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I dedicate this work to my dear grandmother, Shuhua Han; without her unwavering love and encouragement since my childhood this would not have been possible. May she rest in peace.

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Abstract

This dissertation comprises of three essays. The first essay (Chapter 1) examines the short- and long-term stock market reactions to the Made in China 2025 industrial policy both in China and the United States. The second essay (Chapter 2) investigates the impact of venture capitalist directors on CEO compensation incentives at publicly listed and non-venture backed companies. The last essay (Chapter 3) investigates the impact of political connections on corporate insiders' trading behavior.

While governments have been practicing varying forms of industrial policy throughout recorded time, especially in eastern Asian countries during the post-WWII period, the evidence on how financial market investors assess government industrial policy announcements is relatively scant. In Chapter 1, we study the link between industrial policy and asset prices by using the Made in China 2025 industrial policy, announced in May 2015, as an external shock. We track Chinese firms and U.S. firms in ten high-tech industries targeted by the policy. In the short run, stock prices, measured by cumulative abnormal returns (CARs), increase significantly for both Chinese and U.S. firms, by 9.9% and 1.4%, respectively. However, in the long run, Chinese firms' CARs drop heavily, while U.S. firms' CARs continually increase. We further find that after the policy announcement, Chinese firms' profitability declines dramatically by an average of 52.9% and firms' leverage increase significantly, but they do not receive additional government subsidies. We conclude that the policy only boosts market reaction in the short run but does not promote targeted industries longer term.

The value-added role of venture capital on startups has been widely examined. Our understanding is, however, minimal on whether the presence of board members with venture capital experience in publicly listed companies impacts executive compensation contracts. In

Chapter 2, we examine the effect of board members with venture capital experience (i.e., VC directors) on executive incentives at non-venture-backed public firms. VC directors serving on the compensation committee are associated with greater CEO risk-taking incentives (i.e., vega) and pay-for-performance sensitivity (i.e., delta). These effects are more substantial if VC directors are from highly reputable VC firms. Using direct flight to VC hub cities indicators and VC dry powder as instruments, we show that these results are causal. In addition, VC directors are more focused on growth performance goals in CEO compensation contracts. We also document that prior finding of greater research intensity and innovation when VC directors serve on boards of public firms is partly explained by increased risk-taking incentives of the CEO instilled by such directors. Lastly, we find that having VC directors on nominating and/or governance committees is associated with a higher likelihood of forced CEO turnover.

The impact of political connections on firm value has been widely examined. However, there is not much evidence on how political connections affect corporate insider behaviors, especially insider stock transactions. In Chapter 3, I study the impact of political connections on corporate insiders' trading behavior using corporate insiders' employment data. I find that purchases (sales) by politically connected corporate insiders are associated with lower (higher) abnormal returns compared with non-politically connected insiders, indicating that politically connected insiders in general are cautious about potential legal risk. This effect is more significant among purchases. I also find that politically connected insiders are more likely to have longer trading horizons and are more likely to make routine trades. The Stop Trading on Congressional Knowledge (STOCK) Act passed in April 2012 effectively decreases (increases) the abnormal returns associated with insider purchases (sales) made by Congress members and staff in short horizons.

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Chapter 1

Industrial Policy and Asset Prices: Evidence from the Made in China 2025 Policy

1. Introduction

We investigate global stock market reactions to the Made in China 2025 (MC 2025) industrial policy announcement made by the Chinese government on May 8, 2015 and to subsequent evolutions of that policy through year-end 2018. The MC 2025 industrial policy initiative targets ten mostly high-tech industries with promised subsidies, tax benefits, protection from competition, and other preferences with the goal of achieving Chinese industrial prominence by 2025, and dominance by 2040.[1] In contrast to traditional Chinese industrial policies that mostly focus on sectors with comparative advantages in labor-intensive sectors, the MC 2025 policy aims to compete in high-tech and capital-intensive industries.

We examine the initial and long-run stock price reactions of listed, treated-industry Chinese firms to Central government policy announcements from May 2015 through December 2018, as well as to announced western-country policy responses. We also identify US listed firms with significant revenues from China in the ten targeted industries and examine their stock performance over the same period. We particularly evaluate how the patterns of cumulative abnormal returns (CARs) evolve after Donald Trump's election in November 2016 and his administration's subsequent imposition of significant tariffs on Chinese imports into the United States, which were focused on MC 2025 goods from March 2018 onwards (Donnan, 2018; Davis and Zumrun, 2018).[2]

Similar to the previous Chinese industrial policies whose decision-making processes were considered to be opaque to the public, the MC 2025 policy was initiated by the Communist Party of China

[1] Holz (2019) provides a detailed summary on recent industrial policies in China.

[2] We also examine stock performance of Japanese and European companies in targeted industries. Around the announcement, Japanese and European firms do not have significant CARs over [-1,1] or [-5,5] windows. In the long term, Japanese firms have significantly negative CARs from May 2015 through October 2016 and from March 2018 through December 2018, but insignificant CARs over the November 2016-February 2018 period. European firms have insignificant CARs over all three periods. The Japanese and European stock performance results can be found in Table IA7 in the Internet Appendix.

(CPC) and was approved by the State Council, and thus the policy was not endogenously driven by firm level specific conditions. We use event-study methodology to investigate firm stock performance. The event-study approach is a useful and flexible tool that financial economists employ to study how investors view specific policies (Ang and Zhang, 2014; Liu, Shu and Wei, 2017) but, to our knowledge, this paper is the first application of event study methodology to examine the financial market assessment of a major industrial policy's introduction and subsequent development.

We document that the MC 2025 policy announcement day and subsequent policy "event dates," such as President Trump's election and tariff imposition dates, are informationally relevant and then show that the financial response to China's policy initiatives has been exactly the opposite to that intended by the Chinese government. The announcement period excess market adjusted returns (MAR) around May 8, 2015 for the Chinese treated firms in these ten industries are significantly positive (4.5%) over event days [-1,1]. Treated US company stocks rise modestly on the May 2015 announcement, but then dramatically out-perform Chinese treated firms over the next 3.5 years. Announcement-period MARs for the US treated industry competitors (1.0%) are much lower than for Chinese treated firms but are still significantly positive.

While treated Chinese companies' stocks respond positively initially, the intermediate and long-term excess returns are little short of catastrophic. The long-term CARs for Chinese firms become highly *negative* over all three subperiods, namely the period from plan announcement through October 2016, the month before the election of US president Donald Trump in November 2016 (-22.0%); the period from November 2016 through February 2018, the month before now-president Trump launched the US-China trade war (-61.2%); and the trade war period from March 2018 to December 2018 (-21.6%). Treated US listed firms experience significantly positive market-adjusted CARs over two of the three periods (11.3% and 13.0%), and other adjustment models show similarly positive long-run excess returns. It is quite surprising to finding that the long-run CARs for the presumed beneficiaries of the MC 2025, treated-industry Chinese listed companies, are highly negative but CARs for the presumed targets of the policy, American treated-industry listed companies, are highly positive.

A distinctive feature of the MC 2025 policy rollout in May 2015 is that the Chinese government listed ten specific industries as targets for support, rather than presenting a broad plan to support manufacturing or promote exports (Clover, 2017; Economist, 2020). While commentators had foreshadowed an industrial plan being presented (Jephcott, 2015), the detail and specificity of the plan announced was a surprise, as was its broad scope and striking ambition—to achieve Chinese prominence or pre-eminence in ten of the world's most important industries within a decade. Investors and commentators quickly identified the Chinese version of SIC codes for the treated industries, thus enabling identification of listed Chinese firms being impacted, as well as their international competitors. We exploit this feature to identify listed Chinese and US companies in the treated industries and trace how the stocks of each nation's companies perform around and subsequent to the initial MC 2025 policy announcement.

We then use a difference-in-difference approach to investigate why Chinese company-specific long-run returns suffer so dramatically and to test whether these patterns can be explained by changes in the trend levels of specific financial inputs to the treated industries after May 2015. We focus on identifying changing financial performance for treated firms, such as profitability, productivity, and leverage, and on changing government subsidies. We also investigate whether being headquartered in an early-designated Chinese "key city" matters.

Overall, we find scant evidence of any significant changes in government subsidies for treated firms after 2015. However, we find that the leverage of treated Chinese firms increases significantly after the policy announcement, and their profitability drops dramatically, with return on assets (ROA) declines by an average of 52.9%. After examining State-Owned Enterprises (SOEs) and non-SOEs separately, we find that the increased leverage and declined profitability are significantly more pronounced among non-SOEs.

These results indicate that there are distortions in both policy making and policy implementation. First, the Chinese government launched an "aspirational" industrial policy targeting ten industries that are highly capital intensive and thus the policy itself is a diversion from China's comparative advantage in labor intensive sectors. Second, there is resource misallocation between SOEs and non-SOEs. Non-SOEs may suffer more from this policy announcement, since these companies increased their leverage—

presumably in anticipation of further government subsidies, which proved unforthcoming--and their efficiency and profitability declined as a result. Their poor long-term stock performance is associated with the increasing leverage and declining profitability of treated firms after 2015.

Our paper makes several contributions to the literature. First, as noted, we are among the first to examine how financial market investors assess government industrial policy announcements using event study methodology, thus extending an established financial econometric technique to economic policy analysis. Second, we add new empirical evidence to the literature examining how industrial policy announcements and evolutions impact targeted industries, both the presumed domestic industrial beneficiaries and their international competitors. Last, but not least, our paper adds to the rapidly growing literature examining Chinese government policy impacts, summarized in Liu and Megginson (2020).

The rest of the paper is organized as follows. Section 2 introduces the MC 2025 policy details and describes subsequent policy evolutions made by the Chinese and foreign governments. This section also employs economic policy uncertainty methodology (Baker, Bloom, and Davis, 2016) to verify that the announcements and events we study are truly market moving events. Section 3 summarizes our sample construction and data collection procedures, then describes the event-study methods we employ. Section 4 presents and discusses announcement-period and long-run event study results. Section 5 employs difference-in-difference techniques to examine whether post-MC 2025 changes in financial performance and government subsidies of treated firms can explain the extreme negative Chinese CARs. Section 6 discusses robustness checks. Section 7 concludes.

2. The Made in China 2025 Industrial Policy

2.1. The evolution of industrial policy over time

Governments have been practicing varying forms of industrial policy--defined by Stiglitz, Lin, and Monga (2013) as “government policies directed at affecting the economic structure of the economy”--throughout recorded time. Governments promote and protect certain industries, particularly those considered an economy’s “Commanding Heights” (Yergin and Stanislaw, 2002), for national security, industrial development, or comparative advantage reasons. Industrial policy tools include financial subsidies, tax incentives, antitrust enforcement or forbearance, government sponsored research and development, import protection, employment training, and even preferential allocation of scarce resources such as land, electromagnetic spectrum or credit from state-owned banks (Lucas, 2014; Ru, 2018).

In the modern era industrial policy has been most associated with (1) truly planned communist systems--such as the Soviet Union, China, and their satellites--which allocated resources based on economy wide five-year plans; (2) the social market policies of post-World War II Europe, which combined democratic processes with extensive government economic guidance, often exercised through state-owned enterprises (SOEs); (3) the post-colonial industrial policies adopted by African, Asian, and Latin American governments aimed at promoting import substitution and government ownership of infrastructure; and (4) the manufacturing and export industry-focused industrial policies pursued by Japan, Korea, Singapore and other East Asia "Tiger" economies during the post-WWII period. These experiments met with varying degree of success, ranging from disaster and collapse for the Soviet Union to achievement of rich world ("First World") status for Japan, Korea, and Singapore. However, most countries turned away from economy-wide industrial policies after the collapse of the Soviet Union and the triumph of the neoliberal economic model in the West, exemplified by the wave of privatization that swept over, first, western Europe, then the rest of the world after 1989 (Megginson and Netter, 2001; Megginson, 2017). Excellent summaries of modern industrial policies are presented in Geradrdin and Girgenson (2011), Stiglitz, Lin, and Monga (2013), and Holz (2019).

2.2. Industrial policy in China and the Made in China 2025 policy announcement

One major country that never turned away from industrial policy is the People's Republic of China (PRC), though the form and Marxist-Leninist intrusiveness of economic planning changed after Mao Zedong's death in 1976.[3] As discussed in depth in Huang (2016), the political ascendancy of Deng Xiaoping

[3] Arguably, Singapore is another country that still overtly employs industrial policy economy-wide. Although vastly smaller than China, many commentators have noted that Singapore's relatively successful industrial policies have been a major influence on (and inspiration for) Chinese policy-makers. See Ang and Ding (2006), Cheng-Han, Puchniak, and Varottil (2015), and Wildau (2017).

and the policy communique of the Third Plenum of the 11th Party Congress in 1978 put China on a path towards rapid, market-lead but CPC-controlled economic and industrial growth. This growth in turn propelled China's share of global output from 4% of global GDP in 1978 to 15% in 2019 and made it the world's second largest economy since 2010 (the largest since 2016 based on Purchasing Power Parity). Even during this hyper-growth phase, the CPC adopted economy-wide industrial plans every five years, a practice that continues to this day with Plan No.14 coming into force in 2021 (Kubota, Deng, and Wei, 2019). China evolved an economic model widely labeled "State Capitalism," as discussed, usually critically, in Bremmer (2009), Lardy (2014), Huang (2016), Chen, et al (2017), Megginson (2017), and Estrin and Gregorič (2019).[4] In sum, industrial policy has long been an important instrument the Chinese government uses to promote (and guide) economic growth.

In contrast with the previous industrial policies that mostly focused on sectors with comparative advantages on labor-intensive sectors, the Chinese government in 2015 proposed an entirely new and much more focused industrial policy aimed at achieving Chinese industrial prominence in ten selected industries by 2025, and pre-eminence by 2040. The plan was in development from the Fall of 2014 through Spring 2015 (Jephcott, 2015), but full details of the specific industries targeted and tools to be employed to achieve the program's goals were not announced until May 8, 2015. The plan was inspired by Germany's "Industry 4.0" plan but was much broader (Clover, 2017). It was developed by the Ministry of Industry and Information Technology (MIIT) in China over two and a half years, with input from over 150 experts in the China Academy of Engineering. The primary goals of the plan are: (1) to comprehensively upgrade Chinese manufacturing industry, making it innovation-driven, environment friendly, and more efficient and integrated; (2) to optimize the industrial structure in China; and (3) to raise domestic content of core components and materials to 40% by 2020 and 70% by 2025. The policy tools promised to support the ten

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[4] While these authors all define state capitalism somewhat differently, the definitions share key features. For example, Bremmer (2009) defines state capitalist countries as "economies in which the state is the principal actor and judge, and uses the markets for political gains," and further asserts that, "one essential feature of state capitalism is the existence of close ties binding together those who govern a country and those who run its enterprises." Similarly, Huang defines it as, "a capitalist economic system where the role of the state is dominant in investment, production, and distribution in the economy, through its own undertakings in commercial activities or control over corporations."

targeted industries include higher subsidies, lower financing costs, higher tax benefits, more powerful fiscal support, higher human capital investment, better intellectual property protection, more land supply, and simplified government review and approval procedures.[5] The ten targeted industries/sectors are listed in Table 1. We study global stock market reaction to this announcement and subsequent policy evolutions by examining the stock returns of listed, treated-industry Chinese and US companies.

****** Insert Table 1 here ******

2.3. Follow-on Chinese MC 2025-related policy initiatives after 2015 and Western responses

After the MC 2025 policy announcement in May 2015, the Chinese government announced several relevant policies. We summarize the important ones in this section and show the policy timeline in Figure 1. In October 2015, China published the 13th Five-Year Plan (2016-2020). Five-Year Plans are a series of social and economic development initiatives issued since 1953 by the CPC. The 13th plan emphasizes the importance of the MC 2025 policy in developing and upgrading modern industrial systems in China and provides guidance on adopting the MC 2025 to promote innovation for Chinese manufacturing industries. The plan also extends the MC 2025 by creating "Innovation 2030 Key Projects," an initiative to further develop fifteen high-tech projects.[6] Subsequently, in November 2016, the government announced "Strategic Emerging Industries" for the 13th Five-Year Plan, which overlaps somewhat with the targeted industries in MC 2025. In July 2016, the government issued the "National Intellectual Property (IP) Strategic Plan," which promotes the creation, development, and use of IP. In July 2017, September 2017,

[5] The Chinese government uses these financial tools extensively—though not always effectively--as discussed and analyzed in Firth, et al (2009), Chen, et al. (2011), Jin and Zhang (2017), Huang, et al (2018), Chen, et al (2019) and Molnar and Lu (2019). Greenburg (2019) reports that total Chinese government (national and, especially, local) subsidies to industry reached an estimated RMB 800 billion (USD 118 billion) in 2017, accounting for four percent of government spending and one percent of GDP. Khan (2018) describes MC 2025-specific subsidies offered, while Moss (2018) reports the Chinese government spent over USD 48 billion subsidizing electric vehicle and other high-tech sales over 2007-2017. Harris and Jung-a (2018) cites a Gavekal research study suggesting that Chinese government spending could reach USD 300 billion *for MC 2025 alone*, though other estimates are as much as two-thirds lower.

[6] Key Projects are jet engine and gas turbine, deep-sea space station, quantum communication and quantum computer, brain science and brain-inspired intelligence, national cyber security, space probe and space vehicle maintenance, seed industry innovation, clean use of coal, smart grid, space-ground integrated information network, big data, artificial intelligence and robotics, new materials, Beijing-Tianjin-Hebei pollution control, and healthcare.

and December 2018, respectively, detailed development plans for the artificial intelligence industry, the green agriculture industry and high-tech agricultural equipment manufacturing were issued.

****** Insert Figure 1 here ******

From August 2016 through October 2017, thirty Chinese cities were approved by the central government to be the key cities of MC 2025. We analyze the impact of these city-based announcements on the treated Chinese companies in section 5.1. As an upgrade of city-based policies, in November 2017 the government announced the plan to develop MC 2025 "National Pilot and Demonstration Zones," designed to provide stronger policy support to the targeted industries with respect to financing cost, tax benefit, land supply, and human capital investment. These cities are listed and described in the Appendix Table A2.

Initial western media reactions to the launch of MC 2025 were quite muted—though, as we will document, stock markets reacted immediately. The first detailed mention of the policy's adoption in the *Wall Street Journal* (Hong 2015) was on May 19, 2015, while the *Financial Times* (Powley, 2015) did not describe the terms of MC 2025 until June 12. Furthermore, no major national government issued a statement supporting or condemning the plan in the first few months after its announcement. In time, however, the MC 2025 plan began attracting significant international attention, especially from the United States, and most of the reactions were negative. Much of the US media saw (and sees) the MC 2025 initiative as a threat to US leadership in the world economy and, especially, in global high-tech manufacturing industries (Irwin, 2017; Davis and Wei, 2019). The criticism and concerns focus on unauthorized use of intellectual property, commercial cyber espionage, forced technology transfers, rising Chinese investment in the US and Europe, and the increasing numbers and values of Chinese acquisitions of US companies.

Starting from March 2018, the Trump administration began imposing higher tariffs on Chinese goods, escalating the trade tensions between the US and China. The unlawful use (or, to US eyes, theft) of intellectual property (IP) by Chinese companies was one of the administration's principal complaints. Since it targets high-tech manufacturing industries, the MC 2025 plan has also come under increasingly strident attack by US policymakers.[7] Within weeks of Trump's first tariff announcement, the Chinese government started to downplay the importance of MC 2025, such that by March 2019, Premier Li Keqiang did not mention the MC 2025 in his annual government work report for the first time since the plan's announcement.[8] However, although the plan is no longer mentioned explicitly, the practice of providing government support for high-tech industries remains a centerpiece of China's industrial policy.

2.4. Media attention indices: When does MC 2025 attract attention in China and the US?

Before launching our event-study analysis of stock market responses to MC 2025 plan-related news, we must document that the dates specified as informationally relevant are truly impactful. We do this using the economic policy uncertainty (EPU) methodology developed by Baker, Bloom, and Davis (2016) and subsequently employed in Kelly, Pastor, and Veronesi (2016), Huang, et al. (2018), and Bonaime, Gulen, and Ion (2018). Specifically, we develop the media attention indices based on citations of the phrase "Made in China 2025" by collecting articles published in five leading US newspapers[9] from January 2014 to July 2019 from ProQuest Historical Newspapers database. As an alternative, we also report a Google Search-based index constructed by Google Trends. These are presented in Figure 2. Both indices show that media attention to the MC 2025 plan rises modestly around the announcement date (May 2015), and then spikes sharply several times during 2018 and 2019, as US-China trade tensions become the focus of media coverage in both countries. As a comparison, we also collect the Baidu.com search index and media index based on the data from mainland China. Contrary to the US indices, the Baidu search index increases

[7] This is particularly true for acquisitions of US companies. SDC Platinum reports in 2017 and 2018 there were 98 and 75 announced Chinese acquisitions of US firms, respectively, compared with 36 in 2010. According to the Council on Foreign Relations, values of Chinese investments into the US tripled between 2015 and 2016, reaching a value of over $180 billion.

[8] This was only the highest profile of the many steps away from MC 2025 that the Chinese government started signaling in December 2018. A new development plan proposed that month as a replacement for MC 2025 would lower the numerical targets for domestic content of core components and materials from 40% in 2020 and 70% by 2025 (Wei and Davis, 2018). In March 2019, the national legislature quietly amended a draft foreign investment law to tighten up the channels, called "conformity assessments," heretofore used to extract IP secrets from foreign companies (Wei, Paige, and Davis, 2019). By October 2019, both the Chinese and US governments agreed to a truce in the trade war at least partly related to MC 2025, even though the official signing of this "Phase one" deal did not occur until January 15, 2020. This truce came none too soon; Keirnan and Zumbrun (2019) cite an NSF study asserting that the tariffs levied in 2018 saddled US consumers with USD 69 billion in additional costs.

[9] These are the *Chicago Tribune, Los Angeles Times, New York Times, Wall Street Journal*, and *Washington Post*.

substantially at the announcement in May 2015, and then spikes again around the trade war in 2018. However, the Baidu media index does not respond much to the trade war, which is consistent with the fact that the Chinese government downplayed the policy beginning in 2018.

****** Insert Figure 2 here ******

3. Sample, Data and Methodology

3.1. Sample and Data

We use multiple strategies to select treated companies from China and the United States. For the Chinese market, we start with 172 companies that are categorized as "MC 2025 Concept Stocks" listed on the Shanghai or Shenzhen stock exchanges; the requirement of having sufficient data for estimation periods in daily analysis reduces the sample to 169 firms.[10] To select US treated competitors, we identify firms headquartered in the United States whose primary SIC code matches the descriptions of the ten targeted industries/sectors. We further require that the firms: (1) are listed on the NYSE, Nasdaq, or Amex stock exchanges; (2) have segment revenues from China, Asia, and/or Hong Kong; and (3) disclose average research and development expenses exceeding $100 million over 2012-2018.[11] These screens yield a final sample of 131 US listed companies. A sample distribution by country and by industry is presented in Table 1. A detailed list of sample companies is presented in the Internet Appendix Table IA1.

We collect data from multiple sources. Data for Chinese firms are obtained from the China Stock Market and Accounting Research (CSMAR) database. Stock price and financial information for US companies are from CRSP and Compustat, respectively.

[10] The list of MC 2025 Concept Stocks is published on cnstock.com, a designated information disclosure website by China Securities Regulatory Commission. See http://news.cnstock.com/news,zxk-201512-3645927.htm for details. Three Chinese treated firms (stock code 000510, 300008, 600806) are on suspension in May 2015 and have no trading data available. Therefore, we include them in our long-term analysis but not daily analysis.

[11] To ensure a more accurate match, we also match the China official industry code, CIC code (revision 2002), with SIC code (revision 1987). We pick CIC codes based on detailed CIC code definitions (in Chinese), so are able to identify the ten targeted industries in terms of CIC code, and then locate U.S. firms by the corresponding SIC code.

3.2. *Event study methodology: Estimating announcement period excess returns*

As noted in the introduction, event study methodology is perhaps the most powerful and flexible tool that financial economists have for studying how investors view specific corporate finance policies (Brown and Warner, 1985; MacKinlay, 1997; Kothari and Warner, 2007; Ang and Zhang, 2014). For example, event study methods have been used to document that investors view dividend (and share repurchase) initiations and increases positively (Aharony and Swary, 1980; Asquith and Mullins, 1983; Maxwell and Stephens, 2003; Grullon and Michaely, 2004) and cuts negatively; that investors view corporate capital investment budget increases positively (McConnell and Muscarella, 1985); they view leverage-increasing events positively (Masulis, 1980; Andrade and Kaplan, 1998) but leverage-decreasing events, especially seasoned equity offerings, negatively (Asquith and Mullins, 1986; Mikkelson and Partch, 1986; Linn and Pinegar, 1988; Billett, King, and Mauer, 2004); and shareholders view corporate mergers positively overall (Bradley, 1980; Bradley, Desai and Kim, 1988; Rossi and Volpin, 2004; Mulherin, Netter, and Poulsen, 2017), but differentially allocate financial benefits to target and bidding firm shareholders (Moeller, Schlingemann, and Stulz, 2005; Wang, 2018), especially for deals paid in cash (Travlos, 1987). Event studies show that venture capital investment tends to increase value in listed portfolio companies (Brav and Gompers, 1997; Kaplan and Schoar, 2005; Harris, Jenkinson, and Kaplan, 2014); that sovereign wealth fund stock purchases are viewed less favorably than are equity investments by private investors (Bortolotti, Fotak, Megginson, 2015); that commercial bank loans and, especially, loan renewal announcements create value for target borrowers (James, 1987; Lummer and McConnell, 1989); and that use of prestigious investment banks in securities underwriting and merger negotiations creates shareholder value (Fernando, May, Megginson, 2012; Golubov, Petmezas, and Travlos, 2012).

Generalizing event-study methods to examine long-run excess returns (Barber and Lyons, 1997) has allowed researchers to examine the multi-year stock valuation impact of initial public offerings and seasoned equity offerings (Loughran and Ritter, 1995); mergers, spin-offs, and divestitures (Maxwell and Rao, 2003); private placements of stock (Hertzel, Lemmon, Linck, and Rees, 2002); activist investor challenges to corporate managements (Klein and Zur, 2009) and many other financial transactions. Event study techniques have recently been generalized to examine corporate social responsibility (Deng, Kang, and Low, 2013); economic policy uncertainty (Brogaard and Detzel, 2015); the value of political connections (Fan, Wong, and Zhang, 2007; Megginson, 2017); and market reactions to specific political events. Two such studies are especially important for our purpose. Liu, Shu, and Wei (2017) examine how the Chinese stock market assessed the impact of Bo Xilai's fall from grace in 2012, and document that companies politically connected to Bo suffer three-day CARs of -1.75%. Wagner, Zeckhauser and Ziegler (2018) perform a similar analysis of the US stock (and other financial) markets' reaction to the surprise election of Donald Trump as US president on November 6, 2016. They show that companies in industries likely to benefit from President Trump's policies experience significantly positive CARs, at least initially, while stocks that had been bid up in price in anticipation of a Hilary Clinton victory suffer after the election.

We use event-study methodology to examine how global stock market investors react to the Made in China 2025 policy announcement, to subsequent evolutions of this policy, and to foreign (especially US) governmental policy responses to this plan. While similar in spirit to the event study papers cited above, we believe this to be the first application of event study methods to industrial policy announcements. As described above, the fact that the Chinese government explicitly identifies ten industries in May 2015 for support allows us to identify 169 Chinese firms from those industries, as well as 131 US treated firms in the same industries. This allows us to examine not only how treated Chinese companies' stocks react, but also the stocks of their most important foreign competitors. Recent studies show that the informativeness of China's stock prices has significantly improved over the past two decades (Carpenter, Lu and Whitelaw, 2020; Lin et al., 2021). These new findings justify our use of stock movement evidence to investigate the initial policy announcement effect.

To analyze the market reaction to the MC 2025 announcement on May 8, 2015, we examine the excess returns and raw returns over 3-day [-1,+1] and 11-day [-5,+5] windows around the announcement. Market-adjusted returns (MAR) are calculated by subtracting the respective national market index return from the company's raw stock return. Market model (MM) and Fama-

French three-factor model (FF) adjustments require that the estimation period ends 31 days before the event date and has 255 days in length, with a minimum of four days of trading. We use the Shanghai-Shenzhen A-Shares VW index created by the China Center for Economic Research (CCER) at Peking University and the CRSP VW index as benchmarks for Chinese and US firms, respectively. We use the terms "excess" and "abnormal" returns interchangeably. Raw returns are defined simply as the unadjusted, buy-and-hold return for treated stocks over the holding period being examined. We present the results using alternative models and benchmarks in the Internet Appendix.

3.3. Event study methodology: Estimating long-run excess returns

While it is important to examine the immediate market reaction to the MC 2025 policy announcement, the long-run stock performance provides a better picture of the policy's effect over time. We employ similar methods to calculate long-run excess returns. The estimation period for market-model (MM) and Fama-French (FF) estimation ends 13 months before the event month (May 2015) and has 60 months in length with a minimum of four months of trading required. We investigate the long-term CARs starting from the event month (May 2015) to 43 months after (December 2018) with the 2016 US presidential election and 2018 US-China trade war in between. We focus particularly on three study-specific holding periods: the months [0,17] holding period from the MC 2025 plan announcement in May 2015 through October 2016, the month before the US presidential election in November 2016; the months [18,33] from Donald Trump's election through February 2018, the month before the start of US-China trade war; and months [34,43] from the trade war's start to December 2018. Even as a candidate, Mr. Trump was harshly critical of China's trade and industrial policies, and he implemented increasingly severe anti-China trade and investment policies—especially tariffs on ever-widening range of Chinese imports—after becoming president. As mentioned above, the industries targeted for US tariffs overlapped strongly with those promoted by MC 2025, and the link between tariffs imposed and MC 2025 industries became increasingly explicit from early-2018 onwards.

4. Event-Study Empirical Results

4.1. Announcement period raw and excess returns, May 2015

We first investigate the announcement-period returns over 3-day [-1,+1] and 11-day [-5,+5] windows around May 8, 2015. Results are presented in Table 2. Chinese treated firms experience significant raw returns of 9.1% and 12.7%, respectively, over these windows. CARs around the two windows are also significantly positive: the CARs [-1,1] and CARs [-5,5] are 4.5% and 11.5% using market-adjusted returns (MAR), and 4.6% and 9.9% using market model returns (MM). Taken as a whole, we can conclude that the announcement-period CARs for treated Chinese firms are significantly positive, and mostly range between 4.5% and 11.5%.

****** Insert Table 2 here ******

Treated-industry US listed companies have significantly positive market-adjusted excess returns of 1.0% and 1.4%, respectively, over the [-1,+1] and [-5,+5] holding periods. Market model excess returns are somewhat lower, but still significantly positive, with CARs of 0.7% and 0.9% for the [-1,+1] and [-5,+5] holding periods. Fama-French three-factor adjusted CARs are slightly lower, with 0.6% and 0.8% CARs over the two windows, respectively. To summarize, treated US firms experience significantly positive excess returns of about 0.6-1.4% around the May 8, 2015 announcement of MC 2025, but these CARs are substantially below those of Chinese treated firms. Daily CARs for Chinese and US treated firms are plotted over days -30 to +30 in Figure 3.

****** Insert Figure 3 here ******

4.2. Long-run raw and excess returns, May 2015-December 2018

We next examine the long-run monthly returns for up to 43 months--until December 2018--after the MC 2025 policy announcement in May 2015. As noted previously, we focus on three holding periods that are especially relevant for examining the long-run impact of China's adoption of MC 2025, and western governments' reactions to this plan. These are the 18-month period [0,17] from May 2015 through October 2016, the month before Donald Trump's election as US president; the 16-month period [18,33] from Donald

Trump's election through February 2018, the month before the start of the US-China trade war; and the 10-month period [34,43] from the start of trade war through December 2018. Long-run adjusted returns for both countries are presented numerically in Table 3 and graphically in Figure 4.

****** Insert Table 3 and Figure 4 here ******

Due to the Chinese stock market crash happening shortly after May 2015[12], the long-term market-adjusted returns are likely biased upward. We, therefore, focus on market-model CARs in the long-term analysis of the Chinese sample. Long-run excess returns for Chinese treated stocks are positive during the announcement month, AR [0], but turn significantly negative within ten months after the plan's announcement and become steadily worse thereafter. Chinese treated firms experience raw returns of 38.5% in May 2015, though it should be noted that the entire Chinese stock market was booming then. The VW market-model excess return for treated firms is 23.9% in May 2015. From that point on, however, the market's assessment of Chinese firms' prospects darkens dramatically. While all Chinese stocks declined sharply in value during the summer of 2015, MC 2025 stocks performed even worse, and continued declining after the overall market stabilized. The marker-model CAR becomes -22.0% and significant over the [0,17] period.

After the US presidential election, raw returns and all CARs for Chinese treated firms decline dramatically and significantly. The market-model CAR for [18,33], November 2016 through February 2018, is -61.2%, and it continues to decline by another 21.6% after the US-China trade war starts. The raw returns over [18,33] and [34,43] are -19.8% and -27.5%, respectively. The conclusion is inescapable: stock market investors believe that Chinese treated firms have been damaged significantly by adoption and further evolution of the MC 2025 industrial policy.

****** Insert Figure 5 here ******

On the other hand, treated US companies prospered financially from the MC 2025 adoption, especially from the month of the plan's announcement to Trump's presidential election. CARs over [0,17]

[12] The Chinese CSI 300 Index declines by about 40% over the three-month period from June 2015 to September 2015.

are significantly positive, ranging from 9.5% to 11.3% depending on adjustment models. From November 2016 through February 2018, US treated firms continue generating positive CARs, though these are only significant using market-adjusted returns and Fama-French models.[13]

To summarize, different excess return models and benchmarks consistently indicate that Chinese treated firm performance is boosted by the MC 2025 policy in the very short term, but then damaged severely from 2016 onwards. US treated-industry firms benefit in both the short and long terms.

5. Investigate the surprising Chinese firms long-run return patterns

Documenting that treated Chinese listed firms' stocks performed very poorly after adoption of the MC 2025 policy is very surprising and requires an explanation. In this section, we attempt to explain these results in several ways. We hypothesize that if the MC 2025 is an effective policy, then after the announcement, 1) the stocks of treated-industry companies headquartered in cities designated as MC 2025 "key cities" should perform systematically better than stocks of treated companies headquartered in non-key cities, 2) treated companies should experience better financial performance, and 3) excess resources should be allocated to the targeted capital-intensive industries. We examine these hypotheses below.

5.1. Do firms headquartered in key-cities have better stock performance?

From August 2016 to October 2017, thirty Chinese cities were approved by the central government to be MC 2025 key cities, with Ningbo being the first approved city on August 18, 2016. In our treated sample, there are 49 firms from 15 key cities. Treated firm distribution by headquarters city is reported in Table A2. Detailed key city list and city policy announcement dates are presented in Table IA8 in the Internet Appendix[14]. If the MC 2025 plan provides material net benefits to treated industries, then stocks of those companies headquartered in cities targeted first should perform better than treated company stocks of

[13] In unreported tests, we conduct industry-specific analyses of the treated Chinese firms. Among the ten targeted sectors, Information Technology (IT) firms experience the most significant (negative) long-term CARs. If measured by the market model, Chinese IT firms generate -62.3% CARs from the policy announcement to Trump's election, and these worsened further after Trump's election. We report more detailed industry analysis in the Internet Appendix Table IA9.

[14] We thank Ande Shen for assistance in checking the city-based policy announcement dates.

firms headquartered elsewhere; if MC 2025 imposed net costs on treated industries this pattern should be reversed. We conduct city-based analyses to investigate whether treated firms in those key cities outperform other treated firms. To do that, we examine the performance of firms headquartered in a key city versus treated firms headquartered in non-key cities around the policy announcement date for their respective cities. The announcement dates are different for different key cities, so we calculate the average CARs across cities by event ID. We present daily and monthly CARs in Table 4 Panel A and Panel B, respectively, and plot daily and monthly CAR trends of the two groups in Appendix Figure IA7.

****** Insert Table 4 here ******

Both monthly and daily results clearly show that the two groups, Chinese companies headquartered in key cities and companies headquartered in non-key cities, have no significant differences in stock performance. This indicates that city-based MC 2025 policies do not work as planned, at least not on publicly traded firms, nor do they have incremental impact versus the country-level MC 2025 policy.

5.2. Our DID framework to examine firms' response to MC 2025

To investigate the impact of MC 2025 on treated firms, we use a DID framework over 2012-2017[15] as shown in Equation (1) to examine whether the variables of interest change significantly for treated firms after the MC 2025 announcement. *Treated* equals one if a firm is in an industry targeted by MC 2025 and zero otherwise. *After* equals one if a year is greater than or equal to 2015 and zero otherwise. $X_{i,t}$ is a vector of the following control variables. *Size* is natural log of total assets. *Age* is the number of years since a firm was established. We follow Fan, Wong, and Zhang (2007) and Liu, Shu, and Wei (2017) to control for political connections. The indicator variable *Political Connection* equals one if at least one director on the board was or is an official of the central government, a local government, or the military, and zero otherwise. As documented in Jiang and Kim (2020), the major agency problem in Chinese companies is the conflict between controlling and minority shareholders caused by concentrated ownership structures, so we also

[15] The purpose of using observations until 2017 is to rule out the possibility that the results are driven by the China-US trade war, which starts from early 2018. In unreported tables, we find consistent and robust results even after adding the 2018 observations to the regressions and adding year 2018 dummy variables to the triple difference regressions.

control for corporate governance quality. Our main proxy for governance quality is earnings management, which is measured by discretionary accruals following Kothari, Leone, and Wasley (2005) and Giannetti, Liao, and Yu (2015). Higher discretionary accruals indicate lower governance quality. Alternatively, we use the corporate excess perks developed by Chen et al. (2021) as a proxy for governance quality as robustness checks. Higher excess corporate perks indicate lower governance quality.[16] γ_i and γ_t are firm and year fixed effects, respectively. We predict that ownership structure has a fundamental impact on firm performance. Therefore, later in the robustness checks using industry and city fixed effects, we further control for whether a company is a SOE[17]. To obtain the control group, we calculate the propensity score based on size and book-to-market ratio from 2007 to 2014, and control firms are 1-1 matched to treated firms on industry and the propensity score.[18] We further make sure that control and treated pairs are not in the same industry. We report the summary statistics of the sample in Table 5.

$$y_{i,t} = a + b_1 Treated_i + b_2 After_t + b_3 Treated_i * After_t + \delta X_{i,t} + \gamma_i + \gamma_t + \varepsilon_{it} \tag{1}$$

****** Insert Table 5 here ******

5.3. How do treated firms' performance respond to the MC 2025 announcement?

First, we examine whether the financial performance of treated firms changes significantly after the MC 2025 announcement. In Table 5, we report the DID results for three profitability measures, return on assets (*ROA*), return on equity (*ROE*), and net income scaled by number of employees (*NI/EMP*). We also examine the total factor productivity (*TFP*) and firm leverage. *TFP* is constructed following Olley and Pakes (1996). *Leverage* is long term debt scaled by total assets. We find that the interaction terms are negative and significant for *ROA*, *ROE* and *NI/EMP*, indicating that treated firms experience significantly lower profits after 2015. More specifically, taking columns 2 and 4 for example, *ROA* declines on average by 52.9% (=0.018/0.034), and *ROE* declines on average by 59.6% (=0.031/0.052).[19] When examining the direct financing of the treated group, we find that the treated firms have significantly higher leverage after

[16] We thank Yong Li and co-authors for sharing their data on corporate excess perk consumptions.
[17] Due to multicollinearity, the SOE indicator will be removed from regressions if firm fixed effects are included.
[18] We find consistent results if we use the full stock market sample. Full sample results are available upon request.
[19] The sample means of *ROA* and *ROE* are 0.035 and 0.053, respectively.

2015, as shown in column 9 and 10 of Table 6. Taking the results in column 10 for instance, treated firms increase their leverage by 25% (0.011/0.044)[20] after the policy announcement. However, we do not find any significant change in TFP. In other words, the policy induces treated firms to borrow more, and their profitability declines dramatically after the policy announcement.

****** Insert Table 6 here ******

5.4. Do treated firms obtain better government subsidies after the MC 2025 announcement?

We next examine whether government subsidies change significantly for treated firms. We use two sources to collect government subsidies data: one is the amount of government subsidies reported by firms from the CSMAR database, and the other is from the government website which releases the historical information of all direct subsidy support programs. Based on the CSMAR data, we define *GovtSub* as amount of government subsidies scaled by total assets. Based on the government website data, we create an indicator variable *GovtSubDummy* which equals one if a firm receives government subsidies, and zero otherwise. Table 7 reports the DID regression results of government subsidies amount (column 1 and 2) and government subsidies indicator (column 3 and 4). We find that the interaction terms in columns 1 through 4 are not significant, indicating that although the Chinese government aims high at the policy announcement, those firms in the ten targeted sectors do not actually receive better government subsidies as promised.

****** Insert Table 7 here ******

5.5. Heterogeneity analysis on SOEs and non-SOEs

Finally, we explore whether SOEs receive different resources and have difference performance than non-SOEs after the policy announcement. In China, SOEs account for a significant portion of the stock market. According to the Wind-Economic Database, there are 984 non-financial SOEs listed on the Shanghai or Shenzhen Stock Exchanges as of 2017, among which 340 are controlled by the central government and 644 are controlled by local governments. In our Chinese treated sample, 49 firms (29%)

[20] The sample mean of *Leverage* is 0.044.

are SOEs. In Table 8, we split the sample into SOEs and non-SOEs and report the DID results separately. We find that SOEs do not receive more government subsidies compared with non-SOEs. However, in terms of financial performance, only non-SOEs experience significant increases in leverage and significant declines in *ROA, ROE, and NI/EMP* after 2015. The productivity of non-SOEs also significantly declines after 2015. Our interpretations of these results are as follows. After the MC2025 announcement, non-SOEs increase their leverage but receive no further government subsidies, resulting in the decrease in efficiency and profitability. Although we do not have direct evidence that SOEs receive higher government subsidies, it's possible that SOEs receive other external government supports, such as direct government purchasing contracts, which unfortunately are not observable due to data availability. It is also likely that SOEs may have better internal knowledge about the MC 2025 policy than non-SOEs.

****** Insert Table 8 here ******

5.6. Summary: how to explain the poor long-run stock performance of treated Chinese firms?

The DID analysis presented in Tables 6-8 suggests why Chinese treated firms do not perform well after the MC 2025 announcement. First, the policy targets ten industries that are highly capital intensive and thus the policy diverges from China's comparative advantage in labor intensive sectors. Using the dataset from the China Annual Survey of Industrial Enterprises,[21] we find that in the latest available survey wave of 2011-2013, the average capital to labor ratio of all firms is 252.6, while this ratio for firms in the targeted ten industries is 392.7 and deviates significantly from the national level. The Chinese government launched an "aspirational" industrial policy even though the country lacked any true comparative advantage in many/most of the industrial sectors designated for conquest. Second, there is a distortion and resource misallocation in the implementation of the policy. Non-SOEs may suffer more from the policy announcement, since these companies increased their leverage—presumably in anticipation of further government subsidies, which proved unforthcoming--and their efficiency and profitability declined as a

[21] The China Annual Survey of Industrial Enterprises contains universal above-scale industrial firms (the annual sales of the main business are larger than 20 million RMB) in China. For example, there are about 346,900 firms covered in year 2013.

result. The poor long-term stock performance is associated with the increasing leverage and declining profitability of treated firms after 2015.

Although the market reacts to the MC 2025 announcement positively initially, the Chinese government does not provide effective resource allocation to the ten targeted industries which are highly capital-intensive. China does not have comparative advantages in those ten industries, and thus it would be extremely difficult to develop those industries without substantial and sustained government support. Therefore, over the long term, stock market investors react more negatively to the treated firms then to control firms. Our analysis strongly implies that government support is the key factor if a country wants to develop industries in which it has no comparative advantage.

6. Robustness checks

This section discusses the robustness checks we conduct for both the event study results presented in Section 4 and the DID analysis results presented in Section 5.

6.1. Robustness tests for the event study results

To make sure that our event study results are not driven by benchmarks or sample selection bias, we conduct robustness tests using alternative return adjustments and benchmarks, multiple Chinese subsamples, and overseas listed Chinese firms.

First, to mitigate the concerns that the traditional long-term CAR measures can be biased, we apply alternative benchmarks and models as robustness checks. For the Chinese sample, we use the CSI 300 index as the alternative benchmark to run both the MAR and MM estimations. We also employ the Fama-French three-factor model using data from CSMAR and the China factor model developed by Liu, Stambaugh, and Yuan (2019). For the US sample, we follow Bessembinder, Cooper, and Zhang (2019) and test the characteristics-based long-term CARs. We present the results in Table IA2 in the Internet Appendix. The equally-weighted (EW) results are also reported in the table. Our findings are robust to these models and benchmarks.

Second, we examine the results in Chinese subsamples after dropping SOEs. In our Chinese treated sample, 49 firms (29%) are SOEs. From October 2013 to July 2015, as part of its anti-corruption campaign, the Chinese government conducts seven rounds of SOE inspections on 50 parent SOEs, so up to 178 listed SOEs might be affected by the inspections. Among the 49 SOEs in our sample, 23 are inspected during the anti-corruption campaign from November 2014 to July 2015, and 11 are inspected after May 2015. To rule out the possibility that our results might be driven by SOEs or anti-corruption inspections, we examine the daily and long-term CARs and raw returns of non-SOE subsamples. Tables IA4 and IA5 report daily CARs, and monthly CARs of the subsamples, respectively, and Figures IA2 and IA3 show the results graphically. We find that the results are consistent with the full sample analysis after excluding inspected SOEs, after excluding SOEs inspected after May 2015, and after excluding all SOEs.

Finally, we investigate Chinese firms listed overseas. We pick 38 Hong Kong listed and 10 US listed Chinese firms in targeted industries and examine their stock performance around the announcement and in the long-term. We use the Hang Seng Index (value-weighted) and the CRSP VW index as benchmarks for firms listed on the Hong Kong Stock Exchange and US stock exchanges, respectively. We find that overseas-listed Chinese treated firms have stock performance similar to that of the main Chinese sample. Although in the long run they outperform the main sample, the post-election CARs [18,43] are still between -32.1% and -53.5%, depending on the models. These results can be found in Table IA6 and Figures IA5 and IA6 in the Internet Appendix.

6.2. Robustness tests for the DID analysis

First, our results remain unchanged if we replace firm fixed effects with industry and city fixed effects and include the SOE indicator as a control variable. The results are reported in Tables A3 and A4.

Second, we perform the parallel trend tests to make sure the DID setting is valid. Figures 6 and 7 show coefficients and associated 95% confidence intervals from estimating the leads and lags of the MC2025 effects. The omitted category is event year -1. If the interval intersects with zero over all pre-event years, then the parallel trend assumption is valid. As Figures 6 and 7 show, all DID tests meet the parallel trend requirement. We report the parallel trend test results in Tables A5 and A6 in the Appendix.

****** Insert Figures 6-7 here ******

Third, to ensure that the announcement in May 2015 is indeed a shock, we conduct the placebo tests in which we create a pre-announcement placebo event and examine how the placebo event affects the treated and control firms differently. We use the sample from 2009 to 2014 and assume 2012 as the hypothetical event year. The results are presented in the appendix Tables A7-A8.[22] All interaction terms in the *Leverage*, *ROA*, *ROE* and *NI/EMP* regressions, which are significantly negative in our DID results, lose their significance. This verifies that the declining leverage and profitability of treated firms is indeed caused by the MC 2025 announcement.

Fourth, we find that our DID results are not distorted even after including the 2018 observations. Using the triple-difference framework, we show that the 2018 observations do not drive our main results, indicating that Trump's tariff policies announced in 2018 do not affect our results.[23]

Finally, to confirm our results are robust to other corporate governance quality measures, we employ the corporate excess perk measure developed by Chen et al. (2021) as an alternative proxy for governance quality. The results[24] are consistent with the baseline results presented in the paper.

7. Conclusions

This paper examines global stock investors' responses to the detailed announcement of the Made in China 2025 industrial policy in May 2015, and to subsequent policy changes and responses by Chinese and western governments through 2018. We find that most Chinese treated-industry firms benefit during the first few post-announcement months, but then lose heavily in the long-term, especially after the 2016 US presidential election. In contrast, US treated-industry firms benefit in both the short and long-term. DID analyses indicate that treated Chinese firms do not receive additional support from the government.

[22] The results of placebo tests using industry and city fixed effects are available in the online appendix Tables IA10 and IA11.
[23] Results are available in the online appendix Tables IA12-IA15.
[24] Results are available in the online appendix Tables IA16 and IA17.

However, Chinese firms' leverage increases significantly and profitability drops dramatically after the policy announcement, especially for non-SOEs.

Our analysis concludes that there are distortions and resource misallocations in both policy making and policy implementation. Different from the traditional Chinese industrial policies which promote firms with comparative advantages in labor-intensive sectors, the MC2025 policy instead focuses on high-tech industries, which may deviate from the endowment structure. Without further government support being proffered, it is very unlikely the government will achieve its policy goals.

Chapter 2

Venture Capitalist Directors and Managerial Incentives

1. Introduction

Prior research has collectively shown that the use of high-powered incentives (e.g., performance-based executive compensation schemes in the form of restricted stock and option grants) has increased considerably in the last three decades (Murphy, 1999; Perry and Zenner, 2000; Hall and Murphy, 2003; Goldman and Slezak, 2006; Edmans, Gabaix, and Jenter, 2017). The effects of this growth have been the significant rise in the sensitivity of CEO wealth to stock price, i.e., delta (Hall and Liebman, 1998; Jensen and Murphy, 1990), and the rise in the sensitivity of CEO wealth to stock price volatility, i.e., vega (Coles, Daniel, and Naveen, 2006). Earlier studies have also documented that venture capitalists (i.e., VCs) use both cash and equity compensation to align venture-backed startup's CEO incentives with those of the private equity investors (Baker and Gompers, 1999; Hellmann, 2000; Hellmann and Puri, 2002; Hsu, 2004; Wasserman, 2006; Kaplan, Sensoy, and Strömberg, 2009; Puri and Zarutskie, 2012; Chemmanur, Krishnan, and Nandy, 2011; Bengtsson and Hand, 2011; Ewens, Nanda, and Stanton, 2020). Our understanding is, however, minimal on whether the presence of board members with venture capital experience (hereafter VC directors) in publicly listed companies impacts the use of high-powered incentives in their executive compensation contracts.

In this paper, we study the role of VC directors in designing executive compensation contracts in non-VC-backed publicly listed firms. We hypothesize that the appointment of VC directors is associated with stronger pay-risk (vega) and stronger pay-performance (delta) sensitivities. We find support for this hypothesis using executive compensation data over 1998 - 2018. To be more specific, we define a VC director as a director who has prior VC experience, i.e., becomes employed by a VC firm *before* appointment as director in the public firm of interest. Our results show that the membership of VC directors in the compensation committee is associated with greater CEO vega and greater CEO delta after controlling for a series of factors and industry and year fixed effects. However, we do not find similar results if VC directors are on other board committees (i.e., governance, audit, or nomination committees). Using the Lee-Pollock-Jin (2011) VC reputation index, we also document that these effects are more substantial if VC directors are from highly reputable VC firms. Other cross-sectional analysis shows that these effects are more pronounced if firms have more robust governance, higher institutional ownership, lower CEO pay gap, fewer business segments, or a lower percentage of independent board members. In terms of CEO pay components, having VC directors on the compensation committee is associated with higher excess compensation, higher total compensation, and higher option pay. After investigating CEO equity grants and performance goals, we find that the mechanism of how VC directors influence CEO incentives emphasizes growth rather than profitability or market-based performance goals and uses more frequently absolute rather than relative performance goals. Furthermore, the size of performance peer firms with VC directors is significantly larger than firms without, supporting our finding that VC directors emphasize scale rather than short-term profitability. Lastly, we do a placebo test to show that VC directors add stronger CEO incentives through their expertise and experience, rather than VC affiliations which could be coincidental.

In our empirical framework, an identification problem may arise if VC directors are not randomly distributed among public companies' boards, and the presence of VC directors is related to the corporate demand for VC-specific financial expertise. For example, a corporate board wishing to set more aggressive corporate goals may do so by embedding such goals in the executives' incentives. It is also possible that such boards seek to hire VC directors to aid them in setting goals and designing executive compensation to implement those, indicating non-random appointments of VC directors (Hermalin and Weisbach, 2003; Sørensen, 2007; Wintoki, Linck, and Netter, 2012). To mitigate the endogeneity concerns, we use (i) instrument variable (IV) analysis in the Two-Stage Least Squares (2SLS) and the dynamic Generalized Method of Moments (GMM) framework, and (ii) propensity matching methods.

First, in our IV analysis, we employ two groups of instruments. The first group of instruments are constructed based on the industry-year level VC dry powder, which is defined as the committed but un-invested VC capital (Aggarwal et al. 2022). The rationale for using VC dry powder is that it captures the demand by VC investors for new technology unavailable in public markets. Higher VC dry powder indicates higher demand for such technological expertise on the boards of public companies, and thus higher demand for VC directorship. The second group of instruments are indicators of annually available direct flights from a company's headquarter location to the three main VC hub cities - San Francisco, New York City, and Boston. Bernstein, Giroud, and Townsend (2016) show that the availability of direct flight can reduce venture capitalists' travel time and thus reduce VC monitoring cost. Therefore, having direct flights from VC hub cities to company headquarters is positively associated with the likelihood of appointing VC directors, while unrelated to CEO compensation design. Using 2SLS and dynamic GMM framework, we show that our results are robust to the potential endogeneity concerns. Second, we utilize the propensity score matching (i.e., PSM) techniques to determine whether firms with VC directors have higher CEO vega and delta than firms without VC directors. Using a Logit model to estimate a firm's propensity to have VC director(s) on board as a function of control variables and industry and year fixed effects, we perform one-to-one matching with no replacement. The results from this matching procedure are consistent with our baseline results.

Our findings align with Celikyurt, Sevilir, and Shivdasani (2014), who show that firms with VC director(s) have greater research and development intensity, higher innovation output, and increased deal activity with other VC-backed firms. They argue that venture capitalist innovation-specific expertise allows boards to assess better the merits of increasing research initiatives and set the appropriate strategic priorities for such initiatives. Consistent with this result, we show that one possible channel by which VC directors promote corporate innovation is increasing CEO risk-taking incentives. Our results hold for both innovation inputs, measured by R&D intensity, and innovation outputs, measured by the number of patents, patent citations, and patent economic value (Kogan et al., 2017).

In addition, we examine the impact of VC directors on CEO forced turnover. We find that having one or more VC directors on the nomination and/or governance committees is associated with a higher likelihood of forced CEO turnover if firms have poor stock performance. This is consistent with Hochberg (2012), who documents that venture capital firms facilitate good corporate governance. We add new evidence that such an effect operates among non-VC-backed publicly-listed mature companies.

This paper reexamines the advisory roles of VC directors in non-venture-backed public firms to uncover the channel through which such directors affect executive compensation. We contribute to the existing literature in three ways. First, using more recent data on executive compensation, our research examines the role of VC directors in strategic decision-making in listed firms. In particular, our research explores the compensation channels through which directors with VC experience increase corporate risk-taking and complements the literature on how independent directors' characteristics influence executive compensation contracts.[25] Second, our findings contribute to understanding the relationship between boards of directors' expertise and executive compensation. More precisely, we discuss the differences in performance goal-setting when VC directors are involved with the compensation committee and show they emphasize growth objectives as opposed to profitability or stock price objectives. Third, we provide supporting evidence to Celikyurt et al. (2014) suggesting that VC directors improve executive incentives to innovate by changing the design of top management compensation schemes and increasing CEO monitoring which may result in greater CEO turnover propensity following weak shareholder returns.

The paper is organized as follows. Section 2 reviews the literature and develops hypotheses. Section 3 describes the sample and measurement construction and presents sample summary statistics. Section 4 presents the main results and discusses robustness checks. Section 5 concludes.

[25] Within an agency framework, independent directors on corporate boards serve key functions for organizations: selection and monitor of and rewarding managers. The advisory role of independent directors on the board has a prominent place in the literature on CEO compensation. Faleye, Hoitash, and Hoitash (2011) show that directors' effectiveness lowers excess executive compensation. On the other hand, Coles, Daniel, and Naveen (2006) assert that Tobin's Q increases in board size and the fraction of insiders on the board. Along the same lines, Core, Holthausen, and Larcker (1999) find that CEO compensation is a decreasing function of the percentage of inside directors on the board.

2. Prior literature and hypothesis development

Corporate boards can add value to the company by monitoring and advising the executive team (Hermalin and Weisbach, 2003, Adams and Ferreira, 2007) and/or by formally setting executive compensation contracts. For example, Mobbs (2013) shows that board monitoring leads to higher CEO pay-performance sensitivity. Li and Srinivasan (2011) show that firms in which founders serve as a director have more high-powered incentives—higher pay-for-performance sensitivity, lower excess compensation, higher CEO turnover-performance sensitivity—than other firms. Hallock (1997) shows that CEOs who reciprocally interlock their boards earn significantly higher compensation. Other studies investigate the effect of board structure on CEO compensation (Yermack, 1996; Angbazo and Narayanan, 1997; Core, Holthausen, and Larcker, 1999; Cyert, Kang, and Kumar, 2002; Vafeas, 2003; Bertrand and Mullainathan, 2001; Grinstein and Hribar, 2004; Chhaochharia and Grinstein, 2009; Laux and Laux, 2009; Guthrie, Sokolowsky, and Wan, 2012). Yet empirical assessments of the role of board structure on CEO compensation are often deemed inconclusive since board structure is an endogenous variable, determined by an unobservable firm and CEO characteristics that, in turn, affect CEO compensation (Thorburn, 1997; Hermalin and Weisbach, 2003).

Our article is also broadly related to the literature on board expertise and corporate policies. Fama and Jensen (1983) document that providing expertise is an essential role of corporate boards. Among more recent studies, Güner, Malmendier, and Tate (2008) examine how the financial knowledge of bankers on the board affects financing arrangements; Dass et al. (2014) find that directors from related industries significantly impact firm value/performance; and Celikyurt et al. (2014) explore how venture capital expertise affects corporate investment policies, especially R&D and innovation.

On the other hand, venture capitalists have substantial representation on the board of startups in their portfolios (Barry et al., 1990; Megginson and Weiss, 1991; Lerner, 1995) and provide monitoring to limit the opportunistic behavior of managers of their portfolio firms (Rajan, 1992; Admati and Pfleiderer, 1994; Gompers, 1995). In particular, venture capitalists play an essential role in small and private start-ups by performing critical services, including attracting talented executives through recruiting management and resolving compensation issues (Gorman and Sahlman, 1989). More recent literature shows that VCs also play a significant monitoring role as directors of mature public companies (Bottazzi, Da Rin, and Hellmann, 2008). Celikyurt et al. (2014) document that 4.7% of Standard & Poor's (S&P) 1500 companies' directors have VC experience before their board appointments.

Concerning the role of VC investors in unlisted VC-backed companies' boards of directors, prior theoretical literature has identified several ways that the investor/principal can mitigate conflicts of interest between the entrepreneur and an investor with the funds to finance the venture. As summarized by Kaplan and Strömberg (2003), VC investors may perform the following activities: (i) select profitable projects and promising entrepreneurs, (ii) create incentives scheme that aligns the interest of entrepreneur with the those of investors, and (iii) monitor ongoing projects. VCs invest in entrepreneurs who need financing to fund a promising project or company. VCs have strong incentives to maximize value but, at the same time, receive few or no private benefits of control. On the other hand, VCs typically receive at least 20 percent of the profits on their portfolios (Hart, 2001; Gompers and Lerner, 1999).

Several empirical studies document the impact of VCs on entrepreneurs' compensation. For example, by conducting a detailed analysis of 213 actual contracts between VCs and entrepreneurs, Kaplan and Strömberg (2003) document that VCs change the entrepreneur's equity compensation function, making it more sensitive to performance when incentive and asymmetric information problems are more severe while separating cash flow rights and control rights and making both dependent on verifiable measures of performance. Another example is Hellman and Puri (2002). They hand-collect a sample of 173 start-up firms from California's Silicon Valley and find that VC-financed firms are more likely and expeditious to professionalize by adopting stock option plans. Venture-backed firms are more likely and quicker to bring in CEOs from outside the firm. VCs also expect to be active in other areas, such as developing a business plan, assisting with acquisitions, facilitating strategic relationships with other companies, and/or designing employee compensation.

Based on the above literature, we hypothesize that mature non-venture-backed publicly listed firms with VC directors as compensation committee members should be associated with higher CEO vega and CEO delta compared with firms without VC directors on the compensation committee.

3. Variable measurement and sample selection

3.1. Sample selection

The data in this study are gathered from various sources. Data on board characteristics and board committees are from the Institutional Shareholder Services (ISS). Executive compensation data are from the Compustat ExecuComp. The initial sample contains companies at the intersection of the two databases in 1998-2018. Venture capital data are collected from the Securities Data Company (SDC) VentureXpert. The IPOs and firm age data are collected from the SDC and Professor Jay Ritter's website following the procedure to link to conventional data sources (CRSP, Compustat) described by Professor Michelle Lowry. Other firm-level information is obtained from CRSP and Compustat. As a primary screening, we exclude financial institutions (SIC codes between 6000 and 6999) and utilities (SIC codes 4900 and 4999) from the sample to attenuate the potential effect of industry-specific regulation on the director appointments. The requirement of control data availability further reduces the sample size to 19,612 firm-year observations. After excluding VC-backed IPOs, we are left 16,085 observations with 1,707 unique firms, among which 11,064 observations have CEO vega and CEO delta available.

3.2. Measurement of VC experience of directors

To identify directors with VC experience, we extend the approach of Celikyurt et al. (2014). First, we collected detailed director-level employment data from ISS between 1998 and 2018. ISS provides information on the primary employer's name, primary employment category, other employment titles, and the type of employment services for each director. A director serving on the board of a non-venture-backed public firm in our sample is identified as a possible VC director if the keywords 'venture', 'capital', 'partner', 'fund', 'investor', 'angel', 'finance', 'financial', or 'management' is available in any of these data items, and we record the director as a possible candidate for being a VC director. Next, we link the director's primary employer name to the name of the venture capital firm in SDC VentureXpert database using a fuzzy matching method similar to Bernstein, Giroud, and Townsend (2016). Moreover, we also check the biography of each of the possible VC directors from the proxy statements. In particular, we check whether they have worked for a firm registered as a venture capital firm in SDC VentureXpert database. Finally, in our data collection, we manually review the information to refine and evaluate whether the primary employer of each VC director is a venture capital firm that invests in early-stage companies by reading the VC director employer firm's official information from several websites including www.crunchbase.com and www.bloomberg.com.[26]

Importantly, we only consider VC directors who have prior VC experience. If an existing director, at some point in time during his/her tenure with the sample firm, acquires affiliation with a VC firm, he/she is not assigned as a VC director, and we define them as coincidental VC directors instead. Later in the robustness checks, we show that the impact of VC directors is not from the coincidental VC directors. The purpose of this requirement is to capture the incentive spillover effect that VC directors have and to precisely investigate the impact of VC experience rather than coincidental VC affiliation. In addition, if a director only has previous VC experience but is no longer at a VC firm while being a director, it is not considered a VC director for the following reasons. First, VC expertise is dynamic and primarily focused on the commercialization of novel technologies, so it could be less relevant when historic. Second, including past VC experience cases in the control group (i.e., by not including them into the treatment group) biases us toward finding no results if past VC expertise is similar in its implications for CEO compensation as concurrent expertise.

3.3. Measurement of executive compensation incentives

Following previous literature, we measure executives' equity portfolio vega as the change in the risk-neutral Black-Scholes value of the executive's current year option grant for a 0.01 change in the standard deviation of the underlying stock returns (Guay, 1999; Coles, Daniel, and Naveen, 2006, Low,

[26] See **Error! Reference source not found.** for more details about the matching procedure.

2009; Armstrong and Vashistha, 2012, Hayes, Lemmon, and Qiu, 2012). Similarly, we measure executives' equity portfolio delta as the change in the risk-neutral value of the executives' current year equity portfolio for a 1% change in the underlying stock price. We use the natural logarithm of both variables in our regression analysis since both delta and vega are highly skewed.

Table 9 presents summary statistics for the 16,085 firm-year observations. Panel A reports the presence of VC directors by year. The proportion of firms appointing VC directors to the board increases almost monotonically over time. For example, while 0.94% (0.40%) of the firms have at least one VC director on the board (on the compensation committee) in 1998, the ratio increases to 6.90% (3.37%) in 2018. Panel B describes the presence of VC directors by industry. Our sample covers all ten remaining Fama and French 12 industries since financial and utility firms are excluded in our initial screen. The telephone and television transmission industry has the highest percentage of firms with VC directors on board (9.57%), followed by the business equipment industry (6.67%). Firms in the consumer durables industry have the lowest propensity to have a VC director. On average, 4.68 % (2.28%) of the firm year observations in our sample have a VC director (VC director member of the compensation committee).

****** Insert Table 9 about here ******

Table 10 presents summary statistics on CEO compensation, CEO characteristics, board of directors' characteristics, firm characteristics, and other variables used in our regressions. Following past literature (Guay, 1999; Core and Guay, 1999; Coles, Daniel, and Naveen, 2006), we winsorize vega, delta, compensation variables, and other continuous control variables at the 1st and 99th percentile. On average, firms with VC directors on the compensation committee have higher total compensation incentives than firms without VC directors on the compensation committee. Total CEO compensation averages $5,943,182 (1000*e^8.69) for firms with VC directors on the compensation committee and $3,568,854 for firms without. Total CEO vega averages $34,813 per unit of stock volatility change for firms with VC directors on the compensation committee and $17,461 for firms without. Additionally, the Total CEO delta averages $36,234 per unit of stock price change for firms with VC directors on the compensation committee and

$17,637 for firms without. Table 10 also provides summary statistics for the key control variables. On average, firms with VC directors on the compensation committee have larger size, higher leverage, higher R&D intensity, are younger in age, are less profitable, and have lower institutional ownership concentration.[27] Moreover, in Panel B, we can see that the firms with VC directors on the compensation committee have more board members and a greater percentage of independent directors.

At the director level, VC directors are, on average, three years younger and have significantly shorter (5.90 vs. 10.22 years) tenure than non-VC directors. They are also more likely to sit on multiple boards than non-VC directors. Overall, they represent about 0.5% of the pool of corporate directors in the sample. This fraction is significantly below the one documented in Celikyurt et al. (2014) (4.7%) in large part because we have a more conservative sampling of VC-directors as explained below (i.e., excluding VC-backed firms and excluding coincidental directors as noted in Section 4.9.)

****** Insert Table 10 about here ******

4. Empirical Results

To explore the impact of VC directors on a firm's compensation policy, we first investigate whether VC directors affect CEO compensation incentives. Then, to more accurately examine the effect of VC directors on mature listed companies, we exclude VC-backed firms at their IPO stage in our regression analysis. The results using the total sample are similar and consistent with the results presented in the paper and are available upon request.

4.1. Baseline regression results

4.1.1. VC directors and CEO risk-taking incentives

We first estimate the impact of VC directors on CEO risk-taking incentives (vega). Notably, we estimate a panel regression shown as equation (1) in which the dependent variable is the natural logarithm

[27] In the unreported summary statistics of our full sample, 18% of firms are VC-backed at the time of their IPOs. VC-backed IPOs account for 31% of firms with VC directors on the compensation committee, and 18% of firms without.

of CEO vega for a firm *I* in year *t+1*. The primary variable of interest is the VC directors on the compensation committee dummy, the number of VC directors on the compensation committee, or the percentage of VC directors on the compensation committee for firm *i* in year *t*. In addition, we control for several other determinants of executive compensation, including size, leverage, market-to-book ratio, ROA, research and development intensity, tangibility, stock return, stock volatility, firm age, CEO age, CEO tenure, CEO duality, independent board, the board size, institutional ownership concentration, and total institutional ownership (Coles, Daniel, and Naveen, 2006; Hayes, Lemmon, and Qiu, 2012). Furthermore, we follow the prior literature (Celikyurt et al., 2014; Güner et al., 2008; Hochberg, 2012) to include industry (Fama-French 48) fixed effects and year fixed effects to address the possibility that there are other omitted variables.[28] Finally, following Celikyurt et al. (2014), we also include state-level controls (educational attainment, per capita income, and per capita R&D expense) or state fixed effects where a firm is headquartered, to account for the possibility that high managerial incentive firms and VC firms cluster in the same geographical areas. Throughout the study, all associated t-statistics are computed based on standard errors clustered at the firm level.

$$log\ (CEO\ Vega)_{i,t+1} = \alpha + \beta * VC\ Directorship_{i,t} + Controls_{i,t} + \gamma * Year\ dummies + \delta * Industry\ dummies + \varepsilon_{i,t} \quad (2)$$

Table 11 presents the regression results of equation (2). Column (1) reports that the coefficient of VC directors on the compensation committee dummy is 0.225 and statistically significant at the 1% level. This result indicates that CEOs in VC director firms receive higher risk-taking incentives than CEOs in non-VC director firms. In terms of economic magnitude, for a representative firm with CEO compensation

[28] In our sample, the median of VC directors' tenure is 3 years (mean is 4.35 years), so it is difficult to produce consistent estimates using firm fixed effects, due to degrees of freedom per VC director being at most two, for half of the VC directors' sample. Moreover, when we require at least five years of tenure within sample to code VC director's presence, we obtain significant positive coefficient estimates of 1.014 (t=2.325, i.e., statistically significant at 5%) for vega and 0.958 (t=1.689, i.e., statistically significant at 10%) for delta if using the percent of VC directors on the compensation committee as the variable of interest. Additionally, after perusing the list of reasons to hire the VC-directors (from the DEF 14A disclosure) for all VC director cases in our sample, we do not find a mention of any specific reason as to them being hired for a particular firm characteristic or expertise related to such characteristic.

held at the mean level of our sample, switching from a non-VC director firm to a VC director firm implies an increase in the wealth-to-volatility sensitivity by approximately 22%. In column (2), the coefficient of the number of VC directors on the compensation committee is 0.225 and is significant at the 1% level. In column (3), the percentage of VC directors on the compensation committee is significantly positive (0.851) at the 1% level. In columns (4)-(6), we follow Celikyurt et al. (2014) and include state-level controls on educational attainment, per capita income, and per capita R&D expense. The results are similar in magnitude but even stronger. In columns (7)-(9), we use state fixed effects instead of state-level controls, and the results are again consistent. Our estimated coefficients are similar to those reported in earlier studies for other control variables. Firms with larger assets, market-to-book ratios, ROA, and R&D Intensity are associated with higher risk-taking incentives. These results show that the presence of VC directors on the compensation committee has a positive impact on CEO risk-taking incentives (vega). As controls, we include variables significant from prior literature (Coles, Daniel, and Naveen, 2006). As is standard in previous literature, we control for the CEO delta. The signs of the control variable coefficients for CEO tenure, firm sales, market-to-book ratio, book leverage, and R&D intensity are consistent with those in prior literature.

****** Insert Table 11 about here ******

4.1.2. VC directors and CEO pay-performance sensitivity

We perform a similar analysis for CEO pay-performance sensitivity (delta). Estimates of equation (3) contained in Table 12 are consistent with our prediction that CEO delta is positively associated with the presence of a VC director on the compensation committee.

$$log\ (CEO\ Delta)_{i,t+1} = \alpha + \beta * VC\ Directorship_{i,t} + Controls_{i,t} + \gamma * Year\ dummies + \delta * Industry\ dummies + \varepsilon_{i,t} \quad (3)$$

In terms of economic magnitude, for a representative firm with CEO compensation held at the mean level of our sample, switching from a non-VC director firm to a VC-director firm implies an increase in the

CEO wealth-to-price sensitivity by approximately 30%-31%, depending on control variables. Similar to Table 11, CEO delta results become stronger after adding state-level controls or state fixed effects. We also note that the coefficient estimates on the control variables are consistent with those in prior literature.

****** Insert Table 12 about here ******

However, is the impact of the VC director present only when they are part of the compensation committee? To test, we also explore whether having VC directors on other committees has a similar impact. Table 13 reports the effect of the presence of VC directors outside the compensation committee (such as the audit, governance, or nomination committee) on the CEO vega and delta. All the coefficients of non-compensation VC directorship are insignificant, implying that the VC director effect on compensation is exclusively achieved through their membership of the compensation committee. Of note is the fact that in columns (1) and (5) we see that the VC directors who do not have a compensation committee membership have no impact on the CEO vega and delta. Moreover, their impact on CEO compensation through other committees is also insignificant. These results further confirm that the major impact of VC directors on managerial incentives is through compensation contracts.

****** Insert Table 13 about here ******

4.2. The impact of VC reputation on CEO vega and CEO delta

It has been widely documented that VC reputation is an important influence on VC investment. IPOs backed by more reputable VC firms are more likely to exit successfully, access the public market faster, and have better post-IPO firm performance. More reputable VCs also engage more in the corporate governance of their portfolio companies and have higher monitoring ability (Nahata, 2008; Krishnan et al. 2011; Chemmanur, Krishnan, and Nandy, 2011; among others). Therefore, we hypothesize that VC directors from more reputable VC firms have a stronger impact on CEO incentives. We examine this hypothesis using equation (4), where the variables of interest are the interaction terms between *High VC Reputation* dummy variable and VC directorship variables. To measure VC reputation, we assign a VC firm as a high reputation VC if its Lee-Pollock-Jin (2011) VC reputation index is 70 or higher.[29] Since the index is only available until 2010, we assign the 2010 index value to all following years.

$$log\ (CEO\ Vega\ or\ Delta)_{i,t+1} = \alpha + \beta_1 * High\ VC\ Reputation_{i,t} * VC\ Directorship_{i,t} + \beta_2 * VC\ Directorship_{i,t} + \beta_3 * High\ VC\ Reputation_{i,t} + Controls_{i,t} + \gamma * Year\ dummies + \delta * Industry\ dummies + \varepsilon_{i,t} \quad (4)$$

****** Insert Table 14 about here ******

We present the results in Table 14. In Panel A columns (3)-(9) the unconditional effect of VC director's compensation committee membership is significant. The interaction term between *High VC Reputation* and VC directorship variables are all significant at a 1% level across different specifications in models (1)-(9). Taking column (1), for example, a VC director from a reputable VC firm will increase the CEO vega by 70% compared with a VC director from a non-reputable VC firm. Similarly, in Panel B, the impact of VC reputation on CEO delta is also highly significant. For example, in column (1), the increase of CEO delta is 104% higher in a firm with highly reputable VC directors than a firm without. These findings are consistent with the prior literature and add new evidence that high reputation VCs involve more in the corporate governance of non-VC-backed mature firms. Moreover, they show that VC directors' affiliation with higher reputation VCs has stronger effect on the CEO incentives such directors implement.

4.3. Cross-Sectional heterogeneity of VC directors' impact on executive compensation

Table 15 explores heterogeneity in the effect of VC directors on executive compensation. We consider variation in a series of factors that are correlated with directors' roles in the firm, such as corporate governance, institutional ownership, independent directors, business segments, and the extent of CEO power.

[29] We thank Professor Pollock for providing access to his data at https://www.timothypollock.com/vc-reputation-index.

Specifically, we test whether the magnitude of the VC directors' effect on CEO incentives varies depending on the E index (Bebchuk, Cohen, and Ferrell, 2009), institutional ownership (He, Huang, and Zhao, 2019), CEO pay slice, CEO age, number of business segments, and independent board members (Knyazeva, Knyazeva, and Masulis, 2013). We categorize all firms into groups either below or above the median of the abovementioned factors by year and report the results in Table 15. In models (1) and (2) of both panels, we find that the VC directors' impact on CEO vega and delta is more pronounced for firms with a lower E index, indicating that VC directors install stronger CEO risk-taking incentives in better-governed non-VC backed firms. This is consistent with the findings in Hochberg (2012) that public firms backed by VCs have better governance that enhances the reputation of the sponsor VC. In models (3) and (4), we find that VC directors' impact on CEO incentives is stronger if firms have higher institutional ownership. One interpretation of this finding is that institutional investors value the opinions of VC directors in their voting decisions. In terms of CEO pay slice, which is defined as the proportion of the CEO total compensation in the total compensation of top five highest paid executives, a lower pay slice implies lower ability of the CEO to extract rents thus indicating better governance quality. Consistent with Hochberg (2012) therefore, the results from models (5) and (6) show that VC directors' impact is stronger when the CEO pay slice is lower. This is also consistent with Bebchuk, Cremers, and Peyer (2011) finding of firms with lower CEO pay slice having better performance. Other results show that VC directors have a more substantial impact on CEO vega and delta in firms with younger CEO age, fewer business segments, and a lower percentage of independent board members.

****** Insert Table 15 about here ******

4.4. VC directors and the level of CEO compensation

Other than examining CEO vega and delta, we also explore the impact of VC directors on a variety of CEO compensation components, including total compensation, excess compensation, cash pay, stock pay, option pay, inside debt, and termination pay. In Table 16, we document that the firms with VC directors on the compensation committee, on average, change CEO compensation by increasing the level of excess compensation, total compensation, and option compensation.[30] The increase in total and excess compensation is consistent with the prior observation that VC-director firms are also larger ones.

****** Insert Table 16 about here ******

4.5. VC directors and CEO performance goals

To explore how VC directors influence CEO incentives, we investigate the performance goals of CEO equity grants. First, we collect all the absolute and relative performance goals of CEO equity grants from the ISS Incentive Lab dataset. A grant can have one or more performance goals, and each performance goal can have one or more measurement periods. Second, we categorize all performance goals into three groups based on goal metrics. Group one are profitability metrics, including "EPS", "EBITDA", "earnings", "ROIC", "ROE", "EBT", "profit margin", "EBIT", "FFO" (i.e., funds from operations), "EVA", "ROA", "net income", "ROI", "NOI", "gross profit", "ROC", "ROS", or "cost reduction"; group two are growth metrics, including "sales", "operating income", "cashflow", "gross revenues", "same store sales", "IPO of subsidiary", "book value", "sales contracts", or "working capital"; and group three are stock market-based metrics, including "stock price" and "total shareholder returns" (i.e., TSR); all other metrics go to the unassigned group.[31] Importantly, for all three groups, we include both the level and the growth of the relevant metrics (e.g., we include in group two sales level and sales growth as performance goals). Third, we construct a grant-level sample, and define three goal groups' indicators as one if a grant has at least one performance goal with a metric in the respective group in a given fiscal period, and zero otherwise.

In Table 17 Panel A, Part I, we use the grant-level sample and regress the goal group indicators on the VC directorship variables. Since our grant goals panel could include more than one grant per firm, we include CEO fixed effects instead of industry fixed effects. We further include a year fixed effect. We find that firms with VC directors on the compensation committees are more likely to use growth goals and are

[30] When we examine the option pay using pre-2006 and post-2006 (including 2006) subsamples, option pay is more significant in pre-2006 subsamples.
[31] See Appendix D for the distribution and categorization of absolute and relative performance goals.

less likely to set profitability or market-based goals. These findings are consistent with Puri and Zarutskie (2012), who show that the key firm characteristic on which VC focuses is scale or potential for scale, rather than short-term profitability. When we look at individual goals (instead of groups of goals), we confirm that VC-director focus on "sales" goals exclusively. Importantly, in comparing of profitability and growth goals alone, we find that VC-director do not trade these goals off (i.e., there is no substitution of greater growth goals' likelihood for lower profitability goals' likelihood). These findings are further confirmed into Panel A, Part II, whereby we relate the principal companent of the top three - in numerosity - growth goals (i.e., sales, operating income and cash flows) and the top three profitability goals (i.e, EPS, earnings and EBITDA) goals. Our results are confirmed and also statistically stronger, as expected, given that the principal component per goal group is able to capture the common components within each group.

In Panel B, we investigate the likelihood of VC directors using absolute and relative performance goals. *Absolute Goals* is defined as one if a grant has at least one absolute performance goal, and zero otherwise. *Relative Goals* is defined as one if a grant has at least one relative performance goal, and zero otherwise. In models (1)-(6), we include all the grants, and we find that VC directors tend to stay away from relative goals. In models (7)-(9), we remove the grants with both absolute and relative performance goals, and we find that when choosing between absolute and relative goals, VC directors are more likely to use absolute goals. These results together suggest that VC directors are more likely to use absolute rather than relative performance goals, therefore making the CEO bear the industry risk consistent with VC compensation practices for startups with founders bearing the idiosyncratic risk of the firm.[32]

****** Insert Table 17 about here ******

[32] We also examine whether VC directors selectively pick peer group companies (Bizjak, Lemmon and Ngyuen, 2011). We do not find such evidence using peer group data for benchmark compensation comparisons or peer group data for relative performance goals. However, using annual industry peer groups, we find that the firms with VC directors are of larger size as compared to peers.

4.6. Addressing endogeneity concerns

4.6.1. Instrument variable analysis

Most corporate decisions are nonrandom. In our context, VC directors on the compensation committee may be elected by the firms that decide to improve CEO compensation, and a firm compensation decision might be influenced by the CEO network. To control for such potential endogeneity issue, we employ two groups of instruments in the 2SLS model and the dynamic GMM model. The first group of instruments are constructed based on the industry-year level VC dry powder, which is defined as the investible but un-invested VC capital (Aggarwal et al. 2022). We posit that it captures the demand by VC investors for new technology unavailable in public markets. It stands to reason that demand for such technological expertise will be greater on the boards of public companies, hence the anticipated positive relationship of this proxy with the VC director board memberships. The second group of instruments are firm-year level indicators of whether there are direct flights from a company's headquarter location to the three main VC hub cities - San Francisco, New York City, and Boston (Giroud, 2013; Bernstein, Giroud, and Townsend, 2016). The rationale of using direct flight indicators is that venture capitalists can reduce travel time and thus have lower monitoring cost if there are direct flights available between VC firms' location and company headquarters' location, and therefore companies are more likely to have VC directors on board. Moreover, the availability of direct flights does not directly affect companies' compensation design. To construct such indicators, first, we collect the zip code data of all US domestic airports from the Federal Aviation Administration (FAA) website. Second, for each company's headquarter, we get all the airports within 50 miles of the headquarters location by zip code match, and then select the top five home airports by traffic. Third, we use the T-100 aviation segment data collected from the Bureau of Transportation Statistics (BTS) website and get the direct flights with the origin (destination) being one of the headquarter's home airports and the destination (origin) being San Francisco, New York City, or Boston. For any firm-year, we define the direct flight indicator as one if there are direct flights in all twelve months in a year both from a home airport to a hub city and from a hub city to a home airport, and zero otherwise.

In Panel A of Table 18 we report the 2SLS analysis results for both vega and delta. In the first stage, we regress the endogenous VC directorship variables to a group of instruments, which include direct flight indicator from/to San Francisco, direct flight indicator from/to Boston, VC dry power, and VC dry powder squared. In the second stage, we regress CEO vega or delta on estimated VC directorship variables together with the baseline and state-level controls. The second stage results are consistent with our baseline results in Tables 11 and 12. The Cragg-Donald F-stats indicate that our instruments pass the weak instrument test. The Kleibergen-Paap p-values show that the models are not under-identified.

In Panel A, we produce non-robust Sargan tests of overidentification. We note that all six tests are over-identified, and discover that the overidentification is due to the indicator for a direct flight to and from San Francisco to the headquarter of the firm in focus. This implies that firms in locations with direct flight to San Francisco not only tend to have a greater supply of VC directors but also that such firms tend to have higher vegas to start with. To evaluate if the system is overidentified, we remove the direct flight to San Francisco indicator and re-estimate. The results are presented in Table 18 Panel B. In order to be able to estimate a robust Sargan test, it is required that we remove clustering. We do so and produce the estimates in that panel. We note that our results are unchanged and all Sargan tests indicate no overidentification.

In Panel C, we replace industry fixed effects with firm fixed effects. An instrumented VC director variable has greater variability due to the use of predetermined control variables that are firm-specific, and thus allows us to examine the impact of VC directors using firm fixed effects. Our results survive after adding firm effects, especially for CEO vega.

In Panel D of Table 18, we continue the IV analysis with a dynamic GMM model[33]. The coefficients of VC directorship variables across the various specifications are consistently positive, and mostly

[33] Appendix E shows the first stage results. The instruments we employ here include direct flight indicator from/to San Francisco, direct flight indicator from/to Boston, VC dry powder squared, and the minimum distance from company headquarter to any of the top 50 VC cities in the US.

statistically significant. If our instruments are valid, this indicates that our main result is robust to incorporating simultaneous dynamic endogeneity and unobservable heterogeneity.

****** Insert Table 18 about here ******

4.6.2. Matched sample analysis

Next, we employ the propensity score matching (PSM) method, which has become a standard and commonly employed methodology for making causal inferences using observational data that are not produced by controlled experimental settings (e.g., Rosenbaum and Rubin, 1983, 1984). We use the firms with VC directors on board as treated firms in our setting. We match the treated and control firms based on the fiscal year, industry, firm size, CEO vega at time t-1, CEO delta at time t-1, and further control variables[34]. The purpose is to broadly control for firm characteristics and other unobservable factors that may affect the possibility of appointing a VC director. We show in Table A12 that the treated and control groups are indistinguishable along most of the observable control variables, and in particular the lag CEO vega and the lag CEO delta.

Table 19 reports the panel data estimates using the matched sample. Our findings align with those obtained in the baseline panel regressions. Taking column (1) in Panels A and B, for example, the presence of VC directors on the compensation committee increases CEO vega (delta) by 21% (30%). We also note that the results are robust to a battery of fixed effects and controls. For example, in columns (1)-(3) we repeat our main specifications from Table 11 models (1)-(3) with industry and year fixed effects and baseline and no state-level controls. In columns (4)-(6) we include the state controls and in columns (7)-(9) we exclude state controls and include state fixed effects. Overall, our results are consistent across all specifications and are particularly increased in significance when we add controls for state fixed effects.

[34] Other controls in the PSM procedure include leverage, market-to-book ratio, ROA, R&D intensity, tangibility, annual stock return, annual stock volatility, and firm age.

The robust results suggest that non-random assignment of VC directors on the compensation committee does not explain our findings.

****** Insert Table 19 about here ******

4.7. The impact of VC directors on corporate innovation

By examining the board appointments of VC directors on mature public firms, Celikyurt et al. (2014) illustrate that VCs play a broader role than just providing finance, monitoring, and advice for small private firms. In particular, they document that VC directors serving on the board of mature public firms promote corporate innovation, which is measured by R&D intensity and innovation outputs. We argue that such impact can be partly explained by increased CEO risk-taking incentives instilled by VC directors. We use the specification in equation (5) to examine the effect of the interaction between the VC director indicator and CEO vega or delta. The dependent variable is R&D intensity or an innovation outcome measure based on patenting activity. Following Celikyurt et al. (2014), we control for size, leverage, market-to-book ratio, ROA, intangible assets, firm age, board size, and Tobin's Q. If VC directors influence corporate innovation partly through increasing CEO incentives, the interaction term between the VC director indicator and CEO vega or delta should be positive and significant.

$$Innovation\ Measures_{i,t+1} = \alpha + \beta_1 * VC\ Director\ on\ Comp.Committee_{i,t} * log(CEO\ Vega\ or\ Delta)_{i,t} + \beta_2 * VC\ Director\ on\ Comp.Committee_{i,t} + \beta_3 * log(CEO\ Vega\ or\ Delta)_{i,t} + Controls_{i,t} + \gamma * Year\ dummies + \delta * Industry\ dummies + \varepsilon_{i,t} \quad (5)$$

First, in Table 20, we present the results of R&D intensity, which is calculated as R&D expense scaled by total assets. The interaction terms of VC director's dummy and CEO vega or delta are significantly positive at the 5% level, indicating that the positive impact of VC directors on R&D intensity is stronger when CEOs have higher incentives.

****** Insert Table 20 about here ******

Next, we examine the impact of VC directors on several innovation output measures. The leading number of patents is the number of patents that a firm files in year *t+1*. The forward five-year number of citations is the sum of citations from year *t+1* to year *t+5* after a patent is filed. The leading innovation value (real and nominal) is the innovation's economic value in year t+1 developed by Kogan et al. (2017). The forward five-year innovation value (real and nominal) is the sum of innovation's economic value from year *t+1* to year *t+5*. The data on patents, citations, and innovation value are collected from Kogan et al. (2017)[35] and the PatentView website[36]. Because corporate R&D investment takes years to generate innovation outcomes, we use the lagged CEO vega to investigate the impact of CEO incentives on innovation output. Specifically, the lagged CEO vega is calculated as the average of CEO vega from year t-3 to year t-1. Following Celikyurt et al. (2014), we employ the Tobit models to account for the truncation bias in the patent data. Following Hall, Jaffe, and Trajtenberg (2001), all measures are adjusted for year and industry to correct for truncation in the patent data. For example, the number of patents is adjusted by dividing the number of patents obtained by firm *i* in year *t* by the mean number of patents in the same industry-year cohort to which firm *i* belongs. Similar adjustments are made for all other output measures.

****** Insert Table 21 about here ******

The results presented in Table 21 show that the interaction term is statistically significant in five out of six measures in both panels. For example, compared to a firm without VC directors, a firm with VC directors has more patents issued per year, and a significant portion of this increase is through stronger CEO incentives. Similarly, the real economic value of innovation increases if a firm has VC directors, and a significant portion of this increase comes through higher CEO vega and delta. These results further confirm that the impact of VC directors on corporate innovation is stronger when CEOs have higher incentives. Therefore, higher risk-taking incentives are one possible channel through which VC directors influence corporate innovation in mature public firms. In passing, we remark that the controls in Table 21 exclude

[35] Data available at https://github.com/KPSS2017/Technological-Innovation-Resource-Allocation-and-Growth-Extended-Data.
[36] PatentView website provides the patent citation data by year.

R&D intensity, annual stock return and annual stock volatility, CEO age, CEO tenure, CEO duality, percent of independent board members, institutional ownership concentration and institutional ownership total, as well as CEO delta.

4.8. The impact of VC directors on CEO forced turnover

To further explore the presence of VC directors on corporate governance, we examine whether the likelihood of forced CEO turnover is higher if firms have VC board members. We collect the forced CEO turnover data from Professor Florian Peters' website[37]. Given that CEO forced turnovers are more likely to happen during a bad performance (Peters and Wagner, 2014, Jenter and Kanaan, 2015), we interact the VC director dummy variables with the abnormal stock return, as shown in equation (6). *Abnormal return* is the industry and year adjusted excess return calculated by subtracting the S&P 500 annual return from the annual stock raw return. Rather than focusing on the compensation committee, we examine how VC directors impact the nomination and/or governance committees.

$$CEO\ Forced\ Turnover_{\ i,t} = \alpha + \beta_1 * VC\ Directorship_{\ i,t} * Abnormal\ Return_{i,t} + \beta_2 * VC\ Directorship_{\ i,t} + \beta_{\mathbf{3}} * Abnormal\ Return_{i,t} + Controls_{i,t} + \gamma * Year\ dummies + \delta * Industry\ dummies + \varepsilon_{i,t} \quad (6)$$

****** Insert Table 22 about here ******

Results from equation (6) are presented in Table 22. Having VC directors on the nomination and/or governance committee is associated with a higher possibility of forced CEO turnover when firms have bad stock performance. Economically, compared with a firm without VC directors on the nomination and/or governance committee, a firm having such directors is 5% more likely to have a forced CEO turnover conditional on abnormal performance. These findings further confirm that VCs provide monitoring to mature public firms and facilitate their corporate governance.

[37] We thank Professor Peters for providing access to his data at https://www.florianpeters.org/data/

4.9. Robustness checks

An important difference between Celikyurt et al. (2014) and our paper is that we define VC directors as those who have VC experience *before* they are appointed as directors. In contrast, ordinary directors who, at some point in time during their tenure with the company, acquire affiliation with a VC firm are not considered as VC directors in our analysis. We define those as "coincidental" VC directors. Coincidental VC directors do not convey the same concept, and therefore, we expect no effect from those VC directors on executive compensation.

In Table 23, we show the placebo tests of our baseline regressions if 1) only coincidental VC directors are included in the analysis, and 2) if both coincidental VC directors and the VC directors with prior VC experience are included. We find that the effect only comes through VC directors who have prior VC experience, indicating that the impact of VC directors on CEO incentives is indeed through their VC expertise, rather than their coincidental VC affiliation.

****** Insert Table 23 about here ******

To that matter, we remark further that our sampling of VC directorships differs materially from Celikyurt et al. (2014). First, we exclude in our tests VC-backed firms as the VC directorships in such firms may be a consequence of the VC sponsorship in its early life. Second, we exclude any VC directors that are coincidental, i.e., acquire a VC affiliation after the public firm's board membership start date. Our conservative approach causes a substantial reduction in the number of unique firms with VC directors in our regression sample (i.e., 104). Therefore, we verify that our results are consistent when we include coincidental directors and when we include VC-backed firms in line with Celikyurt et al. (2014). For example, when we include VC-backed firms, and non-coincidental directors exclusively, the number of unique firms with VC directors is raised to 147. The results are in the Table A9 Panels A and B and consistent with our main specifications. Taking model (1) for example, CEO vega (delta) increases by 18% (21%) if a firm has a VC director on the compensation committee. Similarly, when we include both coincidental and non-coincidental VC directors in non-VC-backed firms, the number of unique firms with

VC directors is raised to 234, and the results are again consistent with the main specifications. As presented in Table A9 Panels C and D, CEO vega (delta) increases by 13% (15%) if a firm has a VC director on the compensation committee. Lastly, when we include both (i) VC-backed and non-VC-backed firms, and (ii) coincidental and non-coincidental VC directors, there are in total 395 unique firms with VC directors. The results are presented in the Table A9 Panels E and F and are consistent with our main findings. CEO vega (delta) increases by 11% (15%) if a firm has a VC director on the compensation committee.

5. Conclusions

We analyze how VC directors affect the executive compensation policies of mature public firms. Our results indicate that firms with VC directors on the compensation committee are more likely to increase CEO risk-taking incentives and pay-performance sensitivity. On average, having VC directors on the compensation committee increase the CEO vega by 23% and increase CEO delta by 30%. Such effects are more pronounced if directors are from highly reputable VC firms. The increase in CEO vega is more significant when firms have worse governance, higher institutional ownership, a lower CEO pay gap, fewer business segments, and a lower percentage of independent board members. Moreover, the increased CEO incentives – in the presence of VC director(s) – also lead to increased corporate innovation, which is a possible channel to explain the results in Celikyurt et al. (2014). In addition, the positive impact of VC directors on corporate governance is confirmed by the higher likelihood of forced CEO turnover when VC directors are on board.

Our work contributes to the literature on the effect of director experience on corporate policy. Several fruitful avenues exist for future research. One would be to consider the value of VC directors in other settings, such as securities offerings that require helps from venture capitalist.

Chapter 3

Do Corporate Insiders Take Advantage of Their Political Connections? Evidence from Insider Trading

1. Introduction

This paper studies whether political connections affect corporate insiders' trading behavior. Using insiders' employment data, I investigate whether corporate insiders who have current or prior positions in government agencies tend to take advantage of their political connections and trade opportunistically.

In the corporate finance literature, the impact of political connections on firm value is widely examined. Political connections tend to enhance valuations of connected companies, but at the cost of the overall economy and financial system (Goldman, Rocholl, and So, 2009; Cooper, Gulen, and Ovtchinnikov, 2010; Duchin and Sosyura, 2012; Houston et al., 2014; Gao and Huang, 2016; Megginson, 2017; among others). However, there is not much evidence on how political connections affect corporate insider behaviors, especially insider stock transactions. In the insider trading literature, it has been established that insider trades are informative (Seyhun, 1988, 1992; Lakonishok and Lee, 2001; among others). Trades made by certain groups of insiders are more informative than the rest (Ravina and Sapienza, 2010; Wang, Shin, and Francis, 2012; Cohen, Malloy, and Pomorski, 2012; Ali and Hirshleifer, 2017; Cline, Gokkaya, and Liu, 2017; Akbas, Jiang, and Koch, 2020; among others). Therefore, examining trades made by politically connected insiders (hereafter PC insiders) and how they differ from non-politically connected insiders (hereafter non-PC insiders) is an important question to explore.

There is evidence from a single event showing that political connections provide corporate insiders with an information advantage that can facilitate opportunistic behavior by insiders. Jagolinzer et al. (2020) and Akin et al. (2019) examine financial institutions during the 2007-2009 financial crisis, and Bourveau, Coulomb, and Sangnier (2016) examine the 2007 French presidential election. They all document that insiders with political connections experience abnormal returns and make informed trades. In a general setting, Harvison (2020) employs the political contribution data and finds that PC insiders are more likely to make informative sales than non-PC insiders.

However, insiders are subject to high legal risk if they take advantage of political connections and trade on private information (Netter, Poulsen, and Hersch, 1988; Bhattacharya and Daouk, 2002; Seitzinger, 2016; among others). Such risk will be even higher if insiders hold current or prior positions in government agencies. Therefore, whether PC insiders tend to trade on their information advantage and generate abnormal returns compared with non-PC insiders is a theoretically ambiguous question and needs to be empirically tested.

By examining transaction-level data over 1990-2017, I find that PC insiders are more cautious about potential legal risk, and their trading behavior is less opportunistic compared to non-PC insiders. The regression analysis shows that the cumulative abnormal returns (CARs) over 30-day window following PC insider purchases are significantly (annualized 10.8%) lower than CARs of purchases by non-PC insiders, whereas CARs following PC insider sales are significantly (annualized 8.4%) higher than CARs of sales by non-PC insiders. The results are similar if I extend the 30-day CAR window to longer horizons. By comparing purchases with sales, I find that after adding firm fixed effects, CARs of purchases by PC insiders remain significantly lower than that of purchases by non-PC insiders, while the effect is no longer significant for sales. These results are consistent with the cautious PC insider hypothesis.

Next, to explore whether PC insiders perform differently in trading behavior compared with non-PC insiders, I follow Akbas, Jiang, and Koch (2020) and Cohen, Malloy, and Pomorski (2012) to examine whether PC insiders have longer trading horizons and are more likely to make routine trades. Using the order flow-based insider investment horizon measure created in Akbas, Jiang, and Koch (2020), I document that PC insiders tend to trade in longer horizons than non-PC insiders. Using the definition of routine trades in Cohen, Malloy, and Pomorski (2012), I find that PC insiders are more likely to make routine trades than non-PC insiders. In addition to the previous transaction-level results, both findings here provide supporting evidence for the cautious PC insider hypothesis at the insider level.

Last, I employ difference-in-difference (DID) methods to examine the effectiveness of the Stop Trading on Congressional Knowledge (STOCK) Act passed in April 2012, which aims to reduce insider trading by Congress members and staff by prohibiting them from using private information derived from their official positions for personal benefit and other purposes. My sample of corporate insiders with Congressional positions can effectively examine whether the Act functions as expected on corporate insiders. I document that purchases (sales) made by Congress members/staff are associated with more negative (positive) subsequent CARs after the passage of the STOCK Act, compared with non-Congress insiders. These findings show that the STOCK Act is effective in reducing Congress members/staff using their potential information advantage to generate abnormal trading profits.

The contributions of this paper are threefold. First, this is the first paper to examine the impact of corporate insiders' *direct* political connections on their trading behavior in general, by using corporate insiders' employment data. Prior literature using political contribution data does not capture insiders' direct connections. Second, while the existing literature shows that PC insiders use their information advantage to make informed trading around special events such as elections and government bailout during the financial crisis, this is the first paper to provide both transaction-level and insider-level evidence that corporate insiders with political connections are in general more cautious in their trading behavior compared with other insiders. Third, I add new evidence to the literature showing that the STOCK Act is effective in reducing insider trading behavior by Congress members/staff if they are corporate insiders.

The rest of the paper is organized as follows. Section 2 reviews the related literature and develop hypotheses. Section 3 introduces the data and sample construction. Section 4 presents the empirical design and discusses the empirical results. Section 5 concludes.

2. Literature Review and Hypothesis Development

2.1. The literature on political connections

At least since Fisman (2001), many researchers have studied the scope, mechanisms, and private versus social valuation impact of political connections between politicians and owner/managers of companies in the private sector. The principal conclusion from these studies is that political connections tend to enhance valuations of connected companies, but these private benefits are usually associated with significant costs for the overall economy and financial system.

Many prior studies investigate the impact of political connections on corporate outcomes using the data from China. The critical role that China's government play in both state-owned and privately-owned enterprises makes China a unique setting to study political-related economics questions. Fan, Wong, and Zhang (2007) show that companies with politically connected CEOs underperform those without by almost 18% based on 3-year post-IPO stock returns, and have poorer earnings growth, sales growth, and changes in return on sales. Calomiris, Fisman, and Wang (2010) examine a unique setting of two consecutive announcements of sale of government ownership and show that the benefits of political ties outweigh the efficiency costs of government shareholdings. Hung, Wong, and Zhang (2012) find that compared to non-politically connected firms, Chinese state-owned enterprises (SOEs) with strong political connections are more likely to list overseas but have worse listing performance. Using the initial public offering (IPO) data from China, Piotroski and Zhang (2014) document that incentives created by the impending turnover of local politicians can accelerate the pace of IPO activity. Cull, Li, Sun, and Xu (2015) use government appointment of CEOs and state ownership as proxies for firms' government connections and show that investment in firms with strong government connections is less sensitive to internal cash flows than investment in other firms. Liu, Shu, and Wei (2017) use the event of the Bo Xilai political scandal in China as an exogenous shock to identify the impact of political uncertainty on asset prices. They document significant price declines for all stocks upon news of the Bo scandal, and significantly greater declines for stock prices of firms most connected to government policies. Return volatility also increased most for policy-sensitive firms after the scandal hit. Chen, Li, Luo, and Zhang (2017) construct a unique political connections index capturing variations in the strength of firm political relations in China and find an overall negative relation between index level and firm value for the full sample driven by SOEs. In addition to the evidence from China, Khwaja and Mian (2005) use the corporate lending data from Pakistan to investigate rents to politically connected firms in banking. They document that connected companies borrow 45% more than unconnected borrowers, but also have 50% higher default rates. Such preferential treatment only occurs in government owned banks. The economy-wise cost of these rents is estimated as 0.3 to 1.9% of GDP each year.

Although the corporate governance system in the United States is fundamentally different from China, the findings on political connections and their impact on corporate outcomes are similar. Goldman, Rocholl, and So (2009) employ the political position and political contribution data of board members of the S&P 500 companies over 1996-2000 and show a positive abnormal return following the announcement of the nomination of a politically connected insider to the board. They also document that the Republican win in the 2000 presidential election caused the value of Republican connected firms to increase, but values of Democratic connected firms to decrease. Cooper, Gulen, and Ovtchinnikov (2010) use firm-level campaign contribution data over 1979-2004 to document that political contributions are positively correlated with cross-sectional future stock returns. Duchin and Sosyura (2012) use the data of financial institutions applying for government fund support under the Troubled Asset Relief Program (TARP) during the financial crisis and find that politically connected firms are more likely to receive funding than are non-connected firms. Mills, Nutter, Schwab (2013) find that politically sensitive contractors pay higher federal taxes, but firms with greater bargaining power incur fewer tax-related political costs. Houston et al. (2014) examine the impact of political connections on the costs and terms of loan contracts. They document that the cost of loans is significantly lower for companies that have board members with political ties. Smith (2016) uses the corruption convictions from each U.S. federal judicial district and documents that companies headquartered in corrupt locales hold significantly less cash and have greater leverage than firms in less corrupt locales. Gao and Huang (2016) construct a hedge fund holding database and use lobby disclosure information to document that hedge funds with connections to lobbyists tend to tilt their holdings and trading towards politically sensitive stocks, and connected hedge funds perform significantly better on their political holdings than on non-political holdings. Faccio and Hsu (2017) find evidence of higher job-creation, on average, at establishments operated by targets of politically connected private equity firms than at establishments operated by targets of nonconnected private equity firms.

2.2. The literature on insider trading

It has been established in the insider trading literature that insider trades are informative. Seyhun (1988) investigates the information content of aggregate insider trading using open market insider transactions from 1975 to 1981. He finds that net aggregate insider trading activity in a given month is significantly positively correlated with the market return during the subsequent two months. In his follow-up study of aggregate insider trading, Seyhun (QJE, 1992) examines why aggregate insider trading predicts future stock returns. He finds that up to 60% of variations in one-year-ahead aggregate stock returns can be explained by insider transactions. The predictive ability can be attributed to both changes in business conditions and movement away from fundamentals. Lakonishok and Lee (2001) investigate whether insider trades are informative using all companies traded on NYSE, Amex, or Nasdaq over 1975-1995. They document that in general there is very little market movement when insiders trade and when they report to the SEC. Insiders can predict cross-sectional stock returns, especially for small firms. They are also the first to document that insider purchases are more informative than insider sales. Insider sales appear to have no predictive power.

Several papers study the characteristics of insiders and find that trades made by certain groups of insiders are more informative than the rest. Ravina and Sapienza (2010) find that when independent directors purchase stock, they earn positive abnormal returns similar to returns earned by other executives. When they sell, independent directors also earn significant abnormal returns before bad news and around earnings announcements. They also document that executives and independent directors make higher returns in firms with weaker governance. Wang, Shin, and Francis (2012) investigate whether trades made by CFOs reveal more information than CEOs. They find that compared with CEOs, CFOs earn significantly higher abnormal returns after purchasing company shares. Cohen, Malloy, and Pomorski (2012) create measures to differentiate routine insider trades and opportunistic insider trades. Routine traders are insiders who place a trade in the same calendar month for at least three consecutive years in the past, and opportunistic trades are everything else. They find that only the opportunistic trades are informative about firms' future performance. Ali and Hirshleifer (2017) examine opportunistic insider trading and its relationship with insider profits. They find that opportunistic insiders can be identified through the profitability of their trades before quarterly earnings announcements. Cline, Gokkaya, and Liu (2017) study the persistently profitable (PP) insiders and find the trades of PP insiders better predict firm performance than those of other insiders. Akbas, Jiang, and Koch (2020) study the horizon of insider investment and the information content of their trades. They find that the trades of short-horizon insiders are both more unexpected and more informed than those of long-horizon insiders. Short horizon insiders and their firms also tend to display attributes that are associated with a greater focus on short-termism.

2.3. Hypothesis development

In the literature, there is evidence around specific events showing that political connections provide corporate insiders with an information advantage that can facilitate opportunistic behavior by insiders. Jagolinzer et al. (2020) examine one particular industry, financial institutions, during the 2007-2009 financial crisis. They document that insiders with political connections experience abnormal returns prior to the infusion of the Troubled Asset Relief Program (TARP) funds. In a similar setting, Akin et al. (2019) find that insider buying in politically connected banks around the TARP announcement is associated with positive abnormal returns, while there is no such relation for unconnected banks. Bourveau, Coulomb, and Sangnier (2016) examine whether directors' political connections affect their behavior in financial markets using the 2007 French presidential election as an exogenous event. They document positive and significant two-day abnormal returns around the disclosure of purchases by politically connected directors. Harvison (2020) employs the political contribution data and examines the relation of political connections and insider trade informativeness in general. She finds that PC insiders are more likely to make informative sales than non-PC insiders, while there is no such relation for insider purchases.

Although PC insiders may have information advantages which they can use to make abnormal profits, they are also subject to high legal risk if they take advantage of political connections and trade on private information (Netter, Poulsen, and Hersch, 1988; Bhattacharya and Daouk, 2002; Seitzinger, 2016). Such risk is especially high if insiders hold current or prior positions in government agencies. Therefore, whether PC insiders tend to trade on their information advantage and generate abnormal returns compared with non-PC insiders is empirically ambiguous.

If PC insiders are truly opportunistic and tend to take advantage of their information despite the high legal risk, the abnormal returns subsequent to their purchases (sales) should be higher (lower) than returns of non-PC insiders. This is the opportunistic PC insider hypothesis. Alternatively, if PC insiders are sophisticated and cautious about potential legal risk, the abnormal returns subsequent to their purchases (sales) should be indifferent or lower (higher) than returns to non-PC insiders. This is the cautious PC insider hypothesis.

H1a (Opportunistic PC insider hypothesis): Purchases (sales) by PC insiders are associated with higher (lower) abnormal returns than non-PC insiders.

H1b (Cautious PC insider hypothesis): Purchases (sales) by PC insiders are associated with insignificant or lower (higher) abnormal returns than non-PC insiders.

Numerous studies examine the heterogeneity of insider trades and trading patterns. Akbas, Jiang, and Koch (2020) create an insider investment horizon measure based on insiders' average annual net order flow, and they document that trades of short-horizon insiders are both more unexpected and more informed than trades of long-horizon investors. Therefore, I expect that if PC insiders are opportunistic, then they are more likely to have longer investment horizons then non-PC insiders; on the contrary, if PC insiders are cautious, they are more likely to have shorter investment horizons than non-PC insiders.

H2a (Opportunistic PC insider hypothesis): PC insiders are more likely to have shorter investment horizons than non-PC insiders.

H2b (Cautious PC insider hypothesis): PC insiders are more likely to have longer investment horizons than non-PC insiders.

Another study investigating different trading patterns among insiders is Cohen, Malloy, and Pomorski (2012), which categorizes insider trades into routine trades and opportunistic trades based on historical trade timings. They document that opportunistic trades are more informative than routine trades. If PC insiders are opportunistic, I expect to find that they make more opportunistic trades than non-PC insiders; conversely, if PC insiders are cautious, I expect that they make more routine trades than non-PC insiders.

H3a (Opportunistic PC insider hypothesis): PC insiders are more likely to make opportunistic trades.

H3b (Cautious PC insider hypothesis): PC insiders are more likely to make routine trades.

In April 2012, the Congress passes the Stop Trading on Congressional Knowledge (STOCK) Act, which prohibits Congress members and employees from using private information derived from their official positions for personal benefit. The evidence on the effectiveness of the STOCK Act is very limited. Gao and Huang (2016) use hedge fund data and find that the outperformance of politically connected funds declines significantly after the STOCK Act becomes effective. Huang and Xuan (2018) examine US politicians' personal stock investment and provide evidence that after the passage of the Act, politicians no longer exhibit information advantage in trading. Although both studies show that the Act is effective, there is still no clear answer to whether the passage of the STOCK Act has an impact on politically connected corporate insiders. My research design is a suitable setting to answer this question. If the STOCK Act is effective, the subsequent returns for purchases (sales) made by Congress members/staff after April 2012 should be lower (higher) than before.

H4: After the passage of the STOCK Act, the abnormal returns subsequent to purchases (sales) of Congress members/staff are higher (lower) than before.

3. Data and Sample

I obtain data from multiple sources. Director employment and characteristics data are collected from BoardEx. Insider trading data are collected from Thomson Reuters Form 4 Fillings. Stock price and firm level financial data are obtained from CRSP and Compustat, respectively.

Following the insider trading literature, I remove insider transactions with the Thomson Reuter cleanse indicator in A or S categories.[38] I only use open market purchases or sales (transaction code in P or S) which can capture insiders' true intentions (Beneish and Vargus, 2002; Cheng and Lo, 2006; Lauren, Malloy, Pomorski, 2012; Akbas, Jiang, and Koch, 2020; among others). Another important type of insider transaction is option exercise. Because insiders exercise options at a previously determined price, their risk exposure will not be changed by option sales per se, but by subsequent sales of the shares acquired. Therefore, I do not include purchases of shares through the exercise of options. Furthermore, using the Thomson Reuters Amendment Concordance, I remove original filings if there are associated amendment filings available. All the transactions are then aggregated on the insider-firm-transaction date level. For example, if an insider makes multiple purchases or multiple sales on one transaction date, I calculate the aggregated shares that are sold or purchased on that date.

Following prior literature, corporate insiders are defined as officers, directors, or any beneficial owners with more than 10% stock ownership.[39] To identify whether a corporate insider is politically

[38] The Cleanse Indicator indicates Thomson's level of confidence concerning the accuracy of a particular record contained in the database. There are nine cleanse indicators. S and A are the two least reliable cleanse indicators. According to the TFN Insider Data Feed Manual, S indicates that "no cleansing attempted; security does not meet our collection requirements"; A indicates that "numerous data elements were missing or invalid; reasonable assumptions could not be made".

[39] To be more specific, I keep transactions made by corporate insiders whose role code is in CEO, CB, D, DO, H, OD, VC, AC, CC, EC, FC, MC, SC, AV, CFO, CI, CO, CT, EVP, O, OB, OT, P, SVP, or VP.

connected or not, I use the BoardEx employment file, and the screening criteria are based on Goldman, Rocholl, and So (2009). A corporate insider is defined as politically connected if he or she is a current or former member of a government agency when the transaction is made.[40] The advantage of using employment information rather than political contributions data to measure political connections is that employment information can capture the direct political or regulatory ties that a corporate insider has, which is not easy to infer from political contribution data. Those insiders who hold or held a position at government agencies are the ones who are most cautious about insider trading and most sensitive to any possible legal risk.

To construct the main sample, I start with all transactions made by corporate insiders in the Thomson Reuters Form 4 Filings database from 1990 through 2017, then I merge the employment information from BoardEx to insider transactions. I exclude transactions that have no CRPS or Compustat data available for associated stocks or firms. This procedure yields to 1,414,214 transactions aggregated on insider-firm-transaction date level and made by 148,107 unique corporate insiders in 12,843 unique firms. Using BoardEx employment information, I identify 37,078 transactions made by 2,855 unique PC insiders in 2,899 unique firms. Table 24 provides summary statistics of the transaction-level variables, both for the full sample and by PC insider indicator. In addition to the main sample, I also construct an insider-year level sample for the insider-level tests. Summary statistics are also reported in Table 24.

****** Insert Table 24 here ******

To measure the informativeness of insider transactions, I use both the market-adjusted return model (MAR) and the market model (MM) to calculate cumulative abnormal returns (CARs) subsequent to insider transactions over four different trading horizons, namely, CARs over 30-day, 60-day, 120-day, and 255-

[40] To be more specific, an insider is politically connected if he or she has or had a position in US congress, US Department of State, US Executive Office of the President (White House), US Securities and Exchange Commission (SEC), Central Intelligence Agency (CIA), Federal Emergency Management Agency (FEMA), Office of Management and Budget, Internal Revenue Service (IRS), Nuclear Regulatory Commission, United States Social Security Administration, Civil Rights Commission, Food and Drug Administration (FDA), United Nations (UN), presidential campaign, or president’s committee or council, or if he or she is a current or former state governor or mayor.

day windows. To make sure my results are robust to different market benchmarks, I use both equally-weighted (EW) and value-weighted (VW) market indices to calculate daily abnormal returns. To be more specific, CARs are calculated as

$$CAR_j[0,h] = \sum_{d=0}^{h}(R_j^d - R_{expected}^d) \tag{7}$$

where $CAR_j[0,h]$ is the sum of daily abnormal returns for firm *j* over horizon *h*, R_j^d is the daily raw return for firm *j* on day *d*, and $R_{expected}^d$ is the expected return for firm *j* on day *d*, which is the daily CRSP EW or VW market index return in the MAR, or the expected return derived from the MM regressions. MM requires that the estimation period ends 31 days before the transaction date and has 255 days in length, with a minimum of four days of trading. I report the summary statistics of MAR-EW CARs in Table 24, and the rest of CAR measures are summarized in Table A13.

As Table 24 panel A shows, 2.6% of transactions are made by PC insiders. Purchases and sales account for 28.3% and 71.7% of all transactions, respectively. Among PC insider transactions, 0.4% are made by those who are Congress members/staff at the time of the transaction. At firm-year level, there are 7.6 insiders who trade per firm-year, and a firm has on average 0.2 PC insider per year. 17.4% of firms have insider trades made by PC insiders. Among all insiders, 8.1% are CEOs, and 52.4% and 52.7% hold director and officer positions, respectively. Among insiders who have BoardEx data available, 5.7% have a Ph.D., 26.5% have an MBA, 9.9% have a J.D., 11.5% have a CPA, and 0.7% have a CFA. In Panel B, I report summary statistics of PC insider transactions and non-PC insider transactions. On average, compared with non-PC insiders, PC insiders are associated with higher subsequent CARs, tend to make more purchases and fewer sales, and tend to make trades larger in size. PC insiders are more likely to be directors and have a Ph.D. or J.D. degree, but less likely to be CEO, officers, or have an MBA, CPA, or CFA.

4. Empirical Design and Results

4.1. Baseline regressions

To examine the informativeness of politically connected insider purchases and sales separately, I follow Jagolinzer et al. (2020) and Harvison (2020) to apply the baseline regression model in equation (8) on the transaction-level sample,

$$CAR[0,h] = \alpha + \beta_1 PCinsider * Buy + \beta_2 PCinsider * Sell + \beta_3 Buy + \beta_4 PastMonthRet + \beta_5 PastYearRet + \beta_6 Size + \beta_7 BookMarket + \gamma_t + \gamma_j + \varepsilon \quad (8)$$

where *CAR[0,h]* is the cumulative abnormal returns over trading window [0,h] using multiple adjustment models and benchmarks, *PCinsider* is an indicator variable which equals one if a corporate insider is a current or former member of government agencies, *Buy* is an indicator variable which equals one if the transaction is a purchase, *Sell* is an indicator variable which equals one if the transaction is a sale, *PastMonthRet* is the raw return over the month *t*-1 if the transaction happens in month *t*, *PastYearRet* is the raw return over the past year from month *t*-2 to month *t*-12 if the transaction happens in month *t*, *Size* is the natural logarithm of market value at the end of month *t*, *BookMarket* is the book-to-market ratio at the end of month *t*, where book value is from the prior quarter-end, and γ_t and γ_j are year-month and firm fixed effects, respectively. The base group in the regression is the sales made by non-PC insiders.

I report the baseline regression results with year-month fixed effects in Table 25. Panel A shows the results of CAR[0,30] and CAR[0,60], and Panel B shows the results of CAR[0,120] and CAR[0,255]. In Panel A, compared with sales by non-PC insiders, purchases by PC insiders are associated with significantly negative subsequent returns at mostly 5% or higher significance level, whereas sales by PC insiders are associated with significantly positive abnormal returns. The coefficient of *Buy* is positive and highly significant, which means that purchases by non-PC insiders are associated with positive abnormal returns. This may indicate that, compared with PC insiders, non-PC insiders are less cautious and more likely to take advantage of private information. In terms of the control variables, CARs are higher when a firm has lower past month's return, lower past year's return, smaller size, and lower book-to-market ratio.

****** Insert Table 25 here ******

The CAR results are also economically significant. Taking CAR[0,30] measured by MAR EW for example, PC insider purchases are associated with the abnormal return of 0.9% less than non-PC insider purchases over the 30-day period, which is equivalent to -10.8% (-0.9%*12) annualized CAR; PC insider sales are associated with the abnormal return of 0.7% higher than non-PC insider sales over the 30-day period, which is equivalent to 8.4% (0.7%*12) annualized CAR.

CAR[0,120] and CAR[0,255] results in Panel B are similar but weaker than the results over shorter horizons. Purchases by PC insiders are negative and significant only for MAR adjustments, and sales by PC insiders are significantly positive for CARs over a [0,120] horizon, but not over a [0,255] horizon.

In Table 26, I report the baseline regression results with both year-month and firm fixed effects. In Panel A, the interaction terms of purchases are significant across all measures of CAR[0,30] and CAR[0,60]. However, the interaction terms of sales are not significant.

****** Insert Table 26 here ******

The findings that PC insider purchases are associated with negative abnormal returns while sales are associated with positive abnormal returns indicate that rather than taking advantage of private information they might have, PC insiders are sophisticated and cautious about the potential risk associated with insider trading. These findings support the cautious PC insider hypothesis.

Another noteworthy finding is that the t values of purchase interactions are generally higher than those of sale interactions across horizons and adjustment models, especially in Table 26 where firm fixed effects are included.

4.2. PC insider trading patterns

Next, I examine if PC insiders are systematically different from non-PC insiders in terms of their trading patterns. The findings discussed in previous sections indicate that trades by PC insiders are not as informative as those made by non-PC insiders. A number of recent studies in the insider trading literature investigate the heterogeneity of insider trading patterns and document important evidence that certain groups of insiders are more likely to make informed trades. In this section, I examine whether the trading behavior of PC insiders is consistent with the cautious PC insider hypothesis.

4.2.1. Insider trading horizon

Akbas, Jiang, and Koch (2020) create a trading horizon measure and examine the different trading horizons among corporate insiders. They document that the trades of short-horizon insiders are more informed and more unexpected than the trades of long-horizon insiders. Following their methodology, I create the insider investment horizon measure (*HOR*) as

$$HOR_{i,j,y} = \left| \frac{\sum_{y-10}^{y-1} IOF_{i,j,y}}{N} \right| \tag{9}$$

where $IOF_{i,j,y}$ is the annual net insider order flow of insider *i* at firm *j* in year *y*, which is calculated by $\frac{P_{i,j,y} - S_{i,j,y}}{P_{i,j,y} + S_{i,j,y}}$, where *P* is the number of share purchases during year *y*, *S* is the number of shares sold, and *N* is the number of calendar years the insider traded over the 10 years prior to year *y*. I require the insider to have traded in at least two of the past 10 years to be included in the sample. For an insider who only buys in a given year, *IOF* equals *P*/*P*=1; for an insider who only sells in a given year, *IOF* equals -*S*/*S*=-1; for an insider whose purchases and sales exactly offset one another in a given year, *IOF*=0. Therefore, *IOF* in a given year ranges from -1 to 1. *HOR* is then calculated by taking the absolute value of the average of the annual net order flow over the past ten years. The final *HOR* measure ranges from 0 to 1, with 0 indicating insiders who both buy and sell an equal number of shares in a year over the past 10 years, and 1 indicating insiders who either only buy or only sell over the past 10 years. Since the *HOR* measure requires ten years of trading history and TR insider data become available from 1996, *HOR* is available in my sample from 1996 onwards. As reported in Table 24, the average HOR for the insider-firm-year level sample is 0.80.

To examine whether PC insiders are more likely to have longer trading horizons, I use the insider-firm-year level sample and run the regression model in equation (10)

$$HOR_{i,j,y} = \alpha + \beta_1 PCinsider + \beta_2 Size + \beta_3 BookMarket + \gamma_i + \gamma_j + \gamma_y + \varepsilon \quad (10)$$

where *HOR* is the insider trading horizon for insider *i* at firm *j* in year *y*, *PCinsider* is an indicator variable which equals one if a corporate insider is a current or former member of a government agency, *Size* is the natural logarithm of market value at the end of year *y*, *BookMarket* is the book-to-market ratio at the end of year *y*, and γ_i, γ_j, and γ_y are insider, firm, and year fixed effects, respectively.

****** Insert Table 27 here ******

In Table 27, I report the results of equation (10) using multiple fixed effects specifications. The *PCinsider* dummy variable is significantly positive in four of five models, indicating that PC insiders are more likely to have longer trading horizons compared with non-PC insiders. Given that Akbas, Jiang, and Koch (2020) document the negative relation between the trading horizon and informativeness of their trades, the finding that PC insiders on average have longer horizons is consistent with the cautious PC insider hypothesis.

4.2.2. Routine versus opportunistic insiders

Cohen, Malloy, and Pomorski (2012) is the first study to examine routine insider trading patterns. They categorize insider trades into routine trades and opportunistic trades using insiders' past trading history and find that only opportunistic trades are informative about firms' futures. I follow their method to identify routine and opportunistic insider trades and examine whether PC insiders are more likely to make routine trades than non-PC insiders. Specifically, I define a routine trader as an insider who placed a trade in the same calendar month for at least three consecutive years, and everyone else is defined as an opportunistic trader. I require an insider to make at least one trade in each of the three preceding years to be defined as either an opportunistic or a routine trader. Once an insider is categorized as a routine insider at firm *j* in year *y*, he or she is a routine insider for all subsequent years. Using the insider-firm-year level sample, I run the following regression model in equation (11)

$$Routine_{i,j,y} = \alpha + \beta_1 PCinsider + \beta_2 Size + \beta_3 BookMarket + \gamma_i + \gamma_j + \gamma_y + \varepsilon \qquad (11)$$

where *Routine* is an indicator variable which equals one if insider *i* is a routine insider at firm *j* in year *y* and zero otherwise, *PCinsider* is an indicator variable which equals one if a corporate insider is a current or former member of a government agency, *Size* is the natural logarithm of market value at the end of year *y*, *BookMarket* is the book-to-market ratio at the end of year *y*, and γ_i, γ_j, and γ_y are insider, firm, and year fixed effects, respectively.

I report the results of equation (11) in Table 28. *PCinsider* is significant and positively associated with *Routine* in model (1) and (2), where insider and firm fixed effects are included. If I add year fixed effects, the coefficients of *PCinsider* remain positive but become insignificant. In Cohen, Malloy, and Pomorski (2012), they find that only trades by opportunistic insiders are informative about firms' futures. The findings that PC insiders are more likely to be routine insiders and make predictable and identifiable trades indicate that PC insiders in general are sophisticated investors and cautious about potential legal risk, and thus support the cautious PC insider hypothesis.

****** Insert Table 28 here ******

4.3. The effectiveness of the STOCK Act

In April 2012, the Congress passes the Stop Trading on Congressional Knowledge (STOCK) Act, which prohibits Congress members and employees from using private information derived from their official positions for personal benefit. I investigate the effectiveness of the STOCK Act using the difference-in-difference (DID) method shown in equation (12). To examine the impact of the STOCK Act on Congress members/staff versus on other non-Congress PC insiders, I only use transactions made by PC insiders in the DID regression. To make sure that the number of observations is similar before and after the passage of the act, I use transactions made from January 2005 onwards[41]. The treated group includes transactions made by connected insiders who are Congress members/staff at the time of transactions, while the control group includes transactions made by other PC insiders or former Congress members/staff. Among the 20,513 transactions made by PC insiders after January 2005, 59 observations are in the treated group. If the STOCK Act is effective, the purchases (sales) made by the treated group should be associated with lower (higher) subsequent returns after the passage of the STOCK Act. The DID regression is designed as

$$CAR[0,h] = \alpha + \beta_1 Congress * After * Buy + \beta_2 Congress * After * Sell + \beta_3 Congress * Buy + \beta_4 Congress * Sell + \beta_5 Buy + \beta_6 PastMonthRet + \beta_7 PastYearRet + \beta_8 Size + \beta_9 BookMarket + \gamma_t + \gamma_j + \varepsilon \quad (11)$$

where *Congress* is an indicator variable which equals one if a corporate insider is a US Congress member or staff when the transaction is made, and *After* is an indicator variable which equals one if an insider transaction happens after April 2012. The variables of interest are the two triple interaction terms which should be significant if the act is effective in reducing opportunistic insider trading by Congress members/staff.

****** Insert Table 29 here ******

As Table 29 Panel A shows, the triple interaction term for purchases is negative and significant for all CAR[0,30] and CAR[0,60] measures, while the triple interaction term for sales is significantly negative for CAR[0,60] measures. In Panel B, CARs over longer horizons show similar but weaker patterns. In Table 30 where firm fixed effects are added, the effect for sales is significant using CAR[0,30] or CAR[0,120] as the dependent variable, whereas the interaction term for purchases is significant using CAR[0,60]. However, the purchase interaction term becomes positive and significant for CAR[0,255]. Thus, I find that

[41] Results are similar if I use transactions from 2006 onwards or 2007 onwards.

the STOCK Act can effectively reduce opportunistic insider trading by Congress members/staff, while the effectiveness seemingly is limited to short horizons.

****** Insert Table 30 here ******

4.4. Robustness checks

4.4.1. Matched sample analysis

Instead of using the full sample, I construct a one-to-five matched sample to show that my baseline results are robust. For a given transaction made by a PC insider, I match it to five same-type (purchase or sale) transactions made by non-PC insiders from the same firm in the same year with the nearest trade size. The matched sample consists of 148,957 observations, among which 29,552 (19.8%) are transactions made by PC insiders and the rest (80.1%) are made by the control group[42]. I report the regression results based on equation (8) in Table 31. Consistent with the baseline results, purchases by PC insiders are associated with significantly lower CARs, especially over the 30-day window.

****** Insert Table 31 here ******

5. Conclusions

In this paper, I use insiders' employment data to investigate whether corporate insiders who have current or prior positions in one or more government agencies tend to take advantage of their political connections and trade opportunistically. I find that PC insiders are more cautious about potential legal risk, and their trading behavior is less opportunistic compared with non-PC insiders. I document that purchases (sales) by PC insiders are associated with lower (higher) abnormal returns compared with non-PC insiders, and this effect is more significant among purchases. I also find that, compared with non-PC insiders, PC insiders tend to trade in longer horizons and are more likely to make routine trades. The STOCK Act passed

[42] There are PC insider transactions which cannot be matched to any control transaction or can be matched to fewer than five control transactions.

in April 2012 effectively decreases (increases) the abnormal returns associated with insider purchases (sales) made by Congress members and staff in short horizons.

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Figures and Tables

Figure 1. Timeline of industrial policy evolution in China, May 2015-December 2018

This figure shows Chinese industrial policy evolution after the announcement of MC 2025 on May 8, 2015. The 13th Five-Year Plan is issued on October 18, 2015. The Plan for the In-Depth Implementation of the National Intellectual Property Strategy is published on July 8, 2016. The Guidelines for Developing Strategic Emerging Industries is published on November 29, 2016. The Development Plan for New-Generation Artificial Intelligence is published on July 8, 2017. The Opinions on Developing Green Agriculture is issued on September 30, 2017. The Announcement on Establishing Made in China 2025 National Pilot and Demonstration Zones is announced on November 20, 2017. The Opinions on Agriculture Machinery Development is issued on December 21, 2018. We get all the policy information from the website of the State Council of the People's Republic of China (http://www.gov.cn/zhengce/xxgkzl.htm) and the website of the National Development and Reform Commission (http://www.ndrc.gov.cn/).

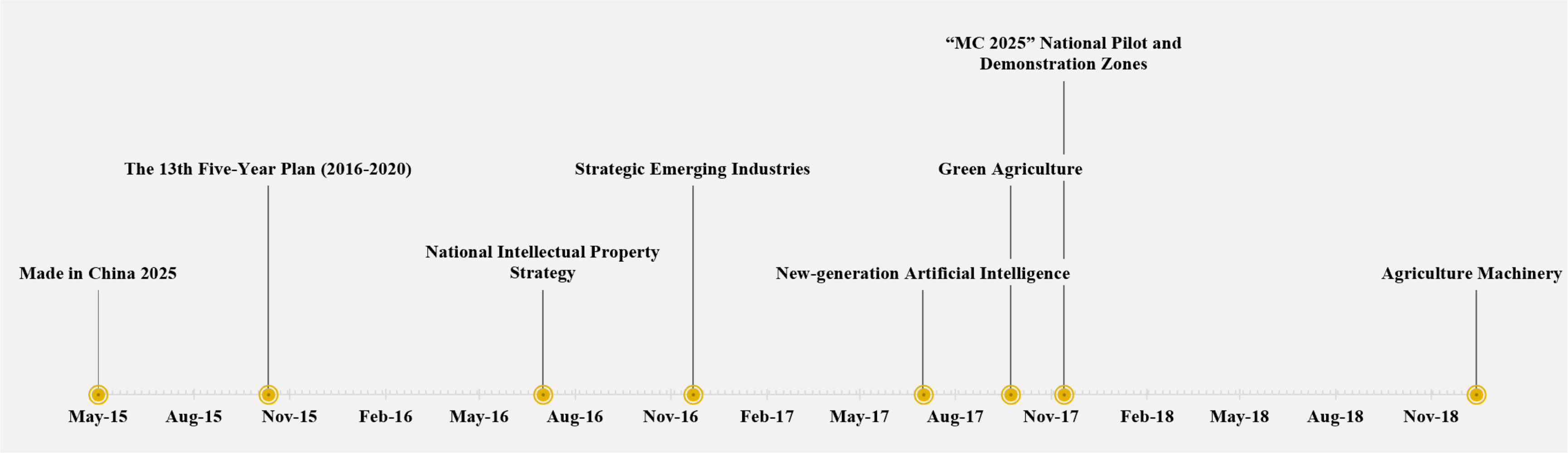


Figure 2. News attention trend indices of “Made in China 2025”

This figure shows monthly news attention trend indices of “Made in China 2025” from April 2015 to December 2018. Panel A presents the newspaper index calculated by the economic policy uncertainty (EPU) methodology. Detailed calculation is reported in Table A1 in the Appendix. Panel B presents the Google Search Index (web search in the US). Google index numbers represent search interest relative to the highest point on the chart for the given region and time. A value of 100 is the peak popularity for the term. A value of 50 means that the term is half as popular. A score of 0 means there was not enough data for this term. Panel C presents the search intensity on Baidu originated from mainland China. Panel D presents the media coverage index on Baidu.

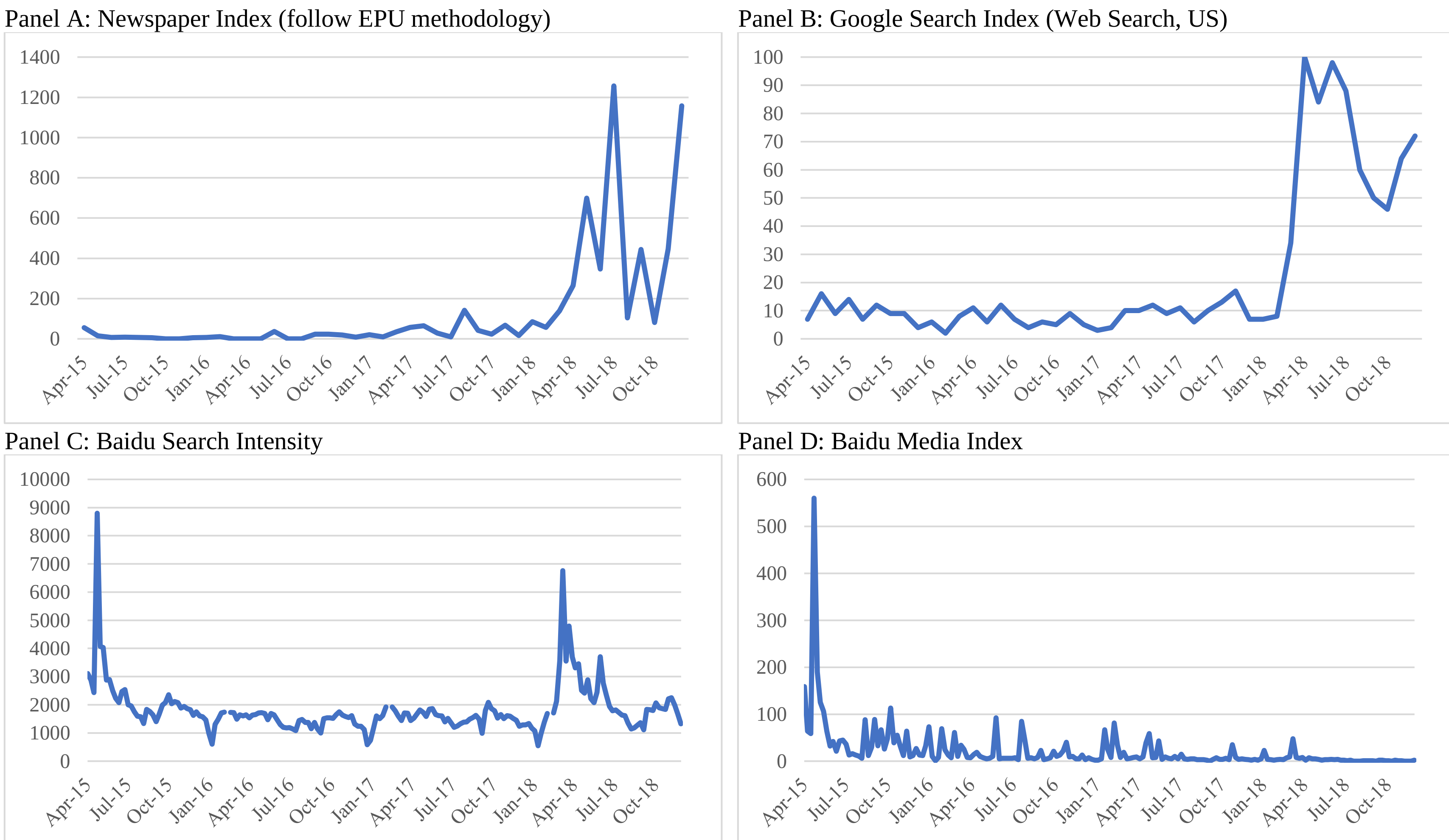
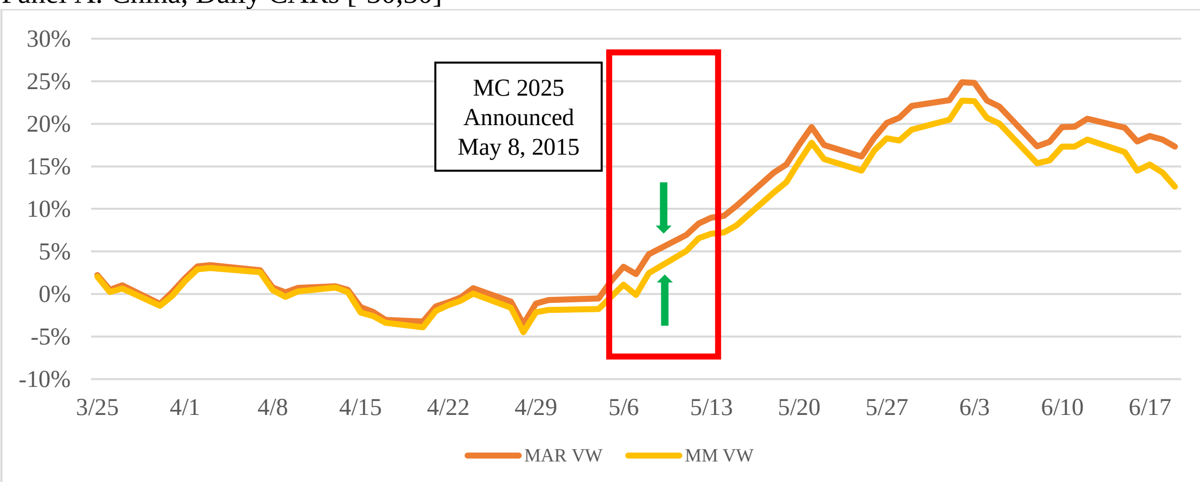


Figure 3. Daily cumulative abnormal returns (CARs) from t-30 to t+30 around the May 8, 2015 announcement date of the Made in China 2025

This figure plots the mean of daily CARs over [t-30, t+30] around May 8, 2015. Market-adjusted return (MAR) is calculated by subtracting the market index return from stock raw return. The market model (MM) and Fama-French three-factor model (FF) require that the estimation period ends 31 days before the event date and have 255 days in length with a minimum of four days trading. For the Chinese firms, we use the Shanghai-Shenzhen A-Shares Value-Weighted index created by China Center for Economic Research (CCER) in Peking University as benchmark. For the US firms, we use the CRSP Value-Weighted index as benchmark. Panel A shows CARs calculated by MAR and MM for Chinese treated firms. Panel B shows CARs calculated by MAR, MM, and FF for US treated firms. Green arrows point to positive significant AR [0] at 5% or higher significance level; red arrows point to negative significant AR [0] at 5% or higher significance level.

Panel A: China, Daily CARs [-30,30]



Panel B: US, Daily CARs [-30,30]

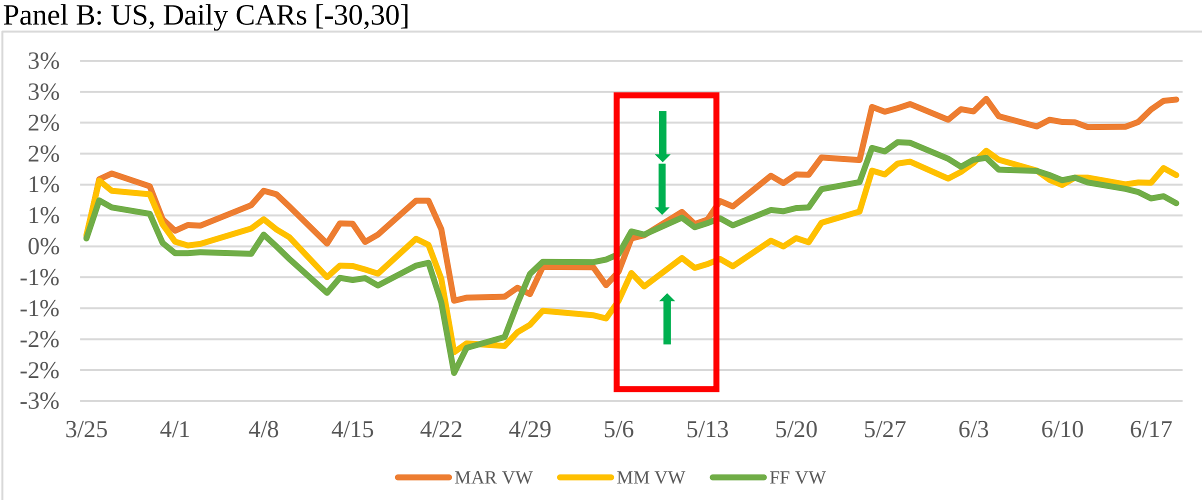
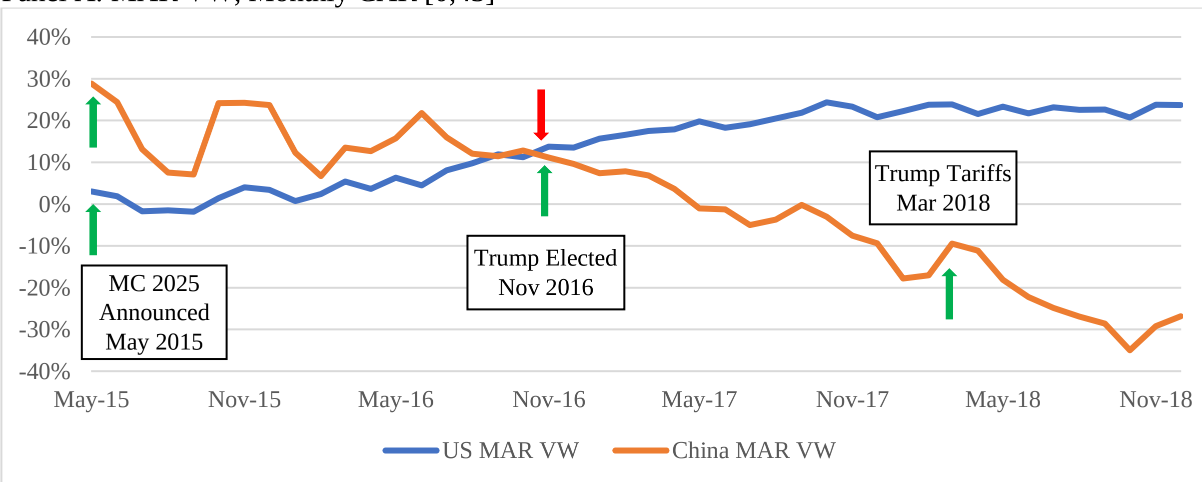


Figure 4. Monthly cumulative abnormal returns (CARs) from t=0 to t+43 around May 2015 announcement of Made in China 2025

This figure plots the mean of monthly CARs over [t=0, t+30] around May 2015. Market-adjusted return (MAR) is calculated by subtracting the market index return from stock raw return. Market model (MM) and Fama-French three-factor model (FF) require that the estimation period ends 13 months before the event date and have 60 months in length with a minimum of four months trading. For China, we use the Shanghai-Shenzhen A-Shares Value-Weighted index created by China Center for Economic Research (CCER) in Peking University as benchmark. For the US market, we use the CRSP Value-Weighted index as benchmark. Panel A shows CARs calculated by MAR for treated firms in China and US. Panel B shows CARs calculated by MM for treated firms in China and US. Green arrows point to positive significant AR [t]; red arrows point to negative significant AR [t].

Panel A: MAR VW, Monthly CAR [0,43]



Panel B: MM VW, Monthly CAR [0,43]

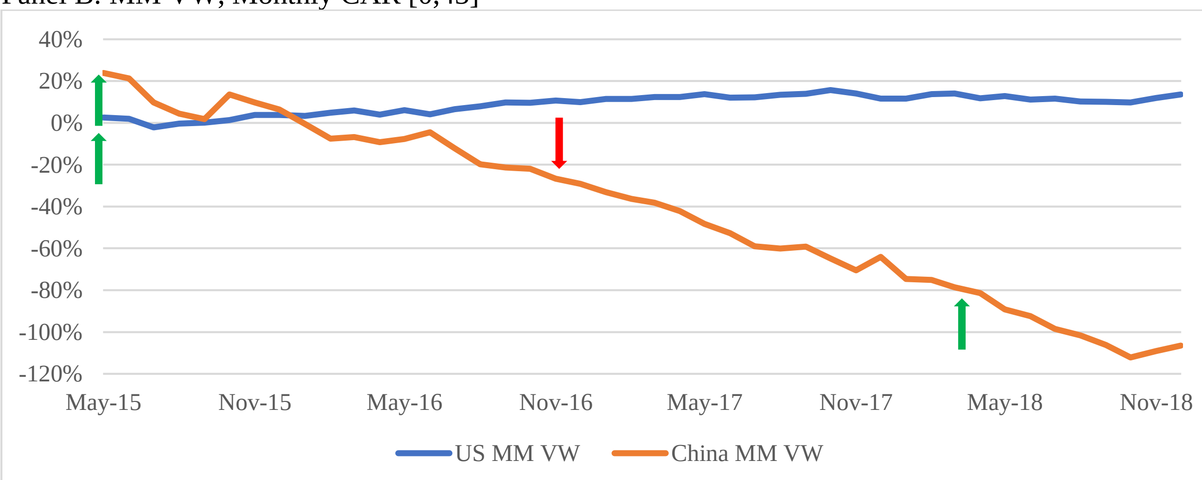
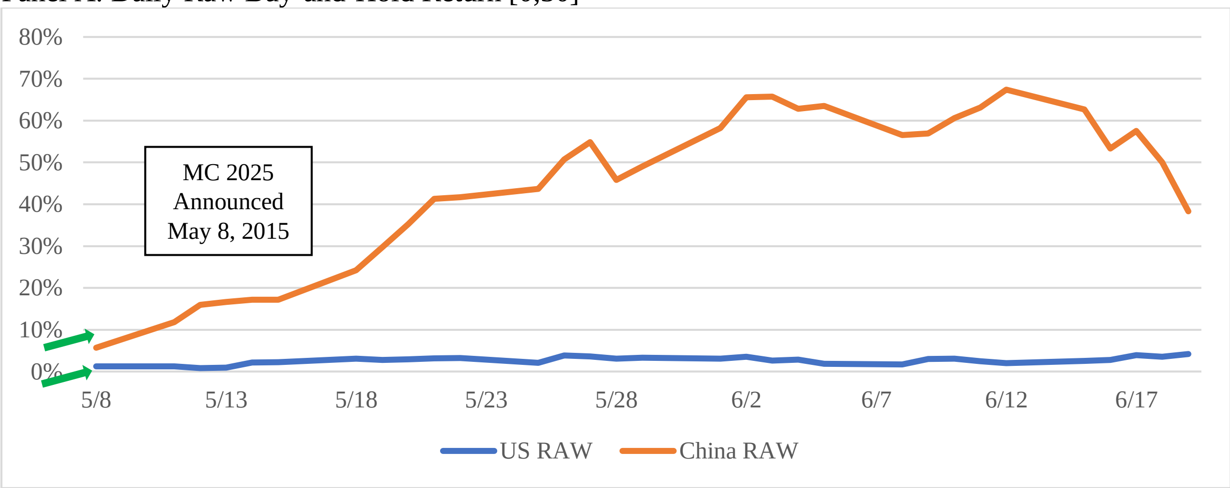


Figure 5. Daily and Monthly Raw Returns for Chinese and US treated firms around May 8, 2015

This figure shows the mean of daily and monthly raw returns for Chinese and US treated firms. Raw return is defined as the unadjusted buy-and-hold return. Green arrows indicate significantly positive AR [t]; red arrows indicate significantly negative AR [t].

Panel A: Daily Raw Buy-and-Hold Return [0,30]



Panel B: Monthly Raw Buy-and-Hold Return [0,43]

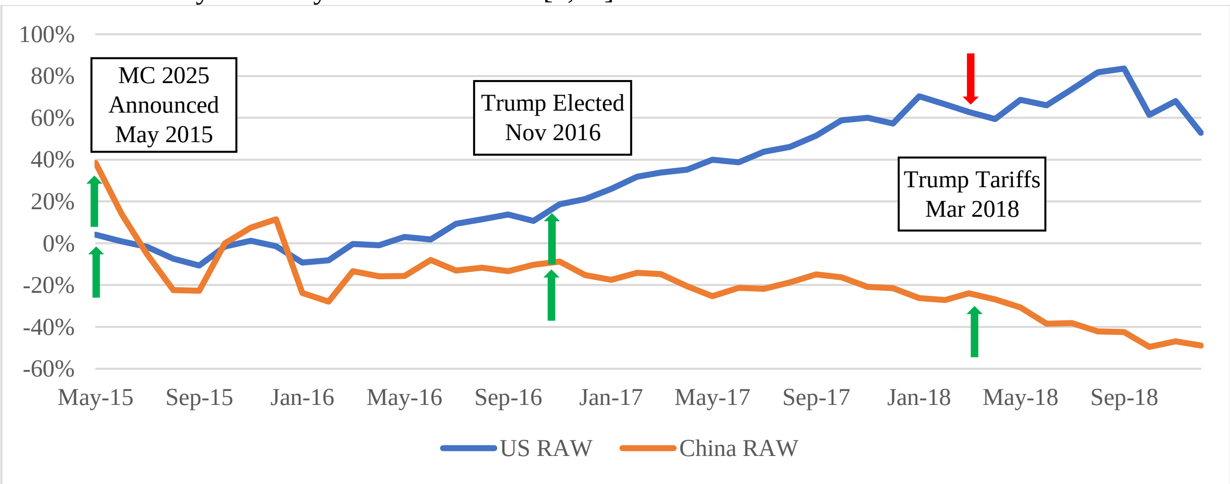
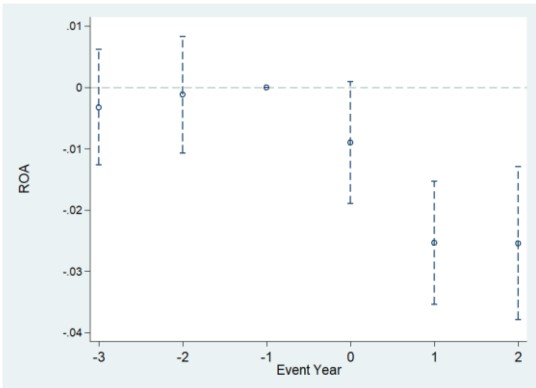


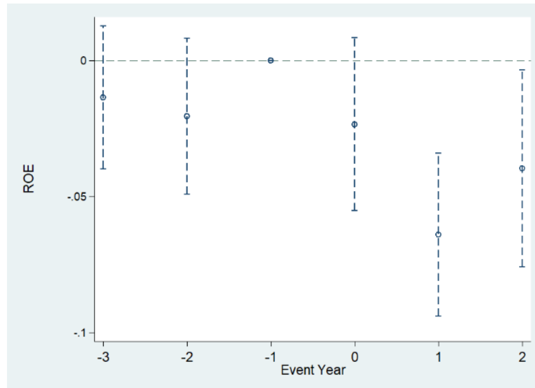
Figure 6. Parallel trend of the financial performance for treated and control firms in China over 2012-2017

This figure plots the coefficients and associated 95% confidence interval from estimating the leads and lags of the MC2025 effects in the propensity score matched sample over 2012-2017, where the dependent variables are *ROA*, *ROE*, *NI/EMP*, *TFP*, and *Leverage* in Panel A, B, C, D, and E, respectively. *ROA* is net income scaled by total assets. *ROE* is net income scaled by total equity. *NI/EMP* is net income scaled by number of employees (labor productivity). *TFP* is the total factor productivity following Olley and Pakes (1996) method. *Leverage* is long term debt scaled by total assets. The base year is event year -1 (year of 2014). All continuous variables are winsorized at 1%.

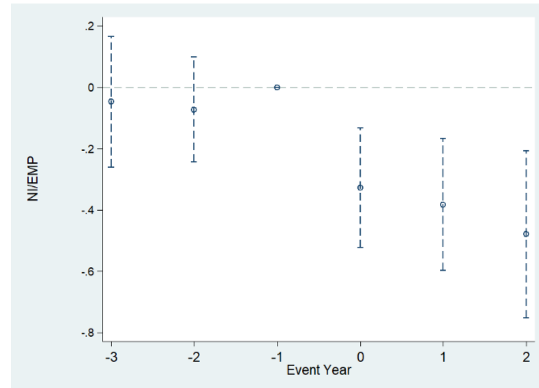
Panel A: ROA



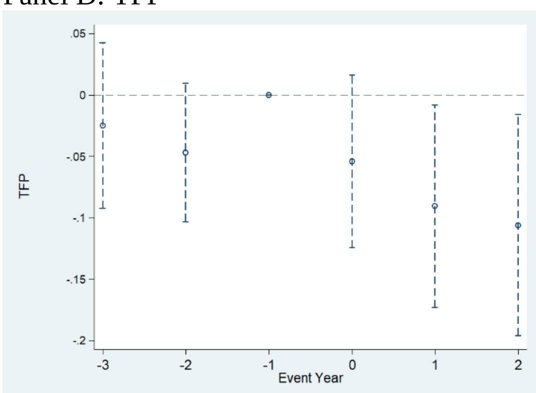
Panel B: ROE



Panel C: NI/EMP



Panel D: TFP



Panel E: Leverage

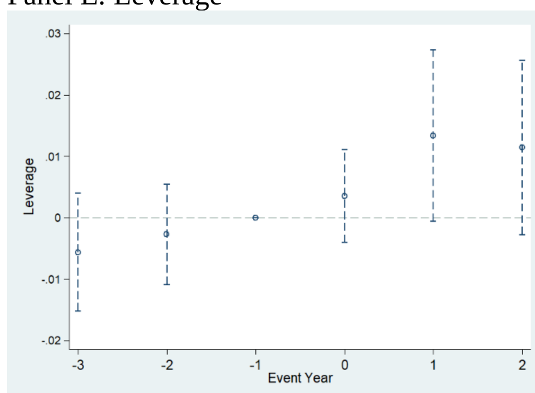


Figure 7. Parallel trend of the government subsidies for treated and control firms in China over 2012-2017

This figure plots the coefficients and associated 95% confidence interval from estimating the leads and lags of the MC2025 effects in the propensity score matched sample over 2012-2017, where the dependent variables are *GovtSub* and *GovtSubDummy*, in Panel A and B, respectively. *GovtSub* is government subsidies scaled by total assets. *GovtSubDummy* equals one if a firm receives government subsidies, and zero otherwise. The base year is event year -1 (year of 2014). All continuous variables are winsorized at 1%.

Panel A: GovtSub

Panel B: GovtSubDummy

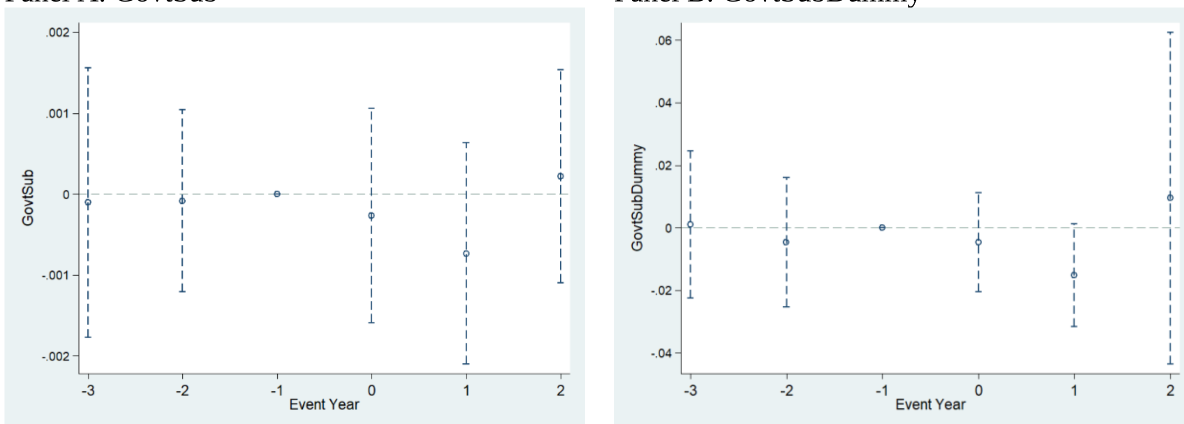


Table 1. Details of the Made in China 2025 Industrial Plan and sample distribution by industry

This table presents the ten industries targeted by the MC 2025 Plan, and the sample distribution by industry. The Chinese sample consists of 169 firms that are categorized as the MC 2025 Concept Stocks by cnstock.com, a designated information disclosure website by China Securities Regulatory Commission. The US sample consists of 131 firms that meet the requirements that (1) are listed on the NYSE, Nasdaq, or Amex stock exchanges, (2) have segment revenues from China, Asia, and/or Hong Kong, and (3) disclose average research and development expenses exceeding $100 million over 2012-2018.

Targeted Industries	Descriptions	Distribution by industry	
		China	**US**
1. Information Technology	Integrated circuit; Telecommunication; Industrial software and operating system	32	44
2. Numerical Control and Robotics	Computer numerical control; Artificial Intelligence; Machine learning	22	24
3. Aerospace Equipment	Aircraft; Aerospace equipment	16	11
4. Marine Engineering and High-tech Ships		20	0
5. Railway Equipment		14	0
6. Green Vehicles	Energy efficiency; Electric vehicles	15	12
7. Power Equipment	Renewable energy	27	2
8. Agricultural Machinery		2	2
9. New Materials		15	5
10. Biomedicine and Medical Devices		6	31
Total		**169**	**131**

Table 2. Announcement period daily abnormal returns (AR) and cumulative abnormal returns (CARs) for Chinese and US treated firms

This table presents the average daily ARs and CARs around 8 May 2015 for Chinese and US treated firms. The market-adjusted return (MAR) is calculated by subtracting the market index return from stock raw return. The market model (MM) and Fama-French three-factor model (FF) require that the estimation period ends 31 days before the event date and have 255 days in length with a minimum of four days trading. For the Chinese market, we use the Shanghai-Shenzhen A-Shares Value-Weighted index created by the China Center for Economic Research (CCER) in Peking University as benchmark. For the US market, we use the CRSP Value-Weighted index as benchmark. Raw return is defined as the buy-and-hold return. Means are tested by t-test, and significance levels at 1%, 5% and 10% are denoted by *, ** and ***, respectively.

China	**Model**	**CAR[-1,1]**	**CAR[-5,5]**
	MAR - SH SZ A share VW index	**4.523%*** **	**11.455%*** **
	MM - SH SZ A share VW index	**4.630%*** **	**9.933%*** **
	Raw returns	**9.144%*** **	**12.656%*** **
US	**Model**	**CAR[-1,1]**	**CAR[-5,5]**
	MAR - CRSP VW index	**0.965%*** **	**1.418%*** **
	MM - CRSP VW index	**0.692%*** **	**0.947%** **
	FF - CRSP VW index	**0.590%*** **	**0.787%** **
	Raw returns	**2.117%*** **	**3.160%*** **

Table 3. Monthly abnormal returns (AR) and cumulative abnormal returns (CARs) for Chinese and US treated firms

This table presents the average monthly ARs and CARs for May 2015 (t=0) to October 2016 (t=17), February 2018 (t=33), and December 2018 (t+43) for Chinese and US treated firms. The market-adjusted return (MAR) is calculated by subtracting the market index return from the stock's raw return. The market model (MM) and Fama-French three-factor model (FF) require that the estimation period ends 13 months before the event month and have 60 months in length with a minimum of four months trading. For the Chinese market, we use the Shanghai-Shenzhen A-Shares Value-Weighted index created by the China Center for Economic Research (CCER) in Peking University as benchmark. For the US market, we use the CRSP Value-Weighted index as benchmark. Raw return is defined as buy-and-hold return. Means are tested by t-test, and significance levels at 1%, 5% and 10% are denoted by *, ** and ***, respectively.

China	**Model**	**AR[0]**	**CAR[0,17]**	**CAR[18,33]**	**CAR[34,43]**
	MAR - SH SZ A share VW index	**28.856%*** **	**12.805%*** **	**-32.941%*** **	**-8.762%*** **
	MM - SH SZ A share VW index	**23.883%*** **	**-21.974%*** **	**-61.249%*** **	**-21.604%*** **
	Raw returns (Buy & Hold returns)	**38.464%*** **	**-10.308%*** **	**-19.787%*** **	**-27.540%*** **
US	**Model**	**AR[0]**	**CAR[0,17]**	**CAR[18,33]**	**CAR[34,43]**
	MAR - CRSP VW	**3.029%*** **	**11.259%*** **	**12.969%*** **	-0.201%
	MM - CRSP VW	**2.650%*** **	**9.548%** **	4.209%	-0.490%
	FF- CRSP VW	**2.049%*** **	**9.837%*** **	5.657%*	-2.994%
	Raw returns (Buy & Hold returns)	**4.063%*** **	**10.726%*** **	**46.504%*** **	**-6.721%*** **

Table 4. City-based policy analysis for Chinese treated firms

This table presents a city-based policy analysis for Chinese treated firms. Market-adjusted return (MAR) is calculated by subtracting the market index return from a stock's raw return. The market model (MM) requires that the estimation period ends 31 days (13 months) before the event day (month) and have 255 days (60 months) in length with a minimum of four days (months) trading to calculate daily (monthly) CARs. We use the Shanghai-Shenzhen A-Shares Value-Weighted index created by China Center for Economic Research (CCER) in Peking University as benchmark. Panel A reports the difference of mean daily CARs between firms headquartered in key cities (key-city firms) and firms headquartered in non-key cities (non-key-city firms). Panel B reports the difference of mean monthly CARs between key-city firms and non-key-city firms. The t-value of difference in the mean of two groups is reported. Differences in means are tested by two-sample t-test. Significance levels at 1%, 5% and 10% are denoted by *, ** and ***, respectively.

Panel A: Daily CARs difference between key-city firms and non-key-city firms

Model	**In key city?**	**CAR[-1,1]**	**CAR[-5,5]**
MAR - VW	No	-0.464%	-1.908%
	Yes	-1.234%	-2.722%
	t	1.01	0.51
MM - VW	No	-1.341%	-3.697%
	Yes	-1.772%	-5.275%
	t	0.59	0.89

Panel B: Monthly CARs difference between key-city firms and non-key-city firms

Model	**In key city?**	**AR[0]**	**CAR[0,3]**	**CAR[0,12]**
MAR - VW	No	-2.428%	-5.658%	-19.796%
	Yes	-1.876%	-6.234%	-24.313%
	t	-0.25	0.10	0.62
MM - VW	No	-4.195%	-13.273%	-43.856%
	Yes	-3.651%	-14.049%	-50.486%
	t	-0.25	0.14	0.86

Table 5. Summary statistics of the Difference-in-Difference sample

This table presents the summary statistics of the propensity score matched sample used in the DID analysis over 2012-2017. Control firms are matched by size, and book-market ratio using observations from 2007 to 2014. *Size* is natural log of total assets. *Age* is number of years since a firm is established. *Political Connection* equals one if at least one director on the board was or is an official of the central government, a local government, or the military following Fan, Wong, and Zhang (2007) and Liu, Shu, and Wei (2017), and zero otherwise. *Earnings Management* is the discretionary accruals following Kothari, Leone, and Wasley (2005) and Giannetti, Liao, and Yu (2015). *Excess Perk* is the corporate excess perk measure following Chen et al. (2021). *ROA* is net income scaled by total assets. *ROE* is net income scaled by total equity. *NI/EMP* is net income scaled by number of employees (labor productivity). *TFP* is the total factor productivity following Olley and Pakes (1996) method. *GovtSub* is government subsidies scaled by total assets. *GovtSubDummy* equals one if a firm receives government subsidies, and zero otherwise. *Leverage* is long term debt scaled by total assets. *R&D* is R&D spending scaled by total assets. *CapEx* is capital expenditures scaled by total assets. *Employees* is the natural logarithm of number of employees. *SOE* equals to one if a firm is state-owned enterprise and zero otherwise. All continuous variables are winsorized at 1%. The table reports t-value in parentheses. All t-values are computed with standard errors clustered at the firm level. Significance levels at 1%, 5% and 10% are denoted by *, ** and ***, respectively.

	All Sample			Treated Firms			Control Firms			Difference
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Size	1997	22.224	1.420	1002	22.181	1.347	995	22.268	1.489	0.088
Age	1997	15.939	5.540	1002	14.916	5.312	995	16.969	5.577	2.053***
Political Connection	1997	0.320	0.467	1002	0.313	0.464	995	0.327	0.469	0.013
Earnings Management	1997	0.001	0.075	1002	0.008	0.073	995	-0.005	0.077	-0.013***
Excess Perk	1500	0.000	0.008	787	0.001	0.007	713	-0.000	0.008	-0.001**
ROA	1997	0.034	0.054	1002	0.038	0.050	995	0.031	0.057	-0.006***
ROE	1997	0.052	0.127	1002	0.056	0.118	995	0.048	0.136	-0.008
NI/EMP	1779	10.915	1.244	913	10.967	1.142	866	10.861	1.343	-0.106*
TFP	1997	15.648	0.978	1002	15.563	0.904	995	15.734	1.041	0.171***
GovtSub	1928	0.007	0.008	974	0.008	0.009	954	0.005	0.008	-0.003***
GovtSubDummy	1946	0.984	0.125	981	0.988	0.110	965	0.980	0.139	-0.007
Leverage	1751	0.044	0.072	867	0.036	0.059	884	0.052	0.081	0.016***
R&D	1717	0.027	0.022	943	0.032	0.023	774	0.021	0.019	-0.011***
CapEx	1962	0.048	0.047	987	0.048	0.045	975	0.047	0.049	-0.001
Employees	1997	7.803	1.337	1002	7.753	1.309	995	7.852	1.364	0.099*
SOE	1997	0.375	0.484	1002	0.276	0.447	995	0.473	0.500	0.197***

Table 6. Difference-in-Difference analysis of the financial performance for Chinese firms

This table presents the DID analysis of the financial performance of Chinese firms in the propensity score matched sample over 2012-2017. Control firms are matched by size and book-market ratio using observations from 2007 to 2014. Dependent variable *ROA* is net income scaled by total assets. *ROE* is net income scaled by total equity. *NI/EMP* is net income scaled by number of employees (labor productivity). *TFP* is the total factor productivity following Olley and Pakes (1996) method. *Leverage* is long term debt scaled by total assets. *Treated* equals one if a firm is in industries targeted by MC 2025 and zero otherwise. *After* equals one if year is greater than or equal to 2015 and zero otherwise. *Size* is natural log of total assets. *Age* is number of years since a firm is established. *Political Connection* equals one if at least one director on the board was or is an official of the central government, a local government, or the military following Fan, Wong, and Zhang (2007) and Liu, Shu, and Wei (2017), and zero otherwise. *Earnings Management* is the discretionary accruals following Kothari, Leone, and Wasley (2005) and Giannetti, Liao, and Yu (2015). All continuous variables are winsorized at 1%. The table reports t-value in parentheses. All t-values are computed with standard errors clustered at the firm level. Significance levels at 1%, 5% and 10% are denoted by *, ** and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	ROA	ROA	ROE	ROE	NI/EMP	NI/EMP	TFP	TFP	Leverage	Leverage
Treated*After	-0.017***	-0.018***	-0.029**	-0.031**	-0.319***	-0.358***	-0.052	-0.060	0.010*	0.011*
	(-3.575)	(-3.751)	(-2.429)	(-2.388)	(-2.941)	(-2.977)	(-1.268)	(-1.274)	(1.803)	(1.935)
Treated	0.014***		0.021**		0.309**		-0.080		-0.016***	
	(2.738)		(2.008)		(2.266)		(-1.384)		(-2.699)	
After	0.003		0.008		0.212**		-0.049		-0.010**	
	(0.967)		(0.813)		(2.267)		(-1.377)		(-2.194)	
Size	0.002	0.008*	0.012***	0.020	0.146***	0.223**	0.571***	0.549***	0.022***	0.023***
	(1.319)	(1.767)	(4.053)	(1.430)	(3.166)	(2.015)	(30.810)	(9.644)	(6.603)	(4.115)
Age	-0.001	-0.001	-0.001	-0.001	0.006	0.049*	0.004	0.006	0.001*	-0.004**
	(-1.596)	(-1.041)	(-1.303)	(-0.344)	(0.640)	(1.781)	(0.658)	(0.592)	(1.691)	(-2.333)
Political Connection	0.005	-0.007	0.012	-0.023**	0.083	0.070	0.010	0.021	-0.007	-0.002
	(1.338)	(-1.410)	(1.452)	(-2.009)	(0.817)	(0.769)	(0.211)	(0.708)	(-1.327)	(-0.323)
Earnings Management	-0.005	0.049**	0.005	0.099	-0.187	0.770*	-0.503**	0.002	0.017	-0.006
	(-0.204)	(2.201)	(0.079)	(1.576)	(-0.317)	(1.901)	(-2.035)	(0.011)	(0.607)	(-0.270)
Observations	1,997	1,997	1,997	1,997	1,779	1,779	1,997	1,997	1,751	1,751
Adjusted R-squared	0.016	0.472	0.021	0.262	0.034	0.653	0.691	0.924	0.234	0.754
Year FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Firm FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Table 7. Difference-in-Difference analysis of the government subsidies for Chinese firms

This table presents the DID analysis of the government subsidies for Chinese firms in the propensity score matched sample over 2012-2017. Control firms are matched by size and book-market ratio using observations from 2007 to 2014. The dependent variable *GovtSub* is government subsidies scaled by total assets. *GovtSubDummy* equals one if a firm receives government subsidies, and zero otherwise. *Treated* equals one if a firm is in industries targeted by MC 2025 and zero otherwise. *After* equals one if year is greater than or equal to 2015 and zero otherwise. *Size* is natural log of total assets. *Age* is number of years since a firm is established. *Political Connection* equals one if at least one director on the board was or is an official of the central government, a local government, or the military following Fan, Wong, and Zhang (2007) and Liu, Shu, and Wei (2017), and zero otherwise. *Earnings Management* is the discretionary accruals following Kothari, Leone, and Wasley (2005) and Giannetti, Liao, and Yu (2015). All continuous variables are winsorized at 1%. The table reports t-value in parentheses. All t-values are computed with standard errors clustered at the firm level. Significance levels at 1%, 5% and 10% are denoted by *, ** and ***, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	GovtSub	GovtSub	GovtSubDummy	GovtSubDummy
Treated*After	0.000	-0.000	0.002	-0.002
	(0.122)	(-0.326)	(0.138)	(-0.173)
Treated	0.002***		0.004	
	(2.941)		(0.538)	
After	-0.000		-0.011	
	(-0.599)		(-1.230)	
Size	-0.001***	-0.000	0.003	0.027**
	(-3.739)	(-0.920)	(1.106)	(2.227)
Age	-0.000*	-0.000**	-0.001***	-0.015***
	(-1.957)	(-2.506)	(-2.885)	(-3.701)
Political Connection	-0.001	-0.001	-0.005	-0.036**
	(-1.565)	(-1.011)	(-0.777)	(-2.175)
Earnings Management	-0.006**	-0.004	0.039	0.043
	(-1.987)	(-1.617)	(0.947)	(0.861)
Observations	1,928	1,928	1,946	1,946
Adjusted R-squared	0.062	0.595	0.005	0.089
Year FE	No	Yes	No	Yes
Firm FE	No	Yes	No	Yes

Table 8. Difference-in-Difference analysis for Chinese SOEs and Non-SOEs

This table presents the DID analysis of Chinese SOEs and non-SOEs in the propensity score matched sample over 2012-2017. Control firms are matched by size and book-market ratio using observations from 2007 to 2014. Dependent variable *GovtSub* is government subsidies scaled by total assets. *ROA* is net income scaled by total assets. *ROE* is net income scaled by total equity. *NI/EMP* is net income scaled by number of employees (labor productivity). *TFP* is the total factor productivity following Olley and Pakes (1996) method. *Leverage* is long term debt scaled by total assets. *Treated* equals one if a firm is in industries targeted by MC 2025 and zero otherwise. *After* equals one if year is greater than or equal to 2015 and zero otherwise. *Size* is natural log of total assets. *Age* is number of years since a firm is established. *Political Connection* equals one if at least one director on the board was or is an official of the central government, a local government, or the military following Fan, Wong, and Zhang (2007) and Liu, Shu, and Wei (2017), and zero otherwise. *Earnings Management* is the discretionary accruals following Kothari, Leone, and Wasley (2005) and Giannetti, Liao, and Yu (2015). All continuous variables are winsorized at 1%. The table reports t-value in parentheses. All t-values are computed with standard errors clustered at the firm level. Significance levels at 1%, 5% and 10% are denoted by *, ** and ***, respectively.

VARIABLES	(1) ROA SOE	(2) ROA Non-SOE	(3) ROE SOE	(4) ROE Non-SOE	(5) NI/EMP SOE	(6) NI/EMP Non-SOE	(7) TFP SOE	(8) TFP Non-SOE	(9) Leverage SOE	(10) Leverage Non-SOE	(11) GovtSub SOE	(12) GovtSub Non-SOE
Treated*After	-0.012	-0.024***	-0.033	-0.038**	-0.228	-0.465***	0.048	-0.142**	0.007	0.012*	-0.001	0.000
	(-1.613)	(-3.623)	(-1.493)	(-2.229)	(-1.079)	(-3.107)	(0.686)	(-2.241)	(0.796)	(1.678)	(-0.938)	(0.644)
Size	0.014	0.007	0.052	0.010	0.187	0.215*	0.598***	0.524***	0.009	0.026***	-0.000	-0.000
	(1.352)	(1.323)	(1.383)	(0.724)	(0.764)	(1.673)	(6.834)	(7.666)	(1.255)	(3.673)	(-0.196)	(-0.670)
Age	-0.001	-0.001	-0.003	0.000	0.035	0.070*	-0.016	0.025*	-0.004*	-0.003	-0.000	-0.001***
	(-0.668)	(-0.663)	(-0.388)	(0.131)	(0.774)	(1.946)	(-1.109)	(1.920)	(-1.708)	(-1.429)	(-1.015)	(-2.883)
Political Connection	-0.001	-0.010	-0.000	-0.036**	0.172	0.014	-0.001	0.031	-0.004	0.001	-0.001	-0.001
	(-0.120)	(-1.459)	(-0.008)	(-2.364)	(1.470)	(0.108)	(-0.014)	(0.794)	(-0.598)	(0.175)	(-0.443)	(-1.214)
Earnings Management	0.026	0.061**	0.058	0.124	0.942	0.697	-0.127	0.076	-0.021	0.002	0.000	-0.006**
	(0.758)	(2.186)	(0.603)	(1.544)	(1.478)	(1.359)	(-0.503)	(0.388)	(-0.774)	(0.088)	(0.032)	(-2.000)
Observations	748	1,249	748	1,249	651	1,128	748	1,249	673	1,078	710	1,218
Adjusted R-squared	0.520	0.448	0.267	0.272	0.703	0.616	0.932	0.906	0.817	0.628	0.650	0.548
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 9. Sample distribution by year and industry

This table provides the summary statistics of the full sample (including firms that are VC-backed at their IPO stage) from 1998 to 2018. Panel A provides the sample distribution by fiscal year, and Panel B provides the sample distribution by the Fama-French 12 industries. See Appendix B for variable definitions.

Panel A: Distribution of observations by year

Year	Number of Observations	With VC Directors (%)	With VC directors on Compensation Committee (%)
1998	743	0.94	0.40
1999	715	1.40	0.70
2000	754	2.52	0.93
2001	799	3.75	2.00
2002	785	3.31	1.53
2003	788	4.06	2.41
2004	779	4.62	2.31
2005	756	3.97	1.98
2006	732	5.19	1.91
2007	795	4.40	1.76
2008	800	4.75	2.13
2009	806	5.09	2.23
2010	790	5.70	3.42
2011	793	5.17	3.15
2012	761	5.39	3.02
2013	785	5.73	3.06
2014	762	6.04	3.15
2015	765	6.01	2.61
2016	765	6.27	2.35
2017	760	6.97	3.42
2018	652	6.90	3.37
Total	16,085	4.68	2.28

Panel B: Distribution of observations by industry

Fama-French 12 Industry	Number of Firms	With VC Directors (%)	With VC directors on Compensation Committee (%)
Business Equipment	2520	6.67	2.74
Chemicals and Allied Products	841	4.16	0.83
Consumer Durables	643	2.18	1.09
Consumer Nondurables	1462	4.51	2.74
Healthcare, Medical Equipment, and Drugs	1319	5.08	2.81
Manufacturing	2960	4.19	2.40
Oil, Gas, and Coal	978	4.81	3.37
Other	2453	3.14	1.51
Telephone and Television Transmission	439	9.57	6.38
Wholesale, Retail, and Some Services	2470	4.53	1.54
Total	16,085	4.68	2.28

Table 10. Summary statistics of the firm, board and director characteristics

This table provides the summary statistics of the firm, board, and director characteristics from 1998 to 2018 in Panel A and B, respectively. Column (10) presents the difference between column (8) and column (5). See Appendix B for variable definitions. The ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Firm Statistics

	All Sample			With VC Directors on Compensation Committee			Without VC Directors on Compensation Committee			Difference
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CEO Total Compensation (Logarithm)	16,074	8.19	1.01	367	8.69	0.80	15,707	8.18	1.01	-0.52***
CEO Vega (Logarithm)	11,064	2.88	1.60	247	3.55	1.37	10,817	2.86	1.60	-0.68***
CEO Delta (Logarithm)	11,064	2.88	1.60	247	3.59	1.44	10,817	2.87	1.60	-0.72***
Size (Logarithm)	16,085	7.71	1.50	367	8.33	1.35	15,718	7.69	1.50	-0.64***
Leverage	16,085	0.20	0.16	367	0.23	0.18	15,718	0.20	0.15	-0.03***
Market-to-Book Ratio	16,085	3.11	3.26	367	3.20	2.76	15,718	3.11	3.27	-0.09
ROA	16,085	0.05	0.09	367	0.04	0.09	15,718	0.05	0.09	0.01***
R&D Intensity	16,085	0.02	0.04	367	0.03	0.04	15,718	0.02	0.04	-0.01***
Tangibility	16,085	0.28	0.22	367	0.23	0.21	15,718	0.28	0.22	0.04***
Stock Return	16,085	0.13	0.44	367	0.13	0.44	15,718	0.13	0.44	0
Stock Volatility	16,085	0.39	0.18	367	0.37	0.16	15,718	0.39	0.18	0.02**
Firm Age (Logarithm)	16,085	2.74	0.83	367	2.61	0.95	15,718	2.74	0.83	0.13***
CEO Age (Logarithm)	16,085	4.04	0.12	367	4.04	0.11	15,718	4.04	0.12	0.01
CEO Tenure (Logarithm)	16,085	1.87	0.78	367	1.78	0.73	15,718	1.88	0.78	0.09**
CEO Duality	16,085	0.63	0.48	367	0.54	0.50	15,718	0.63	0.48	0.09***
Institutional Ownership Concentration	16,085	0.06	0.05	367	0.05	0.04	15,718	0.06	0.05	0.01**
Institutional Ownership Total	16,085	0.77	0.20	367	0.79	0.18	15,718	0.77	0.20	-0.02*
Patent Count	16,085	36.62	244.91	367	52.61	209.60	15,718	36.25	245.66	-16.37
Forward 5-yr Citations	16,085	100.82	1259.71	367	150.41	697.82	15,718	99.66	1269.85	-50.75
Innovation Value (Nominal)	16,085	560.47	2837.41	367	723.35	2507.79	15,718	556.66	2844.61	-166.68
Innovation Value (Real)	16,085	1209.81	5791.00	367	1662.98	5708.26	15,718	1199.23	5792.67	-463.75
CEO Forced Turnover	16,085	0.02	0.14	367	0.02	0.15	15,718	0.02	0.14	-0.01
High VC Reputation	16,085	0.00	0.05	367	0.02	0.14	15,718	0.00	0.04	-0.02***

Panel B: Board Statistics

	All Sample			With VC Directors on Compensation Committee			Without VC Directors on Compensation Committee			Difference
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Board Size	16,085	9.24	2.18	367	9.65	2.41	15,718	9.23	2.17	-0.41***
Number of VC Directors on Board	16,085	0.04	0.21	367	1.00	0.00	15,718	0.02	0.15	-0.98***
% VC Directors on Board	16,085	0.01	0.03	367	0.12	0.06	15,718	0.00	0.02	-0.12***
% Independent Directors on Board	16,085	0.74	0.16	367	0.76	0.14	15,718	0.74	0.16	-0.02***
Number of VC Directors on Compensation Committee	16,085	0.02	0.15	367	1.03	0.16	15,718	0.00	0.00	-1.03***
% VC Directors on Compensation Committee	16,085	0.01	0.04	367	0.28	0.10	15,718	0.00	0.00	-0.28***

Panel C: Director Statistics

	All Sample			VC Directors			Non-VC Directors			Difference
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age	267,196	61.29	8.46	1,261	58.65	8.46	265,935	61.30	8.46	2.65***
Tenure	267,143	10.20	7.76	1,259	5.90	3.88	265,884	10.22	7.77	4.32***
Multiple Boards	267,438	0.47	0.50	1,261	0.59	0.49	266,177	0.47	0.50	-0.12***
Number of Other Board Seats Held	267,324	0.83	1.11	1,261	1.07	1.13	266,063	0.83	1.11	-0.24***

Table 11. The effect of the presence of VC director on the compensation committee on CEO risk-taking incentives (vega)

This table shows the effect of the presence of VC directors on the compensation committee on CEO risk-taking incentives (vega) using the non-VC-backed firm sample from 1998 to 2018. The dependent variable is the natural logarithm of CEO vega in year t+1. See Appendix B for variable definitions. The Fama-French 48 industry fixed effects and year fixed effects are as indicated. Standard errors are clustered at the firm level. T-statistics are in parentheses. The ***, **, and * denote significance levels at 1%, 5% and 10% respectively.

	CEO Vega $_{t+1}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VC Director on Comp. Committee	0.225***			0.236***			0.234***		
	(2.643)			(2.725)			(2.817)		
Number of VC Directors on Comp. Committee		0.225***			0.236***			0.234***	
		(2.882)			(2.965)			(3.058)	
% VC Director on Comp. Committee			0.851***			0.877***			0.859***
			(3.567)			(3.589)			(3.596)
Size	0.321***	0.321***	0.320***	0.321***	0.321***	0.321***	0.328***	0.328***	0.327***
	(18.205)	(18.202)	(18.161)	(17.949)	(17.946)	(17.906)	(17.861)	(17.859)	(17.823)
Leverage	-0.363***	-0.364***	-0.364***	-0.362***	-0.363***	-0.363***	-0.390***	-0.391***	-0.391***
	(-3.427)	(-3.432)	(-3.435)	(-3.356)	(-3.361)	(-3.363)	(-3.574)	(-3.581)	(-3.583)
Market-to-Book Ratio	0.030***	0.030***	0.030***	0.030***	0.030***	0.030***	0.032***	0.032***	0.032***
	(4.978)	(4.979)	(4.988)	(4.819)	(4.821)	(4.828)	(5.110)	(5.111)	(5.118)
ROA	0.864***	0.865***	0.869***	0.854***	0.855***	0.859***	0.817***	0.818***	0.821***
	(4.698)	(4.704)	(4.720)	(4.597)	(4.603)	(4.618)	(4.381)	(4.386)	(4.401)
R&D Intensity	2.255***	2.258***	2.266***	2.304***	2.307***	2.317***	2.297***	2.299***	2.310***
	(5.214)	(5.219)	(5.237)	(5.250)	(5.256)	(5.275)	(4.950)	(4.953)	(4.975)
Tangibility	-0.099	-0.097	-0.095	-0.134	-0.132	-0.130	-0.143	-0.141	-0.139
	(-1.021)	(-0.999)	(-0.980)	(-1.384)	(-1.362)	(-1.344)	(-1.418)	(-1.395)	(-1.381)
Stock Return	-0.098***	-0.098***	-0.098***	-0.095***	-0.095***	-0.094***	-0.092***	-0.092***	-0.092***
	(-3.007)	(-3.007)	(-3.002)	(-2.871)	(-2.872)	(-2.866)	(-2.786)	(-2.787)	(-2.782)
Stock Volatility	-0.561***	-0.560***	-0.561***	-0.560***	-0.559***	-0.560***	-0.542***	-0.541***	-0.542***
	(-5.647)	(-5.639)	(-5.646)	(-5.606)	(-5.597)	(-5.604)	(-5.386)	(-5.378)	(-5.387)
Firm Age	0.014	0.015	0.015	0.016	0.016	0.017	0.010	0.010	0.011
	(0.854)	(0.871)	(0.908)	(0.919)	(0.937)	(0.974)	(0.586)	(0.605)	(0.642)
CEO Age	-0.284**	-0.284**	-0.282**	-0.271**	-0.270**	-0.269**	-0.297**	-0.296**	-0.294**
	(-2.301)	(-2.296)	(-2.279)	(-2.175)	(-2.169)	(-2.154)	(-2.366)	(-2.359)	(-2.344)
CEO Tenure	-0.022	-0.022	-0.022	-0.028	-0.029	-0.029	-0.022	-0.023	-0.023

	(-1.125)	(-1.127)	(-1.129)	(-1.426)	(-1.429)	(-1.430)	(-1.121)	(-1.123)	(-1.125)
CEO Duality	0.065**	0.064**	0.065**	0.061**	0.061**	0.062**	0.060**	0.060**	0.061**
	(2.236)	(2.230)	(2.243)	(2.121)	(2.114)	(2.130)	(2.061)	(2.054)	(2.072)
% Independent Board	0.512***	0.512***	0.513***	0.492***	0.492***	0.494***	0.481***	0.481***	0.483***
	(4.939)	(4.936)	(4.951)	(4.740)	(4.737)	(4.753)	(4.599)	(4.597)	(4.614)
Board Size	0.007	0.007	0.007	0.007	0.007	0.007	0.007	0.007	0.007
	(0.824)	(0.836)	(0.857)	(0.785)	(0.798)	(0.819)	(0.808)	(0.819)	(0.840)
Institutional Ownership Concentration	-0.725*	-0.723*	-0.716*	-0.800*	-0.798*	-0.792*	-0.764*	-0.763*	-0.757*
	(-1.742)	(-1.737)	(-1.721)	(-1.919)	(-1.914)	(-1.899)	(-1.845)	(-1.842)	(-1.829)
Institutional Ownership Total	0.266***	0.268***	0.268***	0.271***	0.273***	0.273***	0.293***	0.294***	0.294***
	(2.811)	(2.827)	(2.828)	(2.849)	(2.867)	(2.865)	(3.098)	(3.115)	(3.108)
Log(1+Delta)	0.376***	0.376***	0.376***	0.376***	0.375***	0.375***	0.370***	0.370***	0.370***
	(20.176)	(20.167)	(20.189)	(19.997)	(19.987)	(20.012)	(19.559)	(19.549)	(19.576)
State Educational Attainment				0.088	0.089	0.081			
				(0.577)	(0.578)	(0.525)			
State per-capita Income				-0.302	-0.305	-0.296			
				(-1.503)	(-1.516)	(-1.467)			
State R&D per Capita				0.022	0.022	0.022			
				(1.199)	(1.206)	(1.202)			
Observations	9,535	9,535	9,535	9,398	9,398	9,398	9,398	9,398	9,398
Adjusted R-squared	0.526	0.526	0.526	0.524	0.524	0.524	0.525	0.525	0.525
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No	No	Yes	Yes	Yes

Table 12. The effect of the presence of VC director on the compensation committee on CEO pay-performance sensitivity (delta)

This table shows the effect of the presence of VC directors on the compensation committee on CEO pay-performance sensitivity (delta) using the non-VC-backed firm sample from 1998 to 2018. The dependent variable is the natural logarithm of CEO delta in year t+1. See Appendix B for variable definitions. The Fama-French 48 industry fixed effects and year fixed effects are as indicated. Standard errors are clustered at the firm level. T-statistics are in parentheses. The ***, **, and * denote significance levels at 1%, 5% and 10% respectively.

	CEO Delta $_{t+1}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VC Director on Comp. Committee	0.301**			0.313**			0.306**		
	(2.313)			(2.394)			(2.473)		
Number of VC Directors on Comp. Committee		0.311**			0.323***			0.318***	
		(2.550)			(2.633)			(2.715)	
% VC Director on Comp. Committee			1.131***			1.158***			1.127***
			(3.195)			(3.233)			(3.283)
Size	0.491***	0.491***	0.490***	0.490***	0.490***	0.490***	0.495***	0.495***	0.495***
	(24.622)	(24.619)	(24.597)	(24.295)	(24.293)	(24.269)	(24.322)	(24.321)	(24.304)
Leverage	-0.473***	-0.474***	-0.474***	-0.471***	-0.472***	-0.472***	-0.538***	-0.539***	-0.539***
	(-3.199)	(-3.205)	(-3.208)	(-3.140)	(-3.147)	(-3.147)	(-3.562)	(-3.571)	(-3.572)
Market-to-Book Ratio	0.047***	0.047***	0.047***	0.047***	0.047***	0.047***	0.049***	0.049***	0.049***
	(5.748)	(5.749)	(5.756)	(5.581)	(5.583)	(5.588)	(5.916)	(5.917)	(5.921)
ROA	1.360***	1.362***	1.365***	1.353***	1.354***	1.358***	1.295***	1.296***	1.300***
	(6.029)	(6.035)	(6.049)	(5.942)	(5.948)	(5.961)	(5.696)	(5.702)	(5.715)
R&D Intensity	4.329***	4.330***	4.342***	4.385***	4.387***	4.400***	4.269***	4.269***	4.285***
	(6.720)	(6.723)	(6.740)	(6.703)	(6.706)	(6.725)	(6.214)	(6.215)	(6.239)
Tangibility	-0.178	-0.174	-0.172	-0.218	-0.214	-0.213	-0.221	-0.217	-0.216
	(-1.261)	(-1.238)	(-1.224)	(-1.558)	(-1.536)	(-1.522)	(-1.539)	(-1.516)	(-1.506)
Stock Return	0.075**	0.075**	0.075**	0.079**	0.079**	0.080**	0.077**	0.077**	0.077**
	(2.304)	(2.302)	(2.308)	(2.415)	(2.413)	(2.420)	(2.346)	(2.342)	(2.351)
Stock Volatility	-0.029	-0.028	-0.029	-0.027	-0.026	-0.027	-0.029	-0.028	-0.029
	(-0.218)	(-0.209)	(-0.218)	(-0.203)	(-0.193)	(-0.202)	(-0.215)	(-0.206)	(-0.217)
Firm Age	-0.031	-0.030	-0.030	-0.031	-0.030	-0.029	-0.036	-0.035	-0.034
	(-1.303)	(-1.284)	(-1.251)	(-1.286)	(-1.266)	(-1.234)	(-1.489)	(-1.470)	(-1.436)
CEO Age	-0.588***	-0.587***	-0.585***	-0.580***	-0.578***	-0.577***	-0.618***	-0.616***	-0.614***
	(-3.308)	(-3.301)	(-3.288)	(-3.248)	(-3.241)	(-3.229)	(-3.496)	(-3.487)	(-3.475)
CEO Tenure	-0.027	-0.027	-0.027	-0.033	-0.033	-0.033	-0.027	-0.027	-0.027
	(-0.952)	(-0.955)	(-0.955)	(-1.146)	(-1.150)	(-1.148)	(-0.920)	(-0.924)	(-0.923)
CEO Duality	0.097**	0.096**	0.097**	0.092**	0.092**	0.092**	0.087**	0.087**	0.087**

	(2.496)	(2.492)	(2.505)	(2.369)	(2.363)	(2.379)	(2.240)	(2.236)	(2.253)
% Independent Board	0.625***	0.624***	0.626***	0.608***	0.608***	0.610***	0.601***	0.601***	0.603***
	(4.105)	(4.103)	(4.116)	(3.976)	(3.973)	(3.988)	(3.908)	(3.906)	(3.921)
Board Size	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.002	-0.002	-0.002
	(-0.270)	(-0.259)	(-0.240)	(-0.293)	(-0.282)	(-0.262)	(-0.190)	(-0.181)	(-0.160)
Institutional Ownership Concentration	-1.302**	-1.299**	-1.292**	-1.380**	-1.377**	-1.370**	-1.310**	-1.307**	-1.302**
	(-2.374)	(-2.366)	(-2.353)	(-2.500)	(-2.493)	(-2.481)	(-2.430)	(-2.424)	(-2.413)
Institutional Ownership Total	0.557***	0.560***	0.559***	0.562***	0.564***	0.563***	0.575***	0.577***	0.576***
	(4.223)	(4.241)	(4.238)	(4.229)	(4.248)	(4.243)	(4.454)	(4.474)	(4.463)
State Educational Attainment				0.260	0.260	0.251			
				(1.282)	(1.283)	(1.238)			
State per-capita Income				-0.486*	-0.490*	-0.479*			
				(-1.798)	(-1.813)	(-1.769)			
State R&D per Capita				0.032	0.032	0.032			
				(1.240)	(1.246)	(1.244)			
Observations	10,384	10,384	10,384	10,242	10,242	10,242	10,242	10,242	10,242
Adjusted R-squared	0.363	0.364	0.364	0.363	0.363	0.363	0.367	0.367	0.367
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No	No	Yes	Yes	Yes

Table 13. The effect of the presence of VC director on non-compensation committees on CEO vega and delta

This table shows the effect of the presence of VC directors on non-compensation committees on CEO risk-taking incentives (vega) and CEO pay-performance sensitivity (delta) using the non-VC-backed firm sample from 1998 to 2018. The dependent variable is the natural logarithm of CEO vega in year t+1 in column (1)-(4), and natural logarithm of CEO delta in year t+1 in columns (5)-(8). See Appendix B for variable definitions. The Fama-French 48 industry fixed effects and year fixed effects are as indicated. Baseline controls include firm size, leverage, market-to-book ratio, ROA, R&D intensity, tangibility, annual stock return, annual stock volatility, firm age, CEO age, CEO tenure, CEO duality, percent independent board, the board size, institutional ownership concentration, and institutional ownership total. For columns (1)-(4), we include CEO delta at time t as a control variable. State controls include state educational attainment, state per-capita income, and state R&D per capita. Standard errors are clustered at the firm level. T-statistics are in parentheses. The ***, **, and * denote significance levels at 1%, 5% and 10% respectively.

	CEO Vega $_{t+1}$				CEO Delta $_{t+1}$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VC Director on Board but not Comp. Committee	0.005				0.086			
	(0.062)				(0.883)			
VC Director on Governance Committee		-0.108				-0.059		
		(-1.247)				(-0.597)		
VC Director on Audit Committee			0.001				0.075	
			(0.005)				(0.584)	
VC Director on Nomination Committee				-0.033				0.047
				(-0.326)				(0.390)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,398	9,398	9,398	9,398	10,242	10,242	10,242	10,242
Adjusted R-squared	0.524	0.524	0.524	0.524	0.362	0.362	0.362	0.362
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 14. The impact of VC reputation on CEO vega and delta

This table shows the effect of the VC reputation on CEO vega and delta using the non-VC-backed firm sample from 1998 to 2018. The dependent variable is the natural logarithm of CEO vega in year t+1 in Panel A, and the natural logarithm of CEO delta in year t+1 in Panel B. See Appendix B for variable definitions. The Fama-French 48 industry fixed effects and year fixed effects are as indicated. Baseline controls include firm size, leverage, market-to-book ratio, ROA, R&D intensity, tangibility, annual stock return, annual stock volatility, firm age, CEO age, CEO tenure, CEO duality, percent independent board, the board size, institutional ownership concentration, and institutional ownership total. In Panel A, we further include CEO delta at time t as a control variable. State controls include state educational attainment, state per-capita income, and state R&D per capita. Standard errors are clustered at the firm level. T-statistics are in parentheses. The ***, **, and * denote significance levels at 1%, 5% and 10% respectively.

Panel A: The impact of VC reputation on CEO vega

	CEO Vega $_{t+1}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
High VC Reputation * VC Director on Comp. Committee	0.700***			0.690***			0.815***		
	(2.694)			(2.624)			(2.855)		
VC Director on Comp. Committee	0.210**			0.221**			0.216***		
	(2.450)			(2.532)			(2.610)		
High VC Reputation * Number of VC Directors on Comp. Committee		0.699***			0.689***			0.814***	
		(2.712)			(2.640)			(2.865)	
Number of VC Directors on Comp. Committee		0.211***			0.222***			0.218***	
		(2.685)			(2.769)			(2.845)	
High VC Reputation * % VC Director on Comp. Committee			2.314***			2.280**			2.710***
			(2.659)			(2.576)			(2.764)
% VC Director on Comp. Committee			0.809***			0.835***			0.812***
			(3.390)			(3.416)			(3.416)
High VC Reputation	-0.057	-0.056	-0.047	-0.057	-0.057	-0.046	-0.101	-0.100	-0.088
	(-0.240)	(-0.237)	(-0.200)	(-0.243)	(-0.240)	(-0.199)	(-0.423)	(-0.420)	(-0.375)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	No	No	No	Yes	Yes	Yes	No	No	No
Observations	9,535	9,535	9,535	9,398	9,398	9,398	9,398	9,398	9,398
Adjusted R-squared	0.526	0.526	0.526	0.524	0.524	0.524	0.525	0.525	0.525
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

State FE	No	No	No	No	No	No	Yes	Yes	Yes

Panel B: The impact of VC reputation on CEO delta

	CEO Delta $_{t+1}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
High VC Reputation * VC Director on Comp. Committee	1.044***			0.995***			1.113***		
	(4.122)			(3.867)			(3.937)		
VC Director on Comp. Committee	0.280**			0.294**			0.285**		
	(2.136)			(2.222)			(2.287)		
High VC Reputation * Number of VC Directors on Comp. Committee		1.032***			0.983***			1.099***	
		(4.148)			(3.885)			(3.930)	
Number of VC Directors on Comp. Committee		0.292**			0.306**			0.299**	
		(2.370)			(2.457)			(2.525)	
High VC Reputation * % VC Director on Comp. Committee			3.239***			3.063***			3.437***
			(3.666)			(3.406)			(3.361)
% VC Director on Comp. Committee			1.077***			1.108***			1.073***
			(3.023)			(3.069)			(3.108)
High VC Reputation	-0.115	-0.113	-0.088	-0.116	-0.115	-0.089	-0.181	-0.180	-0.151
	(-0.624)	(-0.617)	(-0.488)	(-0.633)	(-0.626)	(-0.490)	(-0.934)	(-0.926)	(-0.786)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	No	No	No	Yes	Yes	Yes	No	No	No
Observations	10,384	10,384	10,384	10,242	10,242	10,242	10,242	10,242	10,242
Adjusted R-squared	0.364	0.364	0.364	0.363	0.363	0.363	0.367	0.367	0.367
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No	No	Yes	Yes	Yes

Table 15. The heterogeneity of VC directors on the compensation committee impact on CEO vega

This table shows the heterogeneity of the effect of the presence of VC directors on the compensation committee on CEO risk-taking incentives (vega) using the non-VC-backed firm sample from 1998 to 2018. The dependent variable is the natural logarithm of CEO vega in year t+1 in Panel A and the natural logarithm of CEO delta in year t+1 in Panel B. The variable's median splits high and low groups. See Appendix B for variable definitions. The Fama-French 48 industry fixed effects and year fixed effects are as indicated. Baseline controls include firm size, leverage, market-to-book ratio, ROA, R&D intensity, tangibility, annual stock return, annual stock volatility, firm age, CEO age, CEO tenure, CEO duality, percent independent board, the board size, institutional ownership concentration, and institutional ownership total. We further control for CEO delta in Panel A. State controls include state educational attainment, state per-capita income, and state R&D per capita. Standard errors are clustered at the firm level. T-statistics are in parentheses. The ***, **, and * denote significance levels at 1%, 5% and 10% respectively.

Panel A.CEO vega

	CEO Vega $_{t+1}$											
	E index		% Institutional ownership		CEO pay slice		CEO age		% Independent board members		Business segments	
	High	Low	High	Low	High	Low	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VC Director on Comp. Committee	-0.013	0.500***	0.304**	0.160	0.294**	0.140	0.083	0.332***	0.136	0.346***	0.037	0.244***
	(-0.032)	(3.941)	(2.513)	(1.399)	(2.474)	(1.188)	(0.667)	(3.241)	(1.145)	(3.685)	(0.228)	(2.828)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,172	3,680	4,671	4,727	4,833	4,016	4,256	5,142	4,591	4,807	2,979	5,024
Adjusted R-squared	0.337	0.428	0.406	0.611	0.475	0.519	0.540	0.513	0.537	0.491	0.544	0.496
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B. CEO delta

	CEO Delta $_{t+1}$											
	E index		% Institutional ownership		CEO pay slice		CEO age		% Independent board members		Business segments	
	High	Low	High	Low	High	Low	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VC Director on Comp. Committee	-0.058	0.578***	0.346***	0.201	0.319**	0.227*	0.182	0.340***	0.159	0.408***	0.109	0.286***
	(-0.145)	(4.125)	(2.730)	(1.491)	(2.349)	(1.729)	(1.232)	(3.024)	(1.126)	(4.002)	(0.563)	(2.985)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,172	3,680	4,671	4,727	4,833	4,016	4,256	5,142	4,591	4,807	2,979	5,024
Adjusted R-squared	0.308	0.383	0.348	0.530	0.400	0.442	0.470	0.431	0.462	0.419	0.476	0.425
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 16. The effect of the presence of VC director on the compensation committee on the level of CEO pay

This table shows the effect of the presence of VC directors on the compensation committee on CEO level pay using the non-VC-backed firm sample from 1998 to 2018. The dependent variable total excess pay is the natural logarithm of the difference between CEO total compensation and predicted total compensation. Total pay is the natural logarithm of the total compensation of the CEO. Cash pay is the natural logarithm of CEO salary and bonus. Stock pay is the natural logarithm of the value of restricted stock grants. Option pay is the natural logarithm of the value of option grants to the CEO. Inside debt is the natural logarithm of the present value of each executive's pension benefits under all plans. Termination pay is the natural logarithm of the contractually stipulated severance pay. See Appendix B for variable definitions. The Fama-French 48 industry fixed effects and year fixed effects are as indicated. Baseline controls include firm size, leverage, market-to-book ratio, ROA, R&D intensity, tangibility, annual stock return, annual stock volatility, firm age, CEO age, CEO tenure, CEO duality, percent independent board, the board size, institutional ownership concentration, and institutional ownership total. State controls include state educational attainment, state per-capita income, and state R&D per capita. Standard errors are clustered at the firm level. T-statistics are in parentheses. The ***, **, and * denote significance levels at 1%, 5% and 10% respectively.

	Total excess compensation t+1	Total compensation t+1	Cash pay t+1	Stock pay t+1	Option pay t+1	Inside debt t+1	Termination pay t+1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VC Director on Comp. Committee	0.158**	0.169***	-0.029	0.313	0.655**	-0.206	-0.243
	(2.523)	(3.084)	(-0.321)	(0.888)	(2.356)	(-0.564)	(-0.429)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,630	10,789	10,797	10,640	10,616	5,447	5,451
Adjusted R-squared	0.179	0.584	0.304	0.325	0.125	0.387	0.108
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 17. The effect of the presence of VC director on CEO performance goals

This table shows the effect of the presence of VC directors on CEO performance goals using the non-VC backed firm sample from 1998 to 2018. In Panel A, the sample includes all CEO equity grants. The dependent variable Group One (i.e., Profitability Goals) indicator is equal to one if a grant has at least one performance goal with the metric of EPS, EBITDA, earnings, ROIC, ROE, EBT, profit margin, EBIT, FFO, EVA, ROA, net income, ROI, NOI, gross profit, ROC, ROS, or cost reduction, and zero otherwise; Group Two (i.e., Growth Goals) indicator is equal to one if a grant has at least one performance goal with the metric of sales, operating income, cashflow, gross revenues, same store sales, IPO of a subsidiary, book value, sales contracts, or working capital, and zero otherwise; Group Three (i.e., Market-based Goals) indicator is equal to one if a grant has at least one performance goal with the market-related metric of stock price or total shareholder return (TSR), and zero otherwise. Importantly, for all three groups, we include both the level and the growth of the relevant metrics. In Panel B models (1)-(6), the sample includes all the grants. The dependent variable Absolute Goals equals one if a grant has at least one absolute goal and zero otherwise. The Relative Goal is equal to one if a grant has at least one relative goal and zero otherwise. In Panel B models (7)-(9), the sample excludes the grants with both absolute and relative goals, and the dependent variable is Absolute Goals. See Appendix B for variable definitions. CEO fixed effects and year fixed effects are as indicated. Baseline controls include firm size, leverage, market-to-book ratio, ROA, R&D intensity, tangibility, annual stock return, annual stock volatility, firm age, CEO age, CEO tenure, CEO duality, percent independent board, the board size, institutional ownership concentration, and institutional ownership total. State controls include state educational attainment, state per-capita income, and state R&D per capita. Standard errors are clustered at the firm level. T-statistics are in parentheses. The ***, **, and * denote significance levels at 1%, 5% and 10% respectively.

Panel A Part I: Performance goals as groups

	Group One (Profitability Goals)			Group Two (Growth Goals)			Group Three (Market-based Goals)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VC Director on Comp. Committee	0.056			0.077*			-0.024		
	(0.826)			(1.808)			(-0.544)		
Number of VC Directors on Comp. Committee		0.056			0.077*			-0.024	
		(0.826)			(1.808)			(-0.544)	
% VC Director on Comp. Committee			0.219			0.295**			-0.106
			(0.993)			(2.450)			(-0.709)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,264	8,264	8,264	8,264	8,264	8,264	8,264	8,264	8,264
Adjusted R-squared	0.364	0.364	0.364	0.406	0.406	0.406	0.321	0.321	0.321
CEO FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel A Part II: Performance goals as groups using the Principle Components Analysis (PCA)

	Principle Component Group One (Profitability Goals)			Principle Component Group Two (Growth Goals)			Principle Component Group Three (Market-based Goals)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VC Director on Comp. Committee	-0.060			0.215**			0.017		
	(-0.427)			(2.394)			(0.211)		
Number of VC Directors on Comp. Committee		-0.060			0.215**			0.017	
		(-0.427)			(2.394)			(0.211)	
% VC Director on Comp. Committee			0.086			0.659**			0.006
			(0.185)			(2.397)			(0.025)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,264	8,264	8,264	8,264	8,264	8,264	8,264	8,264	8,264
Adjusted R-squared	0.462	0.462	0.462	0.425	0.425	0.425	0.320	0.320	0.320
CEO FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Absolute goals vs. Relative goals

	Absolute Goals			Relative Goals			Absolute vs. Relative		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VC Director on Comp. Committee	0.030			-0.065*			0.068*		
	(0.628)			(-1.900)			(1.763)		
Number of VC Directors on Comp. Committee		0.030			-0.065*			0.068*	
		(0.628)			(-1.900)			(1.763)	
% VC Director on Comp. Committee			0.089			-0.211*			0.195
			(0.649)			(-1.820)			(1.422)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,264	8,264	8,264	8,264	8,264	8,264	7,075	7,075	7,075
Adjusted R-squared	0.337	0.337	0.337	0.284	0.284	0.284	0.355	0.355	0.355
CEO FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 18. Instrument variable analysis: the effect of the presence of VC director on the compensation committee on CEO vega and delta

This table shows the instrument variable analysis of the effect of the presence of VC director on the compensation committee on CEO risk-taking incentives (vega) and CEO pay-performance sensitivity (delta) using the non-VC backed firm sample from 1998 to 2018. First stage models present the selection equations, with the dependent variable being one of the three VC directorship variables. In second stage models, the dependent variable is the natural logarithm of CEO vega in year t+1 or the natural logarithm of CEO delta in year t+1. See Appendix B for variable definitions. The Fama-French 48 industry fixed effects, firm fixed effects, and year fixed effects are as indicated. Baseline controls include firm size, leverage, market-to-book ratio, ROA, R&D intensity, tangibility, annual stock return, annual stock volatility, firm age, CEO age, CEO tenure, CEO duality, percent independent board, the board size, institutional ownership concentration, and institutional ownership total. In models where dependent variable is CEO vega, we further include CEO delta as a control. State controls include state educational attainment, state per-capita income, and state R&D per capita. Standard errors are clustered at the firm level. T-statistics are in parentheses. The ***, **, and * denote significance levels at 1%, 5% and 10% respectively.

Panel A: 2SLS analysis

	CEO Vega $_{t+1}$						CEO Delta $_{t+1}$					
Stage	First	Second	First	Second	First	Second	First	Second	First	Second	First	Second
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VC Director on Comp. Committee		3.886*						5.564**				
		(1.953)						(2.125)				
Number of VC Directors on Comp. Committee				3.454*						4.932**		
				(1.904)						(2.087)		
% VC Director on Comp. Committee						10.065*						14.007**
						(1.823)						(2.001)
Direct Flight -- SF	-0.004		-0.004		-0.002		-0.005		-0.004		-0.002	
	(-0.619)		(-0.549)		(-0.725)		(-0.656)		(-0.591)		(-0.755)	
Direct Flight -- BOS	0.010		0.010		0.002		0.011		0.010		0.002	
	(1.525)		(1.415)		(1.142)		(1.593)		(1.493)		(1.205)	
VC Dry Powder	-0.002***		-0.003***		-0.001**		-0.002***		-0.003***		-0.001**	
	(-2.664)		(-2.683)		(-2.414)		(-2.663)		(-2.681)		(-2.413)	
VC Dry Powder Squared	0.000***		0.000***		0.000**		0.000***		0.000***		0.000**	
	(2.600)		(2.641)		(2.465)		(2.602)		(2.642)		(2.466)	
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,076	9,076	9,076	9,076	9,076	9,076	9,076	9,076	9,076	9,076	9,076	9,076

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Adjusted R-squared		0.409		0.425		0.448		0.130		0.164		0.216
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-stat for Excluded Instruments		2.41		2.47		2.32		2.45		2.52		2.37
Cragg-Donald F-stat		7.12		7.36		9.22		7.26		7.49		9.33
CD Nearest Lower Critical Value		6.71		6.71		6.71		6.71		6.71		6.71
CD Nearest Lower Critical %		20%		20%		20%		20%		20%		20%
Kleibergen-Paap p-value		0.05		0.04		0.06		0.04		0.04		0.05
Sargan p-value		0.13		0.08		0.09		0.04		0.01		0.02

Panel B: 2SLS analysis with robust Sargan tests

	CEO Vega $_{t+1}$						CEO Delta $_{t+1}$					
Stage	First	Second	First	Second	First	Second	First	Second	First	Second	First	Second
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VC Director on Comp. Committee		3.198*						4.588**				
		(1.868)						(2.253)				
Number of VC Directors on Comp. Committee				2.880*						4.103**		
				(1.805)						(2.174)		
% VC Director on Comp. Committee						8.024*						11.105*
						(1.696)						(1.958)
Direct Flight to BOS	0.008**		0.008**		0.002		0.008**		0.008**		0.002	
	(2.334)		(2.144)		(1.178)		(2.420)		(2.238)		(1.244)	
VC Dry Powder	-0.002***		-0.003***		-0.001***		-0.002***		-0.003***		-0.001***	
	(-4.397)		(-4.479)		(-3.943)		(-4.399)		(-4.480)		(-3.946)	
VC Dry Powder Squared	0.000***		0.000***		0.000***		0.000***		0.000***		0.000***	
	(4.146)		(4.265)		(3.765)		(4.158)		(4.276)		(3.777)	
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,076	9,076	9,076	9,076	9,076	9,076	9,076	9,076	9,076	9,076	9,076	9,076
Adjusted R-squared		0.447		0.455		0.476		0.206		0.226		0.271
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sargan p-value		0.58		0.51		0.38		0.26		0.20		0.11

Panel C: 2SLS analysis with firm and year fixed effects

	CEO Vega $_{t+1}$						CEO Delta $_{t+1}$					
Stage	First	Second	First	Second	First	Second	First	Second	First	Second	First	Second
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VC Director on Comp. Committee		4.997*						4.249				
		(1.877)						(1.562)				
Number of VC Directors on Comp. Committee				4.992*						4.244		
				(1.875)						(1.560)		
% VC Director on Comp. Committee						11.985*						9.164
						(1.702)						(1.265)
Direct Flight -- SF	0.027		0.027		0.008		0.027		0.027		0.008	
	(1.468)		(1.463)		(1.527)		(1.469)		(1.464)		(1.526)	
Direct Flight -- BOS	0.038		0.038		0.011		0.038		0.038		0.011	
	(1.585)		(1.577)		(1.547)		(1.591)		(1.584)		(1.540)	
VC Dry Powder	-0.001		-0.001		-0.000		-0.001		-0.001		-0.000	
	(-1.303)		(-1.315)		(-1.479)		(-1.299)		(-1.310)		(-1.474)	
VC Dry Powder Squared	0.000		0.000		0.000*		0.000		0.000		0.000*	
	(1.528)		(1.540)		(1.678)		(1.523)		(1.534)		(1.671)	
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,872	8,872	8,872	8,872	8,872	8,872	8,872	8,872	8,872	8,872	8,872	8,872
Adjusted R-squared		-0.259		-0.263		-0.141		-0.270		-0.273		-0.174
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-stat for Excluded Instruments		1.87		1.87		1.94		1.87		1.88		1.95
Cragg-Donald F-stat		9.83		9.62		11.14		9.85		9.65		11.13
CD Nearest Lower Critical Value		6.71		6.71		10.27		6.71		6.71		10.27
CD Nearest Lower Critical %		20%		20%		10%		20%		20%		10%
Kleibergen-Paap p-value		0.13		0.13		0.12		0.13		0.13		0.12
Sargan p-value		0.14		0.14		0.05		0.02		0.02		0.01

Panel D: Dynamic GMM analysis

	CEO Vega $_{t+1}$			CEO Delta $_{t+1}$		
	(1)	(2)	(3)	(4)	(5)	(6)
VC Director on Comp. Committee	0.574^{**}			1.000^{***}		
	(2.096)			(2.613)		
Number of VC Directors on Comp. Committee		0.526^{**}			0.985^{***}	
		(2.021)			(2.707)	
% VC Director on Comp. Committee			2.392			3.887^{*}
			(1.590)			(1.864)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,499	8,499	8,499	8,499	8,499	8,499
Number of Firms	1,331	1,331	1,331	1,331	1,331	1,331
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
F-stat for Excluded Instruments	6.12	5.42	3.33	6.29	5.57	3.36
p-value for Excluded Instruments	0.00	0.00	0.00	0.00	0.00	0.00
p-value for AR(1)	0.00	0.00	0.00	0.00	0.00	0.00
p-value for AR(2)	0.60	0.59	0.57	0.02	0.02	0.02
Hensen J-stat	37.17	37.70	36.51	27.80	27.90	27.70
Hensen J-stat p-value	0.12	0.10	0.13	0.53	0.52	0.53

Table 19. Matched sample analysis: the effect of the presence of VC director on the compensation committee on CEO vega and delta

This table shows the effect of the presence of VC directors on the compensation committee on CEO risk-taking incentives using the matched sample. The dependent variable is the natural logarithm of CEO vega in year t+1 in Panel A and the natural logarithm of CEO delta in year t+1 in Panel B. The treatment group is the firms with VC directors on board. The control group is matched on the industry, fiscal year, firm size, CEO vega at time t-1, CEO delta at time t-1, leverage, market-to-book ratio, ROA, R&D intensity, tangibility, annual stock return, annual stock volatility, and firm age. See Appendix B for variable definitions. The Fama-French 48 industry fixed effects and year fixed effects are as indicated. Baseline controls include firm size, leverage, market-to-book ratio, ROA, R&D intensity, tangibility, annual stock return, annual stock volatility, firm age, CEO age, CEO tenure, CEO duality, percent independent board, the board size, institutional ownership concentration, and institutional ownership total. In Panel A, we further include CEO delta at time t as a control. State controls include state educational attainment, state per-capita income, and state R&D per capita. Standard errors are clustered at the firm level. T-statistics are in parentheses. The ***, **, and * denote significance levels at 1%, 5% and 10% respectively.

Panel A: CEO vega

	CEO Vega $_{t+1}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VC Director on Comp. Committee	0.206**			0.219**			0.222**		
	(2.248)			(2.417)			(2.516)		
Number of VC Directors on Comp. Committee		0.205**			0.218***			0.223***	
		(2.441)			(2.614)			(2.727)	
% VC Director on Comp. Committee			0.786***			0.795***			0.819***
			(2.949)			(2.933)			(3.131)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	No	No	No	Yes	Yes	Yes	No	No	No
Observations	788	788	788	785	785	785	785	785	785
Adjusted R-squared	0.533	0.533	0.535	0.531	0.531	0.533	0.554	0.555	0.556
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No	No	Yes	Yes	Yes

Panel B: CEO delta

	CEO Delta $_{t+1}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VC Director on Comp. Committee	0.298**			0.300**			0.287**		
	(2.312)			(2.376)			(2.338)		
Number of VC Directors on Comp. Committee		0.306***			0.308***			0.302***	
		(2.590)			(2.658)			(2.658)	
% VC Director on Comp. Committee			1.062***			1.025***			1.014***
			(3.035)			(2.933)			(3.017)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	No	No	No	Yes	Yes	Yes	No	No	No
Observations	788	788	788	785	785	785	785	785	785
Adjusted R-squared	0.371	0.372	0.373	0.372	0.374	0.374	0.418	0.419	0.420
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No	No	Yes	Yes	Yes

Table 20. The effect of the presence of VC director on the compensation committee on R&D intensity

This table shows the effect of the presence of VC directors on the compensation committee on R&D intensity using the non-VC-backed firm sample from 1998 to 2018. The dependent variable is R&D expenditure scaled by total assets. See Appendix B for variable definitions. The Fama-French 48 industry fixed effects and year fixed effects are as indicated. State controls include state educational attainment, state per-capita income, and state R&D per capita. Standard errors are clustered at the firm level. T-statistics are in parentheses. The ***, **, and * denote significance levels at 1%, 5% and 10% respectively.

	R&D Intensity $_{t+1}$			
	(1)	(2)	(3)	(4)
VC Director on Comp. Committee * CEO Vega $_{t+1}$		0.005**		
		(2.322)		
CEO Vega $_{t+1}$	0.002***	0.001***		
	(3.924)	(3.717)		
VC Director on Comp. Committee * CEO Delta $_{t+1}$				0.004**
				(1.967)
CEO Delta $_{t+1}$			0.002***	0.002***
			(4.203)	(3.996)
VC Director on Comp. Committee	0.005	-0.013	0.005	-0.009
	(1.385)	(-1.603)	(1.349)	(-1.264)
Size	-0.002**	-0.002**	-0.002**	-0.002**
	(-2.332)	(-2.323)	(-2.416)	(-2.404)
Leverage	-0.028***	-0.028***	-0.028***	-0.028***
	(-6.229)	(-6.250)	(-6.243)	(-6.266)
Market-to-Book	0.000	0.000	0.000	0.000
	(1.138)	(1.140)	(1.171)	(1.171)
ROA	-0.116***	-0.117***	-0.116***	-0.116***
	(-4.834)	(-4.852)	(-4.820)	(-4.835)
Tangibility	-0.011***	-0.011***	-0.011***	-0.011***
	(-2.786)	(-2.763)	(-2.766)	(-2.740)
Firm Age	-0.000	-0.000	-0.000	-0.000
	(-0.538)	(-0.560)	(-0.459)	(-0.477)
Board Size	-0.000	-0.000	-0.000	-0.000
	(-0.810)	(-0.814)	(-0.748)	(-0.746)
Tobin's Q	0.008***	0.008***	0.008***	0.008***
	(6.170)	(6.185)	(6.132)	(6.145)
State Controls	Yes	Yes	Yes	Yes
Observations	11,252	11,252	11,252	11,252
Adjusted R-squared	0.404	0.405	0.405	0.405
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 21. The effect of the presence of VC director on the compensation committee on innovation output

This table shows the Tobit model of the effect of the presence of VC directors on the compensation committee on innovation output using the non-VC-backed firm sample from 1998 to 2018. The patent count is the number of patents issued in year t+1. Forward 5-yr Citations is the sum of patent citations from year t+1 to year t+5. Innovation value (real) is the real innovation value in year t+1. Innovation value (nominal) is the nominal innovation value in year t+1. Forward 5-yr innovation value (real) is the real innovation value from year t+1 to year t+5. Forward 5-yr innovation value (nominal) is the nominal innovation value from year t+1 to year t+5. All innovation output measures are adjusted by industry and year. Lag CEO Vega is the average of Ln CEO vega from t-1 to t-3. See Appendix B for variable definitions. The Fama-French 48 industry fixed effects and year fixed effects are as indicated. Baseline controls include firm size, age, leverage, market-to-book ratio, ROA, tangibility, Tobin's Q, and board size. State controls include state educational attainment, state per-capita income, and state R&D per capita. Standard errors are clustered at the firm level. T-statistics are in parentheses. The ***, **, and * denote significance levels at 1%, 5% and 10% respectively.

Panel A. CEO vega

	Patent Count $_{t+1}$		Forward 5-yr Citations		Innovation Value (Real) $_{t+1}$		Innovation Value (Nominal) $_{t+1}$		Forward 5-yr Innovation Value (Real)		Forward 5-yr Innovation Value (Nominal)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VC Director on Comp. Committee * Lag CEO Vega		0.651**		0.631		0.808*		0.807*		0.381**		0.923**
		(2.088)		(1.636)		(1.858)		(1.857)		(2.132)		(2.077)
Lag CEO Vega	0.156	0.142	0.237	0.223	0.181	0.163	0.180	0.163	0.049	0.041	0.111	0.092
	(0.595)	(0.535)	(1.555)	(1.443)	(1.222)	(1.085)	(1.217)	(1.079)	(0.839)	(0.698)	(0.847)	(0.695)
VC Director on Comp. Committee	1.383**	-0.890	1.630**	-0.542	1.375**	-1.453	1.375**	-1.451	0.437*	-0.878	1.050*	-2.139
	(2.012)	(-0.729)	(2.125)	(-0.391)	(2.356)	(-1.021)	(2.360)	(-1.020)	(1.681)	(-1.472)	(1.774)	(-1.471)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,736	13,736	12,571	12,571	13,736	13,736	13,736	13,736	12,859	12,859	12,859	12,859
Pseudo R-squared	0.089	0.089	0.092	0.092	0.113	0.113	0.113	0.113	0.133	0.133	0.103	0.103
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B. CEO delta

	Patent Count $_{t+1}$		Forward 5-yr Citations		Innovation Value (Real) $_{t+1}$		Innovation Value (Nominal) $_{t+1}$		Forward 5-yr Innovation Value (Real)		Forward 5-yr Innovation Value (Nominal)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VC Director on Comp. Committee * Lag CEO Delta		0.630**		0.560		1.021**		1.021**		0.471**		1.113**
		(2.021)		(1.436)		(2.306)		(2.305)		(2.446)		(2.409)
Lag CEO Delta	0.148	0.132	0.194	0.181	0.153	0.128	0.152	0.127	0.043	0.032	0.092	0.067
	(0.600)	(0.531)	(1.345)	(1.237)	(1.085)	(0.900)	(1.078)	(0.893)	(0.771)	(0.577)	(0.734)	(0.532)
VC Director on Comp. Committee	1.378**	-0.841	1.627**	-0.340	1.373**	-2.242	1.373**	-2.243	0.436*	-1.225**	1.049*	-2.880*
	(2.001)	(-0.595)	(2.113)	(-0.206)	(2.358)	(-1.604)	(2.362)	(-1.604)	(1.682)	(-1.975)	(1.776)	(-1.924)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,736	13,736	12,571	12,571	13,736	13,736	13,736	13,736	12,859	12,859	12,859	12,859
Pseudo R-squared	0.088	0.089	0.092	0.092	0.112	0.113	0.113	0.113	0.133	0.133	0.103	0.103
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 22. The effect of the presence of VC director on the governance or nominating committee on CEO forced turnover

This table shows the effect of the presence of VC directors on the governance or nominating committee on CEO forced turnover using the non-VC-backed firm sample from 1998 to 2018. The dependent variable CEO forced turnover is an indicator variable taking the value of one if CEO turnover is forced and zero otherwise. Abnormal return is the industry and year adjusted abnormal return calculated by subtracting the S&P 500 annual return from annual stock raw return. See Appendix B for variable definitions. The Fama-French 48 industry fixed effects and year fixed effects are as indicated. Baseline controls include firm size, leverage, market-to-book ratio, ROA, R&D intensity, tangibility, annual stock volatility, firm age, CEO age, CEO tenure, CEO duality, percent independent board, the board size, institutional ownership concentration, and institutional ownership total. Standard errors are clustered at the firm level. T-statistics are in parentheses. The ***, **, and * denote significance levels at 1%, 5% and 10% respectively.

	CEO Forced Turnover		
	(1)	(2)	(3)
VC Director on Nom. Committee * Abnormal Return	-0.046*		
	(-1.664)		
VC Director on Nom. Committee	0.007		
	(0.799)		
VC Director on Gov. Committee * Abnormal Return		-0.046	
		(-1.508)	
VC Director on Gov. Committee		0.004	
		(0.485)	
VC Director on Nom. or Gov. Committee * Abnormal Return			-0.046*
			(-1.662)
VC Director on Nom. or Gov. Committee			0.007
			(0.791)
Abnormal Return	-0.019***	-0.019***	-0.019***
	(-5.823)	(-5.868)	(-5.823)
Baseline Controls	Yes	Yes	Yes
Observations	16,085	16,085	16,085
Adjusted R-squared	0.024	0.024	0.024
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Table 23. Placebo test: the effect of the presence of coincidental VC director on the compensation committee on CEO vega and delta

This table shows the placebo tests of the baseline results. A coincidental VC director is an ordinary director who eventually gets employed by a VC firm during their tenure (but not before) as a director. The dependent variable is the natural logarithm of CEO vega in year t+1 in column (1)-(6), and the natural logarithm of CEO delta in year t+1 in column (7)-(12). See Appendix B for variable definitions. The Fama-French 48 industry fixed effects and year fixed effects are as indicated. Baseline controls include firm size, leverage, market-to-book ratio, ROA, R&D intensity, tangibility, annual stock return, annual stock volatility, firm age, CEO age, CEO tenure, CEO duality, percent independent board, the board size, institutional ownership concentration, and institutional ownership total. State controls include state educational attainment, state per-capita income, and state R&D per capita. For columns (1)-(6), we also include CEO delta at time t as a control variable. Standard errors are clustered at the firm level. T-statistics are in parentheses. The ***, **, and * denote significance levels at 1%, 5% and 10% respectively.

	CEO Vega $_{t+1}$						CEO Delta $_{t+1}$					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Coincidental VC Director on Comp. Committee	0.053	0.050					0.060	0.053				
	(0.840)	(0.799)					(0.662)	(0.587)				
VC Director on Comp. Committee		0.235***						0.311**				
		(2.710)						(2.379)				
Coincidental Number of VC Directors on Comp. Committee			0.057	0.055					0.060	0.053		
			(0.913)	(0.877)					(0.679)	(0.605)		
Number of VC Directors on Comp. Committee				0.235***						0.321***		
				(2.947)						(2.616)		
Coincidental % VC Director on Comp. Committee					0.298	0.294					0.379	0.363
					(1.333)	(1.318)					(1.278)	(1.236)
% VC Director on Comp. Committee						0.874***						1.150***
						(3.569)						(3.208)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,398	9,398	9,398	9,398	9,398	9,398	10,242	10,242	10,242	10,242	10,242	10,242
Adjusted R-squared	0.524	0.524	0.524	0.524	0.524	0.524	0.362	0.363	0.362	0.363	0.363	0.364
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No	No	No	No	No	No	No	No

Table 24. Summary statistics

This table reports the summary statistics of the full sample in Panel A, and the sample of politically connected insiders and non-politically connected insiders in Panel B. CARs are calculated using market-adjusted returns against CRSP equally-weighted market index. Rest of the CAR measures are reported in Table A13. *PCinsider* is an indicator variable which equals one if a corporate insider is a current or former member of government agencies. *Buy* is an indicator variable which equals one if the transaction is a purchase. *Sell* is an indicator variable which equals one if the transaction is a sale. *Congress* is an indicator variable which equals one if a corporate insider is a US Congress member or staff when the transaction is made. *PastMonthRet* is the raw return over the month *t-1* if the transaction happens in month *t*. *PastYearRet* is the raw return over the past year from month *t-2* to month *t-12* if the transaction happens in month *t*. *Size* is the natural logarithm of firm market value in $millions at the end of month *t*. *BookMarket* is the Book-to-market ratio at the end of month *t*, where book value is from the prior quarter-end. *TradeSize* is the Natural logarithm of insider transaction size in $millions. *HOR* is the measure of insider trading horizon following Akbas, Jiang, and Koch (2020). *Routine* is an indicator variable which equals one if an insider makes trades in the same calendar month for three consecutive years, following Cohen, Malloy, and Pomorski (2012). *CEO* is an indicator variable if an insider is CEO. *Director* is an indicator variable if an insider is a director. *Officer* is an indicator variable if an insider is an officer. *PhD* is an indicator variable if an insider has a Ph.D. *MBA* is an indicator variable if an insider has an MBA. *JD* is an indicator variable if an insider has a JD. *CPA* is an indicator variable if an insider has a CPA. *CFA* is an indicator variable if an insider has a CFA. *Age* is the age of insiders collected from BoardEx. *Networksize* is the size of network collected from BoardEx. Detailed variable definitions are in Appendix C.

Panel A. Summary statistics of full sample

Variables	N	Mean	SD	Min	Max
Transaction-level sample					
CAR[0,30]	1,384,164	0.003	0.176	-2.463	17.728
CAR[0,60]	1,384,215	-0.002	0.246	-2.618	18.184
CAR[0,120]	1,384,297	-0.010	0.351	-3.831	23.657
CAR[0,255]	1,384,340	-0.025	0.529	-7.910	38.447
PCinsider	1,414,214	0.026	0.160	0	1
Buy	1,414,214	0.283	0.450	0	1
Sell	1,414,214	0.717	0.450	0	1
Congress	37,078	0.004	0.060	0	1
PastMonthRet	1,384,573	0.028	0.179	-0.894	10.344
PastYearRet	1,314,245	0.227	1.228	-0.993	248.600
Size	1,408,496	6.405	2.078	-1.765	13.690
BookMarket	1,397,792	0.549	1.225	-383.688	108.073
TradeSize	1,414,196	-2.526	2.215	-19.676	11.375
Insider-firm-year-level sample					
HOR	190,385	0.799	0.331	0	1
Routine	87,271	0.345	0.475	0	1
CEO	494,207	0.081	0.273	0	1
Director	494,207	0.524	0.499	0	1
Officer	494,207	0.527	0.499	0	1
PhD	360,501	0.057	0.233	0	1
MBA	360,501	0.265	0.441	0	1
JD	360,501	0.099	0.299	0	1
CPA	360,501	0.115	0.319	0	1
CFA	360,501	0.007	0.086	0	1
Age	350,963	68.371	10.277	30	105
NetworkSize	334,022	1052.137	1536.329	1	21,294

Panel B. Summary statistics by *PCinsider*

	PCinsider=0					PCinsider=1					Mean(1)-Mean(0)	
Variables	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max	Difference	t-value
						Transaction-level sample						
CAR[0,30]	1,347,633	0.003	0.177	-2.463	17.728	36,531	0.005	0.160	-1.415	2.427	0.002***	2.123
CAR[0,60]	1,347,683	-0.002	0.247	-2.618	18.184	36,532	0.004	0.222	-1.687	2.492	0.005***	4.136
CAR[0,120]	1,347,765	-0.010	0.352	-3.831	23.657	36,532	0.000	0.323	-2.703	6.768	0.010***	5.463
CAR[0,255]	1,347,808	-0.025	0.530	-7.910	38.447	36,532	-0.016	0.487	-4.329	8.225	0.009***	3.217
Buy	1,377,136	0.282	0.450	0	1	37,078	0.314	0.464	0	1	0.032***	13.671
Sell	1,377,136	0.718	0.450	0	1	37,078	0.686	0.464	0	1	-0.032***	-13.671
PastMonthRet	1,348,028	0.028	0.180	-0.894	10.344	36,545	0.024	0.159	-0.880	5.327	-0.004***	-4.683
PastYearRet	1,278,875	0.227	1.175	-0.993	248.600	35,370	0.231	2.467	-0.984	248.600	0.004	0.548
Size	1,371,521	6.381	2.067	-1.765	13.690	36,975	7.299	2.264	0.462	13.485	0.918***	84.081
BookMarket	1,361,031	0.550	1.234	-383.688	108.073	36,761	0.502	0.820	-11.243	33.802	-0.048***	-7.360
TradeSize	1,377,118	-2.535	2.213	-19.676	11.375	37,078	-2.192	2.253	-15.291	7.187	0.343***	29.450
						Insider-level sample						
HOR	184,583	0.799	0.331	0	1	5,802	0.805	0.330	0	1	0.006	1.289
Routine	84,317	0.344	0.475	0	1	2,954	0.372	0.483	0	1	0.027***	3.075
CEO	480,861	0.081	0.274	0	1	13,346	0.072	0.259	0	1	-0.009***	-3.832
Director	480,861	0.516	0.500	0	1	13,346	0.824	0.381	0	1	0.308***	70.641
Officer	480,861	0.534	0.499	0	1	13,346	0.257	0.437	0	1	-0.277***	-63.489
PhD	347,155	0.054	0.225	0	1	13,346	0.154	0.361	0	1	0.100***	48.919
MBA	347,155	0.268	0.443	0	1	13,346	0.194	0.396	0	1	-0.074***	-18.970
JD	347,155	0.095	0.293	0	1	13,346	0.223	0.416	0	1	0.128***	48.762
CPA	347,155	0.118	0.323	0	1	13,346	0.036	0.187	0	1	-0.082***	-29.233
CFA	347,155	0.008	0.086	0	1	13,346	0.004	0.063	0	1	-0.004***	-4.711
Age	337,692	68.120	10.218	31	105	13,271	74.766	9.682	30	103	6.646***	73.636
NetworkSize	322,491	1010.673	1483.181	1	21,294	11,531	2211.748	2335.845	7	18,224	1201.074***	83.341

Table 25. Politically connected insider transactions and their informativeness with time fixed effects

This table reports the regression results of politically connected insider transactions and subsequent returns with time fixed effects using CAR[0,30] and CAR[0,60] in Panel A, and CAR[0,120] and CAR[0,255] in Panel B. *PCinsider* is an indicator variable which equals one if a corporate insider is a current or former member of government agencies. *Buy* is an indicator variable which equals one if the transaction is a purchase. *Sell* is an indicator variable which equals one if the transaction is a sale. *PastMonthRet* is the raw return over the month *t-1* if the transaction happens in month *t*. *PastYearRet* is the raw return over the past year from month *t-2* to month *t-12* if the transaction happens in month *t*. *Size* is the natural logarithm of firm market value in $millions at the end of month *t*. *BookMarket* is the Book-to-market ratio at the end of month *t*, where book value is from the prior quarter-end. Detailed variable definitions are in Appendix C.

Panel A. CAR[0,30] and CAR[0,60]

	(1) [0,30] MAR EW	(2) [0,30] MAR VW	(3) [0,30] MM EW	(4) [0,30] MM VW	(5) [0,60] MAR EW	(6) [0,60] MAR VW	(7) [0,60] MM EW	(8) [0,60] MM VW
*PCinsider*Buy*	-0.009***	-0.009***	-0.006*	-0.007**	-0.014***	-0.015***	-0.011**	-0.011**
	(-2.77)	(-2.93)	(-1.68)	(-2.13)	(-3.16)	(-3.17)	(-2.01)	(-2.18)
*PCinsider*Sell*	0.007*	0.007*	0.005	0.006*	0.016**	0.016**	0.013**	0.014**
	(1.65)	(1.67)	(1.49)	(1.84)	(2.11)	(2.09)	(2.12)	(2.37)
Buy	0.033***	0.033***	0.054***	0.055***	0.046***	0.046***	0.088***	0.089***
	(25.56)	(25.47)	(32.20)	(32.23)	(22.79)	(22.70)	(29.49)	(29.36)
PastMonthRet	-0.013**	-0.013**	-0.044***	-0.042***	-0.012*	-0.011*	-0.070***	-0.069***
	(-2.31)	(-2.33)	(-8.26)	(-7.62)	(-1.70)	(-1.65)	(-9.78)	(-9.70)
PastYearRet	0.002***	0.002***	-0.020***	-0.021***	0.001	0.001	-0.041***	-0.043***
	(2.87)	(2.78)	(-5.02)	(-5.06)	(1.32)	(1.30)	(-5.05)	(-5.07)
Size	-0.003***	-0.003***	-0.003***	-0.003***	-0.006***	-0.006***	-0.006***	-0.005***
	(-9.97)	(-10.09)	(-9.13)	(-8.39)	(-11.68)	(-11.58)	(-9.20)	(-8.53)
BookMarket	-0.001	-0.001	0.004***	0.004***	0.002	0.002	0.010***	0.011***
	(-1.01)	(-0.99)	(3.47)	(3.39)	(1.56)	(1.58)	(4.58)	(4.60)
Observations	1,306,045	1,306,045	1,306,045	1,306,045	1,306,045	1,306,045	1,306,045	1,306,045
Adjusted R-squared	0.020	0.042	0.060	0.086	0.023	0.049	0.087	0.119
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	No	No	No	No	No

Panel B. CAR[0,120] and CAR[0,255]

	(1) [0,120] MAR EW	(2) [0,120] MAR VW	(3) [0,120] MM EW	(4) [0,120] MM VW	(5) [0,255] MAR EW	(6) [0,255] MAR VW	(7) [0,255] MM EW	(8) [0,255] MM VW
*PCinsider*Buy*	-0.019***	-0.020***	-0.016	-0.015	-0.024**	-0.025**	-0.021	-0.019
	(-2.74)	(-2.80)	(-1.50)	(-1.54)	(-2.03)	(-2.08)	(-1.03)	(-0.95)
*PCinsider*Sell*	0.029**	0.029**	0.022**	0.024**	0.039	0.039	0.029	0.033
	(2.21)	(2.19)	(2.13)	(2.37)	(1.46)	(1.46)	(1.26)	(1.47)
Buy	0.055***	0.055***	0.137***	0.140***	0.064***	0.065***	0.237***	0.243***
	(16.39)	(16.45)	(24.48)	(24.43)	(10.71)	(10.76)	(21.05)	(21.05)
PastMonthRet	-0.017**	-0.017**	-0.133***	-0.131***	-0.030**	-0.029**	-0.272***	-0.269***
	(-2.01)	(-1.99)	(-12.28)	(-12.10)	(-2.29)	(-2.22)	(-13.44)	(-13.19)
PastYearRet	0.000	0.000	-0.082***	-0.087***	-0.006**	-0.006**	-0.181***	-0.189***
	(0.17)	(0.15)	(-5.04)	(-5.04)	(-2.44)	(-2.41)	(-5.14)	(-5.13)
Size	-0.012***	-0.012***	-0.011***	-0.011***	-0.027***	-0.027***	-0.025***	-0.023***
	(-12.84)	(-12.77)	(-9.66)	(-8.82)	(-14.68)	(-14.57)	(-10.52)	(-9.62)
BookMarket	0.008***	0.008***	0.025***	0.026***	0.017***	0.017***	0.052***	0.054***
	(3.50)	(3.50)	(5.40)	(5.37)	(3.64)	(3.63)	(5.68)	(5.69)
Observations	1,306,045	1,306,045	1,306,045	1,306,045	1,306,045	1,306,045	1,306,045	1,306,045
Adjusted R-squared	0.028	0.054	0.122	0.157	0.039	0.061	0.168	0.197
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	No	No	No	No	No

Table 26. Politically connected insider transactions and their informativeness with firm and time fixed effects

This table reports the regression results of politically connected insider transactions and subsequent returns with time and firm fixed effects using CAR[0,30] and CAR[0,60] in Panel A, and CAR[0,120] and CAR[0,255] in Panel B. *PCinsider* is an indicator variable which equals one if a corporate insider is a current or former member of government agencies. *Buy* is an indicator variable which equals one if the transaction is a purchase. *Sell* is an indicator variable which equals one if the transaction is a sale. *PastMonthRet* is the raw return over the month *t-1* if the transaction happens in month *t*. *PastYearRet* is the raw return over the past year from month *t-2* to month *t-12* if the transaction happens in month *t*. *Size* is the natural logarithm of firm market value in $millions at the end of month *t*. *BookMarket* is the Book-to-market ratio at the end of month *t*, where book value is from the prior quarter-end. Detailed variable definitions are in Appendix C.

Panel A. CAR[0,30] and CAR[0,60]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	[0,30]	[0,30]	[0,30]	[0,30]	[0,60]	[0,60]	[0,60]	[0,60]
	MAR EW	MAR VW	MM EW	MM VW	MAR EW	MAR VW	MM EW	MM VW
*PCinsider*Buy*	-0.009***	-0.009***	-0.008***	-0.010***	-0.012***	-0.012***	-0.012***	-0.013***
	(-3.08)	(-3.14)	(-2.58)	(-3.11)	(-2.65)	(-2.68)	(-2.75)	(-3.01)
*PCinsider*Sell*	0.002	0.002	-0.001	0.000	0.004	0.004	-0.000	0.001
	(0.93)	(0.98)	(-0.41)	(0.11)	(1.32)	(1.28)	(-0.05)	(0.25)
Buy	0.037***	0.037***	0.052***	0.052***	0.046***	0.046***	0.076***	0.077***
	(30.06)	(29.93)	(34.75)	(34.92)	(25.53)	(25.46)	(31.07)	(30.79)
PastMonthRet	-0.022***	-0.023***	-0.033***	-0.031***	-0.021***	-0.021***	-0.039***	-0.037***
	(-4.35)	(-4.34)	(-6.47)	(-5.82)	(-3.29)	(-3.22)	(-5.63)	(-5.31)
PastYearRet	0.000	0.000	-0.019***	-0.020***	-0.000	-0.000	-0.037***	-0.039***
	(0.15)	(0.12)	(-4.67)	(-4.71)	(-0.45)	(-0.47)	(-4.73)	(-4.75)
Size	-0.023***	-0.023***	-0.040***	-0.040***	-0.065***	-0.064***	-0.095***	-0.097***
	(-22.57)	(-22.56)	(-29.69)	(-29.40)	(-37.72)	(-37.60)	(-37.46)	(-37.18)
BookMarket	-0.001*	-0.001*	0.001	0.001	-0.001	-0.001	0.004*	0.004*
	(-1.82)	(-1.80)	(1.16)	(1.09)	(-1.25)	(-1.23)	(1.69)	(1.69)
Observations	1,305,652	1,305,652	1,305,652	1,305,652	1,305,652	1,305,652	1,305,652	1,305,652
Adjusted R-squared	0.107	0.127	0.146	0.170	0.158	0.181	0.210	0.242
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B. CAR[0,120] and CAR[0,255]

	(1) [0,120] MAR EW	(2) [0,120] MAR VW	(3) [0,120] MM EW	(4) [0,120] MM VW	(5) [0,255] MAR EW	(6) [0,255] MAR VW	(7) [0,255] MM EW	(8) [0,255] MM VW
*PCinsider*Buy*	-0.012*	-0.012*	-0.017**	-0.016**	-0.019*	-0.020*	-0.038***	-0.034**
	(-1.66)	(-1.69)	(-2.24)	(-2.31)	(-1.89)	(-1.94)	(-2.63)	(-2.54)
*PCinsider*Sell*	0.005	0.005	-0.004	-0.003	-0.007	-0.008	-0.023**	-0.020*
	(0.93)	(0.91)	(-0.73)	(-0.44)	(-0.78)	(-0.78)	(-2.05)	(-1.70)
Buy	0.047***	0.048***	0.107***	0.108***	0.049***	0.050***	0.175***	0.176***
	(16.89)	(16.98)	(24.59)	(24.29)	(10.49)	(10.57)	(20.53)	(20.15)
PastMonthRet	-0.027***	-0.027***	-0.065***	-0.060***	-0.034***	-0.034***	-0.116***	-0.107***
	(-3.57)	(-3.53)	(-6.82)	(-6.28)	(-3.34)	(-3.31)	(-7.11)	(-6.50)
PastYearRet	-0.001	-0.001	-0.073***	-0.077***	-0.006***	-0.006***	-0.159***	-0.167***
	(-0.54)	(-0.56)	(-4.73)	(-4.72)	(-2.81)	(-2.81)	(-4.84)	(-4.83)
Size	-0.134***	-0.134***	-0.195***	-0.199***	-0.270***	-0.269***	-0.398***	-0.403***
	(-43.35)	(-43.29)	(-39.25)	(-38.68)	(-46.47)	(-46.36)	(-39.61)	(-38.88)
BookMarket	0.001	0.001	0.011**	0.011**	0.001	0.001	0.022***	0.023***
	(0.51)	(0.53)	(2.56)	(2.57)	(0.25)	(0.27)	(2.72)	(2.75)
Observations	1,305,652	1,305,652	1,305,652	1,305,652	1,305,652	1,305,652	1,305,652	1,305,652
Adjusted R-squared	0.233	0.253	0.298	0.331	0.360	0.375	0.395	0.422
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 27. Politically connected insiders and insider investment horizon

This table reports the regression results of politically connected insiders and their trading horizons. The dependent variable *HOR* is the measure of insider trading horizon following Akbas, Jiang, and Koch (2020). *PCinsider* is an indicator variable which equals one if a corporate insider is a current or former member of government agencies. *Size* is the natural logarithm of firm market value in $millions at the end of year *y*. *BookMarket* is the Book-to-market ratio at the end of year *y*. Detailed variable definitions are in Appendix C.

	(1) HOR	(2) HOR	(3) HOR	(4) HOR	(5) HOR
PCinsider	0.106***	0.087**	-0.011	0.087**	0.078**
	(2.87)	(2.34)	(-1.44)	(2.34)	(2.15)
Size	0.003	-0.001	0.007***	-0.001	-0.001
	(1.52)	(-0.66)	(2.59)	(-0.66)	(-0.49)
BookMarket	0.005***	0.003*	0.002	0.003*	0.002
	(2.95)	(1.93)	(1.36)	(1.93)	(1.47)
Observations	170,648	170,648	188,638	170,648	170,049
Adjusted R-squared	0.527	0.530	0.171	0.530	0.572
Insider FE	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	Yes	No	Yes
Firm FE	No	No	Yes	Yes	Yes

Table 28. Politically connected insiders and routine trades

This table reports the regression results of politically connected insider transactions and routine indicator. The dependent variable *Routine* is an indicator variable which equals one if an insider makes trades in the same calendar month for three consecutive years, following Cohen, Malloy, and Pomorski (2012). *PCinsider* is an indicator variable which equals one if a corporate insider is a current or former member of government agencies. *Size* is the natural logarithm of firm market value in $millions at the end of year *y*. *BookMarket* is the Book-to-market ratio at the end of year *y*. Detailed variable definitions are in Appendix C.

	(1)	(2)	(3)	(4)	(5)
	Routine	Routine	Routine	Routine	Routine
PCinsider	0.207**	0.241***	0.022	0.084	0.022
	(2.29)	(2.60)	(0.23)	(0.82)	(1.26)
Size	0.033***	0.097***	0.005**	0.006	-0.007
	(13.95)	(17.61)	(2.18)	(1.11)	(-1.51)
BookMarket	0.014***	0.033***	0.005**	0.006*	0.005
	(5.27)	(6.00)	(2.26)	(1.84)	(1.55)
Observations	76,456	75,210	76,456	75,210	85,101
Adjusted R-squared	0.599	0.603	0.628	0.629	0.180
Insider FE	Yes	Yes	Yes	Yes	No
Year FE	No	No	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	Yes

Table 29. Difference-in-Difference regressions around the passage of the STOCK Act with time fixed effects

This table reports the DID regression results of around the passage of the STOCK Act with time fixed effects using CAR[0,30] and CAR[0,60] in Panel A, and CAR[0,120] and CAR[0,255] in Panel B. *Congress* is an indicator variable which equals one if a corporate insider is a US Congress member or staff when the transaction is made. *After* is an indicator variable which equals one if an insider transaction happens after April 2012. *Buy* is an indicator variable which equals one if the transaction is a purchase. *Sell* is an indicator variable which equals one if the transaction is a sale. *Congress* is an indicator variable which equals one if a corporate insider is a US Congress member or staff when the transaction is made. *PastMonthRet* is the raw return over the month *t-1* if the transaction happens in month *t*. *PastYearRet* is the raw return over the past year from month *t-2* to month *t-12* if the transaction happens in month *t*. *Size* is the natural logarithm of firm market value in $millions at the end of month *t*. *BookMarket* is the Book-to-market ratio at the end of month *t*, where book value is from the prior quarter-end. Detailed variable definitions are in Appendix C.

Panel A. CAR[0,30] and CAR[0,60]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	[0,30]	[0,30]	[0,30]	[0,30]	[0,60]	[0,60]	[0,60]	[0,60]
	MAR EW	MAR VW	MM EW	MM VW	MAR EW	MAR VW	MM EW	MM VW
*Congress*After*Buy*	-0.728***	-0.743***	-0.516**	-0.572**	-0.739***	-0.741***	-0.405*	-0.444*
	(-2.61)	(-2.62)	(-2.22)	(-2.31)	(-3.11)	(-3.11)	(-1.67)	(-1.77)
*Congress*After*Sell*	0.034	0.035	0.012	0.019	0.060***	0.059***	0.062***	0.060***
	(1.26)	(1.25)	(0.40)	(0.71)	(2.73)	(2.73)	(2.72)	(2.85)
*Congress*Buy*	0.428*	0.430*	0.399*	0.436*	0.483***	0.483**	0.491**	0.530**
	(1.90)	(1.88)	(1.87)	(1.92)	(2.58)	(2.52)	(2.03)	(2.12)
*Congress*Sell*	-0.023	-0.024	0.003	-0.007	-0.050**	-0.049**	-0.041**	-0.044**
	(-0.85)	(-0.86)	(0.12)	(-0.25)	(-2.22)	(-2.28)	(-1.96)	(-2.27)
Buy	0.020***	0.021***	0.030***	0.031***	0.021**	0.021**	0.039***	0.041***
	(3.22)	(3.23)	(5.07)	(5.34)	(1.99)	(2.02)	(4.30)	(4.60)
PastMonthRet	-0.032	-0.033	-0.029	-0.039	-0.038	-0.039	-0.048	-0.059*
	(-1.25)	(-1.28)	(-1.04)	(-1.37)	(-1.28)	(-1.30)	(-1.38)	(-1.67)
PastYearRet	-0.003	-0.003	-0.052***	-0.053***	-0.005	-0.005	-0.100***	-0.104***
	(-0.77)	(-0.77)	(-6.65)	(-6.79)	(-0.80)	(-0.80)	(-6.98)	(-7.09)
Size	0.002*	0.002*	0.001	0.001	0.003	0.003*	0.001	0.002
	(1.69)	(1.72)	(0.80)	(1.10)	(1.63)	(1.68)	(0.56)	(1.16)
BookMarket	0.003	0.003	0.005	0.005	0.005	0.005	0.006	0.009
	(0.90)	(0.88)	(1.17)	(1.14)	(0.93)	(0.94)	(0.90)	(1.22)
Observations	19,853	19,853	19,853	19,853	19,853	19,853	19,853	19,853
Adjusted R-squared	0.060	0.069	0.117	0.124	0.070	0.068	0.161	0.158
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	No	No	No	No	No

Panel B. CAR[0,120] and CAR[0,255]

	(1) [0,120] MAR EW	(2) [0,120] MAR VW	(3) [0,120] MM EW	(4) [0,120] MM VW	(5) [0,255] MAR EW	(6) [0,255] MAR VW	(7) [0,255] MM EW	(8) [0,255] MM VW
*Congress*After*Buy*	-1.046***	-1.061***	-0.431	-0.497	-1.382***	-1.393***	-0.131	-0.188
	(-2.85)	(-2.85)	(-1.55)	(-1.58)	(-2.91)	(-2.91)	(-0.49)	(-0.52)
*Congress*After*Sell*	0.146*	0.150*	0.179***	0.165***	0.334	0.339	0.469**	0.420**
	(1.84)	(1.90)	(4.64)	(3.77)	(1.63)	(1.64)	(2.49)	(2.39)
*Congress*Buy*	0.433**	0.438**	0.488*	0.543*	0.423**	0.430**	0.581**	0.633*
	(2.03)	(1.99)	(1.76)	(1.73)	(2.32)	(2.29)	(2.21)	(1.77)
*Congress*Sell*	-0.159*	-0.161**	-0.169***	-0.164***	-0.399*	-0.399*	-0.488***	-0.442**
	(-1.95)	(-1.99)	(-4.33)	(-3.71)	(-1.95)	(-1.94)	(-2.60)	(-2.53)
Buy	0.022	0.023*	0.061***	0.063***	0.017	0.016	0.100**	0.100**
	(1.63)	(1.66)	(3.43)	(3.64)	(0.65)	(0.63)	(2.23)	(2.23)
PastMonthRet	-0.056	-0.055	-0.076	-0.094*	0.011	0.013	-0.047	-0.070
	(-1.36)	(-1.34)	(-1.40)	(-1.74)	(0.18)	(0.23)	(-0.59)	(-0.81)
PastYearRet	-0.001	-0.001	-0.190***	-0.197***	0.001	0.002	-0.401***	-0.413***
	(-0.07)	(-0.06)	(-7.62)	(-7.76)	(0.07)	(0.10)	(-7.58)	(-7.63)
Size	0.008***	0.008***	0.004	0.006*	0.008	0.008	0.001	0.004
	(3.08)	(3.10)	(1.38)	(1.94)	(1.29)	(1.28)	(0.13)	(0.46)
BookMarket	0.017*	0.017*	0.020*	0.024**	0.039*	0.039*	0.042*	0.050**
	(1.96)	(1.96)	(1.78)	(2.10)	(1.86)	(1.82)	(1.79)	(2.25)
Observations	19,853	19,853	19,853	19,853	19,853	19,853	19,853	19,853
Adjusted R-squared	0.068	0.062	0.196	0.195	0.052	0.056	0.248	0.257
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	No	No	No	No	No

Table 30. Difference-in-Difference regressions around the passage of STOCK Act with firm and time fixed effects

This table reports the DID regression results of around the passage of the STOCK Act with firm and time fixed effects using CAR[0,30] and CAR[0,60] in Panel A, and CAR[0,120] and CAR[0,255] in Panel B. *Congress* is an indicator variable which equals one if a corporate insider is a US Congress member or staff when the transaction is made. *After* is an indicator variable which equals one if an insider transaction happens after April 2012. *Buy* is an indicator variable which equals one if the transaction is a purchase. *Sell* is an indicator variable which equals one if the transaction is a sale. *Congress* is an indicator variable which equals one if a corporate insider is a US Congress member or staff when the transaction is made. *PastMonthRet* is the raw return over the month *t-1* if the transaction happens in month *t*. *PastYearRet* is the raw return over the past year from month *t-2* to month *t-12* if the transaction happens in month *t*. *Size* is the natural logarithm of firm market value in $millions at the end of month *t*. *BookMarket* is the Book-to-market ratio at the end of month *t*, where book value is from the prior quarter-end. Detailed variable definitions are in Appendix C.

Panel A. CAR[0,30] and CAR[0,60]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	[0,30]	[0,30]	[0,30]	[0,30]	[0,60]	[0,60]	[0,60]	[0,60]
	MAR EW	MAR VW	MM EW	MM VW	MAR EW	MAR VW	MM EW	MM VW
*Congress*After*Buy*	0.022	0.014	0.050*	0.031	-0.090**	-0.108***	-0.049	-0.091**
	(0.82)	(0.53)	(1.85)	(1.08)	(-2.24)	(-2.73)	(-1.25)	(-2.24)
*Congress*After*Sell*	0.034*	0.034*	0.042***	0.039***	0.054	0.048	0.072	0.061
	(1.81)	(1.82)	(3.00)	(2.62)	(1.36)	(1.21)	(1.35)	(1.27)
*Congress*Buy*	0.264***	0.265***	0.241***	0.309***	0.752***	0.761***	1.263***	1.232***
	(5.92)	(6.02)	(4.83)	(6.62)	(6.71)	(6.83)	(10.67)	(11.08)
*Congress*Sell*	-0.021	-0.022	-0.036***	-0.035***	-0.025	-0.023	-0.054	-0.050
	(-1.37)	(-1.47)	(-3.48)	(-3.14)	(-0.72)	(-0.65)	(-1.10)	(-1.15)
Buy	0.019***	0.019**	0.029***	0.030***	0.031***	0.032***	0.050***	0.053***
	(2.64)	(2.57)	(3.87)	(3.97)	(3.28)	(3.33)	(4.43)	(4.83)
PastMonthRet	-0.068***	-0.068***	-0.050*	-0.059**	-0.086***	-0.087***	-0.069**	-0.080**
	(-2.77)	(-2.79)	(-1.81)	(-2.12)	(-2.83)	(-2.87)	(-1.98)	(-2.30)
PastYearRet	-0.012*	-0.012*	-0.056***	-0.058***	-0.017	-0.017	-0.105***	-0.109***
	(-1.74)	(-1.74)	(-5.80)	(-5.92)	(-1.43)	(-1.42)	(-5.56)	(-5.67)
Size	-0.017***	-0.018***	-0.022***	-0.022***	-0.054***	-0.054***	-0.065***	-0.064***
	(-3.99)	(-4.01)	(-4.86)	(-4.85)	(-6.14)	(-6.12)	(-5.18)	(-5.03)
BookMarket	0.011*	0.011*	0.013	0.013	0.011	0.011	0.011	0.015
	(1.82)	(1.78)	(1.62)	(1.61)	(1.36)	(1.38)	(1.00)	(1.25)
Observations	19,473	19,473	19,473	19,473	19,473	19,473	19,473	19,473
Adjusted R-squared	0.197	0.206	0.238	0.249	0.239	0.237	0.299	0.301
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B. CAR[0,120] and CAR[0,255]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	[0,120]	[0,120]	[0,120]	[0,120]	[0,255]	[0,255]	[0,255]	[0,255]
	MAR EW	MAR VW	MM EW	MM VW	MAR EW	MAR VW	MM EW	MM VW
*Congress*After*Buy*	0.058	0.053	0.048	-0.001	0.169***	0.165***	0.120	0.058
	(1.22)	(1.12)	(0.93)	(-0.02)	(2.71)	(2.65)	(1.43)	(0.70)
*Congress*After*Sell*	0.106**	0.101**	0.130	0.126*	0.006	0.009	0.071	0.082
	(2.30)	(2.16)	(1.59)	(1.74)	(0.07)	(0.11)	(0.49)	(0.58)
*Congress*Buy*	0.575***	0.614***	1.338***	1.361***	0.817***	0.823***	2.101***	2.152***
	(4.81)	(5.05)	(9.14)	(10.06)	(4.88)	(5.00)	(8.22)	(9.62)
*Congress*Sell*	-0.107**	-0.105**	-0.155**	-0.156**	-0.100	-0.101	-0.213	-0.214
	(-2.52)	(-2.43)	(-2.01)	(-2.32)	(-1.27)	(-1.30)	(-1.50)	(-1.59)
Buy	0.043***	0.044***	0.082***	0.088***	0.063***	0.063***	0.149***	0.156***
	(2.97)	(3.02)	(4.57)	(4.93)	(2.85)	(2.85)	(4.52)	(4.77)
PastMonthRet	-0.128***	-0.127***	-0.092*	-0.110**	-0.125**	-0.124**	-0.071	-0.094
	(-2.62)	(-2.60)	(-1.70)	(-2.07)	(-2.20)	(-2.18)	(-1.16)	(-1.48)
PastYearRet	-0.015	-0.015	-0.190***	-0.197***	-0.014	-0.014	-0.394***	-0.406***
	(-1.18)	(-1.17)	(-6.62)	(-6.72)	(-0.71)	(-0.71)	(-7.07)	(-7.10)
Size	-0.106***	-0.105***	-0.124***	-0.122***	-0.186***	-0.185***	-0.224***	-0.222***
	(-7.05)	(-7.07)	(-5.13)	(-4.98)	(-5.05)	(-5.03)	(-3.93)	(-3.75)
BookMarket	0.018	0.018	0.018	0.026	0.052	0.051	0.053	0.066*
	(1.42)	(1.44)	(1.18)	(1.52)	(1.46)	(1.45)	(1.49)	(1.86)
Observations	19,473	19,473	19,473	19,473	19,473	19,473	19,473	19,473
Adjusted R-squared	0.311	0.306	0.387	0.394	0.428	0.431	0.501	0.517
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 31. Matched sample analysis of base line regressions

This table reports the regression results of politically connected insider transactions and subsequent returns in the matched sample. A transaction made by a PC insider is matched to five same-type (purchase or sale) transactions made by non-PC insiders from the same firm in the same year with the nearest trade size. Panel A and Panel B report the results with year-month fixed effects. Panel C and Panel D report the results with year-month fixed effects and firm fixed effects. *PCinsider* is an indicator variable which equals one if a corporate insider is a current or former member of government agencies. *Buy* is an indicator variable which equals one if the transaction is a purchase. *Sell* is an indicator variable which equals one if the transaction is a sale. *PastMonthRet* is the raw return over the month *t-1* if the transaction happens in month *t*. *PastYearRet* is the raw return over the past year from month *t-2* to month *t-12* if the transaction happens in month *t*. *Size* is the natural logarithm of firm market value in $millions at the end of month *t*. *BookMarket* is the Book-to-market ratio at the end of month *t*, where book value is from the prior quarter-end. Detailed variable definitions are in Appendix C.

Panel A. CAR[0,30] and CAR[0,60] with year-month fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	[0,30]	[0,30]	[0,30]	[0,30]	[0,60]	[0,60]	[0,60]	[0,60]
	MAR EW	MAR VW	MM EW	MM VW	MAR EW	MAR VW	MM EW	MM VW
*PCinsider*Buy*	-0.007*	-0.007*	-0.005	-0.007*	-0.002	-0.002	-0.001	-0.002
	(-1.91)	(-1.89)	(-1.41)	(-1.71)	(-0.38)	(-0.32)	(-0.26)	(-0.32)
*PCinsider*Sell*	0.000	0.000	0.002	0.003	-0.000	-0.001	0.004	0.004
	(0.08)	(0.09)	(0.65)	(0.96)	(-0.11)	(-0.17)	(1.11)	(1.29)
Buy	0.013***	0.012**	0.049***	0.049***	0.009	0.008	0.081***	0.080***
	(2.59)	(2.41)	(7.42)	(7.29)	(1.06)	(0.94)	(7.18)	(6.94)
PastMonthRet	-0.078***	-0.079***	-0.095***	-0.098***	-0.039	-0.040	-0.071	-0.076*
	(-2.86)	(-2.86)	(-3.69)	(-3.48)	(-0.94)	(-0.96)	(-1.63)	(-1.65)
PastYearRet	0.003*	0.003*	-0.014*	-0.015*	0.004*	0.004	-0.029*	-0.031*
	(1.88)	(1.84)	(-1.71)	(-1.74)	(1.65)	(1.62)	(-1.78)	(-1.79)
Size	-0.001	-0.001	0.001	0.001	-0.003*	-0.003*	0.001	0.001
	(-1.40)	(-1.47)	(0.68)	(0.72)	(-1.77)	(-1.79)	(0.65)	(0.66)
BookMarket	0.005	0.005	0.012***	0.013***	0.005	0.005	0.016***	0.019***
	(1.40)	(1.49)	(3.04)	(3.22)	(1.14)	(1.21)	(3.06)	(3.36)
Observations	141,760	141,760	141,760	141,760	141,760	141,760	141,760	141,760
Adjusted R-squared	0.077	0.081	0.133	0.131	0.083	0.094	0.151	0.155
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	No	No	No	No	No

Panel B. CAR[0,120] and CAR[0,255] with year-month fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	[0,120]	[0,120]	[0,120]	[0,120]	[0,255]	[0,255]	[0,255]	[0,255]
	MAR EW	MAR VW	MM EW	MM VW	MAR EW	MAR VW	MM EW	MM VW
*PCinsider*Buy*	0.008	0.009	0.005	0.006	0.008	0.008	-0.003	-0.000
	(1.39)	(1.45)	(0.73)	(0.81)	(1.11)	(1.10)	(-0.26)	(-0.04)
*PCinsider*Sell*	-0.004	-0.004	0.004	0.005	-0.008	-0.008	0.008	0.011
	(-0.92)	(-0.91)	(0.97)	(1.18)	(-0.97)	(-1.02)	(0.99)	(1.28)
Buy	-0.004	-0.005	0.136***	0.137***	0.002	0.002	0.291***	0.295***
	(-0.28)	(-0.33)	(6.51)	(6.44)	(0.10)	(0.09)	(6.99)	(6.95)
PastMonthRet	-0.057	-0.057	-0.120**	-0.129**	-0.073	-0.073	-0.200***	-0.208***
	(-1.16)	(-1.17)	(-2.14)	(-2.26)	(-1.24)	(-1.24)	(-2.61)	(-2.66)
PastYearRet	0.011**	0.011**	-0.056*	-0.060*	0.006	0.006	-0.134*	-0.141*
	(2.00)	(1.97)	(-1.77)	(-1.77)	(0.89)	(0.88)	(-1.83)	(-1.83)
Size	-0.003	-0.003	0.003	0.004	-0.007	-0.007	0.004	0.008
	(-1.20)	(-1.22)	(0.83)	(1.24)	(-1.33)	(-1.30)	(0.64)	(1.17)
BookMarket	0.013	0.013*	0.034***	0.040***	0.031	0.031	0.071***	0.081***
	(1.61)	(1.66)	(3.40)	(3.69)	(1.29)	(1.27)	(3.54)	(4.01)
Observations	141,760	141,760	141,760	141,760	141,760	141,760	141,760	141,760
Adjusted R-squared	0.082	0.081	0.167	0.173	0.095	0.080	0.215	0.235
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	No	No	No	No	No

Panel C. CAR[0,30] and CAR[0,60] with year-month and firm fixed effects

	(1) [0,30] MAR EW	(2) [0,30] MAR VW	(3) [0,30] MM EW	(4) [0,30] MM VW	(5) [0,60] MAR EW	(6) [0,60] MAR VW	(7) [0,60] MM EW	(8) [0,60] MM VW
*PCinsider*Buy*	-0.011***	-0.011***	-0.010***	-0.011***	-0.007	-0.007	-0.007	-0.008
	(-3.15)	(-3.12)	(-2.78)	(-3.02)	(-1.64)	(-1.59)	(-1.43)	(-1.56)
*PCinsider*Sell*	0.002	0.002	0.003	0.003	0.002	0.002	0.005	0.005*
	(0.50)	(0.54)	(0.80)	(1.08)	(0.66)	(0.57)	(1.47)	(1.65)
Buy	0.033***	0.033***	0.071***	0.071***	0.030**	0.029**	0.102***	0.102***
	(4.61)	(4.55)	(6.90)	(7.04)	(2.54)	(2.49)	(5.97)	(6.22)
PastMonthRet	-0.069***	-0.068***	-0.061***	-0.058***	-0.035	-0.036	-0.020	-0.018
	(-3.36)	(-3.43)	(-2.88)	(-2.70)	(-1.24)	(-1.27)	(-0.64)	(-0.57)
PastYearRet	0.000	0.000	-0.013	-0.013	-0.001	-0.001	-0.025*	-0.026*
	(0.11)	(0.08)	(-1.60)	(-1.61)	(-0.32)	(-0.35)	(-1.66)	(-1.67)
Size	-0.024***	-0.023***	-0.035***	-0.036***	-0.068***	-0.068***	-0.085***	-0.088***
	(-5.89)	(-5.72)	(-6.57)	(-6.22)	(-8.16)	(-8.09)	(-7.15)	(-6.96)
BookMarket	0.005	0.005	0.008	0.009	0.003	0.003	0.008	0.010
	(1.07)	(1.06)	(1.52)	(1.63)	(0.41)	(0.44)	(0.94)	(1.14)
Observations	141,746	141,746	141,746	141,746	141,746	141,746	141,746	141,746
Adjusted R-squared	0.239	0.243	0.285	0.285	0.307	0.314	0.349	0.359
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel D. CAR[0,120] and CAR[0,255] with year-month and firm fixed effects

	(1) [0,120] MAR EW	(2) [0,120] MAR VW	(3) [0,120] MM EW	(4) [0,120] MM VW	(5) [0,255] MAR EW	(6) [0,255] MAR VW	(7) [0,255] MM EW	(8) [0,255] MM VW
*PCinsider*Buy*	0.001	0.001	-0.003	-0.003	-0.005	-0.005	-0.018*	-0.016*
	(0.10)	(0.16)	(-0.49)	(-0.49)	(-0.80)	(-0.83)	(-1.78)	(-1.65)
*PCinsider*Sell*	-0.002	-0.002	0.004	0.004	-0.007	-0.007	0.005	0.006
	(-0.48)	(-0.47)	(1.02)	(1.21)	(-1.02)	(-1.08)	(0.67)	(0.89)
Buy	0.014	0.013	0.157***	0.157***	0.031	0.030	0.317***	0.320***
	(0.63)	(0.61)	(4.92)	(5.20)	(0.86)	(0.85)	(5.90)	(6.06)
PastMonthRet	-0.063	-0.061	-0.023	-0.022	-0.040	-0.041	0.037	0.049
	(-1.61)	(-1.59)	(-0.53)	(-0.50)	(-0.85)	(-0.88)	(0.65)	(0.78)
PastYearRet	0.004	0.003	-0.045*	-0.047*	0.001	0.001	-0.100*	-0.104*
	(1.02)	(0.97)	(-1.68)	(-1.68)	(0.16)	(0.15)	(-1.77)	(-1.77)
Size	-0.134***	-0.134***	-0.170***	-0.173***	-0.253***	-0.253***	-0.327***	-0.332***
	(-8.24)	(-8.23)	(-7.04)	(-6.94)	(-7.33)	(-7.33)	(-6.32)	(-6.21)
BookMarket	0.005	0.005	0.015	0.020	0.011	0.011	0.030	0.040
	(0.45)	(0.45)	(0.93)	(1.18)	(0.33)	(0.32)	(0.96)	(1.22)
Observations	141,746	141,746	141,746	141,746	141,746	141,746	141,746	141,746
Adjusted R-squared	0.376	0.376	0.420	0.434	0.529	0.522	0.538	0.559
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Appendix A: Variable Definitions for Chapter 1

Variables	Descriptions
Size	Natural log of total assets.
Age	Number of years since a firm is established.
Political Connection	Equals one if at least one director on the board was or is an official of the central government, a local government, or the military following Fan, Wong, and Zhang (2007) and Liu, Shu, and Wei (2017), and zero otherwise
Earnings Management	Discretionary accruals following Kothari, Leone, and Wasley (2005) and Giannetti, Liao, and Yu (2015).
Excess Perk	Corporate excess perk measure following Chen et al. (2021).
ROA	Net income scaled by total assets.
ROE	Net income scaled by total equity.
NI/EMP	Net income scaled by number of employees (labor productivity)
TFP	Total factor productivity following Olley and Pakes (1996) method.
GovtSub	Government subsidies scaled by total assets.
GovtSubDummy	Equals one if a firm receives government subsidies, and zero otherwise.
Leverage	Long term debt scaled by total assets.
R&D	R&D spending scaled by total assets.
CapEx	Capital expenditures scaled by total assets.
Employees	Natural logarithm of number of employees.
SOE	Equals to one if a firm is state-owned enterprise and zero otherwise.

Appendix B: Variable Definitions for Chapter 2

The variables used in this study are defined in the Appendix below. We winsorize numerical variables at the 1st and the 99th percentiles.

Variables	**Descriptions**	**Data Sources**
Abnormal Return	Industry and year adjusted abnormal return calculated by subtracting the S&P 500 annual return from annual stock raw return.	CRSP
Absolute Goal	The probability of having an absolute performance goal.	ISS
Board Size	Number of directors on the board at year-end.	ISS
Business Segments	Number of business segments.	Compustat
Cash Pay	Natural logarithm of sum of CEO salary and bonus.	Execucomp
CEO Age	Natural logarithm of CEO's age in the sample year.	Execucomp
CEO Duality	Dummy variable taking the value of one if the CEO is the chairperson and zero otherwise.	ISS
CEO Forced Turnover	Dummy variable taking the value of one if CEO is forced.	Florian Peters' website
CEO Pay Slice	Proportion of CEO total compensation in total compensation of top five highest paid executives.	Execucomp
CEO Pay-Performance Sensitivity (Delta)	Natural logarithm of expected dollar change in the value of the CEO's current year annual equity-based compensation (in $ thousands) for a 1% change in the stock price. We compute delta using the sum of all current option grants, number of shares of current restricted stock grants, and number of targeted shares granted. The variable definition is based on Hayes et al. (2012).	Execucomp, CRSP
CEO Risk-Taking Incentives (Vega)	Natural logarithm of expected dollar change in the value of the CEO's current year annual option grant (in $ thousands) for a 1% change in stock price volatility. We compute vega using current year option granted. The variable definition is based on Hayes et al. (2012).	Execucomp, CRSP
CEO Tenure	Natural logarithm of number of years since the director became CEO.	Execucomp
E index	The entrenchment index based on Bebchuk, Cohen, and Ferrell (2009) (available before 2006).	Lucian Bebchuk's website
Firm Age	Natural logarithm of number of years since the firm's IPO.	CRSP
Forward 5-yr Citations	Industry and year adjusted number of citations from year t+1 to year t+5.	KPSS2017, PatentView
Forward 5-yr Innovation Value (Nominal)	Industry and year adjusted sum of nominal innovation value from year t+1 to year t+5.	KPSS2017
Forward 5-yr Innovation Value (Real)	Industry and year adjusted sum of real innovation value from year t+1 to year t+5.	KPSS2017

G Index	The governance index based on Gompers, Ishii, Metrick (2003) (available before 2006).	ISS
High VC Reputation	Dummy variable taking the value of one if a VC firm has 70 or higher Lee-Pollock-Jin (2011) VC reputation index.	Timothy G. Pollock's website
Independent Board	Percentage of outside directors on the board identified as independent of the CEO and firm.	ISS
Innovation Value (Nominal)	Industry and year adjusted nominal innovation value.	KPSS2017
Innovation Value (Real)	Industry and year adjusted real innovation value.	KPSS2017
Inside Debt	Natural logarithm of sum of CEO pension value and deferred benefit.	Execucomp
Institutional Ownership Concentration	Sum of squared individual institutional holdings divided by total institutional holdings.	Thomson Reuters 13F Holdings
Institutional Ownership Total	Aggregate percent of outstanding shares of a company held by all financial institutions.	Thomson Reuters 13F Holdings
Leverage	Ratio of the book value of debt (dlc + dltt) to the sum of the book value of debt (dlc + dltt) and market capitalization (prcc × csho).	Compustat
Market-to-Book Ratio	Ratio of market value of assets (total assets plus market value of equity minus book value of equity) to total assets ((at + csho × prcc_f – ceq)/at).	Compustat
Number of VC Directors on Compensation Committee	Number of VC directors who are compensation committee member or chair.	ISS, SDC VentureXpert
Option Pay	Natural logarithm of Black-Scholes value of CEO option award before 2006 or fair value of CEO option award after 2006.	Execucomp
Patent Count	Industry and year adjusted number of patents filed.	KPSS2017
R&D Intensity	Ratio of research and development expenditures to total assets (xrd/at).	Compustat
Relative Goal	The probability of having a relative performance goal.	ISS
ROA	Earnings before interest, taxes, depreciation and amortization over total assets (ebitda/total assets).	Compustat
Sales Growth Goal	The magnitude of the absolute sales growth goal.	ISS
***Share** Shifter*		
Size	Natural logarithm of total assets (at).	Compustat
State Education Attainment	Natural logarithm of percentage of the population age twenty-five and over with a bachelor's degree or more.	United States Census Bureau

State Income per Capita	Natural logarithm of per-capita income in the state of the firm.	United States Bureau of Economic Analysis
State R&D per Capita	Natural logarithm of total R&D expense per capita in the state of the firm.	National Science Foundation
Stock Pay	Natural logarithm of CEO restricted stock grant before 2006 or fair value of stock awards after 2006.	Execucomp
Stock Price Goal	The probability of having an absolute stock price goal.	ISS
Stock Return	Annual stock return for the fiscal year.	CRSP
Stock Volatility	Annualized standard deviation of daily returns computed over the year.	Compustat
Tangibility	Ratio of net property, plant, and equipment (ppent) to total assets (at).	Compustat
Termination Pay	Natural logarithm of CEO estimated payments in event of involuntary termination.	Execucomp
Tobin's Q	Market value of assets divided by book value of assets.	Compustat
Total Compensation	Natural logarithm of total CEO compensation in $ thousands (tdc1).	Execucomp
Total Excess Compensation	Natural logarithm of difference between CEO total compensation and predicted total compensation.	Execucomp
VC Director	Dummy variable taking the value of one if a director has prior VC experience, and zero otherwise.	ISS, SDC VentureXpert
VC Director on Audit Committee	Number of VC directors who are audit committee member or chair.	ISS, SDC VentureXpert
VC Director on Compensation Committee	Dummy variable taking the value of one if a director is a VC director and is a member or chair of compensation committee, and zero otherwise.	ISS, SDC VentureXpert
VC Director on Governance Committee	Number of VC directors who are governance committee member or chair.	ISS, SDC VentureXpert
VC Director on Nomination Committee	Number of VC directors who are nomination committee member or chair.	ISS, SDC VentureXpert
VC-Backed Firm	Dummy variable taking the value of one if the firm is VC-backed and zero otherwise.	SDC Initial Public Offerings
% Independent Board Members	Percentage of independent members on board.	ISS
% VC Director on Compensation Committee	Number of VC directors who are compensation committee member or chair as a percentage of total number of board members.	ISS, SDC VentureXpert

Appendix C: Variable Definitions for Chapter 3

Variable Name	Definition
Age	The age of an insider in year *y*.
After	An indicator variable which equals one if an insider transaction happens after April 2012.
BookMarket	Book-to-market ratio at the end of month *t*, where book value is from the prior quarter-end.
Buy	An indicator variable which equals one if the transaction is a purchase (*acqdisp*=A).
CAR[0,h] MAR EW	Cumulative abnormal returns over the trading window [0,*h*] calculated by market-adjusted return against CRSP equally-weighted index.
CAR[0,h] MAR VW	Cumulative abnormal returns over the trading window [0,*h*] calculated by market-adjusted return against CRSP value-weighted index.
CAR[0,h] MM EW	Cumulative abnormal returns over the trading window [0,*h*] calculated by the market model against CRSP equally-weighted index. The market model requires the estimation period ends 31 days before the event date and has 255 days in length, with a minimum of four days of trading.
CAR[0,h] MM VW	Cumulative abnormal returns over the trading window [0,*h*] calculated by the market model against CRSP value-weighted index. The market model requires the estimation period ends 31 days before the event date and has 255 days in length, with a minimum of four days of trading.
CEO	An indicator variable which equals one if any of the four role codes of an insider is CEO.
Congress	An indicator variable which equals one if a corporate insider is a US Congress member or staff when the transaction is made.
CPA	An indicator variable which equals one if an insider has a CPA.
CFA	An indicator variable which equals one if an insider has a CFA.
Director	An indicator variable which equals one if any of the four role codes of an insider is CB, D, DO, H, OD, or VC
HOR	The insider investment horizon measure following Akbas, Jiang, and Koch (2020). I create the insider investment horizon measure (*HOR*) as $HOR_{i,j,y} = \left\lvert \frac{\sum_{y-10}^{y-1} IOF_{i,j,y}}{N} \right\rvert$, where $IOF_{i,j,y}$ is the annual net insider order flow of insider i at firm j in year y, which is calculated by $\frac{P_{i,j,y} - S_{i,j,y}}{P_{i,j,y} + S_{i,j,y}}$, where P is the number of shares purchases during year y, S is the number of shares sold, and N is the number of calendar years the insider traded over the 10 years prior to year y. An insider is required to have traded in at least two of the past 10 years to be included in the sample. HOR is then calculated by taking the absolute value of the average of the annual net order flow over the past ten years.
JD	An indicator variable which equals one if an insider has a J.D.
MBA	An indicator variable which equals one if an insider has an MBA.
NetworkSize	The size of an insider's network.
Officer	An indicator variable which equals one if any of the four role codes of an insider is AV, CEO, CFO, CI, CO, CT, EVP, O, OB, OP, OS, OT, OX, P, S, or SVP.
PastMonthRet	Raw return over the month t-1 if the transaction happens in month *t*.
PastYearRet	Raw return over the past year from month *t*-2 to month *t*-12 if the transaction happens in month *t*.

PCinsider	An indicator variable which equals one if a corporate insider is a current or former member of US Congress, US Department of State, US Executive Office of the President (White House), US Securities and Exchange Commission (SEC), Central Intelligence Agency (CIA), Federal Emergency Management Agency (FEMA), Office of Management and Budget, Internal Revenue Service (IRS), Nuclear Regulatory Commission, United States Social Security Administration, Civil Rights Commission, Food and Drug Administration (FDA), United Nations (UN), presidential campaign, or president's committee or council, or if he or she is a current or former state governor or mayor, when the transaction is made.
PhD	An indicator variable which equals one if an insider has a PhD.
Routine	An indicator variable equals one if an insider *i* is a routine insider at firm *j* in year *y* following Cohen, Malloy, and Pomorski (2012). A routine trader is defined as an insider who placed a trade in the same calendar month for at least three consecutive years, and everyone else is defined as an opportunistic trader. An insider is required to make at least one trade in each of the three preceding years to be defined as either an opportunistic or a routine trader. Once an insider is categorized as a routine insider at firm *j* in year *y*, he or she is a routine insider for all subsequent years.
Sell	An indicator variable which equals one if the transaction is a sale (*acqdisp*=D).
Size	Natural logarithm of firm market value in $millions at the end of month *t*.
TradeSize	Natural logarithm of insider transaction size in $millions.

Appendix D: Appendix Tables for Chapter 1

Table A1. Keyword search for news index calculations (EPU methodology)

This table presents the data to calculate news index using Economic Policy Uncertainty methodology documented in Baker et al. (2016). Number of news articles is collected from ProQuest Historical Newspapers. Five leading US newspapers are Chicago Tribune, Los Angeles Times, New York Times, Wall Street Journal, and Washington Post. Column 2-6 shows total number of articles published by month and by newspapers. Column 7-11 shows number of articles which contain the keyword "Made in China 2025" by month and by newspapers. Following Baker et al. (2016), we scale the raw counts by the total number of articles in the same newspaper and month. Next we standardize each monthly newspaper-level series to unit standard deviation from January 2014 to July 2019 and then average across the 5 papers by month. Finally, we normalize the 5-paper series to a mean of 100 from January 2014 to July 2019.

	Total number of articles published					**"Made in China 2025" raw counts**				
	Chicago Tribune	**Los Angeles Times**	**New York Times**	**Wall Street Journal**	**Washington Post**	**Chicago Tribune**	**Los Angeles Times**	**New York Times**	**Wall Street Journal**	**Washington Post**
Jan-14	7774	4709	2626	1306	3862	0	0	0	0	0
Feb-14	6796	4811	2877	1284	3633	0	0	0	0	0
Mar-14	7076	5319	3328	1402	4140	0	0	0	0	0
Apr-14	6794	4696	2915	1390	4704	0	0	0	0	0
May-14	7659	5318	3029	1402	5213	0	0	0	0	0
Jun-14	7018	4966	3034	1319	5156	0	0	0	0	0
Jul-14	7064	4541	2520	1262	4663	0	0	0	0	0
Aug-14	6906	5086	3001	1250	5132	0	0	0	0	0
Sep-14	6972	4867	3291	1384	4594	0	0	0	0	0
Oct-14	7592	4962	3258	1508	5122	0	0	0	0	0
Nov-14	7167	5758	3556	1338	5698	0	0	0	0	0
Dec-14	6938	5318	3231	1381	4262	0	0	0	0	0
Jan-15	6730	4897	2570	1280	4605	0	0	0	0	0
Feb-15	6466	6007	2914	1216	3912	0	0	0	0	0
Mar-15	6976	6884	3348	1320	4880	0	0	0	0	0
Apr-15	7150	6230	2802	1354	4588	0	0	0	1	2
May-15	7026	6982	3147	1326	4826	0	0	0	2	0
Jun-15	6652	4602	2680	1358	4249	0	0	0	1	0
Jul-15	7004	3764	2566	1224	4155	0	0	0	1	0
Aug-15	5732	4151	2925	1394	4266	0	0	0	1	0
Sep-15	5766	5273	3376	1550	4306	0	0	0	1	0
Oct-15	6212	5558	3166	1600	4780	0	0	0	0	0
Nov-15	5479	5800	3457	1482	5129	0	0	0	0	0
Dec-15	5486	5784	3274	1626	4172	0	0	0	1	0
Jan-16	4946	5597	2778	1316	4435	0	0	0	1	0
Feb-16	4490	5152	2751	1328	4476	0	1	0	0	0
Mar-16	5008	5576	3136	1476	4270	0	0	0	0	0
Apr-16	4712	5964	2705	1566	4416	0	0	0	0	0
May-16	5095	5884	3051	1330	4864	0	0	0	0	0
Jun-16	4800	5356	2678	1408	4397	0	1	0	0	1
Jul-16	4689	5671	2725	1168	4363	0	0	0	0	0

Aug-16	4486	5389	2704	1368	4205	0	0	0	0	0
Sep-16	4998	5431	3159	1436	4686	0	0	0	0	1
Oct-16	4805	6006	3097	1456	4892	0	0	0	0	1
Nov-16	4904	5574	3092	1337	5638	0	0	0	0	1
Dec-16	4608	5637	2977	1228	4479	0	0	0	1	0
Jan-17	4034	5109	2625	1086	4520	0	1	1	0	0
Feb-17	3993	4794	2582	1118	3644	0	0	1	0	0
Mar-17	4637	5151	2952	1288	4536	0	1	1	2	0
Apr-17	4099	5317	2840	1314	4626	0	0	3	1	1
May-17	4227	5348	2835	1095	4284	0	2	1	1	1
Jun-17	4385	5019	2492	1222	4452	0	0	3	0	0
Jul-17	3829	5158	2440	935	4038	0	0	1	0	0
Aug-17	3841	4905	2550	1281	3987	0	0	3	4	3
Sep-17	4066	5714	2987	1229	4338	0	0	1	1	1
Oct-17	4023	5630	2979	1247	3980	0	0	0	3	0
Nov-17	4054	5335	2889	1224	3690	0	1	4	3	0
Dec-17	4012	5298	2966	1246	2776	0	0	1	1	0
Jan-18	3327	4920	2355	1090	2911	0	0	0	1	2
Feb-18	3405	4644	2620	1159	2916	0	3	2	0	0
Mar-18	4117	5181	2872	1268	3384	0	0	7	2	2
Apr-18	3501	4828	2770	1165	3188	0	4	10	3	3
May-18	3835	4934	2704	1190	3390	1	9	9	7	5
Jun-18	3657	5368	2553	1263	3067	0	4	11	7	4
Jul-18	3467	4759	2456	1047	2922	3	9	7	4	2
Aug-18	3676	4672	2530	1231	3276	0	0	3	1	2
Sep-18	3679	5129	3148	1234	3241	1	6	3	2	1
Oct-18	4007	5136	2784	1279	3478	0	0	3	3	1
Nov-18	3682	4889	2893	1234	3685	1	5	2	1	2
Dec-18	3369	5005	2977	1206	2833	2	10	5	8	7
Jan-19	3329	4414	2355	1099	3150	0	13	2	1	3
Feb-19	3089	4537	2529	1149	3008	0	0	1	1	0
Mar-19	3435	4885	3032	1224	3307	0	9	2	5	0
Apr-19	3104	4584	2549	1208	3252	0	2	1	1	1
May-19	3655	4584	2691	1120	3210	0	3	4	0	2
Jun-19	3282	5033	2706	1223	3296	0	2	0	2	2
Jul-19	2891	4499	2302	1059	2805	0	3	0	1	0

Table A2. Distribution of Chinese treated firms by headquarter city (key city in bold)

This table reports the distribution of Chinese treated firms by headquarter city. Bold city names indicate MC 2025 key cities.

Headquarter City	Number of firms	%	Headquarter City	Number of firms	%	Headquarter City	Number of firms	%
Beijing	27	15.98	Jilin	2	1.18	Jingshan	1	0.59
Shanghai	19	11.24	Jinan	2	1.18	Jiyuan	1	0.59
Shenzhen	15	8.88	Nantong	2	1.18	Lanzhou	1	0.59
Guangzhou	8	4.73	**Ningbo**	2	1.18	Leshan	1	0.59
Nanjing	6	3.55	Wulumuqi	2	1.18	Liaocheng	1	0.59
Shenyang	6	3.55	**Zhuzhou**	2	1.18	Linzhou	1	0.59
Tianjin	5	2.96	Baoding	1	0.59	Pingdingshan	1	0.59
Chengdu	4	2.37	Baoji	1	0.59	Shantou	1	0.59
Hangzhou	4	2.37	Baotou	1	0.59	Taiyuan	1	0.59
Xiamen	4	2.37	**Changchun**	1	0.59	Taizhou	1	0.59
Zhuhai	4	2.37	Changge	1	0.59	Tiantai	1	0.59
Changsha	3	1.78	**Foshan**	1	0.59	Weihai	1	0.59
Qingdao	3	1.78	Fuzhou	1	0.59	Wenzhou	1	0.59
Wuhan	3	1.78	Guangdong	1	0.59	Xianyang	1	0.59
Xian	3	1.78	Guiyang	1	0.59	Xuchang	1	0.59
Zhengzhou	3	1.78	Haikou	1	0.59	Yantai	1	0.59
Chongqing	2	1.18	Huangshi	1	0.59	Yixing	1	0.59
Dalian	2	1.18	Jiangyin	1	0.59	Zhangjiagang	1	0.59
Haerbin	2	1.18	Jingjiang	1	0.59	**Zhenjiang**	1	0.59
Hefei	2	1.18						
Total	**169**							
Total in key cities	**49**							

Table A3. Difference-in-Difference analysis of the financial performance for Chinese firms with industry and city fixed effects

This table presents the DID analysis of the financial performance of Chinese firms in the propensity score matched sample over 2012-2017. Control firms are matched by size and book-market ratio using observations from 2007 to 2014. Dependent variable *ROA* is net income scaled by total assets. *ROE* is net income scaled by total equity. *NI/EMP* is net income scaled by number of employees (labor productivity). *TFP* is the total factor productivity following Olley and Pakes (1996) method. *Leverage* is long term debt scaled by total assets. *Treated* equals one if a firm is in industries targeted by MC 2025 and zero otherwise. *After* equals one if year is greater than or equal to 2015 and zero otherwise. *Size* is natural log of total assets. *Age* is number of years since a firm is established. *SOE* equals to one if a firm is state-owned enterprise and zero otherwise. *Political Connection* equals one if at least one director on the board was or is an official of the central government, a local government, or the military following Fan, Wong, and Zhang (2007) and Liu, Shu, and Wei (2017), and zero otherwise. *Earnings Management* is the discretionary accruals following Kothari, Leone, and Wasley (2005) and Giannetti, Liao, and Yu (2015). All continuous variables are winsorized at 1%. The table reports t-value in parentheses. All t-values are computed with standard errors clustered at the firm level. Significance levels at 1%, 5% and 10% are denoted by *, ** and ***, respectively.

VARIABLES	(1) ROA	(2) ROA	(3) ROE	(4) ROE	(5) NI/EMP	(6) NI/EMP	(7) TFP	(8) TFP	(9) Leverage	(10) Leverage
Treated*After	-0.017***	-0.016***	-0.030**	-0.028**	-0.330***	-0.276***	-0.051	-0.050	0.013**	0.011**
	(-3.704)	(-3.452)	(-2.492)	(-2.400)	(-3.063)	(-2.593)	(-1.220)	(-1.236)	(2.483)	(2.042)
Treated	0.010	0.008	0.012	0.011	0.447***	0.364**	-0.055	-0.094	-0.008	-0.010*
	(1.578)	(1.269)	(0.946)	(0.807)	(2.642)	(2.250)	(-0.780)	(-1.389)	(-1.505)	(-1.719)
After	-0.001	-0.001	0.001	0.001	0.350***	0.364***	0.013	0.064	-0.006	-0.007
	(-0.106)	(-0.108)	(0.044)	(0.094)	(2.632)	(2.883)	(0.283)	(1.320)	(-1.005)	(-1.268)
Size	0.006***	0.003	0.020***	0.013***	0.182***	0.107**	0.573***	0.559***	0.012***	0.015***
	(3.255)	(1.476)	(5.267)	(3.071)	(3.448)	(2.123)	(26.445)	(23.991)	(5.211)	(5.851)
Age	-0.001	-0.000	-0.001	-0.000	-0.002	-0.004	-0.002	-0.010	0.001	0.000
	(-1.578)	(-0.655)	(-1.024)	(-0.276)	(-0.179)	(-0.306)	(-0.410)	(-1.509)	(1.643)	(0.716)
SOE	-0.003	-0.003	-0.013	-0.012	-0.244*	-0.228	0.163***	0.230***	-0.003	-0.002
	(-0.643)	(-0.596)	(-1.317)	(-1.093)	(-1.772)	(-1.493)	(2.762)	(3.756)	(-0.672)	(-0.322)
Political Connection	0.001	-0.004	0.002	-0.008	-0.012	-0.066	0.004	0.017	-0.004	-0.003
	(0.226)	(-1.223)	(0.283)	(-1.038)	(-0.133)	(-0.729)	(0.099)	(0.446)	(-0.921)	(-0.634)
Earnings Management	0.009	0.030	0.031	0.069	0.259	0.558	-0.319	-0.019	0.010	0.020
	(0.384)	(1.401)	(0.566)	(1.278)	(0.478)	(1.139)	(-1.557)	(-0.103)	(0.490)	(1.015)
Observations	1,997	1,997	1,997	1,997	1,779	1,779	1,997	1,997	1,751	1,751
Adjusted R-squared	0.142	0.295	0.094	0.170	0.187	0.399	0.778	0.830	0.571	0.627
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Table A4. Difference-in-Difference analysis of the government subsidies for Chinese firms with industry and city fixed effects

This table presents the DID analysis of the government subsidies for Chinese firms in the propensity score matched sample over 2012-2017. Control firms are matched by size and book-market ratio using observations from 2007 to 2014. The dependent variable *GovtSub* is government subsidies scaled by total assets. *GovtSubDummy* equals one if a firm receives government subsidies, and zero otherwise. *Treated* equals one if a firm is in industries targeted by MC 2025 and zero otherwise. *After* equals one if year is greater than or equal to 2015 and zero otherwise. *Size* is natural log of total assets. *Age* is number of years since a firm is established. *SOE* equals to one if a firm is state-owned enterprise and zero otherwise. *Political Connection* equals one if at least one director on the board was or is an official of the central government, a local government, or the military following Fan, Wong, and Zhang (2007) and Liu, Shu, and Wei (2017), and zero otherwise. *Earnings Management* is the discretionary accruals following Kothari, Leone, and Wasley (2005) and Giannetti, Liao, and Yu (2015). All continuous variables are winsorized at 1%. The table reports t-value in parentheses. All t-values are computed with standard errors clustered at the firm level. Significance levels at 1%, 5% and 10% are denoted by *, ** and ***, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	GovtSub	GovtSub	GovtSubDummy	GovtSubDummy
Treated*After	-0.000	-0.000	0.004	0.002
	(-0.193)	(-0.321)	(0.309)	(0.187)
Treated	0.002*	0.001	0.004	0.014
	(1.676)	(0.999)	(0.450)	(1.447)
After	-0.001	-0.000	-0.050***	-0.052***
	(-1.375)	(-0.531)	(-2.649)	(-2.748)
Size	-0.001*	-0.001***	0.003	0.002
	(-1.960)	(-2.673)	(0.938)	(0.745)
Age	-0.000	-0.000	-0.001	-0.001
	(-1.435)	(-1.419)	(-1.420)	(-1.236)
SOE	0.001	0.002	-0.010	-0.001
	(0.700)	(1.308)	(-1.313)	(-0.189)
Political Connection	-0.000	-0.000	-0.006	-0.009
	(-0.529)	(-0.327)	(-0.826)	(-1.034)
Earnings Management	-0.006**	-0.004	0.047	0.073*
	(-2.503)	(-1.534)	(1.132)	(1.920)
Observations	1,928	1,928	1,946	1,946
Adjusted R-squared	0.166	0.236	0.070	0.092
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
City FE	No	Yes	No	Yes

Table A5. Parallel trend test of the financial performance for treated and control firms in China over 2012-2017

This table presents the parallel trend of the financial performance of Chinese firms in the propensity score matched sample over 2012-2017, as shown in equation A1. Control firms are matched by size and book-market ratio using observations from 2007 to 2014. Dependent variable *ROA* is net income scaled by total assets. *ROE* is net income scaled by total equity. *NI/EMP* is net income scaled by number of employees (labor productivity). *TFP* is the total factor productivity following Olley and Pakes (1996) method. *Leverage* is long term debt scaled by total assets. *Treated* equals to one if a firm is in industries targeted by MC 2025 and zero otherwise. *Size* is natural log of total assets. *Age* is number of years since a firm is established. *Political Connection* equals one if at least one director on the board was or is an official of the central government, a local government, or the military following Fan, Wong, and Zhang (2007) and Liu, Shu, and Wei (2017), and zero otherwise. *Earnings Management* is the discretionary accruals following Kothari, Leone, and Wasley (2005) and Giannetti, Liao, and Yu (2015). All continuous variables are winsorized at 1%. The table reports t-value in parentheses. All t-values are computed with standard errors clustered at the firm level. Significance levels at 1%, 5% and 10% are denoted by *, ** and ***, respectively.

$$y_{i,t} = a + b_1 Treated_i + \sum_{k=-3}^{3} \tau_k Treated_i * Year_{t,k} + \delta X_{i,t} + \gamma_i + \gamma_t + \varepsilon_{it} \qquad \text{(A1)}$$

VARIABLES	(1) ROA	(2) ROA	(3) ROE	(4) ROE	(5) NI/EMP	(6) NI/EMP	(7) TFP	(8) TFP	(9) Leverage	(10) Leverage
Treated	0.016***		0.034**		0.361**		-0.061		-0.013**	
	(2.790)		(2.409)		(2.481)		(-0.994)		(-2.003)	
Treated*2012	-0.004	-0.003	-0.014	-0.014	-0.035	-0.047	-0.006	-0.025	-0.006	-0.006
	(-0.676)	(-0.568)	(-0.987)	(-0.854)	(-0.246)	(-0.366)	(-0.146)	(-0.612)	(-1.268)	(-0.968)
Treated*2013	-0.002	-0.001	-0.022	-0.021	-0.135	-0.073	-0.053	-0.047	-0.003	-0.003
	(-0.424)	(-0.208)	(-1.393)	(-1.187)	(-1.229)	(-0.705)	(-1.643)	(-1.376)	(-0.642)	(-0.553)
Treated*2015	-0.008	-0.009	-0.022	-0.023	-0.368***	-0.327***	-0.044	-0.054	0.004	0.004
	(-1.407)	(-1.501)	(-1.266)	(-1.219)	(-2.839)	(-2.764)	(-1.169)	(-1.273)	(0.899)	(0.763)
Treated*2016	-0.023***	-0.025***	-0.062***	-0.064***	-0.335***	-0.382***	-0.081*	-0.091*	0.012	0.013
	(-4.035)	(-4.169)	(-3.645)	(-3.528)	(-2.661)	(-2.923)	(-1.754)	(-1.812)	(1.431)	(1.578)
Treated*2017	-0.025***	-0.025***	-0.040**	-0.040*	-0.418***	-0.479***	-0.090*	-0.106*	0.006	0.011
	(-3.506)	(-3.369)	(-1.980)	(-1.806)	(-2.818)	(-2.900)	(-1.807)	(-1.943)	(0.764)	(1.327)
Size	0.002	0.008*	0.012***	0.020	0.145***	0.224**	0.571***	0.549***	0.022***	0.023***
	(1.307)	(1.781)	(4.008)	(1.435)	(3.106)	(2.003)	(30.505)	(9.640)	(6.357)	(4.133)
Age	-0.001		-0.001		0.005		0.003		0.001*	
	(-1.550)		(-1.251)		(0.535)		(0.627)		(1.710)	
Political Connection	0.005	-0.006	0.012	-0.022**	0.081	0.074	0.009	0.022	-0.007	-0.002
	(1.348)	(-1.359)	(1.464)	(-1.977)	(0.801)	(0.815)	(0.188)	(0.729)	(-1.356)	(-0.329)
Earnings Management	-0.006	0.048**	0.004	0.100	-0.190	0.752*	-0.502**	-0.003	0.016	-0.005
	(-0.231)	(2.167)	(0.075)	(1.581)	(-0.319)	(1.840)	(-2.022)	(-0.021)	(0.585)	(-0.226)

Observations	1,997	1,997	1,997	1,997	1,779	1,779	1,997	1,997	1,751	1,751
Adjusted R-squared	0.016	0.474	0.020	0.263	0.032	0.653	0.690	0.924	0.234	0.754
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Table A6. Parallel trend test of the government subsidies for treated and control firms in China over 2012-2017

This table presents the parallel trend test of the government subsidies for Chinese firms in the propensity score matched sample over 2012-2017, as shown in equation A1. Control firms are matched by size and book-market ratio using observations from 2007 to 2014. Dependent variable *GovtSub* is government subsidies scaled by total assets. *GovtSubDummy* equals one if a firm receives government subsidies, and zero otherwise. *Treated* equals to one if a firm is in industries targeted by MC 2025 and zero otherwise. *Year* is a year dummy variable that indicates the period relative to the MC 2025 announcement. *Size* is natural log of total assets. *Age* is number of years since a firm is established. *Political Connection* equals one if at least one director on the board was or is an official of the central government, a local government, or the military following Fan, Wong, and Zhang (2007) and Liu, Shu, and Wei (2017), and zero otherwise. *Earnings Management* is the discretionary accruals following Kothari, Leone, and Wasley (2005) and Giannetti, Liao, and Yu (2015). All continuous variables are winsorized at 1%. The table reports t-value in parentheses. All t-values are computed with standard errors clustered at the firm level. Significance levels at 1%, 5% and 10% are denoted by *, ** and ***, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	GovtSub	GovtSub	GovtSubDummy	GovtSubDummy
Treated	0.002***		0.011	
	(2.892)		(1.168)	
Treated*2012	0.000	-0.000	-0.007	0.001
	(0.392)	(-0.103)	(-0.524)	(0.072)
Treated*2013	-0.000	-0.000	-0.012	-0.005
	(-0.100)	(-0.122)	(-0.939)	(-0.367)
Treated*2015	-0.000	-0.000	-0.007	-0.005
	(-0.173)	(-0.331)	(-0.673)	(-0.478)
Treated*2016	-0.000	-0.001	-0.014	-0.015
	(-0.465)	(-0.886)	(-1.537)	(-1.521)
Treated*2017	0.001	0.000	0.008	0.009
	(1.221)	(0.279)	(0.269)	(0.295)
Size	-0.001***	-0.000	0.004	0.027**
	(-3.666)	(-0.917)	(1.378)	(2.220)
Age	-0.000*		-0.001**	
	(-1.790)		(-2.404)	
Political Connection	-0.001	-0.001	-0.003	-0.035**
	(-1.547)	(-1.003)	(-0.525)	(-2.173)
Earnings Management	-0.006**	-0.004	0.037	0.044
	(-1.974)	(-1.573)	(0.899)	(0.882)
Observations	1,928	1,928	1,946	1,946
Adjusted R-squared	0.060	0.595	0.030	0.087
Year FE	Yes	Yes	No	Yes
Firm FE	No	Yes	No	Yes

Table A7. Placebo test of the financial performance using 2012 as the event year

This table presents the DID analysis of the financial performance of Chinese firms in the propensity score matched sample over 2009-2014. Placebo test assumes that the event year is 2012. Control firms are matched by size and book-market ratio using observations from 2007 to 2014. Dependent variable *ROA* is net income scaled by total assets. *ROE* is net income scaled by total equity. *NI/EMP* is net income scaled by number of employees (labor productivity). *TFP* is the total factor productivity following Olley and Pakes (1996) method. *Leverage* is long term debt scaled by total assets. *Treated* equals one if a firm is in industries targeted by MC 2025 and zero otherwise. *After2012* equals one if year is greater than or equal to 2012 and zero otherwise. *Size* is natural log of total assets. *Age* is number of years since a firm is established. *Political Connection* equals one if at least one director on the board was or is an official of the central government, a local government, or the military following Fan, Wong, and Zhang (2007) and Liu, Shu, and Wei (2017), and zero otherwise. *Earnings Management* is the discretionary accruals following Kothari, Leone, and Wasley (2005) and Giannetti, Liao, and Yu (2015). All continuous variables are winsorized at 1%. The table reports t-value in parentheses. All t-values are computed with standard errors clustered at the firm level. Significance levels at 1%, 5% and 10% are denoted by *, ** and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	ROA	ROA	ROE	ROE	NI/EMP	NI/EMP	TFP	TFP	Leverage	Leverage
Treated*After2012	0.002	-0.004	0.004	-0.007	0.062	-0.087	-0.085**	-0.055	0.008	0.004
	(0.383)	(-0.668)	(0.349)	(-0.472)	(0.482)	(-0.655)	(-2.027)	(-1.258)	(1.285)	(0.614)
Treated	0.012**		0.018		0.239*		0.012		-0.023***	
	(2.116)		(1.601)		(1.691)		(0.183)		(-2.721)	
After2012	-0.006		-0.024**		-0.135		-0.021		-0.023***	
	(-1.462)		(-2.289)		(-1.505)		(-0.659)		(-5.156)	
Size	0.002	0.021***	0.014***	0.048***	0.139***	0.747***	0.583***	0.610***	0.026***	0.020**
	(1.256)	(3.820)	(4.536)	(3.584)	(2.940)	(5.204)	(31.149)	(12.139)	(7.178)	(2.070)
Age	-0.001***	-0.004***	-0.002**	-0.012***	-0.006	-0.112***	0.007	0.003	0.001**	-0.005***
	(-3.114)	(-3.492)	(-2.131)	(-3.269)	(-0.534)	(-3.516)	(1.213)	(0.312)	(2.213)	(-3.221)
Political Connection	0.015***	0.007	0.030***	0.011	0.233**	0.181	0.019	0.145**	-0.008	-0.013
	(3.329)	(0.945)	(3.254)	(0.558)	(2.048)	(1.039)	(0.335)	(2.482)	(-1.220)	(-1.465)
Earnings Management	-0.103***	0.005	-0.195***	-0.025	-1.373***	0.239	-0.289	-0.190	0.095***	0.024
	(-4.523)	(0.318)	(-4.147)	(-0.515)	(-2.619)	(0.759)	(-1.423)	(-1.546)	(4.036)	(1.302)
Observations	1,740	1,740	1,740	1,740	1,571	1,571	1,739	1,739	1,736	1,736
Adjusted R-squared	0.072	0.527	0.069	0.346	0.047	0.675	0.707	0.933	0.270	0.743
Year FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Firm FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Table A8. Placebo test of the government subsidies using 2012 as the event year

This table presents the DID analysis of the government subsidies for Chinese firms in the propensity score matched sample over 2009-2014. Placebo test assumes that the event year is 2012. Control firms are matched by size and book-market ratio using observations from 2007 to 2014. The dependent variable *GovtSub* is government subsidies scaled by total assets. *GovtSubDummy* equals one if a firm receives government subsidies, and zero otherwise. *Treated* equals one if a firm is in industries targeted by MC 2025 and zero otherwise. *After2012* equals one if year is greater than or equal to 2012 and zero otherwise. *Size* is natural log of total assets. *Age* is number of years since a firm is established. *Political Connection* equals one if at least one director on the board was or is an official of the central government, a local government, or the military following Fan, Wong, and Zhang (2007) and Liu, Shu, and Wei (2017), and zero otherwise. *Earnings Management* is the discretionary accruals following Kothari, Leone, and Wasley (2005) and Giannetti, Liao, and Yu (2015). All continuous variables are winsorized at 1%. The table reports t-value in parentheses. All t-values are computed with standard errors clustered at the firm level. Significance levels at 1%, 5% and 10% are denoted by *, ** and ***, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	GovtSub	GovtSub	GovtSubDummy	GovtSubDummy
Treated*After2012	0.000	0.001	0.004	0.001
	(0.592)	(0.671)	(0.304)	(0.039)
Treated	0.002**		0.003	
	(2.363)		(0.265)	
After2012	0.001**		0.005	
	(2.178)		(0.563)	
Size	-0.001***	-0.001	0.007***	0.020
	(-3.996)	(-0.697)	(2.881)	(1.593)
Age	-0.000*	0.000	-0.001**	-0.004
	(-1.945)	(0.135)	(-2.352)	(-0.957)
Political Connection	-0.000	0.000	0.003	-0.003
	(-0.308)	(0.428)	(0.601)	(-0.171)
Earnings Management	-0.005*	-0.003	-0.015	-0.017
	(-1.892)	(-1.302)	(-0.503)	(-0.405)
Observations	1,671	1,671	1,691	1,691
Adjusted R-squared	0.057	0.591	0.006	0.102
Year FE	No	Yes	No	Yes
Firm FE	No	Yes	No	Yes

Appendix E. Merging SDC VentureXpert with ISS Director database for Chapter 2

In this appendix, we describe the process of merging venture capital companies in the VentureXpert database with ISS databases through matching venture capital firm names in VentureXpert with the director's primary employment names in the ISS database.

B.1. Name Standardization

We begin by standardizing company names in VentureXpert and primary employment names from ISS databases using the name standardization algorithm developed by the NBER Patent Data Project. This algorithm standardizes common company prefixes and suffixes and strips names of punctuation and capitalization; it also isolates a company's stem name (the main body of the company name), excluding these prefixes and suffixes.

B.2. The Matching Procedure

With these standardized and stem company names provided by both VentureXpert and the ISS database, we merge the databases following the matching procedures similar to Ma (2020) and DiNardo and Lee (2004) as shown below:

1. We match each standardized ISS company name with standardized names from the VentureXpert data.
    a. If we identify the exact match of standardized names, we consider this as a "successful match".
    b. Otherwise, we consider the rest as "potential match" and follow the next step

2. We match each stem ISS company name with stem names from VentureXpert data.
    a. If we identify the exact match of stem names, we consider this as "successful match".
    b. Otherwise, we consider the rest as "potential match" and follow the next step

3. For the remaining companies, each standardized and stem ISS company name is matched with close standardized and stem names from the VentureXpert data using a Spelling distance method. The criterion is based on the possible matching scenarios by translating a keyword into a query containing the smallest distance value. The method evaluates the query and keyword arguments returning non-negative spelling distance values. A derived value of zero indicates an exact match. Generally, derived values are less than 100. We can control the matching process by specifying spelling distance values greater than zero

    a. As a first pass, we modified the program to match only on the firm name, and discovered that in this application, that same threshold led to "too many" matches. As we describe, we, therefore, augmented the process with a manual review. In these cases, we selected the lowest spelling distance as the candidate match. If there was a tie in spelling distance between two candidate comparisons, we selected one match at random. We reviewed every match and dropped those where they judged the two firm names as different companies and categorizes some as a "potential match" and the remaining as "failed to match"

b. The “potential matches” set identified in the procedures above is reviewed by hand, incorporating information from www.crunchbase.com, www.bloomberg.com, including company business descriptions.
c. Pairs confirmed as successful matches through the manual check are moved to the “successful match” set.

Appendix F: Appendix Tables for Chapter 2

Table A9. Baseline results using alternative samples and VC director definition

This table shows the baseline models presented in Table 3 and Table 4 if including VC-backed IPOs and including coincidental VC directors. Panel A and B present the results if both VC-backed IPOs and non-VC-backed IPOs are included in the sample. There are 147 unique firms with VC directors on board. Panel C and D present the results if both coincidental VC directors and the VC directors with prior VC experience are considered as VC directors. There are 234 unique firms with VC directors on board. Panel E and F present the results if both coincidental VC directors and the VC directors with prior VC experience are considered as VC directors, and if both VC-backed IPOs and non-VC-backed IPOs are included in the sample. There are 395 unique firms with VC directors on board.

Panel A. Table 11 results after including VC-backed IPOs

	CEO Vega $_{t+1}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VC Director on Comp. Committee	0.176**			0.181**			0.192***		
	(2.437)			(2.499)			(2.718)		
Number of VC Directors on Comp. Committee		0.168**			0.173**			0.183***	
		(2.505)			(2.578)			(2.814)	
% VC Director on Comp. Committee			0.587***			0.597***			0.619***
			(2.827)			(2.859)			(3.035)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	No	No	No	Yes	Yes	Yes	No	No	No
Observations	11,542	11,542	11,542	11,378	11,378	11,378	11,378	11,378	11,378
Adjusted R-squared	0.515	0.515	0.515	0.513	0.513	0.513	0.514	0.514	0.514
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No	No	Yes	Yes	Yes

Panel B. Table 12 results after including VC-backed IPOs

	CEO Delta $_{t+1}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VC Director on Comp. Committee	0.208^{*}			0.214^{*}			0.227^{**}		
	(1.895)			(1.957)			(2.153)		
Number of VC Directors on Comp. Committee		0.203^{*}			0.209^{**}			0.222^{**}	
		(1.934)			(2.002)			(2.205)	
% VC Director on Comp. Committee			0.701^{**}			0.710^{**}			0.745^{**}
			(2.205)			(2.243)			(2.434)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	No	No	No	Yes	Yes	Yes	No	No	No
Observations	12,584	12,584	12,584	12,412	12,412	12,412	12,412	12,412	12,412
Adjusted R-squared	0.351	0.351	0.351	0.351	0.351	0.351	0.355	0.355	0.355
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No	No	Yes	Yes	Yes

Panel C. Table 11 results after including coincidental VC directors

	CEO Vega $_{t+1}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VC Director on Comp. Committee	0.126^{**}			0.125^{**}			0.135^{***}		
	(2.386)			(2.357)			(2.588)		
Number of VC Directors on Comp. Committee		0.120^{**}			0.120^{**}			0.130^{**}	
		(2.331)			(2.293)			(2.575)	
% VC Director on Comp. Committee			0.513^{***}			0.515^{***}			0.543^{***}
			(3.004)			(2.967)			(3.191)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	No	No	No	Yes	Yes	Yes	No	No	No
Observations	9,535	9,535	9,535	9,398	9,398	9,398	9,398	9,398	9,398
Adjusted R-squared	0.526	0.526	0.526	0.524	0.524	0.524	0.525	0.525	0.525
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No	No	Yes	Yes	Yes

Panel D. Table 12 results after including coincidental VC directors

	CEO Delta $_{t+1}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VC Director on Comp. Committee	0.158**			0.155**			0.163**		
	(2.034)			(1.980)			(2.156)		
Number of VC Directors on Comp. Committee		0.155**			0.153**			0.160**	
		(2.085)			(2.034)			(2.232)	
% VC Director on Comp. Committee			0.671***			0.666***			0.684***
			(2.869)			(2.816)			(2.977)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	No	No	No	Yes	Yes	Yes	No	No	No
Observations	10,384	10,384	10,384	10,242	10,242	10,242	10,242	10,242	10,242
Adjusted R-squared	0.363	0.363	0.364	0.363	0.363	0.363	0.367	0.367	0.367
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No	No	Yes	Yes	Yes

Panel E. Table 11 results after including coincidental VC directors and VC-backed IPOs

	CEO Vega $_{t+1}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VC Director on Comp. Committee	0.108***			0.106**			0.111***		
	(2.595)			(2.525)			(2.640)		
Number of VC Directors on Comp. Committee		0.089**			0.089**			0.093**	
		(2.367)			(2.340)			(2.506)	
% VC Director on Comp. Committee			0.293**			0.292**			0.299***
			(2.537)			(2.507)			(2.591)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	No	No	No	Yes	Yes	Yes	No	No	No

Observations	11,542	11,542	11,542	11,378	11,378	11,378	11,378	11,378	11,378
Adjusted R-squared	0.515	0.515	0.515	0.513	0.513	0.513	0.514	0.514	0.514
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No	No	Yes	Yes	Yes

Panel F. Table 12 results after including coincidental VC directors and VC-backed IPOs

	CEO Delta $_{t+1}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VC Director on Comp. Committee	0.154**			0.146**			0.151**		
	(2.464)			(2.329)			(2.435)		
Number of VC Directors on Comp. Committee		0.138**			0.132**			0.136**	
		(2.476)			(2.350)			(2.472)	
% VC Director on Comp. Committee			0.483***			0.467***			0.471***
			(2.821)			(2.712)			(2.763)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	No	No	No	Yes	Yes	Yes	No	No	No
Observations	12,584	12,584	12,584	12,412	12,412	12,412	12,412	12,412	12,412
Adjusted R-squared	0.351	0.351	0.352	0.351	0.351	0.351	0.355	0.355	0.355
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No	No	Yes	Yes	Yes

Table A10. Performance goal metrics distributions

This table shows the distribution of all performance goal metrics collected from ISS Incentive Lab. We categorize all goals into four groups, Profit, Growth, Market, and Unclassified. Panel A and Panel B presents the distribution of absolute goals and relative goals, respectively.

Panel A. Absolute goals

Goal Group	**Absolute Goal Metric**	**#**	**%**	**Goal Group**	**Absolute Goal Metric**	**#**	**%**
Unclassified	Other	32,242	26.98	*Market*	Stock Price	344	0.29
Profitability	EPS	11,864	9.93	*Growth*	Same store sales	185	0.15
Growth	Sales	11,206	9.38	*Unclassified*	Business Unit	140	0.12
Unclassified	Individual	8,685	7.27	*Profitability*	Noi	131	0.11
Growth	Operating Income	7,993	6.69	*Unclassified*	Staff Relations, Engagement And Training	126	0.11
Profitability	EBITDA	6,522	5.46	*Unclassified*	Diversity	125	0.1
Growth	Cashflow	6,133	5.13	*Unclassified*	Csr	100	0.08
Profitability	Earnings	5,437	4.55	*Profitability*	Gross Profit	96	0.08
Profitability	ROIC	4,139	3.46	*Growth*	Same Store Sales	92	0.08
Profitability	ROE	2,914	2.44	*Unclassified*	Climate Change And Energy Use	85	0.07
Unclassified	Non-Financial	2,720	2.28	*Unclassified*	Environmental Protection	77	0.06
Profitability	EBT	2,335	1.95	*Unclassified*	Fda Approval	74	0.06
Unclassified	Operational	1,993	1.67	*Unclassified*	Customer And Product Responsibility	60	0.05
Profitability	Profit Margin	1,900	1.59	*Unclassified*	Discretionary	38	0.03
Growth	Gross Revenues	1,864	1.56	*Profitability*	Roc	37	0.03
Profitability	EBIT	1,507	1.26	*Unclassified*	FDA Approval	16	0.01
Profitability	FFO	1,274	1.07	*Growth*	IPO of Subsidiary	12	0.01
Profitability	EVA	1,230	1.03	*Unclassified*	Resource Use	7	0.01
Profitability	ROA	1,167	0.98	*Growth*	Book Value	6	0.01
Unclassified	Customer Satisfaction	1,047	0.88	*Profitability*	Cost Reduction	5	0
Unclassified	Vague	950	0.8	*Unclassified*	Society And Human Rights	5	0
Market	Tsr	809	0.68	*Unclassified*	Balance Sheet Related	1	0
Unclassified	Staff Health And Safety	613	0.51	*Unclassified*	Labor Conditions In Supply Chain	1	0
Profitability	Net Income	409	0.34	*Growth*	Sales Contracts	1	0

Unclassified	Debt Related	396	0.33	*Growth*	Working Capital	1	0
Profit	ROI	373	0.31		**Total**	**119,487**	

Panel B. Relative goals

Goal Group	Relative Goal Metric	#	%	Goal Group	Relative Goal Metric	#	%
Market	Tsr	2,270	27.54	*Profitability*	EBT	39	0.47
Unclassified	Other	1,502	18.22	*Profitability*	EBIT	32	0.39
Profitability	ROIC	713	8.65	*Unclassified*	Customer Satisfaction	30	0.36
Profitability	ROE	616	7.47	*Unclassified*	Staff Health And Safety	28	0.34
Profitability	EPS	610	7.4	*Market*	Stock Price	28	0.34
Growth	Sales	554	6.72	*Profitability*	Net Income	19	0.23
Growth	Operating Income	297	3.6	*Unclassified*	Operational	14	0.17
Profitability	ROA	293	3.55	*Unclassified*	Debt Related	12	0.15
Profitability	Profit Margin	220	2.67	*Profitability*	Roc	9	0.11
Profitability	Earnings	190	2.31	*Profitability*	Gross Profit	6	0.07
Growth	Cashflow	157	1.9	*Profitability*	Ros	4	0.05
Profitability	FFO	127	1.54	*Growth*	Same Store Sales	4	0.05
Profitability	ROI	127	1.54	*Growth*	Same store sales	3	0.04
Profitability	EBITDA	93	1.13	*Unclassified*	Staff Relations, Engagement And	3	0.04
Growth	Gross Revenues	79	0.96	*Unclassified*	Csr	2	0.02
Unclassified	Non-Financial	62	0.75	*Unclassified*	Diversity	1	0.01
Unclassified	Vague	50	0.61				
Profitability	EVA	48	0.58		**Total**	**8,242**	

Table A11. Dynamic GMM analysis first stage level regressions results

This table shows the first stage level regressions results for dynamic GMM estimation. See Appendix B for variable definitions. The Fama-French 48 industry fixed effects and year fixed effects are as indicated. Baseline controls include firm size, leverage, market-to-book ratio, ROA, R&D intensity, tangibility, annual stock return, annual stock volatility, firm age, CEO age, CEO tenure, CEO duality, percent independent board, the board size, institutional ownership concentration, and institutional ownership total. In models (1), (3), and (5), we further include CEO delta as a control. State controls include state educational attainment, state per-capita income, and state R&D per capita. Standard errors are clustered at the firm level. T-statistics are in parentheses. The ***, **, and * denote significance levels at 1%, 5% and 10% respectively.

	VC Director on Comp. Committee		Number of VC Directors on Comp. Committee		% VC Director on Comp. Committee	
	(1)	(2)	(3)	(4)	(5)	(6)
ΔVC Director on Comp. Committee[t-2]	0.569***	0.568***				
	(7.566)	(7.573)				
ΔNumber of VC Directors on Comp. Committee[t-2]			0.599***	0.598***		
			(7.599)	(7.604)		
Δ% VC Director on Comp. Committee[t-2]					0.412***	0.412***
					(3.911)	(3.905)
ΔCEO Vega[t-1]	-0.003*		-0.003*		-0.001*	
	(-1.761)		(-1.913)		(-1.814)	
ΔCEO Delta[t-1]		-0.001		-0.000		-0.000
		(-0.399)		(-0.331)		(-1.014)
Min. Distance to Top 50 VC Cities	0.000**	0.000**	0.000**	0.000**	0.000**	0.000**
	(2.120)	(2.238)	(2.210)	(2.315)	(2.101)	(2.171)
Direct Flight to SF	-0.004	-0.005	-0.004	-0.005	-0.001	-0.001
	(-0.777)	(-0.930)	(-0.777)	(-0.947)	(-0.494)	(-0.626)
Direct Flight to BOS	0.013***	0.014***	0.013***	0.014***	0.003**	0.003**
	(2.833)	(3.025)	(2.783)	(3.001)	(2.376)	(2.533)
VC Dry Powder Squared	-0.000	-0.000	-0.000	-0.000	0.000	0.000
	(-0.716)	(-0.599)	(-1.083)	(-0.957)	(0.143)	(0.201)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,135	5,135	5,135	5,135	5,135	5,135
Adjusted R-squared	0.137	0.136	0.139	0.137	0.088	0.087

Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table A12. Two-sample t-test on the control and treated groups in the PSM sample

This table provides the summary statistics of the control and treated groups in the PSM sample. Column (7) presents the difference between column (2) and column (5). See Appendix B for variable definitions. The ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Treated			Control			Difference
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Size (Logarithm)	394	8.12	1.44	394	7.97	1.48	0.15
Lag CEO Vega (Logarithm)	394	8.25	1.44	394	8.09	1.49	0.16
Lag CEO Delta (Logarithm)	394	1.98	1.01	394	1.94	1.12	0.05
Leverage	394	3.36	1.46	394	3.28	1.46	0.08
Market-to-Book Ratio	394	3.45	1.56	394	3.35	1.49	0.1
ROA	394	0.22	0.15	394	0.20	0.15	0.01
R&D Intensity	394	3.49	3.06	394	3.21	3.29	0.29
Tangibility	394	0.04	0.10	394	0.05	0.09	-0.01
Stock Return	394	0.03	0.04	394	0.02	0.04	0
Stock Volatility	394	0.24	0.19	394	0.26	0.20	-0.02
Firm Age (Logarithm)	394	0.13	0.46	394	0.14	0.49	-0.01
CEO Age (Logarithm)	394	0.40	0.19	394	0.40	0.21	0
CEO Tenure (Logarithm)	394	2.50	0.96	394	2.59	0.90	-0.1
CEO Duality	394	4.03	0.11	394	4.04	0.13	-0.01
% Independent Directors on Board	394	1.79	0.72	394	1.86	0.73	-0.07
Board Size	394	0.67	0.47	394	0.71	0.46	-0.04
Institutional Ownership Concentration	394	0.75	0.14	394	0.74	0.15	0.01
Institutional Ownership Total	394	9.96	2.52	394	9.59	2.30	0.37**
Fiscal Year	394	0.05	0.04	394	0.05	0.04	0
Fama-French 48 Industry	394	0.76	0.17	394	0.78	0.20	-0.02

Appendix G: Appendix Tables for Chapter 3

Table A13. Summary statistics of alternative CAR measures

Panel A. Summary statistics of full sample

Variables	N	Mean	SD	Min	Max
CAR[0,30] MAR VW	1,384,164	0.016	0.179	-2.455	17.784
CAR[0,30] MM EW	1,384,164	-0.007	0.191	-7.068	17.561
CAR[0,30] MM VW	1,384,164	-0.008	0.193	-5.765	17.593
CAR[0,60] MAR VW	1,384,215	0.023	0.25	-2.585	18.306
CAR[0,60] MM EW	1,384,215	-0.022	0.289	-15.488	17.908
CAR[0,60] MM VW	1,384,215	-0.024	0.291	-11.656	17.977
CAR[0,120] MAR VW	1,384,297	0.036	0.356	-3.881	23.74
CAR[0,120] MM EW	1,384,297	-0.048	0.462	-33.399	28.348
CAR[0,120] MM VW	1,384,297	-0.056	0.464	-23.297	19.953
CAR[0,255] MAR VW	1,384,340	0.072	0.534	-7.921	38.656
CAR[0,255] MM EW	1,384,340	-0.106	0.831	-95.335	57.198
CAR[0,255] MM VW	1,384,340	-0.121	0.832	-47.737	37.659

Panel B. Summary statistics by PCinsider

	PCinsider=0					**PCinsider=1**					**Mean(1)-Mean(0)**	
Variables	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max	Difference	t-value
CAR[0,30] MAR VW	1,347,633	0.016	0.179	-2.455	17.784	36,531	0.017	0.161	-1.371	2.513	0.001	0.671
CAR[0,30] MM EW	1,347,633	-0.007	0.192	-7.068	17.561	36,531	-0.005	0.175	-1.666	3.654	0.003	2.771
CAR[0,30] MM VW	1,347,633	-0.008	0.194	-5.765	17.593	36,531	-0.005	0.173	-1.574	3.140	0.003	2.758
CAR[0,60] MAR VW	1,347,683	0.023	0.251	-2.585	18.306	36,532	0.026	0.222	-1.688	2.639	0.004	2.778
CAR[0,60] MM EW	1,347,683	-0.022	0.290	-15.488	17.908	36,532	-0.015	0.258	-3.198	7.331	0.007	4.446
CAR[0,60] MM VW	1,347,683	-0.024	0.292	-11.656	17.977	36,532	-0.016	0.253	-2.100	6.475	0.008	5.286
CAR[0,120] MAR VW	1,347,765	0.036	0.357	-3.881	23.740	36,532	0.043	0.323	-2.609	6.852	0.007	3.621
CAR[0,120] MM EW	1,347,765	-0.049	0.463	-33.399	28.348	36,532	-0.038	0.411	-5.009	16.631	0.011	4.479
CAR[0,120] MM VW	1,347,765	-0.056	0.466	-23.297	19.953	36,532	-0.042	0.403	-4.230	13.395	0.014	5.746
CAR[0,255] MAR VW	1,347,808	0.072	0.536	-7.921	38.656	36,532	0.073	0.485	-4.004	8.225	0.001	0.207
CAR[0,255] MM EW	1,347,808	-0.106	0.833	-95.335	57.198	36,532	-0.093	0.729	-10.981	30.665	0.012	2.828