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Patent pledgeability, trade secrecy, and corporate patenting☆

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ABSTRACT

We identify a positive effect of patent pledgeability on corporate patenting. Our tests exploit staggered city-level policy changes that allow firms to use patents as collateral for financing. We find a significant increase in patents and patent citations for firms headquartered in cities that have adopted such policies relative to firms headquartered in cities that have not. We further show that patent pledgeability increases corporate patenting by inducing firms to shift from secrecy-based innovation to patent-based innovation, rather than by mitigating financial constraints.

1. Introduction

The practice of using patents as collateral for financing is an emerging financial innovation aimed at facilitating innovative firms to raise debt, and many countries are considering regulatory changes to enhance patent pledgeability (Amable et al., 2010; Maskus et al., 2019). However, little empirical evidence exists on whether (or through which channels) enhanced patent pledgeability affects innovation. This question is important for policymakers, the growth of innovative firms, and the long-term competitiveness of the economy. This study sheds light on this issue and identifies the positive effects of patent pledgeability on firms' patenting activities using a quasi-natural experiment in China.

Our analysis is based on the staggered city-level policy change, which allows firms to use patents as collateral for financing. We exploit these policy changes to capture the increase in patent pledgeability, and examine the subsequent changes in corporate patenting activities. The staggered policy changes in various cities provide a group of counterfactuals for how corporate patenting would have been without such changes, and allow us to quantify their effects using a difference-in-differences (DiD) approach. As pointed out by Roberts and Whited (2013), considering that multiple policy changes occur at different times and affect different firms, our setting

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can mitigate the common difficulty faced by studies with a single shock: The shock coincides with possible biases and noise that directly affect corporate patenting. Moreover, due to staggered policy changes, firms can be in both the treatment and control groups at different times, mitigating concerns about large differences between the two groups.

We expect patent pledgeability to increase corporate patenting through the following channels. First, considering that a firm can innovate in the form of either trade secrecy or patenting, an increase in patent pledgeability for external financing could induce firms to shift from secrecy-based to patent-based innovation. Second, an increase in patent pledgeability mitigates innovative firms' financial constraints and helps them increase external financing to support innovation activities.

Using a panel of 20,613 public firm-year observations from 2006 to 2017 and the DiD approach, we find that an exogenous increase in patent pledgeability leads to a significant increase in patent output. On average, firms headquartered in treated cities experienced a 17% increase in the number of patents and a 15% increase in patent citations relative to firms elsewhere.

The assumption behind the DiD identification is that the treated and control groups share parallel trends before policy changes. We show that their trends prior to the treatments are indeed similar, and that the majority of the policy's impact on patenting occurs after its enactment, which suggests a causal interpretation.

It is possible that cities' patent pledgeability policies are triggered by local economic shocks, which in turn affect firms' patenting activities. To investigate this possibility, we exploit the fact that, while city-level policies stop at city borders, economic conditions are likely to be shared by neighboring cities. By comparing treated firms with their neighboring firms in the control group, we can better identify whether the observed treatment effect is driven by the city's patent pledgeability policy rather than by any local confounding economic shocks. After treatment, we still find a significant increase in firms' patenting relative to their neighboring control firms. These results imply that confounding local economic shocks are unlikely to explain our main findings. We further show that our inference remains largely unchanged when we perform DiD analysis using a propensity score-matched sample. This helps mitigate the concern that our main results are driven by differences in firm characteristics between the treated and control groups.

In terms of the channels underlying our findings, we present evidence that our treatment effect is through the channel of inducing firms to shift from conducting innovation via secrecy to doing so via patenting. First, we analyze the frequency of the keywords "trade secrecy/secrecy" used in firms' annual reports to capture the extent to which a firm conducts secrecy-based innovation (relative to patent-based innovation). We subsequently show that patent pledgeability policies lead to a significant reduction in the usage of these keywords. Moreover, firms are more likely to conduct innovation in the form of trade secrecy rather than patents when they produce complex technologies (i.e., it is more difficult for competitors to reverse-engineer) and when they face lower labor mobility (i.e., a lower likelihood of trade secret leakage via employee departure).[1] Considering that these firms tend to have more pre-existing trade secrets, we expect and show that the treatment effect is stronger for these firms. We further find that our treatment effect is driven mainly by newly granted patents in firms' existing technology domains rather than those in new technology domains, suggesting that firms are converting their pre-existing secrecy into patents. Considering that business secrets have long-term strategic value for firms (Hannah, 2005; Hall et al., 2014), we expect and subsequently demonstrate that new patents have greater scientific and economic value. Overall, these results support our proposition that patent pledgeability increases a firm's patenting output by inducing firms to shift from secrecy-based to patent-based innovation.

We further examine whether mitigating the financial constraints faced by innovative firms is another channel. If this channel holds true, we expect our treatment effect to be stronger for firms facing greater financial constraints. However, contrary to this prediction, we show that our treatment effect is stronger for firms facing fewer financial constraints (e.g., larger firms, more profitable firms, and firms with more tangible assets). We also show that following policy changes, treated firms do not increase their R&D-related expenditure, but increase their financial investment in the securities market. These results seem to suggest that only firms with sufficient assets can use their patents as collateral, and that these firms do not direct patent-based loans into R&D activities. Overall, mitigating innovative firms' financial constraints is unlikely to be a channel for our results.[2]

Our study contributes to the literature on the role of collateral in corporate decision-making (e.g., Gan, 2007; Li et al., 2016; Schmalz et al., 2017). For example, Mao (2021) demonstrated that the appreciation of corporate land collateral value helps firms raise debt, thus facilitating innovation. Most existing studies have focused on the role of tangible assets (such as real estate), whereas the role of intangible assets (such as patents) has been underexamined. This lack of evidence prevents us from fully understanding the role of collateral in debt financing for innovative firms, given that intangible assets are their most crucial assets (Zingales, 2000). Our study addresses this gap by providing evidence of the effect of patent pledgeability on the real economy.

Moreover, our paper has important policy implications. Many countries are making increasing efforts to stimulate a more efficient use of patent-based finance (Brassell and King, 2013; UKIPO, 2014).[3] For example, in the U.S., a large number of intellectual property rights (IPR) lawyers, IPR valuation and technology intermediaries, and IPR insurance firms have been actively lobbying for regulatory changes to foster the patent-backed loan industry (Amable et al., 2010). Contributing to these policy debates, we provide empirical evidence that policies aimed at enhancing patent pledgeability could affect firms' choice of trade secrecy versus patents, but may not necessarily induce firms to direct more resources to innovation. Nonetheless, it is worth noting that the shift from trade secrecy to patenting can be potentially positive for society, because it enhances information availability to the general public, avoids others "reinventing the wheel," and fosters a more collaborative innovation landscape (Erkal, 2005; Dass et al., 2021).

This study is not the first to examine the role of patent-backed loans in corporate policies. Mann (2018) showed that patent-backed

[1] See, for example, Cohen et al. (2000), Contigiani et al. (2018), and Klasa et al. (2018).

[2] As detailed in Section 5.2.3, we provide several possible reasons why this channel fails to work.

[3] https://www.oecd.org/officialdocuments/publicdisplaydocumentpdf/?cote=DSTI/ICCP(2014)17/CHAP9/FINAL&docLanguage = En

loans contribute significantly to the financing of innovative U.S. firms. Hochberg et al. (2018) found that patents play an important role in facilitating venture lending to start-ups. Our study complements these studies in three ways. First, previous studies did not focus on the change in market-wide patent pledgeability. By contrast, based on China's policy shocks on patent pledgeability as a quasi-natural experiment, we are likely able to better address the endogeneity of patent pledgeability and identify its effect on innovation. Second, we provide evidence that patent pledgeability spurs patenting not necessarily by mitigating financial constraints, but rather by inducing firms to switch from secrecy-based innovation to patent-based innovation. To the best of our knowledge, this is the first study to document a secrecy-to-patent shift in a firm's innovation strategies in response to an increase in patent pledgeability. Third, Mann (2018) and Hochberg et al. (2018) mainly focused on the *ex-post* effect of patent-pledged loans: conditioning on an innovative firm managing to obtain loans by pledging its patents, the firm's financial constraints can be mitigated. Our study sheds light on the *ex-ante* effect of patent-pledged loans: after patent pledgeability is exogenously increased, it seems easier for firms with sufficient assets-in-place (i.e., firms facing fewer financial constraints) to pledge their patents for loans.

2. Institutional background and hypothesis development

In China, private companies have difficulty acquiring loans from state banks, and thus face severe financial constraints (Poncet et al., 2010). In 1995, China enacted its Law of Guarantee, explicitly stating that intellectual property is valid collateral for loans. Despite this legal structure, patents have remained largely unused as collateral, mainly because of the lack of valuable patent portfolios, banks' risk aversion to accepting patents, and China's weak legal enforcement of intellectual property (IP) protection (Bailey et al., 2011; Fang et al., 2017). Since joining the World Trade Organization (WTO) in 2001, China has strengthened its IP protection and promoted IP financing. In 2007, China's former President Hu Jintao announced the country's "National Intellectual Property Strategy," in which "supporting enterprises to exploit IP value by ownership transferring, licensing, and collateral financing" is central to "construction of an innovative country" (State Council of China, 2008).

From 2009 to 2016, the State Intellectual Property Office of China (SIPO) approved more than 50 cities (listed in Table 1) as "pilot cities" in which firms could use patents as collateral for financing. The list of these pilot cities was obtained from Qiu (2018) and SIPO (2016). According to the "National intellectual property pledge financing pilot work program,"[4] SIPO selected pilot cities largely based on the following four factors: (1) the local government's willingness to participate, (2) a preliminary IP financing management system, (3) support from local financial institutions, and (4) a well-planned local procedure for patent pledging. To become a pilot city, the intellectual property offices of a city must submit an application with recommendations from the provincial intellectual property office. SIPO then organizes experts to review the applications, visit the cities if necessary, and finally select the best applicants. For the pilot cities, SIPO not only assists them in policy guidance, strategic research, personnel training, and informatization construction, but also help in risk control and risk compensation. City governments are willing to participate in the pilot program for at least two reasons. First, this pilot program could incentivize local firms to produce more patent outputs, which is beneficial to local bureaucrats for their political promotion, considering that patent output is a key performance indicator for a local innovation-driven economy (Cull et al., 2017; Hu et al., 2017). Second, as previously mentioned, banks traditionally have less incentive to provide patent-backed loans without policy guidance or government support. Participating in this pilot program helps local firms obtain more bank credit, which usually benefits local economic growth and local officials' promotion (Levine and Zervos, 1998; Li and Zhou, 2005).

As Guo et al. (2020) and SIPO (2010) summarized, typical patent-collateralized financing is conducted as follows. A potential borrower (an innovative firm) reports its financial status, explains the loan's purpose, and provides patents as collateral. The bank consults intermediaries (including IP, accounting, or legal firms) to evaluate the patent value. If the borrower and its patents meet the lending criteria, the bank approves the loan and patent ownership is transferred to the bank. All transactions are registered with the SIPO. When the borrower repays the principal sum and interest, the bank returns the pledged patent. If the borrower defaults, the bank can sell the patents or license them to a third party.

In these pilot cities, the SIPO has implemented several initiatives to facilitate patent-backed loans (Qiu, 2018; SIPO, 2006). First, in collaboration with local governments, the SIPO directly subsidizes the interest payments associated with the patent-backed loans. Second, the SIPO establishes patent-based financing platforms, enabling registered asset appraisers, technical experts in related industries, and relevant legal experts to share information and collaborate. Third, the SIPO has established patent collateral databases. These databases provide dynamic information throughout the patent pledging process, improve the transparency of pledged patents, improve borrower monitoring, and reduce information asymmetry between innovative firms and banks. Fourth, the SIPO actively participates in the entire loan process. During the pre-loan stage, it assists banks in evaluating the patent value and qualifications of innovative firms. After banks issue a loan, the SIPO helps them monitor the patent periodically to ensure that the intellectual property associated with the patent can be secured. Finally, in the case of default on a patent-backed loan, the SIPO, local insurance companies, patent-trading platforms, and related asset appraisal agents collectively act as loan guarantors. They are responsible for deploying the pledged patents and helping banks recover loans.

We find that before the treatment, pilot cities have more patents and citations, greater GDP, larger populations, and more public firms than other cities (see Table 2, Panels C and D). This finding is understandable and indicates that the SIPO tends to select cities with greater economic power and intellectual property for participation in the pilot. Later in the paper (see Table 3), we implement a formal test and prove that this policy indeed leads to a greater use of patents as collateral, supporting the relevance of such policy

[4] See details (in Chinese) on the website: https://www.lawtime.cn/info/zscq/guojiazhengcefagui/20110930109707.html.

Table 1
List of treated cities.

Year	Cities begin pilot patent pledgeability
2009	Beijing (北京), Changchun (长春), Chengdu (成都), Dongguan (东莞), Foshan (佛山), Guangzhou (广州), Nanchang (南昌) Ningxia Province (宁夏省), Wenzhou (温州), Wuxi (无锡) Xiangtan (湘潭), Yichang (宜昌)
2010	Shanghai (上海), Tianjin (天津), Wuhan (武汉), Zhenjiang (镇江)
2011	Chongqing (重庆)
2012	Bengbu (蚌埠), Fuzhou (抚州), Mianyang (绵阳), Quanzhou (泉州), Weifang (潍坊), Weihai (威海), Zhangzhou (漳州)
2013	Binzhou (滨州)
2016	Benxi (本溪), Changde (常德), Changzhou (常州), Chenzhou (郴州), Deyang (德阳), Fuyang (阜阳), Guilin (桂林), Huizhou (惠州), Huzhou (湖州), Jiangmen (江门), Jinan (济南), Jingmen (荆门), Jiujiang (九江), Kunming (昆明), Lianyungang (连云港), Luzhou (泸州), Nantong (南通), Pingxiang (萍乡), Qingdao (青岛), Shenyang (沈阳), Shenzhen (深圳), Xiangyang (襄阳), Xinxiang (新乡), Yantai (烟台), Yuxi (玉溪), Zhengzhou (郑州), Zhongshan (中山), Zhuhai (珠海)

This table reports the years in which each city implemented patent pledgeability, which allows firms to use patents as collateral for financing. The Chinese names of the cities are reported in parentheses.

changes to corporate patenting. In Table 4, we perform a Cox analysis and show that the timing of such policies is unrelated to local firms' pre-existing patent levels and growth.

We posit that patent pledgeability increases corporate patenting by inducing innovative firms to shift from secrecy-based to patent-based innovation. A typical firm usually has two alternative strategies for appropriating returns from innovation: patents and secrets. A patent grants the owner the right to exclude others from using the invention for a limited period, requiring the owner to disclose the invention. Conversely, a trade secret, which can be of unlimited duration, is information that has commercial value and is not generally known, and that the owner deliberately conceals from competitors. In business practice, trade secrecy is often as important as patents for firms to appropriate the returns from innovation (Arundel and Kabla, 1998; Cohen et al., 2000; Arundel, 2001). As Hall et al. (2014) and Png (2017) noted, patents and secrecy are substitutes, and the trade-off between them depends on their relative benefits and costs, such as technological complexity and legal protection. All else being equal, after patents are used as collateral, the relative benefit of patents over secrecy increases (trade secrecy cannot be used as collateral due to its nature). Thus, an innovative firm on the margin may be more likely to conduct innovation in the form of patents rather than trade secrets, leading to greater patenting activity.

Another possible channel is mitigating the financial constraints faced by innovative firms. As the economy becomes increasingly knowledge-driven, intangible intellectual property (such as patents) becomes an innovative firm's primary asset. Therefore, allowing innovative firms to use patents as collateral for loans can mitigate their lack of collateral and facilitate their financing for innovation. However, as detailed in Section 5.2, this channel is unlikely to explain our findings. Our results suggest that banks tend to lend patent-backed loans to firms facing fewer financial constraints (e.g., large firms, firms with more tangible assets, and state-owned firms) and that after obtaining these loans, these firms do not direct the money into R&D expenditure, but into investment in financial securities.

3. Sample construction

We started with all Chinese public companies listed on the Shenzhen and Shanghai stock exchanges obtained from the China Stock Market and Accounting Research (CSMAR) database. Information about patent grants and citations was also obtained from the CSMAR. The CSMAR provides information on each patent's application date, granting date, inventors, and application institutions. We included patent applications filed by the sample firms and their subsidiaries to measure the firms' patenting outcomes. Because citations can be received several years after a patent is granted, patents granted near the end of our sample naturally have less time to accumulate citations. To address this well-known truncation bias, we used an adjustment factor of patent citations based on the average citation count of all patents applied for in the same year. We defined the firm-level citation measure as the sum of the adjusted citation counts of all patents filed by a firm in a given year (Hall et al., 2001). We further obtained information on patent collateral from the Incopat database, which contains detailed legal and operational information on Chinese patents, including litigation, licensing, transfers, and pledges.

Our sample begins in 2006, three years before the first batch of pilot cities in 2009. Considering that there is a typical two- to three-year lag between patent application and approval (Hall et al., 2005) and that the latest year in the CSMAR database is 2020, patents applied for from 2018 to 2020 may not appear in the database. Therefore, we ended the sample period in 2017. Our final sample consisted of 20,613 firm-year observations from 2006 to 2017.

We controlled for a vector of firm characteristics, including firm size, firm age, asset tangibility, leverage, cash holdings, capital expenditure, ROA, and Tobin's *Q*. As richer and larger cities may have more resources to support innovation, we included the city-level logarithms of GDP, per capita income, and the number of listed firms. Additionally, we controlled for city population, city loans, and deposits. Furthermore, investment in education and R&D is another factor driving patenting, and we controlled for expenditure on science and technology. These city-level data were collected from the *China Statistical Yearbooks*. We winsorized all continuous variables at the 1st and 99th percentiles. Detailed variable definitions are provided in the Appendix.

Table 2 provides the summary statistics. Panel A presents the summary statistics for firm-level patenting activities and the control variables in our baseline regressions. On average, our sample firms filed 29 patents (which were subsequently granted) per year and received 25 citations. Our average sample firms have book value assets of RMB 9.77 billion (approximately USD 1.5 billion) and are 16

Table 2
Summary statistics.

Panel A: Firm-level variables

Variable	Mean	SD	P25	Median	P75
Patent	29.409	81.517	0.000	5.000	21.000
Citation	25.439	78.799	0.000	1.796	15.896
Total assets (RMB Billion)	9.767	22.519	1.428	3.070	7.412
Firm age	15.942	5.480	12.000	16.000	19.000
Cash	0.163	0.129	0.072	0.126	0.215
Capex	0.052	0.051	0.014	0.036	0.073
ROA	0.036	0.059	0.013	0.035	0.064
Tobin's Q	2.124	1.492	1.262	1.641	2.374
Leverage	0.470	0.216	0.305	0.469	0.626
Tangibility	0.237	0.170	0.103	0.205	0.341

Panel B: City-level variables

Variable	Mean	SD	P25	Median	P75
City-level aggregate number of patents	299.631	1379.679	3.000	25.000	138.000
City-level aggregate number of citations	301.988	1647.978	0.000	15.896	94.734
△City-level aggregate number of patents in past 3 years	1.161	2.466	−0.171	0.392	1.416
△City-level aggregate number of citations in past 3 years	1.531	3.552	−0.257	0.398	1.612
△City-level aggregate number of patents in past 5 years	3.339	5.906	0.128	1.273	3.898
△City-level aggregate number of citations in past 5 years	4.751	10.821	−0.112	1.081	4.152
City GDP (RMB Billion)	217.421	300.542	65.723	122.603	241.583
City population (Ten Thousand)	464.080	320.501	248.200	391.200	618.100
City expenditure on science and technology	0.002	0.002	0.001	0.002	0.003
City loans and deposits	2.330	1.239	1.502	1.911	2.751
City income per capita (RMB Thousand)	41.122	17.598	26.945	39.405	52.636
Number of public firms	8.658	19.112	1.000	3.000	7.000

Panel C: Comparison of initial firm-level characteristics between the treated and control groups

	Treated Firms		Control Firms		Test of Difference	
	Mean	Median	Mean	Median	*t*-test	Wilcoxon test
	(1)	(2)	(3)	(4)	(1)–(3)	(2)–(4)
Patent	16.603	1.000	9.592	1.000	7.012***	0.000
Citation	11.289	0.000	5.932	0.000	5.357***	0.000
Total assets (RMB Billion)	5.373	1.611	2.911	1.462	2.461***	0.148**
Firm age	11.917	11.000	11.156	10.000	0.761***	1.000***
Cash	0.207	0.154	0.215	0.163	−0.009	−0.008
Capex	0.062	0.043	0.065	0.049	−0.003	−0.005*
ROA	0.040	0.042	0.044	0.048	−0.004	−0.006***
Tobin's Q	1.551	1.328	1.515	1.324	0.036	0.004
Leverage	0.455	0.450	0.424	0.415	0.031***	0.035**
Tangibility	0.259	0.223	0.269	0.239	−0.010	−0.016*

Panel D: Comparison of initial city-level characteristics between the treated and control groups

	Treated Cities		Control Cities	Test of Difference		
	Mean	Median	Mean	Median	t-test	Wilcoxon test
	(1)	(2)	(3)	(4)	(1)–(3)	(2)–(4)
City-level aggregate number of patents	186.529	11.000	14.691	2.000	171.838***	9.000***
City-level aggregate number of citations	221.737	9.243	6.778	0.000	214.960***	9.243***
△City-level aggregate number of patents in past 3 years	0.778	0.307	1.298	0.250	−0.520	0.057
△City-level aggregate number of citations in past 3 years	1.205	0.277	0.859	−0.104	0.347	0.381
△City-level aggregate number of patents in past 5 years	2.151	0.782	3.751	1.636	−1.600	−0.854
△City-level aggregate number of citations in past 5 years	3.330	0.170	2.689	0.431	0.641	−0.261
City GDP (RMB Billion)	190.296	118.390	72.162	47.649	118.134***	70.741***
City population (Ten Thousand)	571.959	484.100	403.495	351.050	168.464***	133.050**
City expenditure on science and technology (in percentage points)	0.039	0.024	0.064	0.028	−0.025	−0.004
City loans and deposits	2.154	1.833	2.014	1.662	0.140	0.171
City income per capita (RMB Thousand)	22.045	20.286	21.289	17.901	0.755	2.384*
Number of public firms	13.745	5.000	3.745	2.000	10.000***	3.000***

This table reports summary statistics for the key variables. Panel A presents firm-level variables based on 20,613 firm-year observations from 2006 to 2017, whereas Panel B presents city-level variables based on 2660 city-year observations during the same period. Panels C and D compare firm and city characteristics between observations that have been piloted and have never been piloted, respectively, when they first appear in our sample. We obtain patent and financial information from the China Stock Market & Accounting Research (CSMAR) database. City-level data were collected from the China Statistical Yearbook. The definitions of all the variables are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles.

Table 3
Effects of patent pledgeability policy on patents collateralized.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(1+ Number of patents collateralized)		Number of patents collateralized)/ number of patents granted		Number of patents collateralized)/ number of patents granted in the past 5 years	
Pledgeability	1.366***	1.288***	0.556**	0.535**	0.152**	0.146**
	(0.180)	(0.183)	(0.257)	(0.259)	(0.067)	(0.068)
Ln (City GDP)		0.127		0.347		0.098
		(0.231)		(0.249)		(0.060)
Ln (City Population)		1.050		−1.049**		−0.294**
		(0.777)		(0.514)		(0.133)
City expenditure on science and technology		39.360**		46.340		12.730
		(19.640)		(29.830)		(8.200)
City loans and deposits		0.008		−0.009		0.003
		(0.032)		(0.037)		(0.010)
City income per capita		−0.641**		0.245		0.079
		(0.312)		(0.323)		(0.083)
Ln(Number of public firms)		0.731***		0.087		0.019
		(0.197)		(0.178)		(0.048)
Constant	0.775***	−4.563	0.441***	3.924	0.115***	1.044
	(0.012)	(4.697)	(0.017)	(3.326)	(0.004)	(0.845)
Province × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3719	3719	3719	3719	3719	3719
R2	0.744	0.751	0.502	0.505	0.494	0.497

This table reports the DiD tests that examine the impact of the patent pledgeability policy on collateralized patents. The dependent variable in columns (1) and (2) is *Ln(1 + Number of patents collateralized)*, the dependent variable in columns (3) and (4) is *Number of patents collateralized* normalized by *number of patents granted*, and the dependent variable in columns (5) and (6) is *Number of patents collateralized* normalized by *number of patents granted in the past 5 years*. For cities that piloted patent pledgeability, the indicator variable *Pledgeability* takes the value of one for the period after the policy change, and zero for the period prior to the policy change. For cities that never piloted patent pledgeability in our sample period, *Pledgeability* always takes the value of zero. The variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Standard errors clustered by city are indicated in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 4
Timing of patent pledgeability and pre-existing corporate patenting.

	(1)	(2)	(3)	(4)
	Ln(Expected time to patent pledgeability implementation)			
Ln (1 + City-level aggregate number of patent)	0.006			
	(0.033)			
Ln (1 + City-level aggregate number of citation)		0.022		
		(0.031)		
△city-level aggregate number of patent			−0.028	
			(0.022)	
△city-level aggregate number of citation				−0.017
				(0.020)
City Controls	Same as those in Table 3 column (2)			
Observations	361	361	286	259
Wald stat	6.920	7.170	8.690	7.270

The dependent variable is *Ln(Expected time to patent pledgeability implementation)*. The sample consists of more than 50 cities with piloted patent pledgeability. Cities are excluded from the sample once they pilot patent pledgeability. The variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Standard errors clustered by city are indicated in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

years old. Cash, capital expenditure, and tangible assets accounted for 16%, 5.2%, and 23.7% of the total assets, respectively. The average firms have a book leverage ratio of 47.0% and perform well, with an ROA of 3.6% and a Tobin's *Q* of 2.12.

Panel B presents the summary statistics for the city-level variables, including city-level aggregate public firms' patenting activities and city-level control variables, based on 2660 city-year observations. On average, their public firms filed 300 patents per year and received 302 citations. Their average growth rates in the past three and five years revealed that both patent numbers and citations grew rapidly in our sample period, which is broadly consistent with China's rapid increase in patenting activities during the last few decades (Fang et al., 2017; Hu et al., 2017). The average sample cities have a GDP of RMB 217 billion, income per capita of RMB 41 thousand, population of 4.6 million, and nine public firms. Their expenditures on science and technology account for 0.2% of the local GDP, and loans and deposits account for twice the local GDP.

Panels C and D compare the firm and city characteristics of the piloted and never-piloted cities. As we have multiple policy changes at different times, we followed Bourveau et al.'s (2018) approach and compared the firm and city characteristics in the year they first appear in our sample to capture any pre-existing differences.[5] Panel C compares the pre-existing firm characteristics (based on 20,613 firm-year observations) and shows that the piloted firms are, on average, more innovative, larger, older, and have higher leverage than control firms. Panel D compares city characteristics (based on 2660 city-year observations). On average, compared with never-piloted cities, piloted cities produce a larger number of patents and citations and have a greater GDP, a larger population, and more public firms. Roberts and Whited (2013) noted that it is ideal for the treatment and control groups to have relatively similar characteristics before treatment. Otherwise, we can directly include the control variables in the regression specification. Despite these differences, the two groups of cities share similar pre-policy growth in patenting activities (such as growth in patents and patent citations over the past three or five years).

4. Empirical results

4.1. Effectiveness of patent pledgeability policy

To provide evidence that the patent pledgeability policy leads to a greater use of patents as collateral, we conducted a standard DiD test at the city level as follows:

$$Patent\ Collateralized_{s,t} = \alpha + \beta_1\ Pledgeability_{s,t} + \beta_2 City\ Characteristics_{s,t} + City\ FE + Province \times Year\ FE + \varepsilon_{s,t}, \tag{1}$$

For the treated cities, the indicator variable *Pledgeability* equals one for the period after the city implemented the patent pledgeability policy, and zero otherwise. For the control group, the indicator variable *Pledgeability* always takes the value of zero. Similar to Acharya et al.'s (2014) method, we controlled for regional time trends through the interaction of province indicators with year indicators (*Province*×*Year FE*). These interactions enable us to nonparametrically control for time-varying differences between provinces in corporate patenting and implementation of the patent pledgeability policy. City fixed effects allow us to control for time-invariant differences across cities. Given that our treatment was defined at the city level, we clustered the standard errors by city.

The key coefficient of interest is the $\beta_1$coefficient. The employed fixed effects lead to β_1 being estimated as the within-city differences before and after the patent pledgeability policy change compared to similar before-after differences in cities that did not experience such a policy change during the same period (Imbens and Wooldridge, 2009).

The results are summarized in Table 3. In columns (1) and (2), the dependent variable is *Ln (1 + Number of patents collateralized),* which measures the number of newly collateralized patents in a given city in a given year. The coefficients of the *Pledgeability* indicator are positive and significant in both columns. Taking column (2) as an example, the coefficient of the *Pledgeability* indicator is 1.288 and is significant at the 1% level, which indicates that the patent pledgeability policy leads to a significant increase in the number of patents collateralized by approximately 263% ($= e^{1.342} - 1$), relative to the cities that did not implement such policies.

To address the possibility that the increase in collateralized patents is driven by an increase in the total number of granted patents in columns (3) and (4), we further normalized the number of newly collateralized patents by the total number of patents granted within the same year. In columns (5) and (6). We also normalized the number of newly collateralized patents by the total number of patents granted in the past five years, considering that a newly collateralized patent may have been granted a few years ago. In all four columns, we find positive and significant coefficients of the *Pledgeability* indicator.

In summary, Table 3 provides evidence that patent pledgeability policy leads to a significant increase in collateralized patents, supporting the relevance condition of these policies.

4.2. Validating tests on the timing of the policy change

Our empirical analysis relied on the assumption that the cross-city timing of patent pledgeability policies is unrelated to pre-existing corporate patenting in these event cities. We followed Beck et al.'s (2010) approach and employed a hazard model to investigate the validity of this assumption.

Specifically, we estimated a city-level regression using the dependent variable *Ln(Expected time to policy implementation)*. The sample consisted of more than 50 event cities. Cities were excluded from the sample once they implemented the policy change. In

[5] We excluded the observations that were already treated when they first appeared in our sample.

columns (1) and (2) of Table 4, the independent variables of interest are *Ln (1 + City-level aggregate number of patent)* and *Ln (1 + City-level aggregate number of citation)*, respectively. We also controlled for the city-level variables listed in Table 3.

None of the coefficients of *Ln (1 + City-level aggregate number of patent)* or *Ln (1 + City-level aggregate number of citation)* are significant, and the magnitudes of these coefficients are also close to zero. Taking column (1) as an example, the coefficient of *Ln (1 + City-level aggregate number of patent)* is small in magnitude (0.006) and statistically insignificant.

In columns (3) and (4) of Table 4, the independent variables of interest are $\triangle$*City-level aggregate number of patent* and $\triangle$*City-level aggregate number of citation*, which are defined as the growth rate of city-level aggregate innovation. Similar to the results in the previous two columns, the coefficients of the changes in innovation are small in magnitude and not statistically significant. For example, in column (4), the coefficient on $\triangle$*City-level aggregate number of citation* is only −0.017 and insignificant. These results indicate that the timing of patent pledgeability policies is not related to the level or change in pre-existing patenting activities. In summary, we demonstrated that city-level patent pledgeability policies are likely to be exogenous to local firms' pre-existing patenting activities.

4.3. Baseline results

Several Chinese cities changed their patent pledgeability policies in different years during the sample period. Thus, we can examine the before-after effect of such policies in affected cities relative to the before-after effect in unaffected cities. This is a DiD test design in multiple treatment groups and time periods and is widely used in the existing literature (e.g., Bertrand et al., 2004; Imbens and Wooldridge, 2009; Gao and Zhang, 2017). Similar to Eq. (1), we implemented this test using the following regression:

Table 5
Effect of patent pledgeability on corporate patenting.

	(1)	(2)	(3)	(4)
	Ln(1 + Patent)		Ln(1 + Citation)	
Pledgeability	0.167***	0.173***	0.133***	0.145***
	(0.047)	(0.046)	(0.049)	(0.049)
Firm size		0.353***		0.328***
		(0.030)		(0.034)
Firm age		0.774***		0.896***
		(0.214)		(0.215)
Cash		−0.091		−0.092
		(0.099)		(0.116)
Capex		0.115		−0.009
		(0.188)		(0.212)
ROA		−0.138		0.014
		(0.171)		(0.177)
Tobin's Q		−0.015		−0.014
		(0.009)		(0.010)
Leverage		−0.115		−0.019
		(0.101)		(0.095)
Tangibility		0.306**		0.194
		(0.122)		(0.128)
Ln(City GDP)		−0.097		−0.086
		(0.198)		(0.199)
Ln(City population)		−0.315**		−0.306
		(0.150)		(0.238)
City expenditure on science and technology		−11.930*		−23.230**
		(7.026)		(10.270)
City loans and deposits		−0.012		−0.009
		(0.034)		(0.043)
City income per capita		−0.476**		−0.457**
		(0.206)		(0.222)
Ln(Number of public firms)		0.045		0.114
		(0.112)		(0.150)
Constant	1.788***	3.634**	1.491***	2.622
	(0.014)	(1.843)	(0.014)	(2.325)
Province × Year FEs	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Observations	20,613	20,613	20,613	20,613
R2	0.780	0.790	0.724	0.733

This table reports the DiD tests examining the impact of patent pledgeability on corporate patenting. The dependent variable in columns (1) and (2) is *Ln(1 + Patent)* and that in columns (3) and (4) is *Ln(1 + Citation)*. For cities that piloted patent pledgeability, the indicator variable *Pledgeability* takes the value of one for the period after the policy change, and zero for the period prior to the policy change. For cities that never piloted patent pledgeability in our sample period, *Pledgeability* always takes the value of zero. The variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Standard errors clustered by city are indicated in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

$$Patenting_{i,t} = \alpha + \beta_1 Pledgeability_{s,t} + \beta_2 Firm\ Characteristics_{i,t} + \beta_3 City\ Characteristics_{s,t} + Firm\ FE + Province \times Year\ FE + \varepsilon_{i,t}, \quad (2)$$

where i indexes firms, s indexes the city in which the firms are headquartered, and t indexes the year.

The regression results are presented in Table 5. The coefficient estimates of the *Pledgeability* indicator are positive and statistically significant in all columns. The dependent variable in columns (1) and (2) is *Ln(1 + Patent)* and we find that the coefficients on the *Pledgeability* indicator are approximately 0.17 and significant at the 1% level, implying a positive effect of the policy change on corporate patenting. The economic magnitude is noticeable. For example, in column (2), allowing patents to be collateralized for financing leads to an increase in the number of patents by approximately 19% ($= e^{0.173} - 1$).

Examining *Ln(1 + Citation)* as the dependent variable in columns (3) and (4), we find that the coefficients on the *Pledgeability* indicator are approximately 0.14 and significant at the 1% level, which implies that allowing patents to be collateralized for financing leads to an increase in the number of citations by approximately 16% ($= e^{0.145} - 1$) (see column (4)).

Atanassov (2013) demonstrated that business combination laws lead to a decrease in the number of patents (citations per patent) by approximately 11% (16%). Acharya et al. (2014) found that wrongful discharge laws increased the number of patents (patent citations) by approximately 12% (19%). Our main results show economic significance, which is similar to the results of these studies. We found that the coefficients of the control variables are broadly consistent with prior findings (see, e.g., Aghion et al., 2005). Taken together, these results indicate that patent pledgeability has a positive effect on patenting activities, thereby supporting our hypothesis.

4.4. The pre-treatment trends

The validity of the DiD framework relies on the parallel trends assumption: without the policy change, the treated firms' patenting would have followed the same pattern as that of the control firms. Table 6 compares the pre-treatment trends of the treated and control firms. In particular, we defined five indicator variables, *Year* − *2*, *Year* − *1*, *Year 0*, *Year 1*, and *Year 2* $^{+}$, to indicate the year relative to the event year. For example, *Year 0* indicates the year in which the patent pledgeability policy is implemented; *Year* − *2* indicates that it is 2 years before the policy; and *Year 2* $^{+}$ indicates that it is at least 2 years after the policy. We then re-estimated Eq. (2) by replacing the *Pledgeability* indicator with the five indicators above.

The coefficients on the *Year* − *2* and *Year* − *1* dummies indicate whether any difference exists in patenting between the treatment and control groups prior to the policy change. The coefficients of both indicators are close to zero and not statistically significant across both columns, suggesting that the parallel trends assumption of the DiD estimation is valid.

The absence of any significant lead effects has three important implications. First, the treated firms do not seem to anticipate the implementation of patent pledgeability policies. Second, even if some treated firms anticipated such policy changes, the actual number of collateralized patents did not change until the policies took effect. Third, the positive effect of patent pledgeability on patenting is not merely due to policymakers reacting to past patenting activities and mitigating any concern about reverse causality (this result is also consistent with Table 4, which provides evidence that patent pledgeability policies are unrelated to firms' pre-event patenting activities).

The coefficients of *Year 0* indicators are significantly positive in both columns. However, the coefficients of *Year 1* and *Year 2*$^{+}$ indicators are larger in magnitude than the coefficients on *Year 0*, indicating that it takes a few years to fully reveal the impact of patent pledgeability on corporate patenting. Taking column (2) as an example (where the dependent variable is patent citations), the coefficient for *Year 0* is 0.120 (significant at the 10% level), and the coefficient for *Year 2*$^{+}$ is approximately 55% larger in magnitude (0.186 and significant at the 5% level). The fact that the patent pledgeability policy takes effect as soon as year 0 is consistent with our proposed channel in which firms tend to convert their pre-existing business secrecy into patents following a policy change (see Section 5.1).

Overall, Table 6 proves that the treated and control firms have a similar trend in patenting prior to the treatment, which supports the parallel trends assumption underlying the DiD estimation. Furthermore, Table 6 shows that most of the policy's impact on patenting occurs one year after its implementation, suggesting a causal interpretation.

4.5. Unobserved local economic conditions

Although we controlled for a vector of observable local economic conditions in our baseline regression shown in Table 5, our results may be driven by unobserved confounding local economic conditions. To address this concern, we exploited the discontinuity of the patent pledgeability policy and examined the patenting activities of treatment firms relative to their neighboring control firms. The logic is described as follows. Suppose that the patent pledgeability policy is driven by unobserved local economic factors, and these factors (instead of the policy itself) drive corporate patenting. Then, both treated firms and their neighboring control firms just across the city border would spuriously react to economic shocks because economic conditions (different from the city-level policy) tend to spill across city borders (Heider and Ljungqvist, 2015; Gao et al., 2020; Tian and Xu, 2022). In this situation, the change in patenting in treated firms should be similar to that in neighboring control firms.

We matched each treated firm to a control firm that belongs to the same industry, is in an adjacent city that has not adopted patent pledgeability policies, and is the closest in distance. A treated firm may not necessarily share the same local business environment with its "closest" control firm if their distance is still large. Therefore, the distance between these pairs of firms was required to be within 50, 100, or 150 miles. If the corresponding distance was greater than that, we excluded this pair from our analysis. Using these criteria, it is likely that the treated and control firms are geographically adjacent, and thus share comparable local economic conditions. We then re-

Table 6
Testing for pre-treatment trends and reversals.

	(1)	(2)
	Ln(1 + Patent)	Ln(1 + Citation)
Year −2	0.072	0.077
	(0.049)	(0.051)
Year −1	0.113	0.057
	(0.072)	(0.076)
Year 0 (event year)	0.193***	0.120*
	(0.063)	(0.064)
Year 1	0.249***	0.243***
	(0.057)	(0.072)
Year 2^{+}	0.231***	0.186**
	0.072	0.077
Controls	Same as those in Table 5 column (2)	
Province × Year FEs	Yes	Yes
Firm FEs	Yes	Yes
Observations	20,613	20,613
R2	0.790	0.733

This table shows the pre-treatment trends between the treated and control groups. The indicator variables *Year − 2*, *Year − 1*, *Year 0*, *Year 1*, and *Year* 2^{+}, indicate the year relative to the patent pledgeability policy. For example, the *Year 1* indicator takes the value of one if it is one year after a city adopts such a policy, and zero otherwise. The variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Standard errors clustered by city are indicated in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

estimated Eq. (2) by focusing on this subsample of firms.

The results are summarized in Table 7. Although focusing on pairs of neighboring treated and control firms within 50 (100 or 150) miles reduced our sample to 5421 (8749 or 9933) firm-year observations, we still find positive and significant coefficients (at or below the 5% level) for the *Pledgeability* indicator in all six columns. Considering that control firms are exposed to similar local economic conditions, and thus the change in control firms' patenting should be similar to that of treated firms, these results indicate that the observed impact of patent pledgeability on corporate patenting is unlikely to be explained by any unobserved confounding local economic conditions.

4.6. Propensity score matching

Following Tian and Xu's (2022) approach, we employed a propensity score matching method to make pilot and control cities more comparable to observable city characteristics. First, we estimated a probit model at the city-year level, in which the dependent variable was the indicator variable *Pledgeability,* taking the value of one for the city that implemented the patent pledgeability policy in a given

Table 7
Treated firms and neighboring control firms.

	(1)	(2)	(3)	(4)	(5)	(6)
	50 miles	100 miles	150 miles	50 miles	100 miles	150 miles
	Ln(1 + Patent)			Ln(1 + Citation)		
Pledgeability	0.261***	0.203***	0.198***	0.235**	0.213***	0.184***
	(0.075)	(0.056)	(0.052)	(0.101)	(0.071)	(0.063)
Controls	Same as those in Table 5 column (2)					
Province × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5421	8749	9933	5421	8749	9933
R2	0.804	0.807	0.808	0.751	0.756	0.755

This table examines whether the unobserved changes in local business conditions confound the effects of patent pledgeability on corporate patenting. For each treated firm, we match to a control firm in the same industry in a city that has not piloted patent pledgeability and is the closest in distance. To ensure that the treated firm and its "closest" control firm are truly close to each other, we further require that the distance between the treated firm and its "closest" control firm be within 50, 100, or 150 miles. For cities that piloted patent pledgeability, the indicator variable *Pledgeability* takes the value of one for the period after the policy change, and zero for the period prior to the policy change. For cities that never piloted patent pledgeability in our sample period, *Pledgeability* always takes the value of zero. The variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Standard errors clustered by city are indicated in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

year and zero otherwise. The dependent variables included city-level control variables in our baseline regression, as well as province- and year-fixed effects. We also controlled for the growth rate of the city-level aggregate number of patents and citations in the previous three years to capture any pre-existing trend in patenting activities.

Column (1) of Panel A of Table 8 presents the marginal effect of the probit regression. The model captures a significant amount of variation in the choice variable, as indicated by a pseudo-R^2 of 30.5% and a p-value from the χ^2 test of the overall model fitness well below 1%. We then used the propensity score obtained from the probit regression to implement propensity score matching. Specifically, for each pilot city in an event year t, we selected the control city as the one that has the nearest propensity score, has not been piloted in year t, and will not be piloted for the following three years. Following Chen et al. (2018), we set the caliper of the matching to 0.25 times the standard deviation of the propensity score. We obtained 39 unique pairs of matched cities.

Next, we re-estimated the probit model in column (1) based on the matched sample. As shown in column (2), the coefficients of all independent variables are trivial in magnitude and not significantly different from zero, indicating that the propensity score matching process has formed a control group of closely matched cities that are highly similar to the pilot cities.

Finally, we re-estimated Eq. (2) based on the matched pairs in Panel B of Table 8. The coefficients of *Pledgeability* remain significantly positive at or below the 5% level, and their economic magnitude is similar to that of the baseline regression. Specifically, when the dependent variable is *Ln(1 + Patent)* in column (1), the coefficient of *Pledgeability* is 0.195 (significant at the 1% level), which is comparable to our baseline results shown in column (2) of Table 5 (0.173). Similarly, when the dependent variable is *Ln(1 + Citation)* in column (2), the coefficient of *Pledgeability* is 0.143 (significant at the 5% level), which is also comparable to our baseline results reported in column (4) of Table 5 (0.145). Overall, these results indicate that our baseline results are unlikely to be driven by differences in characteristics between the pilot and control cities.

5. Channel tests

5.1. Shifting from secrecy-based innovation to patent-based innovation

5.1.1. Secrecy-related analysis

This section examines the channels through which patent pledgeability affects patenting. Following Glaeser's (2018) approach, we used the frequency of the keywords "trade secrecy/secrecy" in firms' annual reports to capture the extent to which a firm relies on business secrecy. Specifically, we constructed three proxies of secrecy-based innovation: the frequency of "trade secrecy/secrecy," the frequency of "trade secrecy/secrecy" per 10,000 words, and the frequency of "trade secrecy/secrecy" normalized by the frequency of "patent." Table 9, Panel A, columns (1) to (3), present the results. The coefficients of *Pledgeability* are negative and significant at or below the 5% level across all three columns, suggesting that the policy change had a negative effect on firms' reliance on secrecy (relative to reliance on patents).

Second, firms with complex technology products (such as telecommunications equipment or semiconductors) tend to prefer trade secrets over patents because they are less likely to be reverse engineered; in contrast, firms commonly prefer patents over trade secrets in discrete or simple product industries, such as chemicals (Ottoz and Cugno, 2008; Sim, 2021). Thus, considering that firms with complex technology products may have more pre-existing trade secrets, we expect the treatment effect on patenting activities to be stronger for these firms. We measured complex technology following Cohen et al. (2000) and Contigiani et al. (2018) and categorized each industry as either complex (SIC between 34 and 39; e.g., fabricated metal, industrial machinery, computer and electronic components and equipment, transportation equipment, and measuring/optical/medical goods) or discrete (SIC between 19 and 33; e.g., food, tobacco, textiles, apparel, lumber, furniture, paper, printing, chemicals, petroleum refining, rubber, leather, and varied material products). Using a restrained sample of the manufacturing industry, we defined the *Complex industry* indicator as 1 if the manufacturer is in complex technology industry, and 0 if the manufacturer is in discrete technology industry. We then re-estimated Eq. (2) by adding the *Complex industry* indicator and its interaction with the *Pledgeability* indicator.

Table 9, Panel A, columns (4) and (5) present the results. The coefficients on *Pledgeability* $\times$ *Complex industry* are positive and significant across both columns, indicating that the positive effect of patent pledgeability on corporate patenting is greater for firms in complex technology industry (i.e., firms that likely have more pre-existing trade secrets). Taking column (4) as an example (where the dependent variable is the number of patents), the coefficient on *Pledgeability* is 0.090 (not statistically significant) and the coefficient on *Pledgeability* $\times$ *Complex industry* is 0.143 (significant at the 10% level). The number of patents increases by 26% ($= e^{(0.143+0.090)} - 1$) for firms in complex technology industry, whereas the number of patents remains unchanged (positive but not statistically significant) for firms in discrete technology industries.

Third, firms facing high labor mobility tend to prefer patents to trade secrets because employee job-hopping is a key reason for secret leakage (Klasa et al., 2018; Chen et al., 2021). Thus, considering that firms facing high labor mobility may have fewer pre-existing trade secrets, we expect our treatment effect to be weaker for these firms. We constructed an industry-level labor mobility measure following Donangelo (2014), and used China's 2005 National Population Sample Survey to estimate the pre-existing level of labor mobility. The detailed calculation method is presented in the Appendix. We defined the *High mobility* indicator as 1 if a firm is located in an industry in which labor mobility is higher than the 75th percentile, and 0 otherwise. We then re-estimated Eq. (2) by adding the *High mobility* variable and its interaction with the *Pledgeability* indicator.

The results in columns (6) and (7) of Panel A in Table 9 show that the coefficients of *Pledgeability* $\times$*High mobility* are negative and significant, implying a weaker treatment effect for firms with high labor mobility. Taking column (6) as an example (where the dependent variable is the number of patents), the coefficient of *Pledgeability* is 0.218 (significant at the 1% level), and the coefficient of

Table 8
DiD tests with propensity score matching.

Panel A: Pre-match propensity score regression and post-match diagnostic regression		
	(1)	(2)
	Pre-match	Post-match
△City-level aggregate number of patents in past 3 years	−0.007	0.002
	(0.005)	(0.007)
△City-level aggregate number of citations in past 3 years	0.001	0.001
	(0.002)	(0.004)
Ln(City GDP)	0.315***	0.213
	(0.071)	(0.134)
Ln(City population)	−0.123**	−0.138
	(0.059)	(0.109)
City expenditure on science and technology	−11.850	−27.030
	(12.260)	(20.900)
City loans and deposits	0.046	0.014
	(0.032)	(0.067)
Ln(City income per capita)	−0.278	−0.152
	(0.187)	(0.326)
Ln(Number of public firms)	−0.072	0.059
	(0.046)	(0.093)
	Yes	Yes
Province FEs	Yes	Yes
	Yes	Yes
Year FEs	Yes	Yes
Observations	1026	487
Pseudo R2	0.305	0.293

Panel B: DiD Test with post-match sample		
	(1)	(2)
Pledgeability	0.195***	0.143**
	(0.058)	(0.060)
Controls	Same as those in Table 5 column (2)	
Province × Year FEs	Yes	Yes
Firm FEs	Yes	Yes
Observations	8125	8125
R2	0.795	0.734

This table reports the results of the DiD tests with propensity score matching. We match cities using a one-to-one nearest-neighbor propensity matching based on a set of observable city-level variables. For cities that piloted patent pledgeability, the indicator variable *Pledgeability* takes the value of one for the period after the policy change, and zero for the period prior to the policy change. For cities that never piloted patent pledgeability in our sample period, *Pledgeability* always takes the value of zero. Panel A reports the parameter estimates from the probit model used to estimate the propensity scores, where the dependent variable is the *Pledgeability* indicator variable. Panel B reports the DiD estimation results using matched samples. The variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Standard errors clustered by city are indicated in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Pledgeability × *High mobility* is −0.183 (significant at the 5% level). The number of patents increases by 24% ($= e^{0.219} - 1$) for firms facing low labor mobility (those that likely have more pre-existing trade secrets), whereas the number of patents increases by only 4% ($e^{(0.219-0.183)} - 1$) for firms facing high labor mobility. In summary, Panel A of Table 9 provides supporting evidence that a patent pledgeability policy increases corporate patenting, possibly by encouraging a shift from secrecy-to patent-based innovation.

5.1.2. *Existing technology domain vs. new technology domain*

If our treatment effect is truly due to firms shifting from secrecy- to patent-based innovation (i.e., converting their business secrecy into patents), we expect that these increased patents are mainly in the firms' existing knowledge domain rather than a new domain. First, following Balsmeier et al. (2017) and Gao et al. (2018), we computed the variable *New-class patent* as the number of patent applications filed and eventually granted in a given year in technology classes where the firm had no other patent filed and was eventually granted in any previous year (starting from 1992, the earliest year of patent data from CSMAR). Similarly, we computed the variable *Existing-class patent* as the number of patent applications filed and eventually granted in a given year in technology classes where the firm has already had patents filed and eventually granted in the same technology classes before (starting from 1992).

As an alternative way to measure a firm's technology domain, we used the combination of a firm's portfolio of patents and citations made by its existing patents over five years to characterize its existing knowledge (e.g., Benner and Tushman, 2002; Almeida et al., 2021). Following Almeida et al. (2021), a patent is categorized as “exploratory” if 60% or more of its citations are based on a firm's new

Table 9
Channel test on trade secrecy.

Panel A: Shifting from secrecy-based innovation to patent-based innovation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Ln(1 + frequency of secrecy)	Frequency of secrecy per 10,000 words	(Frequency of secrecy) / (frequency of patent)	Ln(1 + Patent)	Ln(1 + Citation)	Ln(1 + Patent)	Ln(1 + Citation)
Pledgeability × Complex industry				0.143*	0.434***		
				(0.079)	(0.119)		
Pledgeability × High mobility						−0.183**	−0.233***
						(0.076)	(0.063)
Pledgeability	−0.028**	−0.002**	−0.029***	0.090	−0.102	0.218***	0.210***
	(0.013)	(0.001)	(0.010)	(0.065)	(0.092)	(0.050)	(0.053)
Controls	Same as those in Table 5 column (2)						
Province × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,590	20,590	14,350	12,644	12,644	20,438	20,438
R2	0.544	0.514	0.465	0.776	0.723	0.790	0.734

Panel B: Existing technology domain vs. new technology domain

	(1)	(2)	(3)	(4)
	Ln(1 + New-class patent)	Ln(1 + Existing-class patent)	Ln(1+ Exploratory patent)	Ln(1+ Exploitative patent)
Pledgeability	0.050	0.204***	0.001	0.068**
	(0.036)	(0.047)	(0.022)	(0.034)
P-value of Wald test: coefficient on *Pledgeability* in column (1) = that in (2)	0.006***			
P-value of Wald test:coefficient on *Pledgeability* in column (3) = that in (4)			0.070*	
Controls	Same as those in Table 5 column (2)			
Province × Year FEs	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Observations	20,613	20,613	20,613	20,613
R2	0.479	0.770	0.408	0.714

Panel C: Patents' generality, originality and economic value

	(1)	(2)	(3)
	Generality	Originality	Value
Pledgeability	0.465**	0.439*	0.004***
	(0.203)	(0.250)	(0.001)
Controls	Same as those in Table 5 column (2)		
Province × Year FEs	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes
Observations	20,613	20,613	19,651
R2	0.786	0.700	0.518

This table reports evidence that one possible channel for a pledgeability policy to affect corporate patenting is by inducing firms to shift from secrecy-based to patent-based innovation. In Panel A, the indicator variable *Complex industry* takes the value of one if the firm is in a complex technology industry (SIC between 34 and 39), and zero if the firm is in a discrete technology industry (SIC between 19 and 33). The indicator variable *High mobility* takes the value of one if a firm is located in an industry in which labor mobility is higher than the 75th percentile of the sample. Panel B reports the impact of patent pledgeability on corporate patenting in firms' existing versus new technology domains. Panel C reports the impacts of patent pledgeability on patents' generality scores, originality scores, and economic value. For cities that piloted patent pledgeability, the indicator variable *Pledgeability* takes the value of one for the period after the policy change, and zero for the period prior to the policy change. For cities that never piloted patent pledgeability in our sample period, *Pledgeability* always takes the value of zero. All continuous variables are winsorized at the 1st and 99th percentiles. Standard errors clustered by city are indicated in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 10
Channel test on financial constraints.

Panel A: Heterogeneous treatment effects based on financing constraints

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Ln(1 + Patent)	Ln(1 + Citation)	Ln(1 + Patent)	Ln(1 + Citation)	Ln(1 + Patent)	Ln(1 + Citation)	Ln(1 + Patent)	Ln(1 + Citation)	Ln(1 + Patent)	Ln(1 + Citation)
Pledgeability	0.194***	0.175***	0.206***	0.179***	0.196***	0.158***	0.191***	0.162***	0.269***	0.249***
	(0.048)	(0.052)	(0.046)	(0.052)	(0.051)	(0.053)	(0.047)	(0.050)	(0.057)	(0.063)
Pledgeability×Low Size	−0.130**	−0.170***								
	(0.059)	(0.057)								
Pledgeability×Low Tangibility			−0.153**	−0.151**						
			(0.060)	(0.074)						
Pledgeability×High WW					−0.142***	−0.166***				
					(0.048)	(0.047)				
Pledgeability×Low Cashflow							−0.088***	−0.078*		
							(0.033)	(0.041)		
Pledgeability×Non-SOE									−0.173***	−0.178***
									(0.049)	(0.060)
Controls	Same as those in Table 5 column (2)									
Province × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,613	20,613	20,613	20,613	18,929	18,929	20,613	20,613	20,392	20,392
R2	0.790	0.734	0.791	0.734	0.797	0.744	0.790	0.733	0.790	0.733

Panel B: Effects of patent pledgeability policy on innovation input

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	R&D	SG&A	Capex	R&D+ Capex+ SG&A	Percentage of bachelor	Financial investment	Debt issuance
Pledgeability	0.0005	0.0017	−0.0016	−0.0002	−0.0100	0.0036***	0.0036**
	(0.0004)	(0.0018)	(0.0019)	(0.0026)	(0.0070)	(0.0014)	(0.00180)
Controls	Same as those in Table 5 column (2)						
Province × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,613	20,613	20,613	20,613	19,878	19,679	19,583
R2	0.746	0.857	0.508	0.730	0.772	0.239	0.100

Panel A reports the cross-sectional variation in the treatment effect based on the firms' financial constraints. The indicator variables *Low Size*, *Low Tangibility*, *High WW*, *Low Cashflow* and *Non-SOE* flag for smaller firms, firms with lower tangibility, firms with a higher WW index (Whited and Wu, 2006), less profitable firms, and non-SOE firms, respectively. Panel B examines the effects of patent pledgeability on innovation input. The indicator variable *Pledgeability* takes the value of one for the period after the policy change, and zero for the period prior to the policy change. For cities that never piloted patent pledgeability in our sample period, *Pledgeability* always takes the value of zero. All continuous variables are winsorized at the 1st and 99th percentiles. Standard errors clustered by city are indicated in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

knowledge (i.e., not citing the firm's existing patents or the citations made by those patents), while a patent is categorized as "exploitative" if 60% or more of its citations are based on a firm's existing knowledge. We computed the variable *Exploratory patent* and *Exploitative patent* as the number of exploratory and exploitative patent applications filed and eventually granted in a given year, respectively.

The regression specification in Panel B of Table 9 is the same as in the baseline regression in Eq. (2). The dependent variable in column (1) of Panel B is *Ln(1 + New-class patent)* and the coefficient of *Pledgeability* is 0.050, which is not significantly different from zero. The dependent variable in column (2) is *Ln(1 + Existing-class patent)*, and the coefficient on *Pledgeability* is 0.204 (more than four times as large as that in column (1)) and is significant at the 1% level. The Wald test of the equality of these two coefficients indicated that they are significantly different. These results indicate that increased patenting activity following the treatment is driven mainly by patents in firms' pre-existing technology classes.

We obtained similar results when examining exploratory and exploitative patents. Specifically, when we examine *Ln(1+ Exploratory patent)* in column (3), the coefficient of *Pledgeability* is 0.001 and is not significantly different from zero. By contrast, in column (4), the coefficient of *Ln(1+ Exploitative patent)* is 0.068 and is significant at the 5% level. These two coefficients are significantly different: the increased patenting activity following treatment is mainly driven by firms' exploitative rather than exploratory patents.

In summary, Panel B of Table 9 provides evidence that our treatment effects are mainly driven by patents in firms' existing technology domains rather than those in firms' new technology domains. These results are consistent with our proposed channel, in which firms tend to convert their business secrecy into patents in response to patent pledgeability policies.

5.1.3. *Patents' scientific and economic value*

If patent pledgeability truly affects a firm's patenting activities by inducing a shift from secrecy- to patent-based innovation, we expect that the increased patents will have greater scientific and economic value, considering that business secrets have strategic value for firms in the long run (Hannah, 2005; Hall et al., 2014). We used the originality and generality of a patent to measure its scientific value (Lerner et al., 2011; Hsu et al., 2014). A patent's originality score is computed as one minus the Herfindahl index of the technological classes of all patents it cites, and a patent's generality score is computed as one minus the Herfindahl index of the technological classes that cite the patent. We computed the variables *Originality* and *Generality* as the sum of the originality and generality scores of a firm's patents filed and eventually granted in a given year. The economic value of a patent is calculated as a firm's stock market response to a patent grant based on the method proposed by Kogan et al. (2017). We computed the variable *Economic value* as the sum of the economic value of patents filed and eventually granted in a given year divided by the book value of total assets.

As presented in Panel C of Table 9, the coefficients of *Pledgeability* are positive and significant at or below the 10% level across all three columns, suggesting a positive effect of patent pledgeability policy on firms' scientific and economic value of innovation. Taking column (3) as an example (the dependent variable is *Economic value*), the coefficient of *Pledgeability* is 0.004 and significant at the 1% level. This result indicates that the patent pledgeability policy led to a 27% increase in the economic value of patents from the sample mean (0.015). Overall, these results are consistent with the view that patent pledgeability policy enhances corporate patenting by inducing firms to convert business secrecy into patents.

5.2. *Mitigating financial constraints*

5.2.1. *Heterogeneous treatment effects based on financial constraints*

Another plausible channel is that the patent pledgeability policy mitigates innovative firms' financial constraints. The existing literature shows that the pledgeability of patents contributes significantly to financing for innovative firms (Mann, 2018), and the ability to put financial resources into R&D directly contributes to firms' innovation (Gao and Zhang, 2019). If this channel holds, we expect our treatment effect to be stronger for firms facing greater financial constraints.

In Panel A of Table 10, we use firm size, asset tangibility, the Whited-Wu (WW) index, profitability, and the state-owned enterprise (SOE) indicator to measure a firm's financial constraints, considering that smaller firms, those with lower tangibility, firms with a high WW index, unprofitable firms, and non-state-owned firms usually face greater financial constraints (Cleary, 1999; Allen et al., 2005; Whited and Wu, 2006). Based on these proxies, we found that our treatment effect is stronger for firms facing fewer financial constraints, contradicting our prediction of mitigating financial constraints. Taking column (1) as an example, the interaction *Pledgeability* × *Low size* is −0.130 (significant at the 5% level), and the coefficient on *Pledgeability* is 0.194 (significant at the 1% level). For large firms (which are usually less financially constrained), the number of patents increases by 20% ($= e^{(0.194)} - 1$), whereas the treatment effect is only 7% ($= e^{(0.195-0.13)} - 1$) for small firms. Taking column (10) as another example, the interaction *Pledgeability* × *Non SOE* is −0.178 (significant at the 1% level) and the coefficient on *Pledgeability* is 0.249 (significant at the 1% level). For state-owned firms (which are usually less financially constrained), citations increase by 28% ($= e^{(0.249)} - 1$), whereas the increase is only 7% ($= e^{(0.249-0.178)} - 1$) for non-state-owned firms. Overall, the results in Panel A of Table 10 contradict the proposed channel for mitigating financial constraints.

5.2.2. *Innovation input*

In Table 10 Panel B, we further examined whether patent-backed loans help firms increase their innovation inputs. Following Gao and Zhang (2019), we used R&D, SG&A, and Capex as the three measures of input for innovation. In column (1), we used R&D expenditure to capture firms' innovation inputs.[6] However, we found no significant effect of patent pledgeability on firms' R&D expenditures. This insignificant result is likely because R&D expenditures are only a noisy measure of innovation input, as they can be difficult to evaluate and often represent managers' discretionary choices (Horwitz and Richard, 1980). If managers plan to conceal their R&D activities from competitors, they may try to avoid classifying research-related spending as R&D expenses (Koh and Reeb, 2015; Koh et al., 2017). For example, purchasing lab equipment may be classified as capital expenditure and employee benefits for scientists and engineers may be classified as SG&A expenses (Gao and Zhang, 2019). To address the possible inaccuracy of R&D expenditures, we examined capital and SG&A expenses in columns (2) and (3), respectively. We show that firms do not increase their capital expenditure or SG&A expenses following a patent pledgeability policy. In column (4), we used the sum of R&D expenditures, capital expenditures, and SG&A expenses to capture a firm's overall input that could be (partially) relevant to innovation. The coefficient of *Pledgeability* is small in magnitude (−0.0002) and not statistically different from zero, suggesting that firms' broad innovation input remains unchanged following the patent pledgeability policy.

In addition to monetary innovation input, we investigated the composition of firms' workforce in column (5). The dependent variable was the percentage of employees with a bachelor's degree or higher, which measures the intensity of a firm's human capital. The coefficient of the *Pledgeability* indicator is not significantly different from zero, indicating that firms do not hire more skilled employees following this treatment. Broadly consistent with the results in columns (1) to (4), this finding indicates that firms do not increase their innovation input following a patent pledgeability policy.

Furthermore, the dependent variable in column (6) is *Financial investment*, which is the amount of firms' investment in financial assets normalized by total assets. Examples include money market funds, China's treasury, and various types of bonds and equities. We found a significant increase in *Financial investment* following the treatment, suggesting that treated firms tend to use patent-based loans to make financial investments rather than investing in innovation. The coefficient of *Pledgeability* is 0.0036 and significant at the 1% level, and it leads to an increase in *Financial investment* by 0.36 percentage points relative to the sample mean of 1.07 percentage points (i.e., an increase of 33% = 0.36/1.077).

Finally, we provide evidence that firms increase their debt following the treatment. The dependent variable in column (7) is the firm's *Debt issuance*, measured as the change in the firm's long-term debt normalized by total assets. The coefficient of *Pledgeability* is positive and significant at the 5% level. This finding indicates that firms can increase debt following treatment, supporting the view that patent pledgeability policy helps increase firms' debt capacity by making their patents more pledgeable. Taken together, the pledgeability policy does not increase firms' innovation input despite some evidence that firms borrow more and use these loans to make financial investments.

5.2.3. Possible explanation on why the financial Constraints Channel fails to work

Overall, the above results contradict the view that one possible channel for a patent pledgeability policy to increase patenting is by mitigating financial constraints. Instead, these results suggest that banks tend to issue patent-backed loans only to firms with sufficient assets-in-place, and that after obtaining the loan, these firms tend to use the money to make investments in financial assets rather than increasing innovation input.

This is understandable for several reasons. First, from a theoretical perspective, banks are naturally inclined to accept patents as collateral from a firm with sufficient assets-in-place than from another firm with similar patents but insufficient assets-in-place. This preference arises because borrowers' assets-in-place can help reduce the moral hazard associated with intangible assets and lenders' monitoring costs (Berger and Udell, 1992). Second, patent-backed loans are usually smaller than traditional loans backed by tangible assets (such as land). Thus, it is more efficient for banks to issue patent-backed loans to their pre-existing clients (who are more likely to be mature large firms) to avoid the costs of developing new clients (Loumioti, 2012). Third, in China's business practice, the banking system is dominated by state-owned banks, which tend to favor large mature firms and allocate credit disproportionately to SOEs (Cong et al., 2019). In response to the government's encouragement to issue patent-backed loans, banks are more likely to accept patents as collateral from large mature firms than from small innovative firms.

6. Robustness check and additional investigation

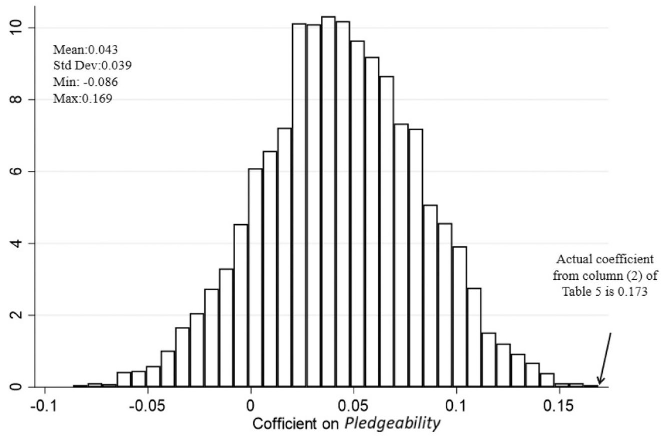
6.1. Placebo tests

In this section, we conduct a placebo test to examine whether our main results are purely due to chance. For each city in the treatment group, we randomly assigned a pseudo-event year from our sample period 2006 to 2017. To ensure that the pseudo-event year is not confounded with the actual event year, we ensured that the pseudo-event years be either at least three years before or at least three years after the actual event year. We then re-estimated the baseline regression in Eq. (2) based on the pseudo-event years and saved the corresponding coefficient on *Pledgeability*. This procedure was repeated 5000 times.

Fig. 1 plots the empirical distribution of the coefficients of those pseudo-events. The figure clearly shows that the coefficient estimates for columns (2) and (4) of Table 5 lie well to the right of the entire distribution of coefficients from the placebo test. In Graph A (where we examined the number of patents), the coefficient estimated from Table 5, column (2) (0.173) is more than three times the

[6] CSMAR provides R&D information only starting in 2007; we collect firms' R&D expenditure from WIND for the year 2006.

Graph A. Ln(1 + Patent)



Graph B. Ln(1 + Citation)

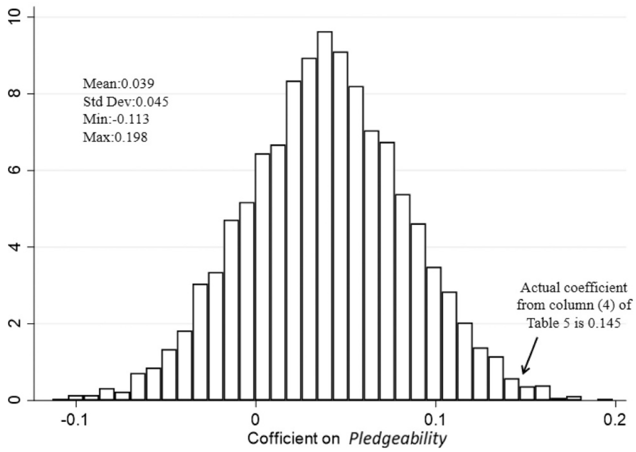


Fig. 1. Placebo tests.
This figure plots the histogram of coefficient estimates on the indicator *Pledgeability* from 5000 bootstrap simulations of the baseline model in column (2) of Table 5. For each treated firm, we assigned a pseudo–event year chosen randomly from the sample period 2006–2017. We further require the pseudo–event year to be at least three years before or after the actual event year so that the pseudo–event year is not confounded by the actual year. We then re-estimate the baseline regression based on the pseudo-pledgeable cities and save the coefficient estimates on indicator *Pledgeability*. We repeat this procedure 5000 times. Graph A shows the distribution of the coefficient estimates when the dependent variable is ***Ln (1 + Patent)***. Graph B shows the distribution of the coefficient estimates when the dependent variable is ***Ln (1 + Citation)***. The variable definitions are provided in the Appendix.

standard deviation (0.039) above the mean (0.043) of the distribution. In Graph B (where we examined the number of patent citations), the coefficient estimated from Table 5, column (4) (0.145) is approximately 2.3 times the standard deviation (0.045) above the mean (0.039) of the distribution. These results suggest that our findings are indeed driven by the patent pledgeability policy and are unlikely driven by chance.

6.2. Alternative difference-in-differences methods

Goodman-Bacon (2021) showed that standard DiD estimates can be biased when multiple treatments occur at different times, partially because earlier treatment cohorts serve as controls for later treatment groups. Given that we exploited staggered policy changes in different years, we applied three alternative DiD methods to address the heterogeneity in the timing of treatment. These include: (1) the method proposed by Sun and Abraham (2021), (2) the imputation strategy proposed by Borusyak et al. (2021), and (3) the stacked DiD method proposed by Cengiz et al. (2019).

For the estimator developed by Sun and Abraham (2021), we first computed the individual cohort time-specific treatment effects, allowing for treatment effect heterogeneity, and then aggregated these treatment effects to produce the overall treatment effects. The key idea for the estimator developed by Borusyak et al. (2021) is to use a regression of the outcome of group- and time-fixed effects in a sample of untreated observations to predict the counterfactual outcome of treated observations. Based on these predicted results, we can obtain an estimated treatment effect for each treated observation and finally a weighted average of the treatment effect estimates. The third estimator proposed by Cengiz et al. (2019) showed that the idea for stacked DiD is to create event-specific clean 2×2 datasets for the treated groups and "clean" control groups within the treatment window. We then stacked all clean 2×2 datasets and estimated a DiD regression with dataset-specific firm- and year-fixed effects.

Panel A of Table 11 reports the static effect estimates of the impacts of the patent pledgeability policy on the number of patents. The sample includes cities that were treated during the sample period and clean control cities (never-treated observations). The coefficients on *Pledgeability* are 0.190 (Sun and Abraham's (2021) method), 0.158 (Borusyak et al.'s (2021) method), and 0.182 (the stacked DiD method), respectively; each coefficient is significant at or below the 5% level. The economic magnitude of these coefficients is comparable to that from our baseline regression in column (2) of Table 5 (0.173).

Panel B of Table 11 reports the static effect estimates of the impacts of the patent pledgeability policy on the number of patent citations. The coefficients on *Pledgeability* are 0.183 (Sun and Abraham's (2021) method), 0.167 (Borusyak et al.'s (2021) method), and 0.176 (the stacked DiD method), respectively; each coefficient is significant at or below the 5% level. The economic magnitude of these coefficients is comparable to that from our baseline regression in column (4) of Table 5 (0.145). Overall, these results indicate that our main inference is largely unchanged (both statistically and economically) under alternative DiD methods.

6.3. Poisson regression

Cohn et al. (2022) demonstrated that estimating the linear regressions of the log of one plus the outcome may produce estimates with no natural interpretation that can have the wrong sign in expectation. To investigate whether our findings were driven by this potential bias, we followed their approach and estimated the baseline regressions using Poisson models.

Columns (1) and (2) of Table 12 report the results. The independent variable is *Patent* in column (1) and *Citation* in column (2) (without taking the log transformation). The coefficients on *Pledgeability* are positive and significant at or below the 5% level in both columns, indicating that our inference is largely unchanged under Poisson models.

6.4. Other confounding events

The existing literature has documented two types of local policies in China that also affect innovation: high-tech zones (Tian and Xu, 2022) and rural-urban migration (Chen et al., 2020). Tian and Xu (2022) showed that the establishment of high-tech zones in a city leads to an increase in local innovation, which could support our main findings if patent pledgeability policies are confounded by high-tech zones. Chen et al. (2020) found that China's city-level hukou relaxation (which facilitates rural-urban migration) reduces corporate innovation, which could contradict our main findings if patent pledgeability policies are confounded with hukou relaxation.

As a robustness check, we excluded cities where one of the two confounding policy changes occurred within a three-year window around the pilot year of patent pledgeability, and then re-estimated Eq. (2). As shown in columns (3) and (4) of Table 12, we continue to find positive and significant coefficients of *Pledgeability*. These results indicate that confounding local policy shocks are unlikely to have driven our main findings.

6.5. Controlling for pre-existing time trend

One potential problem with DiD estimation is the possibility of pre-existing differences in time trends across the treated and untreated groups. Although a comparison of pre-trends did not indicate any evidence of significant differences (Table 6), we followed the approach of Moser and Voena (2012) and re-estimated Eq. (2) by controlling for city-specific time trends.

In columns (5) and (6) of Table 12, we controlled for the city-specific linear year trend. In columns (7) and (8), we further controlled for the city-specific quadratic time trend. We found that our inference remains unchanged. Taking column (7) as an example (where the dependent variable is the number of patents), the coefficient of *Pledgeability* is 0.155 (significant at the 1% level), which is comparable to our baseline results reported in column (2) of Table 5 (0.173). Similarly, in column (8) (where the dependent variable is patent citations), the coefficient of *Pledgeability* is 0.127 (significant at the 5% level), which is similar to our baseline results reported in column (4) of Table 5 (0.145). In summary, our main findings remain robust when we control for pre-existing time trends (if any).

6.6. City-level innovation and spillover effects

Thus far, we have focused only on the patent outputs of public firms, while patenting activities in a city can be performed by other institutions. To better understand the effect of a patent pledgeability policy on a city's overall patenting activities, we examined total city-level patent outputs, which include the number of patents from not only public firms but also private firms, universities, research institutions, and individuals.[7] Following Ning et al. (2016) and Berkes and Gaetani (2021), total city-level patenting activities were scaled by the total population of a city. The regression equation is as follows:

[7] We obtained the patent data from SIPO and the citation data from *CnOpenData* database.

Table 11
Alternative difference-in-differences methods.

Panel A: Ln(1 + Patent)

	(1)	(2)	(3)
	Sun and Abraham (2021)	Borusyak et al. (2021)	Stacked DiD
Pledgeability	0.190***	0.158**	0.182***
	(0.065)	(0.068)	(0.044)

Panel B: Ln(1 + Citation)

	(1)	(2)	(3)
	Sun and Abraham (2021)	Borusyak et al. (2021)	Stacked DiD
Pledgeability	0.183***	0.167**	0.176***
	(0.065)	(0.066)	(0.047)

This table reports the static effect estimates from the alternative DiD methods that examine the impacts of patent pledgeability on corporate patenting. The dependent variable in Panel A is *Ln (1 + Patent)* and that in Panel B is *Ln (1 + Citation)*. Columns (1)–(3) apply the methods of Sun and Abraham (2021), Borusyak et al. (2021) and the stacked DiD approach, respectively. For cities that piloted patent pledgeability, the indicator variable *Pledgeability* takes the value of one for the period after the policy change, and zero for the period prior to the policy change. For cities that never piloted patent pledgeability in our sample period, *Pledgeability* always takes the value of zero. The variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Standard errors clustered by city are indicated in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 12
Poisson regressions, confounding local events, and pre-existing time trend.

	Poisson models		Confounding Local Events		Pre-existing Time Trend			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Patent	Citation	Ln(1 + Patent)	Ln(1 + Citation)	Ln(1 + Patent)	Ln(1 + Citation)	Ln(1 + Patent)	Ln(1 + Citation)
Pledgeability	0.159***	0.152**	0.174***	0.148***	0.135**	0.112**	0.155***	0.127**
	(0.049)	(0.068)	(0.051)	(0.056)	(0.052)	(0.054)	(0.055)	(0.056)
Controls	Same as those in Table 5 column (2)							
City × Linear year trends					Yes	Yes	No	No
City × Quadratic year trends					No	No	Yes	Yes
Province × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,388	17,242	19,885	19,885	20,613	20,613	20,613	20,613
R2	0.855	0.836	0.788	0.731	0.797	0.743	0.797	0.742

This table reports the DiD tests examining whether the impacts of patent pledgeability on corporate patenting are robust to Poisson models, confounding local events and pre-existing time trends. Columns (1) and (2) report the regression results based on the Poisson models. The dependent variable in column (1) is *Patent*, and the dependent variable in column (2) is *Citation*. Columns (3) and (4) examine whether the effect of patent pledgeability on corporate patenting is confounded by establishment of high-tech zones (Tian and Xu, 2022) and hukou relaxation (Chen et al., 2020). We delete the city if it had one of the confounding effects three years before or after the pilot year. Columns (5)–(8) investigate whether the effect of patent pledgeability on corporate patenting is driven by pre-existing time trends. In columns (5) and (6), we control for city-specific linear-year trends. In columns (7) and (8), we control for city-specific quadratic year trends. For cities that piloted patent pledgeability, the indicator variable *Pledgeability* takes the value of one for the period after the policy change, and zero for the period prior to the policy change. For cities that never piloted patent pledgeability in our sample period, *Pledgeability* always takes the value of zero. The variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Standard errors clustered by city are indicated in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

$$City\ Innovation_{s,t} = \alpha + \beta_1 Pledgeability_{s,t} + \beta_2 City\ Characteristics_{s,t} + City\ FE + Province \times Year\ FE + \varepsilon_{s,t}. \tag{3}$$

Panel A of Table 13 reports the results. The coefficients on the *Pledgeability* indicator are positive and significant in both columns. Taking column (1) as an example, the dependent variable is the city-level total patent outputs, computed as the total number of patents by all institutions and individuals in a city divided by the city's total population. The coefficient on *Pledgeability* is 0.141 and is significant at the 1% level, indicating an increase by approximately 15% ($= e^{0.141} - 1$)in the average number of patents. In column (2), the dependent variable is the city-level total patent citations normalized by the city's population, and we show that the treatment policy leads to a 43% ($= e^{0.357} - 1$) increase in patent citations. In summary, we found a significant increase in the city-level total number of patents and citations following the patent pledgeability. This result is consistent with our baseline results using firm-level analysis.

Table 13
City-level patenting and spillover effects.

Panel A: City-level Patenting	(1)	(2)
	Ln (1 + City Patent)	Ln (1 + City Citation)
Pledgeability	0.141***	0.357***
	(0.0386)	(0.0556)
	Same as those in Table 3 column (2)	
Province × Year FEs	Yes	Yes
City FEs	Yes	Yes
Observations	3884	3884
R2	0.971	0.948

Panel B: Spillover Effect	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(1 + Patent)	Ln(1 + Citation)	Ln(1 + Patent)$_{+1}$	Ln(1 + Citation)$_{+1}$	Ln(1 + Patent)$_{+2}$	Ln(1 + Citation)$_{+2}$
Close	−2.128	−7.234	7.063	8.470	−2.162	−1.869
	(4.973)	(5.603)	(4.982)	(5.680)	(6.990)	(7.057)
Controls	Same as those in Table 5 column (2)					
Province × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,549	14,549	12,113	12,113	10,144	10,144
R2	0.782	0.721	0.793	0.741	0.802	0.752

Panel A reports the DiD tests examining the impact of patent pledgeability on city-level innovation. Panel B estimates the spillover effects of the patent pledgeability policy. The subscript $+i$ for the independent variables indicates that they take the value in the next i period. *Close* is the natural logarithm of the reciprocal of a city's distance from its closest pilot city within the province. The variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Standard errors clustered by city are indicated in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Additionally, we followed Tian and Xu's (2022) method to examine the potential spillover effects of patent pledgeability policies on nearby cities. From an *ex-ante* perspective, the direction of the spillover effect can go either way. On one hand, one may expect a positive spillover effect because firms in nearby cities may learn better from innovation progress in the piloted cities (Bottazzi and Peri, 2003; Feldman and Kogler, 2010). Conversely, one may also expect a negative spillover effect because pilot cities may absorb talent and bank credit from nearby cities (Mukherji and Silberman, 2013; Miguélez and Moreno, 2015).

Specifically, we used a sample of nearby (within a 250-km radius) cities without a patent pledgeability policy piloted and re-estimated Eq. (2) by replacing the *Pledgeability* indicator with *Close*, which is defined as the natural logarithm of the reciprocal of a city's distance to the closest pilot city within the province.

As reported in columns (1) and (2) of Panel B in Table 13, the coefficients of *Close* are not significantly different from zero, suggesting no obvious spillover effects. Considering that spillover effects could take longer than the main effects (Tian and Xu, 2022), we used patenting outputs in the next one and two years as dependent variables. As shown in columns (3) to (6), we still do not find any significant spillover effects. Overall, Panel B of Table 13 indicates that patent pledgeability policies have no spillover effects.

7. Conclusions

In this paper, we find that patent pledgeability positively affects corporate patenting activities. We exploit quasi-exogenous shocks from China's staggered city-level policy change, which allows firms to use patents as collateral for financing. Using a DiD approach, we find a significant increase in firms' patents and patent citations following policy changes, relative to firms in cities that do not implement such policy changes. We then conduct several tests suggesting a causal interpretation of our results. Our pre-trend tests demonstrate that there is no time trend difference in patenting between the treated and control firms before treatment. Our analysis comparing the treated firms and their geographically adjacent control firms indicate that our results are unlikely to be due to unobserved local economic factors (because these factors would have affected both groups of firms). Our propensity score matching analysis show that our results are not driven by differences in characteristics between the treated and control groups.

Further, we present evidence that the potential channel for patent pledgeability to affect corporate patenting induces firms to shift from secrecy-based innovation to patent-based innovation: (1) following the treatment, firms rely less on secrecy; (2) treatment effects are stronger for firms with greater pre-existing trade secrecy; (3) increased patent outputs are mainly driven by patents in firms' pre-existing technology domains rather than those in new technology domains; and (4) newly granted patents have greater scientific and economic value. We examine whether the treatment effect is also through the channel of mitigating financial constraints faced by innovative firms and showed that this channel is unlikely to hold: (1) our treatment effect is stronger for firms facing weaker financial constraints, (2) firms do not increase their innovation input following treatment, and (3) firms seem to direct the money from patent-backed loans to financial investments rather than R&D.

Overall, our findings indicate that policies aimed at enhancing patent pledgeability could increase firms' propensity to apply for patents (instead of keeping innovation secret) but may have little impact on firms' overall incentive to innovate.

CRediT authorship contribution statement

Yanke Dai: Investigation, Formal analysis, Data curation. **Ting Du:** Validation, Formal analysis, Data curation. **Huasheng Gao:** Writing – review & editing, Writing – original draft, Project administration, Methodology, Conceptualization. **Yan Gu:** Methodology, Investigation, Formal analysis, Data curation. **Yongqin Wang:** Writing – review & editing, Resources, Data curation.

Data availability

Data will be made available on request.

Appendix A. Variable definitions

Variable	*Definition*
Capex	Capital expenditures normalized by book value of total assets.
Cash	Cash and short-term investments normalized by book value of total assets.
Citation	Total number of patent citations in a given year normalized by the average citation count of all patents applied for in the same year.
City citation	Total number of patent citations by all firms in a city, normalized by total population in a city.
City expenditure on science and technology	The expenditure on science and technology normalized by GDP in a city.
City GDP	City-level GDP.
City income per capita	Per capita income of city residents.
City-level aggregate number of citations	Total number of patent citations by all public firms in a city.
City-level aggregate number of patents	Total number of patent applications filed and eventually granted by all public firms in a city.
City loans and deposits	Loans and deposits normalized by GDP in a city.
City patent	Total number of patent applications filed and eventually granted by all firms in a city, normalized by total population in a city.
City population	Total population in a city.
Close	Nature logarithm of the reciprocal of a city's distance to the closest city piloting patent pledgeability policy within the province if this closest pilot city has begun the pilot, and zero otherwise.
Complex industry	An indicator variable that takes the value of one if the firm is in a complex technology industry, and zero if the firm is in a discrete technology industry. We focus on the manufacturing industries and categorize each industry as either discrete technology (SIC between 19 and 33) or complex technology (SIC between 34 and 39).
Debt issuance	Change in long-term debt normalized by book value of total asset.
Economic value	The sum of the economic value of a firm's patents filed and eventually granted in a given year divided by the book value of total assets. The economic value of a patent is calculated as the firm's stock market response to patent grant based on the method proposed by Kogan et al. (2017).
Existing-class citation	Total number of patent citations in a given year for patents in technology classes where the firm had no other patent filed and eventually granted in any previous years (starting 1992), normalized by the average citation count of all patents applied in the same year.
Existing-class patent	Total number of patent applications filed and eventually granted in a given year in technology classes where the firm had other patents filed and eventually granted in any previous years (starting 1992).
Expected time to patent pledgeability implementation	Number of years ahead for a city to implement the patent pledgeability policy.
Exploitative patent	Total number of exploitative patent applications filed and eventually granted in a given year. Following Almeida et al. (2021), a patent is categorized as "exploitative" if at least 60% of the patents it cites are from the firm's existing knowledge.
Exploratory patent	Total number of exploratory patent applications filed and eventually granted in a given year. Following Almeida et al. (2021), a patent is categorized as "exploratory" if at least 60% of the patents it cites are from the firm's new knowledge (i.e., patents not in the firm's existing knowledge).
Financial investment	Investment in financial assets normalized by book value of total assets. Financial assets consist of available-for-sale financial assets, held-to-maturity financial assets, held-for-trading financial assets, long-term equity, long-term debt, and financial derivatives.
Firm age	Natural logarithm of number of years since the firm's foundation.
Firm size	Natural logarithm of total assets.
Frequency of patent	Number of times "patents (专利)" are mentioned in an annual report.
Frequency of secrecy	Number of times "trade secrecy (商业机密 or 商业秘密)" or "technology secrecy (技术机密 or 技术秘密)" is mentioned in an annual report.

(continued on next page)

(*continued*)

Variable	*Definition*
Generality	The sum of the generality scores of a firm's patents filed and eventually granted in a given year. We compute the generality score of a patent as one minus the Herfindahl index of the technological classes that cite the patent, following Hsu et al. (2014).
High mobility	An indicator variable that takes the value of one if a firm locates in an industry for which labor mobility is higher than the 75th percentile of the sample. Industry-level measures of labor mobility are calculated using the data from the 1% National Population Sample Survey in 2005, based on the method proposed by Donangelo (2014).
High WW	An indicator variable that takes the value of one if the WW index is higher than the 75th percentile. Following Whited and Wu (2006), WW = −0.091 × Ratio of cash flow to total assets−0.062× Index of cash dividends +0.021 × Long-term liability rate − 0.044 × Firm size+0.102 × Industry average sales growth rate − 0.03× Sales revenue growth rate.
Low cashflow	An indicator variable that takes the value of one if *Cashflow* is lower than the 25th percentile of the sample. *Cashflow* is defined as net cash flow normalized by the book value of total assets.
Low size	An indicator variable that takes the value of one if *Firm size* is lower than the 25th percentile of the sample.
Low tangibility	An indicator variable that takes the value of one if *Tangibility* is lower than the 25th percentile of the sample.
New-class citation	Total number of patent citations in a given year for patents in technology classes where the firm had other patents filed and eventually granted in any previous years (starting 1992), normalized by the average citation count of all patents applied in the same year.
New-class patent	Total number of patent applications filed and eventually granted in a given year in technology classes where the firm had no other patent filed and eventually granted in any previous years (starting 1992).
Non-SOE	An indicator variable that takes the value of one if the firm is not state-owned.
Number of patents collateralized	Newly collateralized patents in a city.
Number of patents granted	Newly granted patents in a city.
Number of patents granted in the past 5 years	Newly granted patents in a city in the past 5 years.
Number of public firms	Number of public firms in a city.
Originality	The sum of the originality scores of a firm's patents filed and eventually granted in a given year. We compute the originality score of a patent as one minus the Herfindahl index of the technological classes of all the patents it cites, following Hsu et al. (2014).
Patent	Number of patent applications filed and eventually granted in a given year.
Pledgeability	An indicator variable that takes the value of one for the period after a city piloted patent pledgeability policy, and zero otherwise. For cities that never piloted patent pledgeability policy, the indicator variable *Pledgeability* always takes the value of zero.
R&D	R&D expenditures normalized by the book value of total assets. If the R&D expenditures variable is missing, we set the missing value to zero.
ROA	Operating income normalized by the book value of total assets.
SG&A	Selling, general, and administrative expenses normalized by book value of total assets.
Tangibility	Property, plant and equipment normalized by the book value of total assets.
Tobin's Q	Market value of equity plus book value of assets minus book value of equity, normalized by book value of total assets.
△City-level aggregate number of citations	Annual growth rate of number of citations of all public firms in a city.
△City-level aggregate number of citations in past 3 years	Growth rate of number of citations of all public firms in a city in the past three years.
△City-level aggregate number of citations in past 5 years	Growth rate of number of citations of all public firms in a city in the past five years.
△City-level aggregate number of patents	Annual growth rate of number of patents of all public firms in a city.
△City-level aggregate number of patents in past 3 years	Growth rate of number of patents of all public firms in a city in the past three years.
△City-level aggregate number of patents in past 5 years	Growth rate of number of patents of all public firms in a city in the past five years.

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