The Journal of FINANCE
The Journal of THE AMERICAN FINANCE ASSOCIATION

THE JOURNAL OF FINANCE • VOL. LXIX, NO. 5 • OCTOBER 2014

Does Stock Liquidity Enhance or Impede Firm Innovation?

VIVIAN W. FANG, XUAN TIAN, and SHERI TICE*

ABSTRACT

We aim to tackle the longstanding debate on whether stock liquidity enhances or impedes firm innovation. This topic is of interest because innovation is crucial for firm- and national-level competitiveness and stock liquidity can be altered by financial market regulations. Using a difference-in-differences approach that relies on the exogenous variation in liquidity generated by regulatory changes, we find that an increase in liquidity causes a reduction in future innovation. We identify two possible mechanisms through which liquidity impedes innovation: increased exposure to hostile takeovers and higher presence of institutional investors who do not actively gather information or monitor.

INNOVATION PRODUCTIVITY is of interest to a large number of stakeholders including firm managers, employees, investors, and regulators. As Porter (1992, p. 65) states, "To compete effectively in international markets, a nation's businesses must continuously innovate and upgrade their competitive advantages. Innovation and upgrading come from sustained investment in physical as well as intangible assets." Given the importance of innovation for firm- and national-level competitiveness, investigation into factors that increase or decrease innovation is warranted. There has been much debate on whether stock liquidity enhances or impedes innovation. This topic is of particular interest to regulators, since stock liquidity can be altered by changing financial market regulations and securities laws.[1] The goal of this paper is to further our understanding of this

*Fang is with the Carlson School of Management, University of Minnesota. Tian is with the Kelley School of Business, Indiana University and PBC School of Finance, Tsinghua University. Tice is with the A.B. Freeman School of Business, Tulane University. We are grateful for helpful comments from an anonymous referee, an anonymous Associate Editor, Cam Harvey (the Editor), Utpal Bhattacharya, Francois Degeorge, Valentin Dimitrov, Alex Edmans, Eitan Goldman, Craig Holden, Carolyn Levine, Alexander Ljungqvist, Gustavo Manso, Gordon Phillips, Scott Smart, Noah Stoffman, Charles Trzcinka, Michael Usher, Xiaoyun Yu, and seminar and conference participants at Indiana University, the 2011 Western Finance Association meetings, the 2011 Paris Spring Corporate Finance Conference, and the 2011 Financial Management Association meetings. Tice acknowledges research support from the A.B. Freeman Chair in Finance at the A.B. Freeman School of Business, Tulane University. We remain responsible for all errors and omissions.

[1] Examples of regulations and securities laws that promote liquidity are disclosure requirements, insider trading rules, rules to eliminate price manipulation, reduction of minimum tick size, and deregulation of stock commissions.

DOI: 10.1111/jofi.12187

issue by using a set of novel experiments to examine the effect of stock liquidity on firm innovation.

Stock liquidity may impede firm innovation for two reasons. First, Stein (1988) shows that, in the presence of information asymmetry between managers and investors, takeover pressure could induce managers to sacrifice long-term performance (like investment in innovation) for current profits to keep the stock from becoming undervalued. Shleifer and Summers (1988) suggest that managers have less power over shareholders when hostile takeover threats are higher, which leads to fewer managerial incentives to invest in innovation. Kyle and Vila (1991) argue that, when liquidity is high, the presence of liquidity traders allows the entry of an outsider who can camouflage her buying in an attempt to take over a firm. Since high liquidity increases the probability of a hostile takeover attempt, it can exacerbate managerial myopia and lead to lower levels of long-term intangible investment such as innovation.

Second, due to lower trading costs, high liquidity facilitates entry and exit of institutional investors who trade based on current earnings news and whose trading may lead to misvaluation and underinvestment in innovation (Porter (1992)). Bushee (2001) shows that a group of institutional investors presumably chase short-term performance as they tend to invest more heavily in firms with greater expected near-term earnings. Bushee (1998) provides evidence that managers feel pressure to cut R&D to manage earnings. Managerial myopia is consistent with executive survey findings in Graham, Harvey, and Rajgopal (2005). In their survey, Chief Investment Officers (CFOs) reveal that they are frequently willing to sacrifice long-term sustainability to meet short-term earnings targets. They explain that meeting earnings benchmarks (analyst consensus or same quarter earnings last year) helps maintain a firm's stock price.

On the other hand, stock liquidity may enhance firm innovation as liquidity facilitates the entry of blockholders (e.g., Maug (1998), Edmans (2009)). While Maug (1998) predicts more monitoring activities by blockholders in highly liquid firms, Admati and Pfleiderer (2009), Edmans (2009), and Edmans and Manso (2011) show that the mere act of gathering and trading on private information by blockholders can discipline managers when managerial compensation is closely tied to stock price. This is because blockholders' collection of private information and trading on such information make liquid stocks' prices more efficient. If high liquidity leads to better monitoring and/or more efficient prices, managers may be willing to forgo short-term profits to invest in long-term investments such as innovation.

The question of whether stock liquidity enhances or impedes investment in innovation has been difficult to test due primarily to simultaneity between stock liquidity and innovation. In other words, liquidity may affect innovation but innovation could also affect liquidity. To address this simultaneity we run tests during periods surrounding exogenous shocks to liquidity such as decimalization and other regulatory changes in the minimum tick size using a difference-in-differences (hereafter, DiD) approach. Changes in tick size are good quasi-natural experiments for a number of reasons. First, they directly affect stock liquidity as liquidity rises on average surrounding changes in tick size and the increase in liquidity exhibits variation in the cross-section of stocks (Bessembinder (2003), Furfine (2003)). However, it is unlikely that changes in tick size directly affect innovation. Second, it is unlikely that a change in expected future innovation influences the cross-sectional changes in liquidity brought about by changes in tick size. In addition, the unobservability of investment in intangible assets has been an impediment to research on whether liquidity enhances or impedes innovation. To surmount this challenge, we use an observable investment output (patenting) in our tests as this helps us assess the success of investment in long-term intangible assets, which have traditionally been difficult to observe.[2]

We document a positive relation between stock illiquidity (measured by the relative effective spread) and innovation output (measured by patents and citations per patent) one, two, and three years in the future. To establish causality, we employ three identification tests using the DiD approach. First, we make use of the exogenous variation in stock liquidity generated by decimalization surrounding 2001 and show that firms with a larger increase in liquidity due to decimalization experience a bigger drop in innovation output than those with a smaller increase in liquidity. For example, firms with an increase in liquidity in the top tercile of the sample due to decimalization produce 18.5% fewer patents per year in the first three years following decimalization than matched firms of similar characteristics but with an increase in liquidity in the bottom tercile. Second, we use the exogenous shock to liquidity that occurred in 1997 when the minimum tick size moved from the 8th regime to the 16th regime. We obtain similar results. Finally, we explore the phase-in feature of decimalization and exploit the exogenous variation in liquidity generated by staggered shifts from the fractional pricing system to the decimal pricing system on the NYSE exchange. We find that pilot firms that converted to decimal pricing in 2000 experience a significantly larger drop in one-year-ahead patent output than nonpilot firms that went decimal in 2001. Overall, our identification tests suggest that stock liquidity has a negative causal effect on firm innovation.

We next explore possible mechanisms for how increased stock liquidity causes a drop in firm innovation. To do so we use the DiD approach to examine if changes in hypothesized mechanisms are more significant for firms with a larger increase in liquidity than for firms with a smaller increase in liquidity due to decimalization. Using the takeover exposure model of Cremers, Nair, and John (2009), we find that firms with a larger exogenous increase in liquidity from decimalization have a higher probability of facing a hostile takeover in the next three years. An increased hostile takeover threat could put pressure on managers to boost current profits and cut long-term investment in innovation as a strategy to prevent a hostile takeover attempt. We also find that firms with a larger exogenous increase in liquidity experience a larger increase in the

[2] Due to the difficulty in identifying whether there is underinvestment in unobservable activities, Stein (2003) points out that most studies in managerial myopia tend to examine firm operating performance surrounding equity issues. This is because managers face short-term pressure to increase earnings and boost the current stock price prior to equity issues.

holdings of nondedicated institutional investors.[3] An increase in the holdings of nondedicated institutional investors may put increased pressure on managers to boost current profits and cut long-term investment in innovation or risk the exit of these investors.

Our paper's main contribution is to shed light on the longstanding theoretical and policy debate on whether stock liquidity enhances or impedes firms' long-term intangible investments such as innovation. To the best of our knowledge, this paper is the first in the literature to provide causal evidence that stock liquidity impedes firm innovation. Thus, our paper uncovers a previously under-identified adverse consequence of regulatory effort to enhance stock liquidity.

Our paper differs from other papers that examine innovation as we provide direct and causal evidence that stock liquidity affects firm innovation. Our finding of higher levels of innovation for illiquid stocks complements the findings of recent papers on innovation. Aghion, Van Reenen, and Zingales (2013) show that firms with higher institutional ownership have more innovation as higher institutional ownership lowers manager career concerns that arise with riskier innovation. We provide insights into their paper by showing that their results could be due to dedicated institutional investors who trade for reasons other than liquidity. Lerner, Sorensen, and Stromberg (2011) find that leveraged buyout (LBO) firms generate more important patents after the LBO transaction, consistent with the theoretical prediction of Ferreira, Manso, and Silva (2014). They do not directly test the link between stock liquidity and innovation, but their findings can be viewed as consistent with our paper as an LBO can be interpreted as an extreme case where a firm's stock liquidity is gone.[4] Lastly, while Atanassov (2013) finds a drop in innovation for firms incorporated in states that pass antitakeover laws during the 1980s and early 1990s, Chemmanur and Tian (2013) find a rise in innovation for firms that have more antitakeover defenses during 1990 to 2006. We add to the debate by finding an increase in the probability of a hostile takeover following exogenous increases to stock liquidity in 2001. Our findings suggest the higher probability of a takeover caused by the increase in liquidity may be one mechanism that reduces innovation as liquidity rises.

Our paper also adds to the small but growing literature linking liquidity to firm performance. Fang, Noe, and Tice (2009) find that an exogenous shock to liquidity leads to an increase in firm performance (higher firm Q) by creating

[3] Bushee (1998) classifies institutional investors into transient investors, quasi-indexers, and dedicated investors. Transient investors are characterized by high portfolio turnover and momentum trading, quasi-indexers by following indexing strategies and holding fragmented diverse portfolios, and dedicated investors by concentrated portfolio holdings and low portfolio turnover. Porter (1992) argues that a higher presence of transient investors and quasi-indexers exacerbates managerial myopia as these investors have low incentives to collect fundamental information or monitor managers. We thus group transient investors and quasi-indexers together as nondedicated investors.

[4] In a similar vein, Asker, Farre-Mensa, and Ljungqvist (2011) find that publicly traded firms are subject to stock market pressures, which results in underinvestment and lower sensitivity to investment opportunities compared to private firms.

a more efficient feedback mechanism from investors to managers via prices or by enhancing the efficiency of stock-based managerial compensation. Bharath, Jayaraman, and Nagar (2013) add to their work and find that exogenous shocks to liquidity lead to greater increases in firm value for stocks with a higher level of blockholders. Two recent papers find evidence of more governance with

B. Variable Measurement

B.1. Measuring Innovation

Existing literature has developed two proxies to capture firm innovation: R&D expenditures and patenting activity. Between the two measures, patenting activity is considered a better proxy, as it measures innovation output and captures how effectively a firm has utilized its innovation inputs (both observable and unobservable). In contrast, R&D expenditures are only one particular observable input and fail to capture the quality of innovation. Therefore, following previous studies, for example, Seru (2014) for publicly traded firms and Lerner, Sorensen, and Stromberg (2011) for privately held firms, we use a firm's patenting activity to measure innovation.

We obtain information on firms' patenting activity from the latest version of the NBER Patent Citation Data File, which provides annual information from 1976 to 2006 on patent assignee names, the number of patents, the number of citations received by each patent, a patent's application year, a patent's grant year, etc. Based on the information retrieved from the NBER patent database, we construct two measures of a firm's innovation productivity. The first is the number of patent applications a firm files in a year that are eventually granted. We use a patent's application year instead of its grant year as the application year is argued to better capture the actual time of innovation (Griliches, Pakes, and Hall (1988)). Although straightforward to compute, this measure cannot distinguish groundbreaking innovations from incremental technological discoveries. To further assess a patent's influence, we construct a second measure of corporate innovation productivity by counting the number of non-self-citations each patent receives in subsequent years. Controlling for firm size, the number of patents captures innovation productivity while citations-per-patent captures the importance of innovation output. To reflect the long-term nature of investment in innovation, both measures of innovation productivity are measured one, two, and three years in the future.

Following existing innovation literature, we adjust the two measures of innovation to address the truncation problems associated with the NBER patent database. The first truncation problem arises as the patents appear in the database only after they are granted. In fact, we observe a gradual decrease in the number of patent applications that are eventually granted as we approach the last few years in the sample period. This is because the lag between a patent's application year and a patent's grant year is significant (about two years on average). Many patent applications filed during the latter years in the sample were still under review and had not been granted by 2006. Following Hall, Jaffe, and Trajtenberg (2001, 2005), we correct for this truncation bias by first estimating the application-grant lag distribution for the patents filed and granted between 1995 and 2000. This is done by calculating the time interval (in years) between a patent's application year and its grant year. We define W_s, the application-grant lag distribution, as the percentage of patents applied for in a given year that are granted in s years. We then compute the truncation-adjusted patent counts, P_{adj}, as $P_{\text{adj}} = \frac{P_{\text{raw}}}{\sum_{s=0}^{2006-t} W_s}$, where P_{raw} is the raw (unadjusted) number of patent applications at year t and $2001 \leq t \leq 2006$. The second type of truncation problem relates to the citation counts, as a patent can keep receiving citations over a long period of time, but we only observe citations received up to 2006. Following Hall, Jaffe, and Trajtenberg (2001, 2005), we correct for this truncation bias by dividing the observed citation counts by the fraction of predicted lifetime citations actually observed during the lag interval. More specifically, we scale up the citation counts using the variable "*hjtwt*" provided by the NBER patent database, which relies on the shape of the citation lag distribution. The truncation-adjusted measures of patents and citation counts are used in all of our tests.

The distribution of patent grants in the pooled sample is right-skewed, with the 75th percentile of the distribution at zero.[6] Due to the right-skewed distributions of patent counts and citations per patent, we use the natural logarithm of the weight-adjusted patent counts and the natural logarithm of the citation lag-adjusted citations per patent, *INNOV_PAT* and *INNOV_CITE*, as the main innovation measures in our analysis. To avoid losing firm-year observations with zero patents or citations per patent, we add one to the actual values when calculating the natural logarithm.

It is important to note that using patenting activity to measure innovation is not without limitations. In particular, different industries have various innovation propensity and duration. For example, the innovation process by nature is longer and riskier in the pharmaceutical industry than in the software development industry. One might therefore observe fewer patents generated in the pharmaceutical industry in a given time period, but this does not necessarily imply that pharmaceutical firms are less innovative than software firms. However, we believe that an adequate control for heterogeneity in industries and firms should alleviate this concern and lead to reasonable inferences that can be applicable across industries and firms.

B.2. Measuring Stock Liquidity

We use the relative effective spread during fiscal year t as our primary proxy for stock liquidity (higher relative effective spread means lower liquidity), where relative effective spread is defined as the absolute value of the difference between the execution price and the midpoint of the prevailing bid-ask quote (effective spread) standardized by the midpoint of the prevailing bid-ask quote. While the market microstructure literature has proposed a handful of liquidity measures, the effective spread is generally considered the best proxy for liquidity as it is based on realized high-frequency trading data. In fact, it often serves as a benchmark to evaluate the effectiveness of other liquidity measures

[6] Firm-year observations with zero patents represent roughly 77% of our sample, which is comparable to the 84% reported in Atanassov, Nanda, and Seru (2007) and the 73% reported in Tian and Wang (2014). Their samples include the universe of Compustat firms between 1974 and 2000 and venture capital–backed IPO firms between 1985 and 2006, respectively.

computed using low-frequency price and volume data (see, for example, Hasbrouck (2009), Goyenko, Holden, and Trzcinka (2009)).

To construct relative effective spreads, we follow the procedures detailed in Chordia, Roll, and Subrahmanyam (2001) and Fang, Noe, and Tice (2009). Specifically, for each stock in our sample, we first calculate the relative effective spread for each matched quote/trade during a trading day. To do so, we match any trade from 1993 to 1998 to the first quote at least five seconds before the trade and any trade after 1998 to the first quote prior to the trade.[7] Trades out of sequence, trades recorded before the open or after the close, and/or trades with special settlement conditions are dropped. To minimize matching errors, trades with a quoted spread (i.e., quoted bid-ask spread of the transaction) larger than five dollars, a ratio of effective spread to quoted spread larger than four, or a ratio of quoted spread to execution price larger than 0.4 are further deleted from the sample.

Next, the arithmetic mean of the relative effective spreads for each matched quote/trade over a trading day for a stock is defined as its daily relative effective spread. Each daily relative effective spread within a month is then weighted equally to calculate the monthly relative effective spread. Finally, the annual relative effective spread is defined as the arithmetic mean of the monthly relative effective spreads over a stock's fiscal year. Due to the nonnormality of effective spreads, the natural logarithm of the annual relative effective spread (denoted as *ILLIQ*) is used in all regression analyses.

B.3. Measuring Control Variables

Following the innovation literature, we control for a vector of firm and industry characteristics that may affect a firm's future innovation productivity. All variables are computed for firm i over its fiscal year t. In the baseline regressions, the control variables include firm size, *LN_MV*, measured by the natural logarithm of firm market capitalization; profitability, *ROA,* measured by return on assets; investment in innovation, *RDTA*, measured by R&D expenditures scaled by total assets; asset tangibility, *PPETA*, measured by net property, plant, and equipment scaled by total assets; leverage, *LEV*, measured by total debt-to-total assets; investment in fixed assets, *CAPEXTA*, measured by capital expenditures scaled by total assets; product market competition, *HINDEX*, measured by the Herfindahl index based on annual sales; growth opportunities, Q, measured by Tobin's Q; financial constraints, *KZINDEX*, measured by the Kaplan and Zingales (1997) five-variable KZ index; and firm age, *LN_AGE,* measured by the natural logarithm of one plus the number of years the firm is listed on Compustat. To mitigate nonlinear effects of product market competition (Aghion et al. (2005)), we also include the squared Herfindahl index in our

[7] Lee and Ready (1991) note that trade reports are generally delayed and suggest using quotes lagged five seconds. However, this observed delay has dissipated in recent years. Chordia, Roll, and Subrahmanyam (2001) suggest matching any trade to the first quote prior to the trade after 1998.

baseline regressions. Detailed variable definitions are described in Panel A of Table I.

C. Descriptive Statistics

To minimize the effect of outliers, we winsorize all variables at the top and bottom 1% of each variable's distribution. Panel B of Table I provides summary statistics of the main variables used in this study. On average, a firm in our final sample has 6.5 granted patents per year and each patent receives 3.4 non-self-citations. The stock illiquidity measure *ILLIQ* has a mean value of −4.482 and a median value of −4.377 (the mean relative effective spread for the sample is 0.022 and median relative effective spread is 0.013), which is comparable to previous studies (e.g., Fang, Noe, and Tice (2009)). Panel B also reports summary statistics of the control variables. In our sample, an average firm has market capitalization of $2.21 billion, return on assets of 7.8%, property, plant, and equipment scaled by total assets of 28.5%, total debt-to-total assets of 20.9%, Tobin's Q of 2.1, and is 9.9 years old since its IPO date.

Panel C of Table I reports the number and fraction of firms with and without patents by industry. In our sample, firms with patents are spread broadly across industries. Using the Fama-French 12-industry classification obtained from Kenneth French's website (http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html), we show that all 12 industries have firms with nonzero patents during our sample period and the fraction of firms with nonzero patents ranges from a low of 4.1% to a high of 61.4%.

II. Empirical Results

A. OLS Specification

To assess whether stock liquidity enhances or impedes corporate innovation, we estimate

$$INNOV\_PAT_{i,t+n}(INNOV\_CITE_{i,t+n}) = a + bILLIQ_{i,t} + c'CONTROLS_{i,t} + YR_t + FIRM_i + error_{i,t}, \quad (1)$$

where i indexes firm, t indexes time, and n equals one, two, or three. The dependent variables—the natural logarithm of one plus the number of patents filed and eventually granted (

Table I

Variable Definitions, Summary Statistics, and Patents by Industry

Panel A provides definitions of the main variables. Panel B reports summary statistics for variables constructed using a sample of U.S. public firms. Innovation variables are measured from 1994 to 2005. Illiquidity and control variables are measured from 1993 to 2004. Panel C reports the number and percentage of firms that generate at least one patent and the number and percentage of firms that generate zero patents over the sample period of 1994 and 2005 in each industry. In Panel C, industries are defined following the Fama and French 12 industry group classification system.

Panel A: Variable Definitions

Variable	Definition
Measures of Innovation	
$INNOV\_PAT_{t+n}$	$INNOV\_PAT_{t+1}$, $INNOV\_PAT_{t+2}$, and $INNOV\_PAT_{t+3}$ denote the natural logarithm of one plus firm *i*'s total number of patents filed (and eventually granted) in year $t+1$, year $t+2$, and year $t+3$, respectively.
$INNOV\_CITE_{t+n}$	$INNOV\_CITE_{t+1}$, $INNOV\_CITE_{t+2}$, and $INNOV\_CITE_{t+3}$ denote the natural logarithm of one plus firm *i*'s total number of non-self-citations received on the firm's patents filed (and eventually granted), scaled by the number of the patents filed (and eventually granted) in year $t+1$, year $t+2$, and year $t+3$, respectively.
Measure of Stock Liquidity and Control Variables Used in Baseline Specifications	
$ILLIQ_t$	Natural logarithm of annual relative effective spread, *RESPRD*, measured over firm *i*'s fiscal year *t*. *RESPRD* is defined as (the absolute value of the difference between the execution price and the midpoint of the prevailing bid-ask quote) divided by the midpoint of the prevailing bid-ask quote.
LN_MV_t	Natural logarithm of firm *i*'s market value of equity (#25×#199) measured at the end of fiscal year *t*.
$RDTA_t$	Research and development expenditures (#46) divided by book value of total assets (#6) measured at the end of fiscal year *t*, set to zero if missing.
ROA_t	Return on assets defined as operating income before depreciation (#13) divided by book value of total assets (#6), measured at the end of fiscal year *t*.
$PPETA_t$	Property, plant, and equipment (net, #8) divided by book value of total assets (#6) measured at the end of fiscal year *t*.
LEV_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*.
$CAPEXTA_t$	Capital expenditures (#128) scaled by book value of total assets (#6) measured at the end of fiscal year *t*.
$HINDEX_t$	Herfindahl index of four-digit SIC industry *j* to which firm *i* belongs, measured at the end of fiscal year *t*.
$HINDEX^2{}_t$	The square of $HINDEX_t$.
Q_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 zero if missing)) divided by book value of assets (#6).

(Continued)

Table I—*Continued*

Panel A: Variable Definitions	
Variable	Definition
$KZINDEX_t$	Firm i's KZ index measured at the end of fiscal year t, calculated as $-1.002 \times$ cash flow ((#18+#14)/#8) plus $0.283 \times Q$ ((#6+#199×#25−#60−#74)/#6) plus $3.139 \times$ leverage ((#9+#34)/(#9+#34+#216)) minus $39.368 \times$ dividends ((#21+#19)/#8)

Table I—*Continued*

Panel C: Number and Percentage of Firms with and without Patents by Industry					
FF	Industry Name	Description	Firms with Positive Patents	Firms with Zero Patents	Total No. of Firms
6	BusEq	Business equipment (computers, software, and electronic equipment)	887 (49.6%)	901 (50.4%)	1,788
7	Telcm	Telephone and television transmission	43 (15.8%)	229 (84.2%)	272
8	Utils	Utilities	23 (10.7%)	191 (89.3%)	214
9	Shops	Wholesale, retail, and some services (laundries, repair shops)	74 (9.9%)	673 (90.1%)	747
10	Hlth	Healthcare, medical equipment, and drugs	490 (54.1%)	416 (45.9)	906
11	Money	Finance	29 (4.1%)	671 (95.9%)	700
12	Other	Mines, construction, building materials, transportation, hotels, business services, entertainment	160 (15.1%)	899 (84.9%)	1,059

innovation productivity as discussed in Section I.B.3. We include year fixed effects to account for intertemporal variation that may affect the relation between stock liquidity and innovation, and firm fixed effects to control for omitted firm characteristics that are constant over time. Innovation (our dependent variable) is likely to be autocorrelated over time. We therefore cluster standard errors by firm to avoid inflated t-statistics (Petersen (2009)).

In Table II, Panel A, we examine the effect of a firm's stock liquidity (*ILLIQ*) on its number of patents filed (and eventually granted) in one year.[8] The coefficient estimate on *ILLIQ* is positive and both economically and statistically significant. Increasing relative effective spread from its median (0.013) to the 90th percentile (0.052) is associated with a 42.3% increase in the number of patents filed in one year. We also find that a larger innovation input, measured by a higher R&D-to-assets ratio in year t, is associated with more innovation output in future years. In columns (2) and (3), we replace the dependent variable with the natural logarithm of the number of patents filed in two and three years, respectively. The coefficient estimates on *ILLIQ* continue to be positive and significant at the 1% level. Panel B of Table II reports the regression results estimating equation (1) with the dependent variable replaced

[8] In addition to the pooled OLS regression we use a Tobit model and a Poisson model to account for the nonnegative nature of patent and citation counts, the nontrivial fraction of sample firms with patent and citation counts equal to zero (corner solution response), and the fact that patents are a count variable. The results remain robust to the use of the Tobit model and the Poisson model with industry fixed effects instead of firm fixed effects.

Table II

OLS Specifications

Panel A (B) reports pooled OLS regression results of the model $INNOV\_PAT_{i,t+n}$ ($INNOV\_CITE_{i,t+n}$) $= a + bILLIQ_{i,t} + c'CONTROLS_{i,t} + YR_t + FIRM_i + error_{i,t}$. The dependent variable is $INNOV\_PAT_{i,t+1}$ ($INNOV\_CITE_{i,t+1}$) in column (1), which is replaced with $INNOV\_PAT_{i,t+2}$ ($INNOV\_CITE_{i,t+2}$) and $INNOV\_PAT_{i,t+3}$ ($INNOV\_CITE_{i,t+3}$) in columns (2) and (3), respectively. Variable definitions are provided in Table I

Table II—*Continued*

Panel B: Innovation Measured by *INNOV_CITE*

Dependent Variable	(1) $INNOV\_CITE_{t+1}$	(2) $INNOV\_CITE_{t+2}$	(3) $INNOV\_CITE_{t+3}$
$RDTA_t$	0.169**	0.149*	0.175*
	(0.080)	(0.090)	(0.098)
ROA_t	0.137**	0.299***	0.250***
	(0.061)	(0.062)	(0.074)
$PPETA_t$	0.168**	0.143	0.164*
	(0.077)	(0.087)	(0.095)
LEV_t	−0.197***	−0.266***	−0.313***
	(0.052)	(0.060)	(0.064)
$CAPEXTA_t$	0.240**	0.229*	0.243*
	(0.113)	(0.120)	(0.126)
$HINDEX_t$	0.129*	0.105	0.088
	(0.077)	(0.082)	(0.086)
$HINDEX^2{}_t$	−0.167	−0.035	−0.082
	(0.126)	(0.132)	(0.136)
Q_t	0.004	0.008	0.008
	(0.006)	(0.006)	(0.006)
$KZINDEX_t$	−0.000	−0.000	0.000
	(0.000)	(0.000)	(0.000)
LN_AGE_t	0.091***	0.063**	0.084***
	(0.025)	(0.029)	(0.031)
INTERCEPT	0.661***	1.010***	1.119***
	(0.080)	(0.089)	(0.098)
Year and firm fixed effects	Included	Included	Included
number of obs. used	39,469	33,098	27,363
Adjusted R^2	0.652	0.653	0.653

by *INNOV_CITE*. The coefficient estimates on *ILLIQ* remain economically and statistically significant. For example, column (1) suggests that increasing relative effective spread from its median to the 90th percentile is associated with a 31.2% increase in the number of citations received by each patent in one year.

The results in Table II are robust to replacing the proxy for firm size (the market capitalization of equity) with either the book value of total assets or firm sales, to excluding lagged R&D-to-assets from the regression, and to the use of alternative measures of stock liquidity.[9] The results using alternative measures of stock liquidity are tabulated in the Internet Appendix.[10]

To provide additional insights, we conduct a number of tests to examine whether various subsamples are driving the OLS results. In summary, we show that the negative relation between stock liquidity and firm innovation

[9] Myopic managers may cut investment in a project too early, which would reduce innovation productivity in years $t+1$, $t+2$, and $t+3$, controlling for the level of lagged R&D expense in year t. Similarly, myopic managers may select projects with a faster payback-to-R&D ratio even though the projects may ultimately create less innovation and value for the firm. Thus, controlling for lagged R&D gives a better idea of innovation productivity.

[10] The Internet Appendix may be found in the online version of this article.

is not driven by firms acquiring or merging with other firms, is not driven by small-cap firms, is not driven by firms with no innovation, and is increasing over time. These results are tabulated and discussed in the Internet Appendix. In the next section we present our baseline model.

B. Baseline Model: DiD Approach

In the previous section, we show that there is a negative relation between stock liquidity and firm innovation controlling for other factors that have been shown to affect innovation. In this section, we use the DiD approach to determine the effect of a change in stock liquidity on firm innovation. This methodology compares the innovation output of a sample of treatment firms whose stock liquidity increases the most to that of control firms whose stock liquidity increases the least but that are otherwise comparable, before and after policy changes that cause an exogenous shock to stock liquidity.

The DiD methodology has some key advantages. First, the DiD methodology rules out omitted trends that are correlated with stock liquidity and innovation in both the treatment and the control groups. As an example of an omitted trend, firms may rely on acquisitions to foster and grow innovation (Sevilir and Tian (2012)). Mergers tend to come in waves and may simultaneously increase innovation and lower stock liquidity. The DiD approach rules out the possibility that a shift in mergers is driving the change in innovation rather than a change in liquidity. Second, the DiD approach helps establish causality as tests are conducted surrounding policy changes that cause exogenous variation in the change in liquidity (the main independent variable). As an example of a reverse causality concern, high levels of R&D and innovation may make firms more opaque, which in turn could reduce stock liquidity. Lastly, as with the inclusion of firm fixed effects in the OLS specifications discussed in Section II.A, the DiD approach controls for constant unobserved differences between the treatment and the control groups. For example, management quality could be correlated with both stock liquidity and innovation and may drive the negative relation between them. Though the use of the DiD methodology is very powerful at ruling out alternative explanations, it does not entirely eliminate the possibility of an unobservable that affects the treatment and control groups differently and is correlated with the outcome variable (innovation). We address this concern in several ways in Sections II.B.1 through II.B.3.

B.1. The DiD Approach Exploiting Decimalization

We start by identifying a large exogenous shock to stock liquidity during our sample period. Prior to 2001, the minimum tick size for quotes and trades on the three major U.S. exchanges was \$1/16. Over the period of August 28, 2000 to January 29, 2001, NYSE and Amex reduced the minimum tick size to pennies and terminated the system of fractional pricing. NASDAQ decimalized shortly thereafter over the period of March 12, 2001 to April 9, 2001. Prior studies show significant increases in liquidity as a result of decimalization, especially among actively traded stocks (Bessembinder (2003), Furfine (2003)).

Decimalization appears to be a good candidate to generate exogenous variation in liquidity since it directly affects liquidity, it is unlikely to directly affect innovation, and changes in liquidity surrounding decimalization exhibit variation in the cross-section of stocks. Regarding the reverse causality concern, we do not expect changes in future innovation to affect the change in liquidity brought about by decimalization. Hence, examination of the change in innovation productivity following the change in liquidity due to decimalization provides a quasi-natural experiment for our tests.

We construct a treatment group and a control group of firms using propensity score matching. Specifically, we start by measuring the change in the annual relative effective spread ($\Delta RESPRD$) from the predecimalization year (year -1) to the postdecimalization year (year $+1$), where year zero indicates the fiscal year during which decimalization occurred for a firm. Based on $\Delta RESPRD_{-1\text{ to }+1}$, we then sort the 3,375 sample firms into terciles and retain only the top tercile representing the 1,125 firms experiencing the largest drop in relative effective spread surrounding decimalization and the bottom tercile representing the 1,125 firms experiencing the smallest drop in relative effective spread. Finally, we employ a propensity score matching algorithm to identify matches between firms in the top tercile and firms in the bottom tercile.

When applying propensity score matching, we first estimate a probit model based on the 2,250 sample firms in the top and bottom terciles. The dependent variable is equal to one if the firm-year belongs to the treatment tercile (top tercile) and zero otherwise. The probit model includes all control variables from equation (1), measured in the year immediately preceding decimalization; institutional ownership measured as the natural logarithm of firm i's institutional ownership measured in the year before decimalization (Thomson's CDA/Spectrum database (form 13F)); the Fama-French 12 industry dummies; as well as the predecimalization innovation growth variables (i.e., the growth in the number of patents *PAT_GROWTH* and the growth in the number of non-self-citations each patent receives *CITE_GROWTH*, both computed over the three-year period before decimalization). These variables are included to help satisfy the parallel trends assumption as the DiD estimator should not be driven by differences in any industry or firm characteristic.[11]

Table III, Panel A provides definitions of the new variables used in Table III. The probit model estimates are presented in column (1) of Table III, Panel B, with robust standard errors adjusted for heteroskedasticity. The results show that the specification captures a significant amount of variation in the choice variable, as indicated by a pseudo-R^2 of 15.7% and a p-value from the χ^2 test of

[11] As stated in Lemmon and Roberts (2010), the parallel trends assumption does not require the level of outcome variables (innovation variables in our setting) to be identical across the treatment and control firms or across the two regimes, because these distinctions are differenced out in the estimation. Instead, this assumption requires similar trends in the innovation variables during the pre-event regime for both the treatment and the control groups.

Table III

Difference-in-Differences (DiD) Analysis Using 2001 Shift to Decimalization

This table reports DiD tests examining how exogenous changes in stock liquidity due to decimalization affect firm innovation. Panel A provides variable definitions for new variables used in the DiD tests. Other variables are defined in Table I, Panel A. Firms are sorted into terciles based on their change in the annual relative effective spread from the predecimalization year to the postdecimalization year. The top (bottom) tercile is the treatment (control) group. We match firms using one-to-one nearest neighbor propensity score matching, without replacement. Panel B presents parameter estimates from the probit model used to estimate propensity scores for firms in the treatment and control groups. The dependent variable is one if the firm-year belongs to the treatment group and zero otherwise. Heteroskedasticity robust standard errors are displayed in parentheses. Industry fixed effects are included in both columns in Panel B. Panel C reports the distribution of estimated propensity scores for the treatment firms, control firms, and the difference in estimated propensity scores post matching. Panel D reports the univariate comparisons between the treatment and control firms' characteristics and their corresponding t-statistics. Panel E provides the DiD test results. *PAT (CITE)* is the sum of firm i's number of patents (number of citations per patent) in the three-year window before or after decimalization. Standard errors are given in parentheses below the mean differences in innovation outcomes. Panel F reports regression estimates of the innovation dynamics of treatment and control firms surrounding decimalization. The dependent variable is *PAT**, firm i's number of patents in a given year, or *CITE**, firm i's number of citations per patent in a given year. Bootstrapped standard errors are displayed in parentheses. Panel G shows DiD test results (falsification test) for variables that should be unaffected by decimalization. In all panels *** (**) (*) indicate significance at the 1% (5%) (10%) two-tailed level.

Panel A: New Variable Definitions

Variable	Definition
Measures of Innovation	
$PAT\_GROWTH_{-3\text{ to }-1}$	Change in the number of firm i's patents over the three-year period before the decimalization year defined as the number of patents in year 0 minus the number of patents in year −3.
$CITE\_GROWTH_{-3\text{ to }-1}$	Change in the number of firm i's number of non-self-citations each patent receives over the three-year period before the decimalization year defined as the number of non-self-citations in year 0 minus the number of non-self-citations patents in year

Table III—*Continued*

Panel A: New Variable Definitions

Table III—*Continued*

Panel E: Difference-in-Differences Test

	Mean Treatment Difference (after – before)	Mean Control Difference (after – before)	Mean DiD Estimator (treat – control)	t-statistic for DiD Estimator
PAT	−5.169	−1.682	−3.487**	−2.265
	(1.103)	(1.074)	(1.540)	
CITE	−11.14	−8.522	−2.616**	−1.976
	(0.986)	(0.884)	(1.324)	

Panel F: Difference-in-Difference Analysis for Innovation Dynamics

Dependent Variable	(1) *PAT**	(2) *CITE**
$TREAT \times BEFORE^{-1}$	−0.031	0.002
	(0.061)	(0.073)
$TREAT \times CURRENT$	−0.092	−0.050
	(0.078)	(0.072)
$TREAT \times AFTER^{1}$	−0.164*	−0.099
	(0.085)	(0.074)
$TREAT \times AFTER^{2\&3}$	−0.191*	−0.141*
	(0.098)	(0.079)
$BEFORE^{-1}$	−0.064*	−0.138***
	(0.039)	(0.047)
CURRENT	−0.054	−0.212***
	(0.049)	(0.047)
$AFTER^{1}$	−0.230***	−0.342***
	(0.055)	(0.046)
$AFTER^{2\&3}$	−0.478***	−0.514***
	(0.067)	(0.054)
TREAT	0.156	0.132
	(0.110)	(0.082)
INTERCEPT	0.640***	0.586***
	(0.077)	(0.056)
Number of obs. used	5,836	5,836
Adjusted R^2	0.035	0.073

Panel G: Difference-in-Differences Test for Capital Expenditure, Employees, and Acquisition

	Mean Treatment Difference (after – before)	Mean Control Difference (after – before)	Mean DiD Estimator (treat – control)	t-statistic for DiD Estimator
CAPEX	−0.023	−0.138	0.115	0.956
	(0.102)	(0.064)	(0.120)	
EMPLOYEES	−0.002	−0.000	−0.002	−0.284
	(0.004)	(0.003)	(0.005)	
PPE	−0.003	−0.060	0.057	0.211
	(0.139)	(0.229)	(0.269)	
SALEPPE	0.001	0.001	−0.001	−0.086
	(0.002)	(0.006)	(0.007)	
ACQUISITION	0.013	0.086	−0.073	−0.558
	(0.018)	(0.123)	(0.130)	
SALEINV	0.022	1.209	−1.187	−1.174
	(0.088)	(1.007)	(1.012)	

overall model fitness well below 0.001. We then use the predicted probabilities, or propensity scores, from column (1) to perform nearest-neighbor propensity score matching. In particular, each firm in the top tercile (labeled treatment firms) is matched to a firm from the bottom tercile with the closest propensity score (labeled control firms). If a firm from the bottom tercile is matched to more than one treatment firm, we retain the pair for which the distance between the two firms' propensity scores is the smallest. We end up with 508 unique pairs of matched firms.[12]

Since the validity of the DiD estimate critically depends on the parallel trends assumption, we conduct a number of diagnostic tests to verify that we do not violate the assumption. In the first test, we re-run the probit model restricted to the matched sample. The probit estimates are presented in column (2) of Table III, Panel B. None of the independent variables are statistically significant. In particular, the coefficient estimates on the preshock innovation growth variables are not statistically significant, suggesting there are no observable different trends in innovation outcomes between the two groups of firms predecimalization. Also, the coefficient estimates in column (2) are much smaller in magnitude than those in column (1), suggesting that the results in column (2) are not simply an artifact of a decline in degrees of freedom due to the drop in sample size.[13] In addition, the pesudo-R^2 drops drastically from 15.7% prior to the matching to 0.8% following the matching, and a χ^2 test for overall model fitness shows that we cannot reject the null hypothesis that all of the coefficient estimates on independent variables are zero (with a p-value of 0.985).

In our second diagnostic test, we examine the difference between the propensity scores of the treatment firms and those of the matched control firms. Panel C of Table III demonstrates that the difference is rather trivial. For example, the maximum distance between two matched firms' propensity scores is only 0.024, while the 95th percentile of the distance is only 0.001.

Finally, we report the univariate comparisons between the treatment and control firms' predecimalization characteristics and their corresponding t-statistics in Panel D of Table III. As shown, none of the observed differences between the treatment and control firms' characteristics is statistically significant in the predecimalization regime. In particular, the two groups of

[12] As a robustness test we examine what happens if we allow treatment firms matched to the same control firm to remain in the sample. Matching with replacement results in 1,125 matched pairs and 253 control firms matched to multiple treatment firms. The DiD estimates become even more statistically significant than those obtained if we keep the matched pair with the smallest distance between propensity scores. However, there are differences in firm characteristics in the predecimalization period across the treatment and control groups (i.e., violating the parallel trends assumption), making it less likely that a comparison of treatment and control groups provides an accurate estimate of the effect of stock liquidity on innovation. Our choice of matching procedure (without replacement) improves matching precision at the expense of a loss of sample observations. We choose to stay on the conservative side and sacrifice power in our DiD tests to ensure that we obtain precise matches with comparable firms.

[13] In addition, none of the industry dummies are statistically significant in column (2), whereas a majority of them are statistically significant in column (1). We do not report the coefficient estimates on industry dummies for brevity.

firms have similar levels of liquidity preshock, even though they are affected by decimalization differently. Moreover, the univariate comparisons for the innovation growth variables suggest that the parallel trends assumption is not violated. Overall, the diagnostic tests reported above suggest that the propensity score matching process removes meaningful observable differences (other than the difference in the change in liquidity surrounding decimalization). This increases the likelihood that the changes in innovation are caused only by the exogenous change in stock liquidity due to decimalization.

Table III, Panel E presents the DiD estimators. Column (1) reports the average change in the number of patents (denoted *PAT*) and the average change in the number of non-self-citations each patent receives (denoted *CITE*) for the treatment group. These measures are computed by first subtracting the total number of patents (citations) counted over the three-year period immediately preceding decimalization from the number of patents (citations) counted over the three-year period immediately following decimalization for each treatment firm. The differences are then averaged over the treatment group. Similarly, we calculate the average change in the number of patents and the number of citations for the control group and report them in column (2). In columns (3) and (4), we report the DiD estimators and the corresponding two-tailed t-statistics testing the null hypothesis that the DiD estimators are zero.

Two findings emerge. First, the innovation productivity of both the treatment and the control firms decreases after decimalization, which is consistent with our preliminary findings that liquidity is negatively related to firm innovation on average. Second, and more importantly, the decline in innovation productivity is larger for the treatment group than for the control group as the DiD estimators on *PAT* and *CITE* are both negative and statistically significant at the 5% level. The magnitude of the DiD estimators on *PAT* suggests that, on average, the exogenous shock to liquidity due to decimalization results in a decrease of about 3.5 more patents in the three-year period immediately following decimalization relative to the three-year period immediately preceding decimalization for the treatment firms than for the control firms (i.e., a drop of approximately 3.5/3 = 1.2 more patents per year, 18.5% of 6.5 patents, the sample average of the number patents granted per year). Similarly, treatment firms experience a decrease of about 2.6 more citations per patent than the control firms in the three-year period immediately following decimalization (relative to the three-year period immediately preceding decimalization). This corresponds to a drop of 2.6/3 = 0.9 more citations per patent per year, 26.4% of 3.4, the sample average of the number of non-self-citations each patent receives per year.[14]

[14] The relative effective spread for the treatment firms drops on average by 0.032 more than the relative effective spread for the control firms around decimalization. For a similar drop in the spread (2.46 times the median sample relative effective spread of 0.013), the OLS model in Section II.A estimates a 34.7% drop in one-year-ahead patents and a 25.6% drop in one-year-ahead non-self-citations per patent.

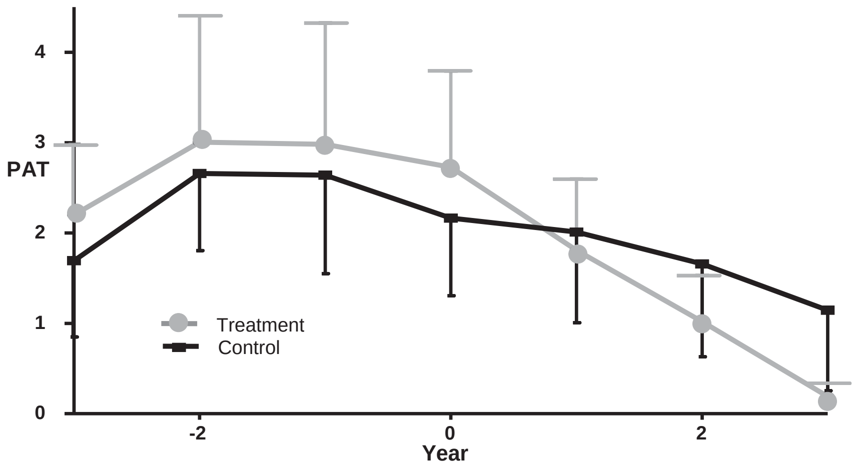
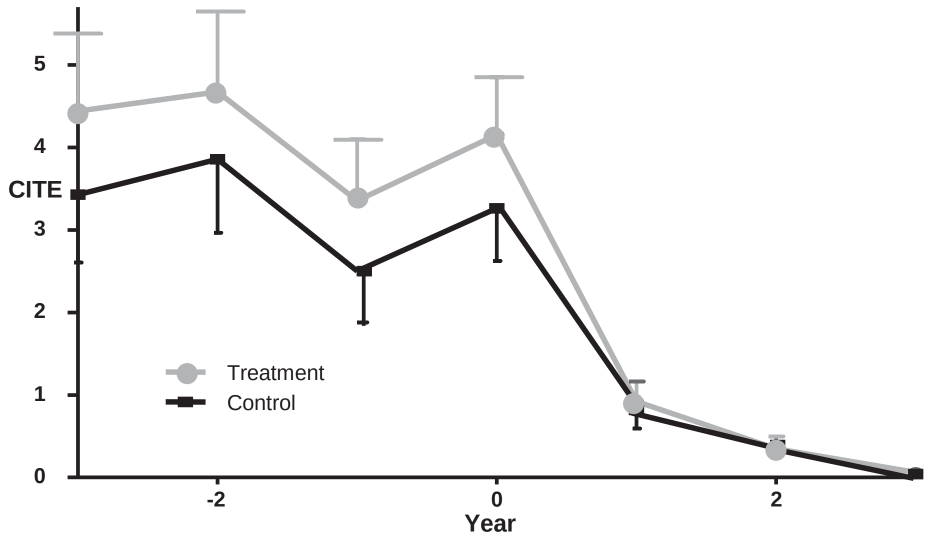


Figure 1. Number of patents surrounding decimalization. This figure shows the average innovation captured by the mean number of patents for treatment and control firms, from three years before decimalization to three years after decimalization. Decimalization year is denoted as year 0. The sample comprises 508 treatment firms and 508 unique control firms matched based on the procedures described in Table III. Two standard errors are represented by the vertical lines from each of the annual mean nodes.

These trends can be seen more clearly in Figures 1 and 2. Figure 1 depicts the number of patents for the treatment and control groups over a seven-year period centered on the decimalization year (denoted as year 0) and Figure 2 depicts the number of non-self-citations per patent for both groups of firms over the same period. The vertical lines from each node reflect two standard errors of the mean values. As shown, the two lines representing the number of patents (citations) for the treatment group and the control group trend closely in parallel in the three years leading up to decimalization. After decimalization, the two lines start to decline and converge, indicating a drop in innovation productivity for both groups and an even larger drop for the treatment group.

We also show the dynamics of Figures 1 and 2 as well as our main DiD results (reported in Table III, Panel E) in a regression framework. Specifically, in the spirit of Bertrand and Mullainathan (2003), we retain firm-year observations for both treatment and control firms for a seven-year window centered on the decimalization year and estimate

$$\begin{aligned} PAT^*(CITE^*) = a &+ bTREAT^*BEFORE^{-1} + cTREAT^*CURRENT \\ &+ dTREAT^*AFTER^1 + eTREAT^*AFTER^{2\,\&\,3} \\ &+ fBEFORE^{-1} + gCURRENT + hAFTER^1 \\ &+ iAFTER^{2\,\&\,3} + jTREAT + error. \end{aligned} \tag{2}$$


CITE
5
4
3
2
1
0
Treatment
Control
-2
0
2
Year

One concern regarding the use of decimalization as an exogenous shock to liquidity is that the burst of the dot-com bubble and the economic recession that followed the dot-com bubble are contemporaneous with decimalization. A possibility exists that the dot-com bubble and the subsequent recession differently affect the treatment and control groups and are correlated with innovation. We address this concern in several ways. First, we run a falsification test. We examine whether capital expenditures *CAPEX* (Compustat #128), the number of employees *EMPLOYEES* (#29), and property, plant, and equipment *PPE* (#8) change significantly for treatment firms (relative to control firms) surrounding decimalization. All three variables are divided by annual sales (#12). If the observed negative relation between stock liquidity and innovation in the treatment firms is driven by the dot-com bubble and the recession, we are likely to observe a drop in these variables similar to what we observe for innovation. We examine the changes in *CAPEX*, *EMPLOYEES*, and *PPE* in our DiD framework and report the results in Panel G of Table III. The DiD estimators demonstrate that the difference between treatment firms and control firms is not statistically significant in terms of the change in capital expenditures, number of employees, and PPE surrounding decimalization. Also in Panel G, we examine the change in other components of net investment: *SALEPPE*, *ACQUISITION*, and *SALEINV*, which denote a firm's sale of property, plant, and equipment (#107), cash acquisition expenditures (#129), and sale of other investments (#109), respectively, all divided by annual sales. Again, no significant difference is observed between the two groups of firms for changes in these variables. The results are similar if we divide all six variables by total assets instead of annual sales.

Second, we estimate equation (1) within each of the Fama-French 12 industry groups to check whether high-tech industries are driving the negative relation between liquidity and innovation.[15] We report the coefficient estimates on the illiquidity variable, *ILLIQ*, in Table IV. Standard errors are clustered by firm and are displayed in parentheses below the coefficient estimates. We observe positive coefficient estimates on *ILLIQ* in 11 industries, and six of them are statistically significant. The evidence suggests that the relation between liquidity and innovation is not purely driven by high-tech firms (such as Healthcare, Drugs, Computers, and Software), which are likely most affected by the dot-com bubble, but is also driven by old-economy low-tech firms (such as Household Appliances, Machinery, Oil, Gas, and Coal Extraction and Products, and Chemical and Allied Products). These results demonstrate that our findings do not appear to be explained away by the dot-com bubble or the economic recession that occurred following the burst of the bubble. In the next section, we repeat our DiD analysis using a different policy change that occurred several years before the decimalization shock and also resulted in an exogenous shock to liquidity.

[15] This test is conducted using our OLS specification on the entire sample as we do not have sufficient power to test this in the DiD framework with only 508 pairs of firms.

Table IV
Within-Industry Regressions

This table reports coefficients on the illiquidity variable, *ILLIQ*, from OLS regression estimates of the model $INNOV\_PAT_{i,t+1}$ ($INNOV\_CITE_{i,t+1}$) $= a + bILLIQ_{i,t} + c'CONTROLS_{i,t} + YR_t + FIRM_i + error_{i,t}$ within each of the Fama-French 12 industry groups. The *ILLIQ* coefficient estimates are shown, and their standard errors are clustered by firm and displayed in parentheses below. Variable definitions are provided in Table I, Panel A. *** (**) (*) indicate significance at the 1% (5%) (10%) two-tailed level.

FF	Industry Name	Description	$INNOV\_PAT_{t+1}$	$INNOV\_CITE_{t+1}$	No. of Obs.
1	Nodur	Consumer Nondurables (food, tobacco, textiles, apparel, leather, toys)	0.098 (0.071)	0.067 (0.055)	2,465
2	Durbl	Consumer durables (cars, TVs, furniture, household appliances)	0.336** (0.135)	0.305*** (0.116)	1,068
3	Manuf	Manufacturing (machinery, trucks, planes, office furniture, paper, commercial printing)	0.251*** (0.085)	0.215*** (0.050)	4,761
4	Enrgy	Oil, gas, and coal extraction and products	0.131* (0.067)	0.099** (0.044)	1,679
5	Chems	Chemicals and allied products	0.480** (0.221)	0.321*** (0.115)	966
6	BusEq	Business equipment (computers, software, and electronic equipment)	0.235*** (0.048)	0.186*** (0.038)	8,860
7	Telcm	Telephone and television transmission	0.083 (0.085)	0.079 (0.063)	1,423
8	Utils	Utilities	−0.004 (0.010)	−0.024 (0.043)	1,387
9	Shops	Wholesale, retail, and some services (laundries, repair shops)	0.030 (0.029)	0.025 (0.025)	4,117
10	Hlth	Healthcare, medical equipment, and drugs	0.328*** (0.064)	0.164*** (0.046)	4,613
11	Money	Finance	0.001 (0.024)	0.010 (0.024)	2,987
12	Other	Mines, construction, building materials, transportation, hotels, business services, entertainment	0.030 (0.036)	0.016 (0.031)	5,143

average effective spread due to the move from the 16ths regime to the decimal regime (decimalization). More specifically, Chordia, Roll, and Subrahmanyam (2008) report that the mean (median) effective spread is 0.1176 (0.1175) in the 8th regime, which decreases by 28.4% (28.3%) to 0.0838 (0.0842) in the 16th regime and then by 58.4% (60.5%) to 0.0349 (0.0333) in the decimal regime. Nevertheless, we use the 1997 shock as a separate exogenous shock to liquidity to further identify the causal effect of liquidity on innovation.

We repeat the propensity score matching and the DiD approach outlined above for the 1997 shock and match 338 treatment-control pairs without re-

Table V

Difference-in-Differences (DiD) Analysis Using 1997 Shift to 16ths

This table reports diagnostics and results of DiD tests on how exogenous changes in stock liquidity due to the shift in minimum tick size in 1997 from the 8ths regime to the 16ths regime affect firm innovation. The sample selection begins with all firms with nonmissing matching variables and nonmissing innovation outcome variables in the preshift year (year −1) and the postshift year (year +1), with year zero indicating the fiscal year during which the shift occurred for firm *i*. Based on the change in the annual relative effective spread from the preshift year to the postshift year, $\Delta RESPRD_{-1 \text{ to } +1}$, we sort firms into terciles and retain only the top tercile firms experiencing the largest drop in relative effective spread (treatment group) and the bottom tercile experiencing the smallest drop in relative effective spread (control group). We match firms by adopting one-to-one nearest neighbor propensity score matching following the procedure outlined for the decimalization test in Table III. Panel A reports univariate comparisons between the treatment and control firms' characteristics and their corresponding *t*-statistics. Panel B gives the DiD test results. *PAT* is the sum of firm *i*'s number of patents in the three-year window before or after the 1997 shock. *CITE* is the sum of firm *i*'s number of citations per patent in the three-year window before or after the 1997 shock. Variable definitions are provided in Table I, Panel A and Table III, Panel A. Standard errors are given in parentheses below the mean differences in innovation outcomes. *** (**) (*) indicates significance at the 1% (5%) (10%) two-tailed level.

Panel A: Differences in Pre-1997 Shift Characteristics

	Treatment	Control	Difference	*t*-statistic
$ILLIQ_{-1}$	−3.628	−3.618	−0.009	−0.207
LN_MV_{-1}	4.582	4.551	0.032	0.297
$RDTA_{-1}$	0.067	0.064	0.003	0.403
ROA_{-1}	0.063	0.068	−0.005	−0.367
$PPETA_{-1}$	0.271	0.252	0.019	1.113
LEV_{-1}	0.172	0.159	0.013	0.905
$CAPEXTA_{-1}$	0.069	0.070	−0.001	−0.104
$HINDEX_{-1}$	0.126	0.111	0.015	1.211
Q_{-1}	2.111	2.239	−0.128	−0.999
$KZINDEX_{-1}$	−7.960	−7.532	−0.428	−0.253
LN_AGE_{-1}	2.070	1.963	0.106	1.584
LN_INST_{-1}	0.257	0.248	0.008	0.542
$PAT\_GROWTH_{-3 \text{ to } -1}$	0.005	−0.021	0.026	0.601
$CITE\_GROWTH_{-3 \text{ to } -1}$	0.127	0.020	0.107	1.549

Panel B: Difference-in-Differences Test

Mean Treatment Difference (After – Before)	Mean Control Difference (After 6011Tm(0.127)-57187.970TBT3.227cm0100((Afo859Tc7.h.47467284.e3	Mean DiD Estimator	*t*-statistic for DiD

pilot, in which 52 firms (representing 57 issues) were priced in decimals starting September 25, 2000. On December 4, 2000, the pilot program was once again expanded to include an additional 94 securities in Phase 3. The rest of the nonpilot securities listed on the NYSE were converted to decimals in January 2001.

According to an August 16, 2000 NYSE news release,[18] "The NYSE chose the Phase 2 stocks based on several criteria that the Exchange developed with a securities-industry committee, of which the NYSE is a member. These criteria included choosing stocks that have different levels of daily trading activity. In addition, the Phase 2 stocks are located throughout the trading floor to give more traders experience with decimals, as compared with the Phase 1 stocks, which for ease of implementation are assigned to one workstation at the specialist firm of Spear, Leed, and Kellogg." The NYSE news release goes on to state, "Approximately 60 days after the end of Phase 2, the NYSE and the industry in consultation with the SEC will evaluate the pilot results, focusing on the impact on liquidity, trading patterns, and systems capacity. Following that, a decision will be made to extend decimal pricing to all NYSE-listed stocks."

Since Phase 1 stocks are chosen for ease of implementation and Phase 2 stocks are selected to have varying liquidity levels and physical trading locations, it appears unlikely that the order in which the stocks are selected to be phased in at the exchange is correlated with factors that drive firm innovation productivity. Hence, the variation in liquidity generated by the phase-in feature of decimalization is likely to be exogenous. The phase-in implementation of decimalization allows us to apply the DiD approach comparing pilot firms with nonpilot firms to further establish the causal effect of liquidity on innovation outcomes.

To apply the phase-in implementation of decimalization test, we focus on the sample of firms traded on the NYSE. First, we perform a thorough search of the news coverage on the phase-in implementation of decimalization to identify the tickers of the 158 pilot securities and the name of the listed companies to which these pilot securities belong. We then use the tickers to match the pilot securities to the Center for Research in Security Prices's (CRSP) PERMNO numbers. Since ticker is not a unique identifier in CRSP, we manually check the accuracy of the ticker-PERMNO matches using company names. For the pilot securities that we are unable to match using tickers, we hand collect their PERMNO numbers. Finally, we remove firms with dual stock listings. These procedures yield 140 unique firm-PERMNO matches.

After identifying the pilot firms, we use the DiD approach in a multivariate regression framework because the shifts to decimalization affect pilot and nonpilot firms at different times. In this framework, the NYSE pilot firms are the treatment firms and the nonpilot NYSE firms are the control firms. We restrict our sample period in this analysis to 1999 and 2000, so essentially each firm corresponds to two observations: one in 1999 (pretreatment period) and the other in 2000 (posttreatment period). The intuition behind this analysis is that, while the stocks of both groups of firms were traded in 16ths in 1999, only the stocks of the pilot firms went decimal in 2000 (recall the stocks of nonpilot

[18] Available at http://www1.nyse.com/pdfs/decimal081600.pdf.

Table VI

Difference-in-Differences Analysis Comparing Pilot and Nonpilot Stocks

This table reports pooled regression results of the model $INNOV\_PAT_{i,t+1}(INNOV\_CITE_{i,t+1}) = a + bPILOT_i \times YR\_2000 + cPILOT_i + dYR\_2000 + e'CONTROLS_{i,t} + IND_j + error_{i,}$. *PILOT* is a dummy variable equal to one if a firm's stock is in NYSE's decimalization pilot program and zero otherwise. *YR_2000* is a dummy variable equal to one for 2000 and zero for 1999. *PILOT× YR_2000* is the interaction term between these two variables. The sample includes 122 NYSE decimalization pilot firms (Phase 1, 2, and 3) as treatment firms and 2,038 NYSE nonpilot firms as control firms. Definitions of all other variables are listed in Table I, Panel A and Table III, Panel A. Fama-French 12 industry fixed effects are included in all regressions. Coefficient estimates are shown, and their standard errors are clustered by firm and displayed in parentheses below. *** (**) (*) indicates significance at the 1% (5%) (10%) two-tailed level.

Dependent Variable	(1) $INNOV\_PAT_{t+1}$	(2) $INNOV\_CITE_{t+1}$
$PILOT_i \times YR\_2000$	−0.485**	−0.309*
	(0.213)	(0.164)
$PILOT_i$	0.289	0.313*
	(0.243)	(0.166)
YR_2000	−0.014	0.091
	(0.165)	(0.097)
Control variables	Included	Included
Industry fixed effects	Included	Included
Number of obs. used	2,160	2,160
Adjusted R^2	0.550	0.481

firms were still traded in 16ths in 2000). Therefore, if there is a causal effect from stock liquidity to innovation, we might observe a drop in innovation productivity for pilot firms in 2000 (which is reflected in their innovation output in 2001), but such a drop, if any, should be significantly smaller for nonpilot firms.

We construct three new variables for the DiD analysis: *PILOT,* a dummy variable that equals one if a firm's security is in the decimalization pilot program (i.e., pilot firms), and zero otherwise (i.e., nonpilot firms); *YR_2000,* a dummy variable that equals one for year 2000 and zero for year 1999; *PILOT×YR_2000,* the interaction term between these two variables. We then estimate

$$INNOV\_PAT_{i,t+1}(INNOV\_CITE_{i,t+1}) = a + bPILOT_i \times YR\_2000 + cPILOT_i + dYR\_2000 + e'CONTROLS_{i,t} + IND_j + error_{i,t}, \quad (3)$$

where i indexes firm, t indexes year (1999 or 2000), and j indexes industry. We control for the Fama-French 12 industry fixed effects.

Table VI reports the regression results estimating equation (3). These regressions include 122 pilot firms and 2,038 nonpilot firms with patenting and control variables available. The dependent variable is *INNOV_PAT* in column (1). Since the nonpilot firms' stocks went decimal in 2001 (only one year after pilot firms' stocks), we focus on one-year-ahead innovation outcomes to avoid comparing these two groups of firms when they are both trading in the decimal regime. Doing so helps to separate out the effect of decimalization on pilot firms compared to nonpilot firms. The coefficient estimate on *PILOT* is positive but not statistically significant, suggesting that there is no significant difference between the number of patents filed by pilot firms and those by nonpilot firms before the pilot firms' stocks switched to decimal pricing. The coefficient estimate on *YR_2000* is negative but not statistically significant, which suggests that the nonpilot firms do not experience a significant change in the number of patents filed across these two years. Most importantly, the coefficient estimate on *PILOT*$\times$*YR_2000*, the variable of interest in the DiD approach, is negative and significant at the 5% level. The coefficient suggests that the pilot firms experience an average 48.5% larger drop in one-year-ahead number of patents filed after their conversion to decimal pricing than nonpilot firms. We do not report the coefficient estimates of the control variables for brevity.

In column (2), we replace the dependent variable with one-year-ahead *INNOV_CITE*. The coefficient estimate on *PILOT*$\times$*YR_2000* is negative and significant at the 10% level, suggesting that, compared to nonpilot firms, pilot firms generate patents with 30.9% lower impact in the first year after they went decimal. In this specification, the coefficient estimate on *PILOT* is positive and statistically significant, suggesting there is a difference between the number of patent citations for pilot firms and nonpilot firms before the pilot firms' stocks switched to decimal pricing.

Given the small number of pilot firms, there is a concern that outliers may be driving the results. Since the seven stocks included in Phase 1 are chosen for ease of implementation and monitoring, there is no particular reason to suspect outliers in this phase. Phase 2 might introduce outliers as it includes some of the most actively traded stocks at the time, such as Compaq Computer Corporation. Phase 3 is chosen similarly to Phase 2.

We run several tests to make sure that outliers are not driving our results. These results are tabulated in the Internet Appendix. Since decimalization coincides with the burst of the dot-com bubble, we drop pilot firms that are classified in the Business Equipment industry (Computers, Software, and Electronic Equipment) based on the Fama-French 12-industry classifications, as these firms are most prone to the dot-com bubble and thus are likely to be outliers. The eight pilot firms we drop are Iomega Corp., LSI Logic Corp., Thermo Electron Corp., Compaq Computer Corp., Solectron Corp., Gateway Inc., Factset Research Systems Inc., and Midway Games Incorporated. We continue to observe negative DiD estimates that are statistically and economically significant. Pilot firms experience an average 40.7% larger drop (significant at the 10% level) in one-year-ahead patent counts after their conversion to decimal pricing than nonpilot firms and generate patents with 28.8% lower impact (significant at the 10% level) in the first year after they convert to decimals.

Second, we re-run the DiD tests dropping seven pilot firms that fall in the bottom 10% of relative effective spreads (i.e., pilot firms with the most liquid stocks) one year before decimalization using the final sample for the regressions in Table VI of the paper. These firms are Compaq Computer Corp., American Home Products Corp., Kimberly Clark Corp., Colgate-Palmolive Co., Cigna Corp., Gateway Inc., and Daimler Chrysler AG. Bessembinder (2003) shows that firms that are heavily traded before decimalization also experience the greatest change in liquidity during decimalization. To the extent that the predecimalization level of stock liquidity is not fully controlled for in our DiD test comparing pilot and nonpilot stocks, firms having higher liquidity predecimalization may be driving our results. The results dropping these firms show that pilot firms experience an average 40.8% larger drop (significant at the 10% level) in one-year-ahead number of patents filed after their conversion to decimal pricing than nonpilot firms and generate patents with a lower impact (t-statistic of the DiD estimate is 1.55) in the first year after they went decimal.

Third, we rely on Cook's distance (i.e., Cook's D) to detect potential outliers that drive our results. Specifically, we identify 11 pilot firms with a Cook's D greater than the cutoff of 4/n (i.e., 4/2160).[19] Among the 11 firms, we conjecture that the three auto manufacturing firms (i.e., Daimler Chrysler AG, General Motors Corp., and Toyota Motor Corp) could be potential outliers as they may happen to cut back on innovation in the subsequent economic recession. We re-run the DiD tests dropping these three firms. We show that pilot firms experience an average 33.0% larger drop (significant at the 10% level) in one-year-ahead number of patents filed after their conversion to decimal pricing than nonpilot firms and generate patents with a lower impact (t-statistic of the DiD estimate is 1.50) in the first year after they went decimal.

In our final robustness check, we re-run the DiD tests dropping pilot firms in Phase 3. We find even stronger results. Pilot firms in Phase 1 and Phase 2 experience an average 70.2% larger drop (significant at the 10% level) in one-year-ahead number of patents filed after their conversion to decimal pricing than nonpilot firms and generated patents with a 52.6% lower impact (significant at the 5% level) in the first year after they convert to decimals.

Despite these robustness checks, given the small sample of pilot firms and the short time interval over which the pilot program was completed, our results in Section II.B.3 could potentially be driven by outliers that we fail to detect.

In summary, in Section II of the paper, we use the DiD approach and exploit the exogenous variation in liquidity caused by decimalization of minimum tick size surrounding 2001, the shift in minimum tick size from 8ths to 16ths in 1997, and the phase-in feature of decimalization. We show that firms that experience a larger increase in liquidity surrounding decimalization or the 1997 shock produce significantly fewer patents and patents with lower impact in the following years. We also find that pilot firms that switch to decimal pricing

[19] These firms are Colgate-Palmolive Co., Daimler Chrysler AG, General Motors Corp. (New Class H), Lockheed Martin Corp., Lone Star Technologies Inc., Toyota Motor Corp., Ashland Inc., Biovail Corp., Kimberly Clark Corp., Midway Games Inc., and Thermo Electron Corp.

earlier experience a larger drop in innovation output in the first year after their conversion than firms not in the pilot program. Overall, our identification tests suggest a negative causal effect from stock liquidity to firm innovation.

III. Possible Mechanisms

In this section, we run several tests to examine if the hypothesized mechanisms through which liquidity may impede innovation change surrounding an exogenous shock to stock liquidity (decimalization). It is of course challenging to provide definitive proof of underlying mechanism(s) through which liquidity reduces innovation, so our tests are only suggestive.

A. Takeover Exposures

Stein (1988) argues that shareholders cannot properly evaluate a manager's investment in long-term intangible assets. In the presence of information asymmetry, they tend to undervalue the stocks of companies investing in innovative projects. This leads to a higher probability of the firm facing a hostile takeover. To protect current shareholders against expropriation, managers tend to put more effort in short-term projects that offer quicker returns instead of investing in long-term innovative projects. Shleifer and Summers (1988) suggest that managers have less power over shareholders when takeover threats are high, which leads to fewer incentives granted to managers to invest in activities with only long-run payoffs. Furthermore, Kyle and Vila (1991) argue that high liquidity increases a firm's exposure to takeovers. Therefore, takeover exposures could be an underlying economic mechanism through which stock liquidity impedes firm innovation.

We obtain attempted and completed mergers and acquisitions (M&As) deals from the SDC database. We distinguish between friendly and hostile deals based on the information provided by SDC. Following Cremers, Nair, and John (2009), we estimate a firm's takeover exposure by running a logit regression.[20] Next, we examine the effect of stock liquidity on a firm's takeover exposure in the DiD framework using the matched sample constructed in Section II.B.1. In Table VII, we report the DiD estimators. We calculate the change in takeover exposure by subtracting its average calculated over the three years before decimalization from its average over the three years after decimalization and scaling the change by a firm's average takeover exposure over the three years before decimalization. The DiD estimator on *Hostile Takeovers* shows that the hostile takeover exposure of treatment group firms increased 17.7 percentage points (significant at the 1% level) more than the hostile takeover exposure of

[20] We use the probability of takeovers rather than actual takeovers. According to the theories of Stein (1988) and Shleifer and Summers (1988), on which our tests are based, it is the ex ante threat (or likelihood) of takeovers instead of the actual incidence of takeovers that alters managers' incentives to invest in innovation. Furthermore, actual takeovers would be a lot noisier than the predicted takeover likelihood.

Table VII

Possible Mechanisms: Takeover Exposure

This table reports DiD tests on how exogenous changes in stock liquidity due to decimalization affect firms' takeover exposures. *Hostile Takeover* is the average probability of being a target in a hostile takeover in the three-year window before or after decimalization, with the probability being the predicted value of *TARGET* based on the coefficients estimated in the logit regression $TARGET_{i,t+1} = a + bQ_{i,t} + cPPETA_{i,t} + dLN\_CASH_{i,t} + eBLOCK_{i,t} + fLN\_MV_{i,t} + gINDMA\_DUM_{i,t} + hLEV_{i,t} + iROA_{i,t} + YR_t + IND_j + error_{i,t}$, where *TARGET* is a dummy variable equal to one if the company is a target of an attempted or completed hostile acquisition and zero otherwise. *All Takeover* is the average probability of being a target in any takeover in the three-year window before or after decimalization, with the probability being the predicted value of *TARGET* based on the coefficients estimated in the logit regression $TARGET_{i,t+1} = a + bQ_{i,t} + cPPETA_{i,t} + dLN\_CASH_{i,t} + eBLOCK_{i,t} + fLN\_MV_{i,t} + gINDMA\_DUM_{i,t} + hLEV_{i,t} + iROA_{i,t} + YR_t + IND_j + error_{i,t}$, where *TARGET* is a dummy variable equal to one if the company is the target of an attempted or completed acquisition regardless of takeover attitudes and zero otherwise. Standard errors are given in parentheses below the mean differences in takeover exposures. *** (**) (*) Significance at the 1% (5%) (10%) two-tailed level.

	Mean Treatment Difference (After – Before)	Mean Control Difference (After – Before)	Mean DiD Estimator (Treat – Control)	*t*-statistic for DiD Estimator
Hostile takeovers	0.212	0.035	0.177***	5.036
	(0.021)	(0.024)	(0.032)	
All takeovers	0.040	0.019	0.022*	1.828
	(0.008)	(0.009)	(0.012)	

control group firms after decimalization. We find a smaller and less significant DiD estimator on *All Takeovers*. Our evidence suggests that a vibrant hostile takeover market may be an underlying mechanism through which stock liquidity impedes firm innovation.[21]

B. Nondedicated Institutional Investors

Next, we examine whether the presence of nondedicated institutional investors could cause a larger drop in innovation activities surrounding decimalization in the treatment group. Porter (1992) argues that investment in long-term intangible assets tends to depress short-term earnings due to the

[21] Information asymmetry theories predict that takeover exposure reduces managerial incentives to invest in innovation as it may lead to temporary undervaluation (e.g., Shleifer and Summers (1988), Stein (1988), while moral hazard theories argue that takeover exposure disciplines managers and promotes innovation (e.g., Jensen and Ruback (1983), Jensen (1988)). Our findings in this section differ from the findings in Atanassov (2013), who finds that antitakeover legislation results in less innovation. We conjecture that the difference may arise from differing empirical settings and conditions under which we examine these theories. Our sample period is later than the one used in Atanassov (2013). With increased monitoring by institutional investors (Yermack (2010)) and more intense product market competition in recent years (e.g., Bils and Klenow (2004)), the positive effect of takeover exposure on innovation in earlier decades documented by Atanassov (2013) may now be dominated by a negative effect of takeover exposure on innovation caused by information asymmetry.

Table VIII

Possible Mechanisms: Nondedicated Institutional Investors

This table reports DiD tests on how exogenous changes in stock liquidity due to decimalization affect ownership of different types of institutional investors. Based on the Bushee (1998, 2001) classification, *TRAPCT* is the average institutional holdings (%) held by transient institutional investors in the three-year window before or after decimalization; *QUAPCT* is the average institutional holdings (%) held by quasi-indexers in the three-year window before or after decimalization; and *DEDPCT* is the average institutional holdings (%) held by dedicated institutional investors in the three-year window before or after decimalization. Standard errors are given in parentheses below the mean differences in institutional holdings. *** (**) (*) Significance at the 1% (5%) (10%) two-tailed level.

	Mean Treatment Difference (After – Before)	Mean Control Difference (After – Before)	Mean DiD Estimator (Treat – Control)	t-statistic for DiD Estimator
TRAPCT	0.040	−0.012	0.052***	11.42
	(0.004)	(0.002)	(0.005)	
QUAPCT	0.064	0.009	0.055***	9.127
	(0.005)	(0.004)	(0.006)	
DEDPCT	0.013	0.007	0.005	1.586
	(0.002)	(0.003)	(0.003)	

significant initial outlays (R&D expenditures are often expensed under U.S. GAAP). He stresses that a significant proportion of American institutional investors are transient shareholders who chase short-term price appreciation and may exit in response to a low quarterly earnings report or quasi-indexers who use passive indexing strategies and have little or no incentives to monitor. If managers have incentives to keep the stock price high, they may cut investment in long-term projects to boost short-term profits. This effect should be more pronounced when liquidity is high because high liquidity makes it easier for dissatisfied institutional investors to exit (Bhide (1993)). Bushee (1998) highlights the possibility of cutting R&D expenditures as a way to reverse an earnings decline, especially when transient institutional ownership is high. Matsumoto (2002) demonstrates that firms with higher transient institutional ownership (among other characteristics) are more likely to meet or exceed analyst forecasts at quarterly earnings announcements and may manage earnings upward to meet earnings targets. Thus, pressure exerted by nondedicated institutional investors could be a channel through which liquidity impedes firm innovation.

We identify the change in institutional ownership, from its average over the three years before decimalization to its average over the three years after decimalization, for the treatment and control groups. We follow the institutional investor classification scheme created by Bushee (1998, 2001) and report the changes for the institutional holdings owned by transient investors (*TRAPCT*), quasi-indexers (*QUAPCT*), and dedicated investors (*DEDPCT)*, respectively. The results in Table VIII show that transient institutional ownership increases by 4% in the treatment group but decreases by 1.2% in the control group surrounding decimalization, leading to a DiD estimator of 5.2%, significant on the decimalization year or 0.4% out of 7,112 firm-year observations), which makes drawing meaningful statistical inferences difficult.[22] These results are tabulated in the Internet Appendix.

D. Mechanisms and Explanatory Power

In Sections III.A and III.B, we find evidence that a change in liquidity may affect innovation by causing a change in the probability of a hostile takeover or a change in the fraction of nondedicated institutional ownership. In this

Table IX

Residual Effect of Liquidity on Innovation after Controlling for Hypothesized Mechanisms

This table regresses DiD estimators of patents (citations) derived from the exogenous changes in stock liquidity surrounding the 2001 shift to decimalization from Table III, Panel E on DiD estimators of the hostile takeover probability from Table VII and DiD estimators of the three types of institutional ownership from Table VIII. *PAT* (*CITE*) is the sum of firm *i*'s number of patents (citations) in the three-year window before or after decimalization. *TAKEOVER_H* (*Hostile Takeover*) is the probability of being a target in a hostile takeover in the three-year window before or after decimalization, with the probability being the predicted value of *TARGET* based on the coefficients estimated in the logit regression $TARGET_{i,t+1} = a + bQ_{i,t} + cPPETA_{i,t} + dLN\_CASH_{i,t} + eBLOCK_{i,t} + fLN\_MV_{i,t} + gINDMA\_DUM_{i,t} + hLEV_{i,t} + iROA_{i,t} + YR_t + IND_j + error_{i,t}$, where *TARGET* is a dummy variable equal to one if the company is a target of an attempted or completed hostile acquisition and zero otherwise. Using the Bushee (1998, 2001) classification, *TRAPCT* is the average institutional holdings (%) held by transient institutional investors, *QUAPCT* is the average institutional holdings (%) held by quasi-indexers, and *DEDPCT* is the average institutional holdings (%) held by dedicated institutional investors in the three-year window before or after decimalization. The *DiD* function calculates the difference between the change in the treatment firm's variable of interest from three years before the liquidity event (decimalization) to three years after the liquidity event (decimalization) and the corresponding change in the matched control firm's variable of interest. Coefficient estimates are divided by 100 for ease of presentation, except for the intercept. Bootstrapped standard errors are given in parentheses below. *** (**) (*) Significance at the 1% (5%) (10%) two-tailed level.

Dependent Variables	(1) *DID_PAT*	(2) *DID_CITE*
DID_TAKEOVER_H	−0.067*	−0.055*
	(0.035)	(0.030)
DID_TRAPCT	−0.520**	−0.271**
	(0.258)	(0.134)
DID_QIXPCT	−0.102	−0.269***
	(0.118)	(0.101)
DID_DEDPCT	−0.055	−0.141
	(0.184)	(0.156)
INTERCEPT	−1.533*	−1.888*
	(0.815)	(1.141)
Number of Obs. Used	508	508
R^2	0.119	0.180

becomes smaller in magnitude (i.e., −1.888, representing a 28% drop from the benchmark DiD estimator of −2.616 reported in Table III, Panel E).

In summary, the two hypothesized economic mechanisms are able to explain a significant proportion of the causal effect of stock liquidity on innovation. However, we still observe a sizeable causal effect of stock liquidity on firm innovation even after controlling for the two economic mechanisms identified in the previous sections.

IV. Conclusion

This study investigates the effect of stock liquidity on corporate innovation. We first document a strong negative relation between stock liquidity and firm innovation. Using a DiD approach and exploiting the variation in stock liquidity generated by three exogenous shocks (the decimalization of the minimum tick size in 2001, the shift in minimum tick size from 8ths to 16ths in 1997, the phase-in of NYSE decimal-pricing pilot stocks in 2000), we show that stock liquidity has a causal negative effect on firm innovation. We then examine two possible mechanisms that could contribute to this finding. First, high stock liquidity makes firms more prone to hostile takeovers and this takeover threat may pressure managers to cut long-term intangible investment such as innovation. Second, high liquidity attracts transient investors who trade frequently to chase current profits or quasi-indexers who follow passive indexing strategies and fail to govern. As a result, managers may be pressured to cut investment in innovation to boost short-term earnings.

We acknowledge that, although we follow prior literature and capture innovation using patents and citations, our results may not extend to other types of nonpatentable innovation, such as process innovation. Thus, it may be the case that illiquidity pressures managers to shift toward rapidly visible forms of innovation (that generate patents within three years) and away from invisible forms of innovation (that are nonpatentable) or long-term innovations that take more than three years to generate patents. We highlight this issue as an interesting and important area for future research, particularly if measures of nonpatentable innovation and longer-term investment outputs become available.

With the rapid increase in the liquidity of U.S. stock markets, it should be a concern to academics, government regulators, firm managers, investors, and all Americans that stock liquidity may lead to underinvestment in long-term investments in innovation as, ultimately, this could affect the health and growth of the U.S. economy. Our results suggest that this is a valid concern as it appears that the promotion of stock liquidity can come with a cost to corporate innovation. More research in this area is therefore warranted.

Initial submission: February 3, 2011; Final version received: March 10, 2014
Editor: Campbell Harvey

REFERENCES

Admati, Anat, and Paul Pfleiderer, 2009, The Wall Street walk and shareholder activism: Exit as a form of voice, *Review of Financial Studies* 22, 2645–2685.

Aghion, Philippe, Nicholas Bloom, Richard Blundell, Rachel Griffith, and Peter Howitt, 2005, Competition and innovation: An inverted U relationship, *Quarterly Journal of Economics* 120, 701–728.

Aghion, Philippe, John Van Reenen, and Luigi Zingales, 2013, Innovation and institutional ownership, *American Economic Review* 103, 277–304.

Asker, John, Joan Farre-Mensa, and Alexander Ljungqvist, 2011, Comparing the investment behavior of public and private firms, Working paper, New York University.

Atanassov, Julian, 2013, Do hostile takeovers stifle innovation? Evidence from antitakeover legislation and corporate patenting, *Journal of Finance* 68, 1097–1131.

Atanassov, Julian, Vikram Nanda, and Amit Seru, 2007, Finance and innovation: The case of publicly traded firms, Working paper, University of Oregon.

Bertrand, Marianne, and Sendhil Mullainathan, 2003, Enjoying the quiet life? Corporate governance and managerial preference, *Journal of Political Economy* 111, 1043–1075.

Bessembinder, Hendrik, 2003, Trade execution costs and market quality after decimalization, *Journal of Financial and Quantitative Analysis* 38, 747–777.

Bharath, Sreedhar, Sudarshan Jayaraman, and Venky Nagar, 2013, Exit as governance: An empirical analysis, *Journal of Finance* 68, 2515–2547.

Bhide, Amar, 1993, The hidden costs of stock market liquidity, *Journal of Financial Economics* 34, 31–51.

Bils, Mark, and Peter Klenow, 2004, Some evidence on the importance of sticky prices, *Journal of Political Economy* 112, 947–985.

Bushee, Brian, 1998, The influence of institutional investors on myopic R&D investment behavior, *The Accounting Review* 73, 305–333.

Bushee, Brian, 2001, Do institutional investors prefer near-term earnings over long-run value? *Contemporary Accounting Research* 18, 207–246.

Chemmanur, Thomas, and Xuan Tian, 2013. Anti-takeover provisions, innovation, and firm value: A regression discontinuity analysis, Working paper, Boston College.

Chordia, Tarun, Richard Roll,C387.97013nieedha, 2001, M2(arket)5381.5(liquidi1,)-2561s.7nd(Ct(a)0.radrkin.)TJE

Journaf

Jensen, Michael, and Richard Ruback, 1983, The market for corporate control: The scientific evidence, *Journal of Financial Economics* 11, 5–50.

Kaplan, Steven, and Luigi Zingales, 1997, Do investment-cash flow sensitivities provide useful measures of financing constraints? *Quarterly Journal of Economics* 112, 169–215.

Kyle, Albert, and Jean-Luc Vila, 1991, Noise trading and takeovers, *RAND Journal of Economics* 22, 54–71.

Lee, Charles, and Mark Ready, 1991, Inferring trade direction from intraday data, *Journal of Finance* 46, 733–746.

Lemmon, Michael, and Michael Roberts, 2010, The response of corporate financing and investment to changes in the supply of credit, *Journal of Financial and Quantitative Analysis* 45, 555–587.

Lerner, Josh, Morten Sorensen, and Per Stromberg, 2011, Private equity and long-run investment: The case of innovation, *Journal of Finance* 66, 445–477.

Matsumoto, Dawn, 2002, Management's incentives to avoid negative earnings surprises, *The Accounting Review* 77, 483–514.

Maug, Emst, 1998, Large shareholders as monitors: Is there a trade-off between liquidity and control? *Journal of Finance* 53, 65–98.

Norli, Oyvind, Charlotte Ostergaard, and Ibolya Schindele, 2010, Liquidity and shareholder activism, Working paper, Norwegian School of Management.

Petersen, Mitchell, 2009, Estimating standard errors in finance panel data sets: Comparing approaches, *Review of Financial Studies* 22, 435–480.

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

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

Sevilir, Merih, and Xuan Tian, 2012, Acquiring innovation, Working paper, Indiana University.

Shleifer, Andrei, and Lawrence Summers, 1988, Breach of trust in hostile takeovers, in A. J. Auerbach, ed.: *Corporate Takeovers: Causes and Consequences* (University of Chicago Press, Chicago, Illinois).

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

Stein, Jeremy, 2003, Agency, information and corporate investment, in George Constantinides, Milton Harris, and René Stulz, eds.: *Handbook of the Economics of Finance* (North Holland, Amsterdam).

Tian, Xuan, and Tracy Wang, 2014, Tolerance for failure and corporate innovation, *Review of Financial Studies* 27, 211–255.

Yermack, David, 2010, Shareholder voting and corporate governance, *Annual Review of Financial Economics* 2, 103–125.

Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.