIN) over the three pre-event years. 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. The last two rows report the Chi-squared test statistics and the corresponding p-values for the difference in the DiD estimators between columns (1) and (2), and between columns (3) and (4). ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Panel A: Subsample partitioned by price nonsynchronicity (*Nonsync*)

Dep. Var.	*LnPatent*		*LnCitePat*	
	High_ Nonsync	*Low_ Nonsync*	*High_ Nonsync*	*Low_ Nonsync*
	(1)	(2)	(3)	(4)
*Pilot*Post*	-0.001	0.065***	0.013**	0.035***
	(0.016)	(0.017)	(0.005)	(0.012)
Pilot	-0.021	-0.037	-0.010	-0.019
	(0.042)	(0.049)	(0.007)	(0.018)
Controls	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	4,085	3,838	4,085	3,838
R-squared	0.375	0.609	0.264	0.140
Chi -squared Test	14.309		2.947	
P-value	0.000		0.086	

Panel B: Subsample partitioned by probability of informed trading (*PIN*)

Dep. Var.	*LnPatent*		*LnCitePat*	
	High_ PIN	*Low_ PIN*	*High_ PIN*	*Low_ PIN*
	(1)	(2)	(3)	(4)
*Pilot*Post*	0.009	0.045***	0.014**	0.037**
	(0.015)	(0.015)	(0.006)	(0.016)
Pilot	-0.006	-0.007	-0.014*	0.000
	(0.038)	(0.051)	(0.007)	(0.017)
Controls	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	4,070	3,845	4,070	3,845
R-squared	0.336	0.583	0.262	0.166
Chi -squared Test	9.406		1.530	
P-value	0.002		0.216	

Table 6: Cross-sectional tests for the degree of information asymmetry

This table reports the results of the multivariate difference-in-differences (DiD) tests using subsamples partitioned on a firm's degree of information asymmetry. Panel A examines subsamples based on analyst coverage. Panel B examines subsamples based on firm size (natural logarithm of total assets). 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. The last two rows report the Chi-squared test statistics and the corresponding p-values for the difference in the DiD estimators between columns (1) and (2), and between columns (3) and (4). ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Panel A: Subsample partitioned by analyst coverage (*Analyst*)

Dep. Var.	*LnPatent*		*LnCitePat*	
	High_Analyst	*Low_ Analyst*	*High_ Analyst*	*Low_ Analyst*
	(1)	(2)	(3)	(4)
*Pilot*Post*	0.023	0.059**	0.033**	0.061***
	(0.021)	(0.026)	(0.014)	(0.023)
Pilot	-0.032	-0.005	-0.002	-0.067**
	(0.055)	(0.046)	(0.016)	(0.027)
Controls	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	3,681	3,706	3,681	3,706
R-squared	0.618	0.372	0.188	0.143
Chi -squared Test	3.624		4.873	
P-value	0.057		0.027	

Panel B: Subsample partitioned by firm size (*LnAssets*)

Dep. Var.	*LnPatent*		*LnCitePat*	
	High_ LnAssets	*Low_ LnAssets*	*High_ LnAssets*	*Low_ LnAssets*
	(1)	(2)	(3)	(4)
*Pilot*Post*	-0.001	0.062**	0.020**	0.091***
	(0.017)	(0.026)	(0.008)	(0.035)
Pilot	0.024	-0.005	0.000	-0.087**
	(0.052)	(0.038)	(0.010)	(0.036)
Controls	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	3,964	3,793	3,964	3,793
R-squared	0.636	0.390	0.156	0.143
Chi -squared Test	2.954		4.839	
P-value	0.086		0.028	

Table 7: Cross-sectional tests for the degree of agency problem

This table reports the results of the multivariate difference-in-differences (DiD) tests using subsamples partitioned on the degree of agency problem within a firm. Panel A examines subsamples based on institutional ownership. Panel B examines subsamples based on product market competition (industry Herfindahl index). Panel C examines subsamples based on corporate governance (the E-index). 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. The last two rows report the Chi-squared test statistics and the corresponding p-values for the difference in the DiD estimators between columns (1) and (2), and between columns (3) and (4). ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Panel A: Subsamples partitioned by institutional ownership (*InstOwn)*

Dep. Var.	*LnPatent*		*LnCitePat*	
	High_InstOwn	*Low_InstOwn*	*High_InstOwn*	*Low_InstOwn*
	(1)	(2)	(3)	(4)
*Pilot*Post*	-0.011	0.070***	0.024	0.054**
	(0.026)	(0.022)	(0.017)	(0.024)
Pilot	-0.002	-0.013	-0.006	-0.058*
	(0.047)	(0.042)	(0.017)	(0.031)
Controls	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	4,127	3,630	4,127	3,630
R-squared	0.506	0.523	0.209	0.135
Chi -squared Test	5.889		2.907	
P-value	0.015		0.088	

Panel B: Subsamples partitioned by product market concentration (*HIndex*)

Dep. Var.	*LnPatent*		*LnCitePat*	
	High_ HIndex	*Low_ HIndex*	*High_ HIndex*	*Low_ HIndex*
	(1)	(2)	(3)	(4)
*Pilot*Post*	0.046**	0.016	0.075***	0.025
	(0.018)	(0.020)	(0.024)	(0.016)
Pilot	-0.018	-0.008	-0.036	-0.014
	(0.044)	(0.049)	(0.031)	(0.016)
Controls	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	4,029	3,728	4,029	3,728
R-squared	0.506	0.534	0.122	0.226
Chi -squared Test	2.152		4.923	
P-value	0.142		0.027	

Panel C: Subsamples partitioned by corporate governance (*E-Index*)

Dep. Var.	*LnPatent*		*LnCitePat*	
	High_E-Index	*Low_ E-Index*	*High_ E-Index*	*Low_ E-Index*
	(1)	(2)	(3)	(4)
*Pilot*Post*	0.075***	0.007	0.058**	0.028***
	(0.021)	(0.012)	(0.027)	(0.008)
Pilot	0.011	-0.012	-0.033	-0.008
	(0.047)	(0.052)	(0.033)	(0.014)
Controls	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	2,906	3,255	2,906	3,255
R-squared	0.522	0.593	0.130	0.221
Chi -squared Test	5.324		2.209	
P-value	0.021		0.137	

Table 8: Cross-sectional tests for the availability of option trading and the distance to SEC regional offices

This table reports the results of the multivariate difference-in-differences (DiD) tests using subsamples partitioned on whether there are available options traded on a firm's stock (Panel A) or subsamples partitioned on the distance between firm headquarters and the nearest SEC regional office (Panel B). 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. The last two rows report the Chi-squared test statistics and the corresponding p-values for the difference in the DiD estimators between columns (1) and (2), and between columns (3) and (4). ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Panel A: Subsample partitioned by availability of traded options (*Option*)

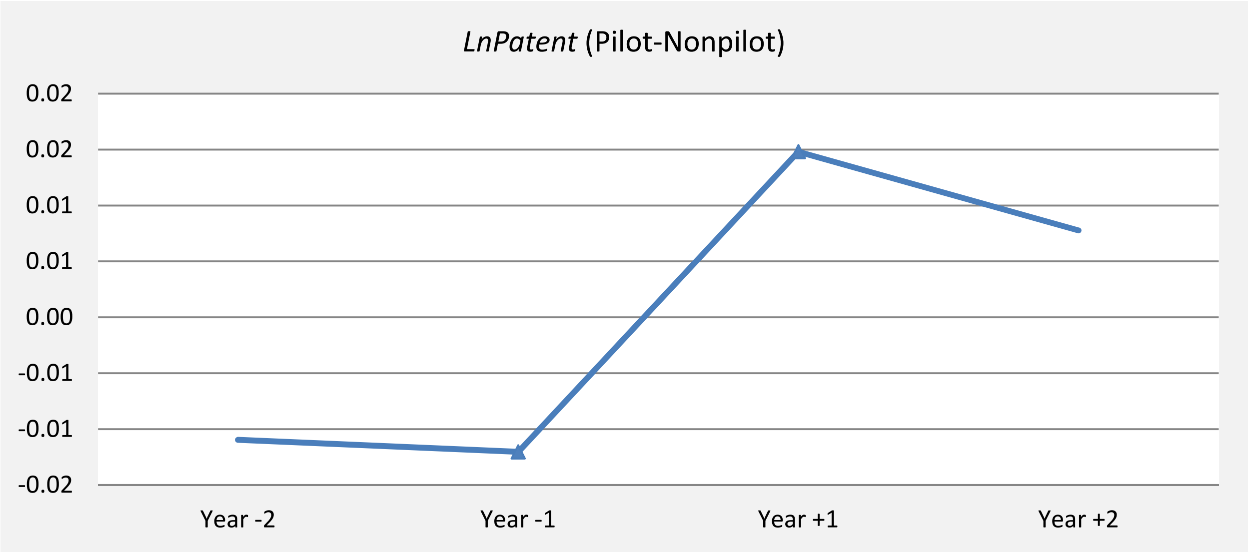
Dep. Var.	*LnPatent*		*LnCitePat*	
	With_ Option	*Without_ Option*	*With_ Option*	*Without_ Option*
	(1)	(2)	(3)	(4)
*Pilot*Post*	0.011	0.053***	0.019	0.066***
	(0.017)	(0.019)	(0.016)	(0.016)
Pilot	-0.022	-0.046	0.004	-0.043**
	(0.045)	(0.042)	(0.020)	(0.019)
Controls	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	4,436	3,497	4,436	3,497
R-squared	0.565	0.421	0.144	0.204
Chi -squared Test	23.808		5.581	
P-value	0.000		0.018	

Panel B: Subsample partitioned by distance to SEC regional offices (*SECDist*)

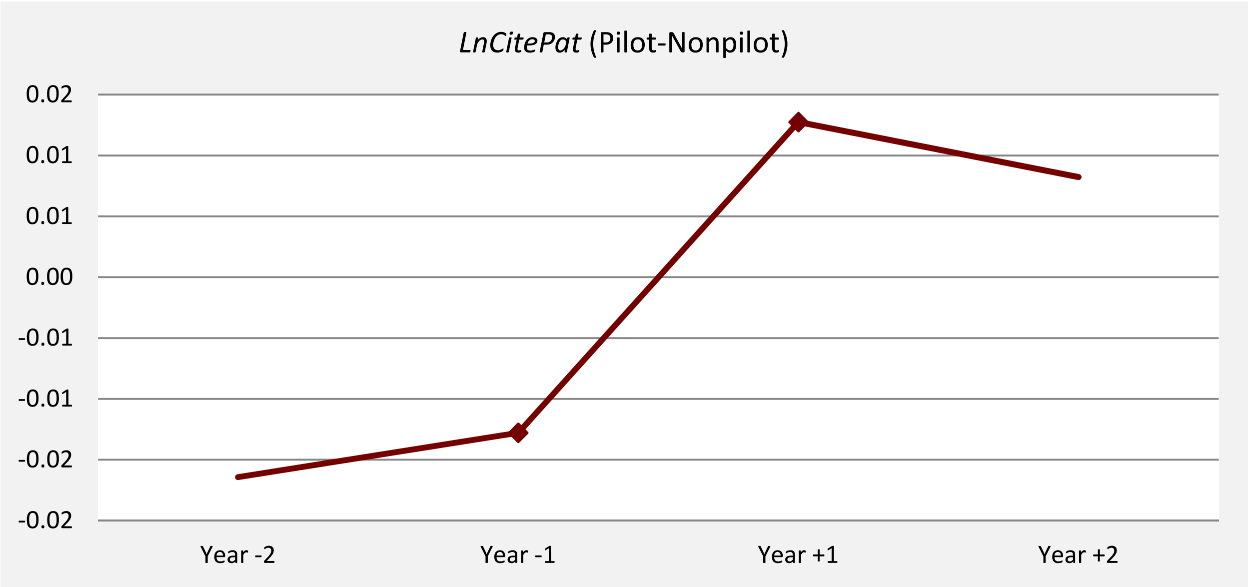
Dep. Var.	*LnPatent*		*LnCitePat*	
	Large_ SECDist	*Small_ SECDist*	*Large_ SECDist*	*Small_ SECDist*
	(1)	(2)	(3)	(4)
*Pilot*Post*	0.026	0.034**	0.034***	0.051***
	(0.024)	(0.017)	(0.011)	(0.019)
Pilot	-0.037	0.013	-0.019	-0.027
	(0.048)	(0.050)	(0.019)	(0.023)
Controls	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	4,053	3,832	4,053	3,832
R-squared	0.524	0.507	0.162	0.175
Chi -squared Test	0.041		1.019	
P-value	0.839		0.313	

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

This figure shows the trend of innovation activities 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 total number of patents and Panel (b) reports the mean logarithm of total number of citations per patent.



(a)



(b)