Two Essays on Corporate Finance

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ABSTRACT

This dissertation investigates the impact of political factors on firm corporate policies. In the first essay, I investigate whether political uncertainty affects firm innovation, using United States gubernatorial elections as a source of plausibly exogenous variation in uncertainty. I find that firm innovation productivity, captured by patent counts and citations, declines 3.8% and 5.5% respectively in the year leading up to an election and quickly reverses afterward. This finding is robust to various specifications and endogeneity concerns. Incumbent Republican regime is negatively associated with innovation, and the negative effect of political uncertainty on innovation only exists in elections where the incumbent governor is a Republican. Finally, I find that the uncertainty effect is more pronounced in elections with high levels of uncertainty, in politically sensitive and non-regulated industries, and in firms subject to less binding financing constraints.

The second essay (jointly with Jerry Cao, Brandon Julio and Sili Zhou) examines the impact of political influence and ownership on corporate investment by exploiting the unique way provincial leaders are selected and promoted in China. The tournament-style promotion system creates incentives for new provincial governors to exert their influence over capital allocation, particularly during the early years of their term. Using a neighboring-province difference-in-differences estimation approach, we find that there is a divergence in investment rates between state owned enterprises (SOEs) and non-state owned enterprises (non-SOEs) following political turnover. SOEs experience an abnormal increase in investment by 6.0% in the year following the turnover, consistent with the incentives of a new governor to stimulate investment. In contrast, investment rates for non-SOEs decline significantly post-turnover, suggesting that the political influence exerted over SOEs crowds out private investment. The effects of political turnover on investment are mainly driven by normal turnovers, and turnovers with less-educated or local-born successors. Finally, we provide evidence that

the political incentives around the turnover of provincial governors represent a misallocation of capital as measures of investment efficiency decline post-turnover.

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Chapter 1

Political Uncertainty and Innovation: Evidence from U.S. Gubernatorial Elections

1.1. Introduction

Corporate innovation plays a vital role in determining firm competitive advantages (Porter (1992)) and is an important driver of a nation's long-term economic growth (Solow (1957) and Baumol (2001)). Although many existing studies have looked at links between firm-specific or macroeconomic factors and innovation, surprisingly little attention has been paid to how institutional factors, such as political forces, shape firm innovation.[1] According to the Global Innovation Index (GII) 2014 report, political stability and government effectiveness

[1]Current research on the determinants of innovation has focused on firm- and market-specific factors such as incentive compensation for managers (Manso (2010)), institutional ownership (Aghion et al. (2013)), anti-takeover provisions (Atanassov (2013) and Chemmanur and Tian (2013)), access to the equity market (Gao et al. (2014) and Hsu et al. (2014)), firms' information environment (He and Tian (2014)), banking competition (Cornaggia et al. (2014)), investors' tolerance (Tian and Wang (2014)) and stock liquidity (Fang et al. (2014)), among many others.

under the political environment category are rated as the top two most influential factors in fostering innovation.[2] Politics affect firm innovation because politicians make policies and regulatory decisions that frequently alter the economic environments and external boundaries in which innovative firms operate. For example, politicians can support innovation through a variety of channels, such as a direct financial grant, a government procurement policy (e.g., buying national products or services) and/or a tax concession for R&D for small and private businesses. In this paper, I seek to fill the void in the literature by empirically examining the real effects of politics on innovation. Specifically, I investigate the link between political uncertainty and firm innovation, using U.S. gubernatorial elections between as a source of plausibly exogenous variation in uncertainty.

Starting from Bernanke (1983), the real option theories establish that uncertainty increases the value of the option to defer an (at least partially) irreversible investment expenditure (Dixit and Pindyck (1994), Tigeorgis (1996), Seth and Chi (2005) and Bloom, Bond and Van Reenen (2007)). The value of the option to defer is particularly meaningful for investment in innovation, given that innovation is costly and the process is long, idiosyncratic and often involves a high failure probability, representing the exploration of new untested approaches (Holmstrom (1989), Aghion and Tirole (1994) and Manso (2011)). One important way in which political uncertainty induced by elections influences real innovation decisions is through the channel of increasing uncertainty about changes in state leadership and government policies. The idea is simple: State governor has the ability to influence state policies (Peltzman (1987) and Ang and Longstaff (2012)) and different government policies (e.g., R&D, tax concession, healthcare, safety, environment and economic regulations) have different effects on the payoffs to firm investment in innovative projects, depending on the type of industry and the type of firm (Marcus (1981)). Without certainty about election outcomes and government future policies, firms become cautious and tend to hold back on investment in innovative projects in the face

[2] The report is available at https://www.globalinnovationindex.org/userfiles/file/reportpdf/GII-2014-v5.pdf

of political uncertainty, because they are unable to assess risk and opportunity and make the tradeoffs necessary for investment. The more uncertain the election, the higher the value of deferral, and the more likely firms are to postpone investment in innovation projects. The above discussion leads to my primary hypothesis that firm innovation activities are expected to decline in the year leading up to an election due to the increase in political uncertainty related to elections.

Similarly, after an election, firms should resume investment in innovation projects as soon as possible. The logic is that firm managers observe the policy positions of the respective candidates in these gubernatorial races well in advance of the actual election. By the day of the election, firms likely know which innovative project they should pick up if each candidate wins. Given the time-to-build considerations and the costs of deferral, it makes no sense for firms to wait until the election has occurred to start to consider their investment decisions. Therefore, the post-election increase in innovation should follow relatively shortly after the election uncertainty is resolved. Motivated by these observations, my second hypothesis is that after the political uncertainty related to elections is resolved and the new government policy becomes clear, firms increase their innovation activities accordingly in the one-year post-election period immediately following the election, to the extent that they make up the foregone innovation projects by adapting to policy changes and switching their innovation trajectory to fit new policy.

To illustrate some key aspects of the relations between political uncertainty, changes in government policies, and firm innovation decisions examined in this paper, consider the 2014 gubernatorial election in Florida. The two candidates, Democrat Charlie Crist and Republican Rick Scott, show stark differences on the principal issues from education and immigration to the environment. For example, on the energy and environmental regulation, Crist believes that man-made pollution is contributing to the decline of Earth's environment.[3] Unlike Scott, he

[3]For example, he was quoted as saying that "I didn't only read about climate change and discuss it with my friends. I led by example. In April I ordered an energy audit at the Governor's Mansion and outfitted the place

is a supporter of cap-and-trade style pollution regulation. He supports incentives to increase Florida's renewable energy mix, especially solar expansion and opposes fracking for natural gas. Crist also wants to do away with the monopoly of big utility companies and support smaller businesses that may offer energy alternatives at competitive prices.[4] Unlike Crist, Scott is a climate change skeptic and believes that cap-and-trade has no impact on global temperatures. He repealed solar installation rebates and supports fracking for natural gas. Scott has expressed his preference to seek new domestic energy sources, like natural gas and offshore oil drilling, to keep energy prices low. The two candidates also differ greatly on the healthcare policy. Crist is a supporter of President Barack Obama's Affordable Healthcare Act. As governor of Florida, he once floated a plan to offer $150-per-month health plans to insure nearly $4 million uninsured Floridians. Scott opposes government intervention in healthcare, and has joined with other states in suing the federal government over the implementation of the Healthcare Act.[5] Scott has stated that he believes the "free market" should be the only marketplace for healthcare. For the past few months before the election, the two candidates together have spent more than $83 million in television advertisements to attack each other, and the race is dead even. The gubernatorial race is so closely contested that the likely winner for Florida's next governor is difficult to forecast.

The above example highlights several key considerations in a firm's decision to innovate in the face of political uncertainty induced by elections. The first one is the presence of uncertain election outcomes associated with possible changes in government future policies. The two candidates differ widely in their policy positions and the winner of the election has the ability to influence state polices that affect the payoffs to firm investment in innovative

with high-efficiency lightbulbs and a Heliocol solar heater for the outdoor swimming pool. I also got a new car: an ethanol-fueled Chevy Tahoe. There was only one ethanol station in Tallahassee, but the effort was worth it, I thought." See http://www.ontheissues.org/governor/Charlie_Crist_Energy_+_Oil.htm

[4]For example, in his 2008 state of the state address, Crist once recommended allocating $200 million toward alternative energy development.

[5]Scott was once quoted as claiming that "The Obama's Affordable Care Act would kills jobs, bankrupt America, and – who knows? – maybe even cause halitosis."

projects. Before the election, firms tend to hold back on investment in innovation projects, as they don't know which candidate will likely win the race and which policy will be adopted. On the other hand, once the election has occurred and the electoral uncertainty is resolved, firms switch their innovation trajectory to fit the new policy and make up the foregone investment accordingly. Therefore, elections change the status quo by increasing uncertainty about future policy outcomes. The second consideration is the presence of significant policy differences between Republican and Democratic parties, as highlighted in the gubernatorial race for the governor's office of Florida. It is well known that the Democratic and Republican parties in the U.S. political system have different political agendas that influence economic outcomes and corporate activities differently (Kahin and Hill (2013)). For example, the partisan view of politics suggests that the two parties differ significantly with respect to almost all major issues on economic, regulatory, and social policies, e.g., taxation rules, government expenditures, national defense and welfare reforms (Alesina (1987)). The above discussion leads to my final hypothesis: the impact of political uncertainty induced by elections should be highly dependent on which party is in power as well as how the power is transferred surrounding the elections.

There are several empirical challenges in estimating a causal relationship between political uncertainty and innovation. The first one is how to measure political uncertainty. In this paper, I employ U.S. gubernatorial elections as the primary proxy for political uncertainty. Political uncertainty arises from gubernatorial elections because state policies highly depend on the governor's preferences and actions, directly or indirectly affecting firm innovation decisions. Using gubernatorial elections, which occur in some states every year, rather than national elections or the economic policy uncertainty index developed by Baker et al. (2013), as a source of political uncertainty avoids potential concerns that the changes in firm innovation are driven by changes in the business cycle or global or national economic uncertainty. Detailed discussion on the appropriateness of using gubernatorial elections as the proxy for political uncertainty is provided in §1.2.1. The second challenge is how to measure firm innovation. Following He and Tian (2013) and Chemmanur and Tian (2013), I use patents rather than R&D spending as a proxy to gauge firm innovation, because patents are the ultimate innovation outputs and represent the successful usage of all (both observable and unobservable) innovation inputs. On the other hand, R&D spending only reflects one particular dimension of observable quantitative innovation inputs and may suffer significant measurement errors in the Compustat database.[6] The third challenge is how to time the impact of political uncertainty on firm innovation, proxied as its patenting activity. Existing literature documents that on average, the lag between R&D expenditures and patent applications is often less than one year.[7] Given that gubernatorial elections usually take place in November followed by inaugurations of the new governors in the following January, it is reasonable to assume that political uncertainty created by elections in year t affect *contemporaneous* firm innovation behavior also in year t. The National Bureau of Economic Research (NBER) Patent and Citation database provides a clean, detailed source of firm-level patenting activities between 1976 and 2006 that allows me to directly measure innovation and its economic value along multiple dimensions, such as quantity versus quality. I therefore test my main hypotheses by using data on gubernatorial elections and patents of a comprehensive sample of public U.S. firms for the period from 1976 to 2006.

In the empirical tests, I first examine the impact of political uncertainty induced by gubernatorial elections on firms' innovation behavior around the election cycle. My findings highlight an interesting pattern in corporate innovation activity around the gubernatorial elec-

[6]Chemmanur and Tian (2013) point out that "more than 50% of firms do not report R&D expenditures in their financial statements. However, the fact that a firm does not report its R&D expenditures does not necessarily mean that the firm is not engaging in innovation activities. Replacing missing values of R&D expenditures with zeros, a common practice in existing literature, introduces additional noise that may bias the coefficient estimates."

[7]For example, Hausman, Hall and Griliches (1984) and Hall, Griliches and Hausman (1986) investigate the lag in the patent-R&D relationship and find that there is little evidence of anything but contemporaneous movement of patents and R&D. Similar findings have been documented for U.S. manufacturing firms in a recent study by Gurmu and Sebastian (2008). This point of view has been widely adopted in recent studies examining drivers of innovation (Lerner and Wulf (2007), Aghion et al. (2013) and Cao et al. (2014)).

tion cycles. First, I find a negative relationship between innovation and the election years. This is consistent with the findings of existing literature (e.g., Julio and Yook (2012) and Jens (2013)) in the sense that firms face political uncertainty prior to political leadership changes. I also find a robust increase in innovation rates following the election years. The innovation productivity, measured as patent counts and citation counts, increases by approximately 3.7% and 7.6% relative to their respective sample means. The post-election increase in innovation is a novel finding in the literature. The increase in post-election innovation is of roughly the same magnitude as the election year decline in innovation, suggesting that innovation appears to have been postponed from election year to post-election year. Collectively, the evidence supports the view that political uncertainty captured by gubernatorial elections represents a temporary reallocation in innovation productivity: firms become cautious and tend to pull back on innovation prior to elections due to the increase in political uncertainty related to election outcomes; after the uncertainty is resolved and government policy becomes clear, they make up the foregone innovation projects by adapting to policy changes and switching their innovation trajectory to fit new policy. These results are robust to an alternative measure of political uncertainty using presidential election cycles, a placebo (falsification) test with randomly generated election events, and various susbsample analyses.

My identification strategy behind the primary results assumes that political uncertainty is on average higher in the year leading up to an election than in non-election years. While this seems to be a reasonable assumption, there is some concern for possible reverse causality in this estimation. In order to establish causality and cross-validate my main hypothesis, I further exploit variation in the degree of political uncertainty induced by elections and their likely economic impact across states and over time. The impact of electoral uncertainty on innovation should depend on both the predictability of an election's outcomes and the probability that a policy shift will occur. Closely contested elections introduce exogenous shocks and are considered as better proxies for political uncertainty. (e.g., Snowberg, Wolfers and Zizewitz (2007)). The intuition is that closely contested elections entail more uncertainty about the eventual winner and future policy and therefore can be associated with a higher level of political uncertainty, which should in turn create a greater decline in election year innovation. Consistent with the prediction, I find that the dampening effect of political uncertainty on innovation is mainly driven by elections in which the electoral uncertainty and competition are likely to be high. Although my identification strategy is less vulnerable to potential reverse causality, this finding further helps strengthen the causality that indeed runs from political uncertainty to innovation, confirming my main hypothesis.

It is well known that the Democratic and Republican parties in the U.S. political system have different political agendas that influence economic outcomes and corporate activities differently (Kahin and Hill (2013)). For example, the two major parties differ greatly in their core philosophies and ideals on almost all major issues such as taxes, the role of government, entitlements, national defense and healthcare policy, among many others (Alesina (1987)). As such, a natural question that follows is whether and how incumbent party affiliation alters the patterns of innovation around elections. To address this question, I explore the interactions between political uncertainty and political regime and examine how such interactions affect firm innovation. First, I find that on average, Republican regime is associated with less innovation. Further investigation reveals that the magnitude of innovation sensitivity to electoral uncertainty indeed varies with political regimes. Specifically, I find that the reduction in election-year innovation only exists in Republican regime, while there is a weak increase in innovation in election years under Democrat regime; In post-election years, Republican regime experience an increase in innovation, but the increasing pattern is more noticeable for Democrat regime. In a more in-depth analysis, I show that the change in innovation surrounding elections is also highly dependent on whether and how the political regime is transferred. Overall, my findings appear to support the view that Republicans hinder innovation. This is consistent with economics literature showing that on average, annual GDP is higher under

Democratic term (Alesina and Rosenthal (1995) and Alesina et al. (1997)). The contrasting evidence of the differential impact of Republican and Democratic regimes on the innovation dynamics around the election cycle is likely to be driven by the differences in innovation s-trategies and polices between the two parties. Overall, my findings appear to support the view that Republican party stifles innovation (Khanna (2014)).

There are firm characteristics that should result in firms being particularly sensitive to increases in political uncertainty. I expect that the election year reduction in innovation is more pronounced for firms operating in *politically sensitive industries*, because these firms are more likely to face regulatory changes that affect their business operations and corporate decisions (Kostovetsky (2009)). Consistent with the expectation, I find that politically sensitive industries have approximately a 9.2% (16.0%) decline in innovation measured as patent quantity (quality) in election years, while is figure is only 3.7% (5.1%) in other industries. Interestingly, I also find that politically sensitive industries do not experience an increase in post-election innovation. These results support the view that firms operating in politically sensitive industries are particularly sensitive to increases in political uncertainty. On the other hand, considerable evidence suggests that firms operating in *heavily regulated industries* have strong incentives to manage and mitigate political risks via lobbying activities and/or through the capture of regulators and politicians (Liu and Ngo (2013)). Not surprisingly, I find that regulated industries are generally immune to political uncertainty: the election year decline in innovation and the post-election spike in innovation do not exist in regulated industries. These results indicate that while firms in regulated industries face possible legislation and policy changes induced by elections, this electoral uncertainty may be largely mitigated/hedged through their lobbying activities.

My final empirical analysis examines whether financial constraints exacerbate or mitigate the effects of political uncertainty on firm innovation. Existing literature points to lack of financing as one of the major barrier for innovation (e.g., Canepa and Stoneman (2008), Navarro et al. (2010) and Gorodnichenko and Schnitzer (2013)). Firms facing tighter financial constraints spend less for their innovative projects than unconstrained ones, as financial constrained firms often experience debt overhang or underinvestment problems (Myers (1977)). Motivated by these observations, I hypothesize that there is a more pronounced impact of political uncertainty on innovation productivity for less financially constrained firms, to the extent that these firms are on average more innovative and thus should be more negatively affected. To test the hypothesis, I employ three proxies for financial constraints: the Kaplan and Zingales (1997) index, firm size, and dividend payer, with the last two proxies popularized by Almeida et al. (2004). Across all three measures, I find that the negative coefficient estimate of the election year indicators are more significant and larger in the less financially constrained subsample than in the more constrained subsample, suggesting that the election year reduction in innovation is concentrated in firms subject to less binding financial constraints.

This paper contributes to the literature in several dimensions. First, it contributes to the literature on drivers of corporate innovation. Existing research on this topic has focused on firm- and market-specific factors such as incentive compensation for managers (Manso (2010)), institutional ownership (Aghion et al. (2013)), anti-takeover provisions (Atanassov (2013) and Chemmanur and Tian (2013)), access to the equity market (Gao et al. (2014) and Hsu et al. (2014)), firms' information environment (He and Tian (2014)), banking competition (Cornaggia et al. (2014)), investors' tolerance (Tian and Wang (2014)) and stock liquidity (Fang et al. (2014)), among many others. Although these studies enhance the understanding of the mechanisms that motivate firms to innovate, the role of politics, such as political uncertainty, is largely overlooked. My thus paper helps to fill this void in the literature by documenting that political uncertainty negatively affects firm-level innovation.

Second, the paper contributes to a growing literature on the role of politics in shaping firm performance and corporate decisions. From the perspective of asset pricing, Boutchkova et al. (2013), Brogaard and Detzel (2012), Pastor and Veronesi (2012, 2013) and Belo et al. (2013)

study the impact of political uncertainty on stock returns. From the perspective of corporate finance, Julio and Yook (2012) show that firms tend to reduce investment prior to national elections around the world, due to the rising political uncertainty related to elections. Similar findings have been documented for U.S. public firms around U.S. gubernatorial elections by Jens (2013). Using U.S. gubernatorial election data, Liu and Ngo (2013), Colak et al. (2014), Dai and Ngo (2014), and Gao and Qi (2014) further investigate the impact of political uncertainty on bank failure rate, IPO activity, accounting conservatism, and the financing costs of public debts respectively. As far as I know, I am the first to relate firm-level innovation behavior to political uncertainty induced by U.S. gubernatorial elections.

Third, the paper adds to the burgeoning literature on the differential effects of political regimes (Republican vs. Democrat) on financial markets. Prior studies on this topic document that financial markets behave differently under different political regimes. For example, Santa-Clara and Valkanov (2003) find that excess stock returns are higher and real interest rates are lower under Democratic than Republican presidencies after controlling for business-cycle variables and risk factors. Belo et al. (2013) report that during Democratic presidencies, firms with high government exposure experience higher cash flows and stock returns, and that the opposite is true during Republican presidencies. Giuli and Kostovetsky (2014) further show that Democratic-leaning firms are more socially responsible than Republican-leaning firms. In this study, I explore whether the innovation sensitivity to political uncertainty around the election cycle varies with political regimes. I show that on average, firms are less innovative under Republican regime. I also document that the election year reduction in innovation only exists in states in which the incumbent governor is a Republican. Taken together, my findings are consistent with economics literature showing that on average, annual GDP is higher under Democratic term (Alesina and Rosenthal (1995) and Alesina et al. (1997)).

The only paper I am aware of that directly study the relation between political uncertainty and innovation is Bhattacharya et al. (2014). They show that political uncertainty adversely affects a nation's innovation growth, but policy (e.g., left, center and right) does not matter. They thus conclude that businesses adapt to different policies but face a problem when they do not know which policy to adapt to. My results differ in important ways from those found in Bhattacharya et al. (2014). First, I provides direct evidence of the causal effect of political uncertainty on firm innovation in a sample consisting of only U.S. public firms, while Bhattacharya et al. (2014) examine whether it is policy or political uncertainty that affects a nation's aggregate innovation growth in the context of national elections around the world. Second, I use gubernatorial elections as a source of uncertainty, while they focus on political uncertainty created by national elections. Third, my finding that changes in innovation around elections are highly dependent on which party is in power as well as how the power is transferred, is novel and does not exist in Bhattacharya et al. (2014). This is likely because of differences in how political uncertainty arises and is resolved in presidential and parliamentary election systems.[8]

The remainder of the paper proceeds as follows. §1.2 describes the sources of data and the sample construction process. §1.3 discusses the identification strategy and presents my main empirical results related to firm innovation dynamics around elections, including various subsample analyses, multiple robustness checks, and an examination of political uncertainty under different political regimes and its impact on innovation. §1.4 concludes.

1.2. Data and Summary Statistics

I collect a large amount of data from various sources. The primary source for gubernatorial election data is the CQ Press Electronic Library. I supplement the election data and check for data quality by cross-referencing Wikipedia information. I collect information on firm-level

[8]The United States follows a presidential election system, while the international sample used in Bhattacharya et al. (2014) consists of approximately 50% parliamentary systems. See Stepan and Skach (1993) for a general discussion on the trade-off between presidential and parliamentary systems.

innovation activity from the National Bureau of Economic Research (NBER) Patent and Citation Database with data coverage from 1976 to 2006. Following the innovation literature, I set the number of patents and citations to zero for firms that have no patent information available in the NBER database. Firm financials and industry characteristics are obtained directly from the Compustat database. I omit firms not headquartered in any of the 48 U.S. states and firms with missing data for the main variables used in the analysis.[9] I match the NBER patent data with firm characteristics using patent application year and a bridge file provided by the NBER database in which gvkey is the common identifier. I use patent application year instead of its grant year as the former is argued to better capture the actual timing of innovation (Griliches, Pakes, and Hall (1988)). I further exclude firms that Compustat does not cover and then match this sample with gubernatorial election data by election year and state. I collect information on state macroeconomic conditions from the U.S. Bureau of Economic Analysis (BEA). Finally, I require the firm to be domiciled in the U.S. for at least four consecutive years. By applying these selection criteria, I end up with a sample of 90,870 firm-year observations between 1976 and 2006. Below, I describe main variables, sample selection and data collection procedures. Appendix A provides detailed information on definitions, construction, and data sources of variables.

1.2.1. Gubernatorial Elections

The timing of U.S. gubernatorial elections are exogenously determined by law. Every state but Louisiana holds its gubernatorial election on the first Tuesday following the first Monday in November.[10] Currently, the vast majority of the states hold gubernatorial elections every four years, with the exception of Vermont and New Hampshire, which choose to run their gu-

[9] New Hampshire and Vermont are excluded in the analysis as they follow a two-year gubernatorial term throughout the sample period.

[10] The election timing of Louisiana may differ every year due to the adoption of the open primary system, where all the candidates for an office run together in one election. See Wikipedia for more detailed discussion about elections in Louisiana: http://en.wikipedia.org/wiki/Elections_in_Louisiana

bernatorial elections every two years. Five states, including Louisiana, Kentucky, Mississippi, New Jersey, and Virginia, elect their state governors in odd-numbered years just preceding a presidential election. Other states run their gubernatorial elections in even-numbered years to coincide either mid-term elections or presidential elections. In thirty-eight states, governors are limited to two consecutive terms. In some cases, states have changed the length of their gubernatorial election cycle. For example, the state of Arizona and the state of Rhode Island switched from a two-year election cycle to a four-year election cycle in 1986 and 1994 respectively.[11]

I use U.S. gubernatorial elections as the main proxy for the measures of political uncertainty. I focus on gubernatorial elections in the baseline analysis, instead of the presidential elections or the economic policy uncertainty index developed by Baker, Bloom and Davis (2012) for several reasons. First, gubernatorial elections are pre-scheduled and thus can be viewed as mostly exogenous events where political uncertainty arises. Using such a setting mitigates possible endogeneity between political uncertainty and general economic conditions, which also affect corporate innovation decisions, and allows us to make causal inference regarding the real impact of political uncertainty on innovation. Second, unlike presidential elections, gubernatorial elections in different states occur in different years. Therefore, substantial across- and within-state variations exist in addition to the time series variation in the timing of gubernatorial elections. For example, there are total 366 gubernatorial elections conducted in 48 states during the sample period of 1976 to 2006.[12] In contrast, there are only 8 president elections during the same period, which is not an adequate sample to yield meaningful statistical inferences. On the other hand, as a country level index, there is little cross-sectional variation in the economic policy uncertainty index by construction. Besides, the index itself

[11]The only special election in the sample period took place in California in 2003. It resulted in voters replacing incumbent Democratic Governor Gray Davis with Republican Arnold Schwarzenegger. I treat this observation as all other election observations, and its inclusion has no effect on the results.

[12]Since I am interested in analyzing the change in innovation productivity dynamics in both pre- and post-election years, I exclude New Hampshire and Vermont in the analysis, which follow a two-year gubernatorial term.

may not be purely exogenous in the sense that firm behavior could also impact news coverage, government policy and economic forecasts, which constitute the key underlying components of the index.[13]

My study considers 366 gubernatorial elections in 48 states held between 1976 and 2006. Detailed election information is obtained from a variety of sources. The primary source for election and regime change data is the CQ Press Voting and Elections Collection, which is part of the CQ Press Electronic Library.[14] This database contains information on election date, the names of Republican/Democract candidates and the independent candidate (if any), incumbent party affiliation, whether the incumbent governor seeks re-election, whether the incumbent is subject to term limit, other reasons if the incumbent doesn't participate in the election (e.g., defeated in primary or retired or simply not running for re-election), the winning candidate/party affiliation, the percentage vote for each candidate and the vote margin of the election. I supplement the gubernatorial election data with Wikipedia for cases in which election information is missing from the CQ Electronic Library.

Panel A of Table 1 summarizes the election data by state for the sample period from 1976 to 2006. There are 366 gubernatorial elections in total, distributed quite evenly across the 48 states. The distribution of elections offers a great deal of cross-sectional and time-series variations to test their effects on firm innovation productivity. Following the identification and classification of Julio and Yook (2012), Jens (2013) and Colak et al. (2014), I classify an election as being more uncertain if it is a close election, where the victory margin, defined as the vote difference between the first place candidate and the second place candidate, is less than 5%. I also expect elections with absence of incumbents or party changes (D$\rightarrow$R or R$\rightarrow$D) to be more uncertain. Further discussion on the appropriateness of the measures of the degree of electoral uncertainty is provided in §1.3.5. Of the 366 elections, 80 are defined as close. In

[13] See Baker, Bloom and Davis (2012) for more detailed discussion on the construction of the the economic policy uncertainty index: http://www.policyuncertainty.com/methodology.html

[14] The CQ Press Electronic Library database is available at http://library.cqpress.com/elections/

67 elections, incumbent governors do not seek re-election due to reasons other than term-limit expiration (such as retired or defeated in primary). Finally, 128 elections are associated with party changes where incumbent governor and the successor are from different parties.

1.2.2. Innovation Productivity

Following recent innovation literature such as Seru (2012) on publicly traded firms and Lerner, Sorensen, and Stromberg (2011) on privately held firms, I proxy for firm innovativeness using its patenting activity, which indicates how effectively the firm transforms innovation inputs into outputs. Patent-related data are obtained from the latest version of the NBER Patent and Citation Database originally developed by Hall, Jaffe, and Trajtenberg (2001).[15] The database contains detailed information on more than three million U.S. patents granted by the United States Patent and Trademark Office (USPTO) between January 1976 and December 2006. It provides patent information such as patent number, patent application dates, grant dates, patent assignee names and identifiers, patent technology class, firms' Compustat-matched identifiers, the number of citations received by each patent, and other details. Patents appear in the database only if they are eventually granted by the USPTO by the end of 2006. Using patent information retrieved from the NBER Patent and Citation Database, I therefore construct two measures to gauge a firm's innovation productivity

The first measure is a firm's total number of patent applications filed in a given year that are eventually granted. This measure captures the quantity of innovation output. I focus on the patent's application year instead of its grant year because previous studies (e.g., Griliches, Pakes, and Hall (1988) and Hall, Jaffe and Trajtenberg (2001, 2005)) have shown that the former are better aligned with the actual time of innovation. As patents vary widely and significantly in their technological and economic importance, my first innovation measure

[15] See Hall, Jaffe and Trajtenberg (2001) for more detailed discussion about the database and the updated database is available at https://sites.google.com/site/patentdataproject/Home/downloads

based on simple patent counts do not distinguish ground-breaking inventions from incremental technological discoveries and thus may not reflect innovation success adequately. To assess a patent's influence, I follow Hall, Jaffe and Trajtenberg (2001, 2005) and use forward non-self citations each patent receives in subsequent years as measures for the extent of its quality and importance. My results continue to hold when I include self-citations. As a robustness check, I also construct citations per patent as an alternative measure of innovation productivity to further gauge the patent's quality.

As clearly pointed out by prior studies, the two raw measures of innovation productivity suffer from truncation problems due to the finite length of the sample period associated with the NBER Patent and Citation Database. The first truncation problem on patent counts originates from the fact that only successfully granted patents will enter the NBER database. Considering the average two-year lag between a patent's application year and its granted year reported by USPTO, I observe a gradual declining trend in the number of patent applications towards the end of the sample period, especially in the last two to three years, as many patent applications filed during this period were still under review and had not been granted by 2006. To correct for this truncation bias, I follow the recommendations of Hall, Jaffe and Trajtenberg (2001) and adjust the patent counts using the "weight factor" estimated from the application-grant empirical distribution between 1995 and 2000.[16] The second truncation problem is regarding the citation counts. As it usually takes time for patents to receive citations from other patents, patents created in the later years of the sample period have less time to accumulate citations. Following Hall, Jaffe, and Trajtenberg (2001), I correct for this type of truncation bias in citation counts by multiplying it with the scaling factor "hjtwt" provided by the NBER patent database, which is estimated using the shape of the citation-lag empirical distribution. Due to the observed high level of right skewness of the patent grant data, I use the natural logarithm of adjusted patent counts and the natural logarithm of adjusted citation

[16]See Fang, Tian and Tice (2014) for detailed methodology on computing the weight factor.

counts, *# Patents* and *# Citations*, as the primary innovation productivity measures used in my analysis. To avoid losing firm-year observations with zero patent counts and citation counts, I further add one to the actual values when computing the natural logarithm.

Finally, I acknowledge that using patent activity to measure firm innovation is not without limitations. Patent activity captures only one dimension in which a firm protects returns resulting from innovation. Many inventions are protected as trade secrets, such as the formula for Coca-Cola, and different industries have different innovation cycles and patenting propensities. Nonetheless, patents remain the most direct measure of the extent and quality of firms' innovation output (Griliches (1990)), and the use of patent activity to measure of innovation productivity is widely accepted in the existing literature (Lerner, Sorensen, and Stromberg (2011)). I believe that adequate controls for heterogeneity in firm financials, firm industries, and firm locations should lead to reasonable inferences that can be applicable across firms in different industries.

1.2.3. Firm and State Variables

To isolate the effect of political uncertainty and firm innovation, I control for an array of firm and industry characteristics that have been demonstrated by existing literature to influence innovation. Hall and Ziedonis (2001) document that the number of patent applications and the number of patent citations are positively correlated with firm size. I therefore control for firm size, measured as the natural logarithm of total assets. The results are robust to alternatively using the natural logarithm of net sales. To control for the effect of a firm's life cycle on its innovation ability and propensity, I include firm age, measured as the natural logarithm of one plus the number of years elapsed between the IPO year and the current year t. Return on assets (ROA) and Tobin's Q are added to capture firms' operating profitabilities and growth opportunities. In addition, I control for R&D expenses scaled by lagged property, plant, and equipment (PPE), as Atanassov (2012) argues that R&D expenditures are the main determinant of the innovative output of firms.[17] Cash flow, capital expenditure (CAPEX) and leverage ratio are further added to account for possible effects of cash holdings, fixed asset investment and capital structure decisions on innovation. I additionally include Herfindahl index calculated at the three-digit SIC industry level as measures of the extent of product market competition (e.g., Aghion et al. (2005), Chemmanur and Tian (2013), Atanassov (2012), Chang et al. (2013), He and Tian (2013), Tian and Wang (2013) and Van Reenen and Zingales (2013)). All firm financials and industry characteristics are obtained directly from the Compustat database from 1976 to 2006. The sample period is chosen to match the availability of firm innovation data from the NBER database. Finally, state-level annual GDP growth rate is included to take into account the general economic conditions within a state and the data is collected from the U.S. Bureau of Economic Analysis (BEA).[18] In order to minimize the impact of data errors and outliers, I winsorize all firm characteristics at the 1st and 99th percentiles. Appendix A provides detailed variable descriptions as well as the variable sources.

1.2.4. Summary Statistics

Panel B of Table 1 presents descriptive statistics on the main variables used in the regression analyses. On average, firms in my final sample have approximately 4 patents filed (and subsequently granted) per year and receive 28 citations. Moreover, an average firm has total assets of 2.3 billion, ROA of 2.4%, Tobin's Q of 2.1, cash flow of -3.7%, leverage of 25.0%, R&D ratio of 7.3%, capital expenditure ratio (CAPEX) of 6.2%, Herfindahl Index (HHI) of 0.17, and is 15.5 years old since its listing date. The reported firm characteristics are typical

[17] Following existing innovation literature (e.g., Chemmanur and Tian (2013)), I set missing R&D expense to zero. Results are similar when I use R&D intensity, an indicator variable that takes on a value of one if R&D expense is non missing and zero otherwise.

[18] Bureau of Economic Analysis website is available at http://www.bea.gov/

of Compustat public firms and are generally comparable to previous studies (e.g., Atanassov (2012) and Cornaggia et al. (2013)).

[Insert Table 1 about here]

Panel C of Table 1 reports the pair-wise Pearson correlation coefficients among election event time period dummies, innovation measures and control variables, with * indicating significance at the 1% level. Not surprisingly, I first note that the two innovation measures, *# Patents* and *# Citations*, are highly correlated with each other. Consistent with my hypothesis, *Election dummy* is negatively and significantly correlated with both innovation measures, suggesting that political uncertainty discourages firm innovation. In contrast, correlation coefficients between *Post-election dummy* and the two innovation measures are positive and significant at 1% level, indicating that there exists a post-election rebound in innovation after the election uncertainty is resolved. As expected, the signs and significance of correlation coefficients between other control variables and the two measures of innovation are largely consistent with existing innovation literature (e.g., Chang et al. (2013), He and Tian (2013), Tian and Wang (2013)). Although the evidence appears to support my hypothesis, these unconditional relations should be interpreted with caution as the effects of other control variables are not taken into consideration.

[Insert Table 2 about here]

To further understand the dampening effect of elections on innovation, I conduct a univariate analysis by comparing the mean difference in innovation productivity between election years (0) and non-election years (-1, +1, +2) for each state. Panel A of Table 2 presents results for the measure of innovation quantity, *# patents*. Of the 48 states, 36 have lower innovation rates in election years compared to non-election years, which is consistent with my hypothesis. In addition, I find that election years are associated with significantly lower innovation rates for 3 states: California (CA), Florida (FL) and Minnesota (MN). The only state in which innovation is significantly higher in election years is Arkansas (AR). More importantly, when aggregating all states, I find that innovation rate in election years has a mean value significantly lower than non-election years. Panel B of Table 2 reports results for the measure of innovation quality, *# citations*. Results are largely consistent with those reported in Panel A and thus explanations are omitted for the sake of brevity. Overall, Table 2 indicates that political uncertainty, captured by gubernatorial elections, is associated with less innovation quantity and lower innovation quality.

[Insert Table 3 about here]

Table 3 compares the mean difference in innovation productivity between election years and non-election years by industry, where Fama French 48 industries is used as the industry classification. Panel A considers innovation quantity, measured as *# patents*. Consistent with my hypothesis, 39 out of the 48 industries have lower innovation rates in election years compared to non-election years. I find that election years are associated with significantly lower innovation rates for 3 industries: machinery (Mach), electronic equipment (Chips) and transportation industries (Trans). Importantly, there is no industry in which innovation is significantly higher in election years than non-election years. Panel B of Table 3 further considers innovation quality *# citations* and documents similar industrial patterns comparable to those reported in Panel A. Overall, Table 3 confirms Table 2 and supports the notion that political uncertainty hinders firm innovation.

1.3. Empirical Results

This section presents my empirical findings related to changes in innovation productivity around gubernatorial election cycles. I begin with the univariate analysis, followed by a multiple regression framework controlling for firm characteristics and state economic conditions. I then perform a number of additional tests to ensure that my baseline results are robust to alternative model specifications and variable definitions. Finally, I exploit variation in the sensitivity of innovation productivity to political uncertainty across elections, governor affiliations, industries, and firms.

1.3.1. Univariate Evidence

Table 4 provides a more detailed examination of corporate innovation dynamics across the gubernatorial election cycle for the full sample, the Republican subsample and Democrat subsample respectively. For each event year in the [-1, 0, +1, +2] election cycle, the table reports the mean innovation rates in that year (denoted by *Mean*) along with the mean innovation rates in the rest of sample years (denoted by $Mean^R$), where year 0 indicates the gubernatorial election year. The mean difference in innovation rates ($Mean\ diff \doteq Mean - Mean^R$) and the associated t-statistics of the difference are also reported. Panel A presents descriptive statistics for the full sample. Panel B shows the equivalent under the Republican regime. Panel C shows the equivalent under the Democrat regime. I first consider Panel A using the full sample. Unconditionally, firm innovation rates, measured by both patent counts and citation counts, are significantly lower in the election years [0] than those in other years. On the other hand, post-election years [+1] are associated with significantly higher innovation rates. The increase in post-election innovation seems to be roughly equal to the decline in innovation in election years. Further, I note that both the pre-election years [-1] and the post-election years [+2] show no significant pattern in innovation. Consistent with the full sample results reported in Panel A, the mean innovation rates under Republican (Democrat) regime show a similar pattern in the election years and post-election years, aside from a small increase (decrease) in innovation in the pre-election years. The univariate analysis, while not controlling for firm and state characteristics, provides preliminary evidence supporting the view that firms temporarily reduce innovation productivity in the year leading up to an election outcome and increase innovation once the election uncertainty is resolved.

[Insert Table 4 about here]

To visualize the univariate analysis results and to better understand the innovation dynamics around the full election cycle, I construct Figure 1. Panel A of Figure 1 considers the full sample and plots the time series difference between the mean innovation rates for a given year in the [-1, +2] election cycle and the mean innovation rates for other sample years. Panel B and Panel C show the equivalent figures for the Republican subsample and Democrat subsample respectively. The 95% confidence intervals (in gray dashed lines) are also added to each figure. The patterns shown in Figure 1 confirms Table 4 and indicates that there is a sharp decline in innovation rates in the election years followed by a considerable spike in innovation in the one-year post-election periods.

[Insert Figure 1 about here]

Panel D of Figure 1 further compares the mean innovation rates under Republican regime versus the mean innovation rates under Democrat regime over the full election cycle. A clear pattern emerges from Panel D of Figure 1: Republican regime is associated with slightly higher innovation rates than Democrat regime and the wedge between innovation rates under Republican regime and Democrat regime is much less noticeable in election years. Later in this section, I investigate the impact of different political regimes on innovation patterns in more detail. Since omitted variable bias in the univariate analysis may mask the true relations between elections and innovation, I next rely on multivariate analysis to formally examine the effects of political uncertainty on innovation dynamics.

1.3.2. Regression Specification

In this section, I investigate corporate innovation policy in a multivariate setting to control for firm characteristics and state economic conditions. I employ a standard difference-in-difference (DD) framework to evaluate changes in corporate innovation productivity across gubernatorial election cycles that cannot be explained by other explanatory variables. The main regression model is specified as follows:

$$\text{Innovation}_{ijt} = \alpha_i + \gamma_t + \beta_0 \times \text{Election}_{j,t} + \beta_1 \times \text{Post-election}_{j,t+1} + \sum \varphi_\mathbf{i} \mathbf{X_i} + \sum \delta_\mathbf{j} \mathbf{S_{jt}} + \varepsilon_{ijt} \quad (1.1)$$

where i indexes firms, j indexes states, and t indexes years. The dependent variable, the firm-level innovation productivity, is measured as the natural logarithm of adjusted patent counts and the natural logarithm of adjusted citation counts, *# Patents* and *# Citations*. The first measure captures innovation quantity and the latter captures innovation quality. The primary explanatory variables of interest are the time-state dummies measuring the periods before and after the gubernatorial election event. First is the election year dummy, which takes on a value of one if a gubernatorial election occurred in that state in that year. The post-election year is defined as the one-year period immediately following the election year. The timing of the indicator variables is set to capture the firms' innovation dynamics around the election cycle. The above DD model uses firm in states without an upcoming election as the control group for a treated sample of firms in states about to elect a governor. The coefficient estimates of interest are β_0 and β_1, which capture the changes in innovation productivity during the election years and the post-election years between the treated and control samples. To control for firm characteristics and state economic conditions, I include a set of control variables motivated by Cao et al. (2014) and Fang, Tian, and Tice (2014), who identify potential determinants of innovation, both in the cross-section and over time. $\mathbf{X_i}$ is a vector of firm characteristics, which include firm size, firm age, ROA, Tobin's Q, cash flow, leverage ratio, capital expenditure (CAPEX), R&D expense ratio and product market competition (Herfindahl Index). $\mathbf{S_{jt}}$ is a vector of state macroeconomic variables, including annual state gross domestic product (GDP) growth rate and state unemployment rate. Appendix A provides details on variable descriptions as well as variable sources. In addition, I include both firm and year fixed effects in the baseline innovation regression to account for any time-invariant unobservable variation. This specification captures the within-firm variation in corporate innovation around gubernatorial election event years. Standard errors are clustered at the firm level in all specifications.

There are potential concerns with the one-way clustering of regression standard errors used in my analysis. However, as pointed out by Thompson (2011) and Petersen (2009), two-way clustering is only valid provided: (i) Both N and T are "large"; and (ii) The aggregate shocks must dissipate over time. In such cases, clustering by two dimensions will likely produce unbiased standard errors. Apparently, my sample only satisfies the second requirement but doesn't fit the first, as in my sample N exceeds 6,500 firms but the average T is around 15 years with a maximum of 37 years. In view of this, I first report the baseline results with standard errors clustered at firm level only. For robustness, I repeat my analysis with standard errors clustered at both state and year levels and find slightly weaker but qualitatively the same results.[19]

1.3.3. Innovation Productivity around Election Years

Table 5 presents the baseline regression results on the impact of gubernatorial elections on corporate innovation productivity using the full sample. I estimate panel regressions and include firm and year fixed effects in all regression specifications. Standard errors are clustered at firm level. The dependent variable in columns (1) to (3) is the natural logarithm of one plus patent counts, *# patents*, which measures innovation quantity. In columns (4) to (6),

[19]To save space, the robustness with alternative standard error estimates are not reported here. Results are available upon request.

the dependent variable is the natural logarithm of one plus citation counts, *# citations*, which measures innovation quality. I estimate three specifications for each innovation measure that differ only in whether the election year dummy and the post-election year dummy are included separately or jointly.

[Insert Table 5 about here]

Consistent with the hypothesis that political uncertainty dampens innovation productivity in election years, I find that the coefficient estimates on the election year dummy is negative and statistically significant across all specifications. Depending on the specification, the reductions in conditional innovation productivity range between 0.037 to 0.050 for the innovation quantity measure and between 0.054 and 0.080 for the innovation quality measure. On the other hand, I see a large and robust increase in innovation rates in the post-election years, which is consistent with the univariate analysis results reported in Table 4. The negative significant coefficients of the election dummy and the positive significant coefficients on the post-election dummy together suggest that firms exhibit a tendency to pull back innovation activity in the year leading up to gubernatorial elections due to political uncertainty but scale up innovation immediately after the political uncertainty is resolved. The estimates in column 3 (6) show that innovation productivity, captured by patent counts (citation counts), first decrease by 0.037 (0.054) in election years and then increase right away by 0.036 (0.073) on average in the one-year post-election period, after controlling for firm characteristics and state macroeconomic conditions. In terms of economic magnitude, the coefficient estimates reported in column (3) translates into a 3.8% ($= 100 \times (e^{\beta_0} - 1)$) decrease and a 3.7% ($= 100 \times (e^{\beta_1} - 1)$) increase in patent counts in the election years and post-election years respectively, relative to mean patent counts in the full sample years. Similarly, estimates in column (6) represent a 5.5% drop and a 7.6% rebound in citation counts right before and after elections. Interestingly, the average reduction in innovation in election years appears to be roughly equal to the post-election spike in innovation rates in absolute terms. The coefficient estimates on the control variables are consistent with the literature in terms of signs and magnitudes. Corporate innovation productivity is positively related to firm size, Tobins' Q, R&D expense and CAPEX, but negatively related to firm age and product market competition. Other control variables are generally not related. For robustness, I also estimate panel regressions with standard errors double-clustered at both state and year levels and find similar results.

Given the asymmetry in the coefficient estimates between the election year and the post-election year, it is interesting to see whether the pre-election drop and the subsequent increase in innovation around the election cycle reflect a real distortion or a temporal pull-back/reallocation of innovation. To formally evaluate the net change in innovation around the gubernatorial election cycle, I perform a test of a linear combination of the regression coefficients on the election and post-election variables. I test the null hypothesis that the coefficients on the election and post-election variables sum to zero, which would suggest a temporal pull-back/reallocation of innovation activity. The final two rows of Table 5 presents the sum of those coefficients along with the corresponding t-statistics for the null hypothesis that they sum to zero. For innovation productivity measured as both patent counts in column (3) and citation counts in column (6), I cannot reject the null hypothesis that the election year reduction in innovation is offset in magnitude by the post-election increase.

Overall, my baseline regression results reported in Table 5 highlight an interesting pattern in corporate innovation activity around the gubernatorial election cycles. First, I find a negative relationship between innovation and the election years. This is consistent with the findings of existing literature (e.g., Julio and Yook (2012) and Jens (2013)) in the sense that firms face political uncertainty prior to political leadership changes. I also find a robust increase in innovation rates following the election years. The innovation productivity, measured as patent counts and citation counts, increases by approximately 3.7% and 7.6% relative to their respective sample means. The post-election increase in innovation is a novel finding in the literature. The evidence is consistent with the view that political uncertainty captured by gubernatorial elections represents a temporary reallocation in innovation productivity: firms tend to become cautious and are pulling back on innovation prior to elections due to the increase in political uncertainty related to elections; after the uncertainty is resolved and the policy becomes clear, they make up the foregone innovation projects by adapting to policy changes and switching their innovation trajectory to fit new policy.

For the reminder of the paper, I only indicate which control variables are included in the specifications but do not report the coefficient estimates to preserve space. Moreover, the coefficient estimates for the control variables remain largely unchanged for my various specifications.

1.3.4. Robustness Tests

Before moving to additional analyses, I perform a series of robustness tests on my main findings that political uncertainty dampens firm innovation productivity in election years. Throughout this subsection, I use the baseline innovation regression specification and control for both firm and year fixed effects. Standard errors are clustered at the firm level and corrected for heteroskedasticity in all specifications. The first set of robustness tests uses presidential election cycle, instead of gubernatorial elections, as an alternative proxy for political uncertainty. To capture changes in corporate innovation around presidential elections, I create two additional indicator variables to describe the timing of presidential election events. *President election* is a binary variable set equal to one if the year coincide with a presidential election year during my sample period. The *post-president election* is measured as the one-year period immediately following the presidential election year. As discussed earlier, there are potential drawbacks using presidential election cycle as measures of political uncertainty. Although the timing of presidential elections is also exogenous, this measure suffers from a lack of cross-sectional variation. For example, my sample period from 1976 to 2006 covers only 8 presidential elections, compared to 366 gubernatorial elections.

[Insert Table 6 about here]

I replace gubernatorial election event dummies with presidential election indicators and re-estimate the baseline model. Panel A of Table 6 presents the results for the alternative measures of political uncertainty. Consistent with the baseline results, I find that the coefficients on the presidential election dummy are negative and statistically significant across all specifications, suggesting that firms reduce their innovation productivity significantly in the year leading up to an presidential election. I also find that there is a robust increase in innovation in the one-year period immediately following the presidential elections. It is interesting to note that the average decline in innovation rates in presidential election years is roughly three times as large in absolute terms as the post-election spike in innovation rates. In unreported analysis, I show that the sum of changes in innovation rates around the presidential election cycle is negative and statistically significant from zero, suggesting a net reduction in innovation productivity due to the increase in political uncertainty related to presidential elections.

While I control for various measures of time-varying firm characteristics and state macroeconomic conditions, there are still potential concerns that my results might be coming from nonlinear temporal trends in the data. To address this concern, in the second robustness test, I re-estimate the baseline regressions over the sample period using random "placebo" dummy variables. Specifically, I falsify the gubernatorial election dates by randomly assigning the election years to each state within a four-year cycle. I further require that the relative frequency of random assigned election events each state matches the relative frequency of actual gubernatorial elections as reported in Panel A of Table 1. In doing this, I end up with two random placebo dummy variables, $Election^P$ and $Post\text{-}election^P$, that looks like the gubernatorial election event indicators used in the previous regressions, except that the timing is randomly allocated across states. Thus, if a temporal trend were driving the results in the earlier specifications, I would expect a significantly negative coefficient on $Election^P$ dummy along with a significantly positive coefficient on $Post\text{-}election^P$ dummy. Panel B of Table 6 reports the estimates of this random placebo test. All of the estimates on the control variables are similar as in the other specifications. As expected, the coefficients on the two placebo dummy variables, $Election^P$ and $Post\text{-}election^P$, are insignificant, indicating that the effects in the baseline regressions are systematically related to gubernatorial election events and not to temporal nonlinear trends in the sample.

In the last three panels of Table 6, I experiment with additional robustness checks. As indicated in Table 2 and Table 3, a small number of states and industries are associated with significantly lower innovation rates in election years. To ensure that my results are not driven by a few dominant states or industries, I drops firms headquartered in California, Florida and Minnesota (Panel C) and firms operating in machinery, electronic equipment and transportation industries (Panel D) from the full sample respectively, and re-estimate the baseline regressions. Results confirm that my main findings are robust to the exclusion of a few outliers. In Panel E, I remove observations during the 1999-2000 dot-com bubble period and results remain virtually unchanged.

1.3.5. Degree of Electoral Uncertainty and Innovation

I have so far established the fact that innovation productivity is systematically lower in the period leading up to a gubernatorial election in the overall sample, which supports the hypothesis that firms tend to pull back innovation when facing political uncertainty. In order to cross-validate the main hypothesis and deepen the understanding of political uncertainty, I further exploit variation in the degree of political uncertainty induced by elections and their likely economic impact across states and over time. The impact of electoral uncertainty on innovation should depend on both the predictability of an election's outcomes and the probability that a policy shift will occur. High uncertain elections introduce exogenous shocks and thus are better proxies for political uncertainty. (e.g., Snowberg, Wolfers and Zizewitz (2007)). Motivated by these arguments, I expect that the negative effects of political uncertainty on innovation should be more pronounced in elections characterized by higher levels of electoral uncertainty. I examine these predictions in this subsection.

I consider three *ex post* measures that capture the degree of electoral uncertainty. The first measure is the the victory margin, defined as the vote difference between the first place candidate and the second place candidate. The basic idea is that closer elections, indicated by smaller victory margins, entail more uncertainty about the eventual winner and future policy and therefore can be associated with higher levels of political uncertainty, which should create a greater decline in election year innovation. To incorporate differences in the degree of electoral uncertainty, I create an indicator variable, *close election*, which takes on a value of one if the victory margin is less than 5% and zero otherwise. It is an *ex post* measure of how close the election was, but should capture the *ex-ante* uncertainty levels of election outcomes well. Of the 366 elections covered in my analysis, 80 (approximately 22%) are identified as close elections. This metric has been used extensively in the literature. For example, Julio and Yook (2012) and Jens (2013) use this measure to analyze changes in corporate investment around close elections, Colak et al. (2014) investigate whether close elections amplify the dampening effect of political uncertainty on IPO activity, and Dai and Ngo (2014) examine the relationship between close elections and accounting conservatism. In a related paper, Bhattacharya et al. (2014) use close elections to assess the differential effects of policy uncertainty on country-level innovation growth.

The second measure considers elections in which incumbent governors are not seeking re-elections for reasons other than term-limit expirations (such as retired or defeated in primary). Previous studies extensively document that the advantage of incumbency is an important predictor of any executive or legislative elections outcomes (e.g., Cover (1977), Gelman and King (1990) and Ansolabehere and Snyder (2002)). Consistent with their observations, I find that incumbents in my sample win more than 80% of the gubernatorial races when they run for re-elections. Thus, it is reasonable to expect that if an incumbent governor is not a candidate on the election ballot for reasons other than term limits, the political uncertainty and competition around the election are likely to be high. To capture the variation in incumbency advantage, I define an indicator variable, *absence of incumbent*, which takes a value of one if incumbents don't seek re-elections for reasons other than term limits. I identify 67 gubernatorial elections (approximately 18%) with the indicator variable equal to one.

The third measure explores whether an election leads to a change in the governing party (R→D or D→R). Although Democratic and Republican parties dominate the U.S. political landscape, they differ greatly in their core philosophies and ideals on major issues such as taxes, the role of government, entitlements, national defense and healthcare, among many others. Motivated by these observations, I thus expect that elections characterized by party changes should cause a higher level of political uncertainty and a greater drop in innovation leading up to the election. The indicator variable, *party change*, takes on a value of one if an election is associated with a change in the governing party and zero otherwise. Based on the classification, I identify 128 (approximately 35%) elections with the *party change* indicator equal to one.

[Insert Table 7 about here]

To test whether electoral uncertainty attenuates or exacerbates the election year reduction in innovation, I split the full sample into two groups according to the three measures of political uncertainty, and rerun the baseline regression on each group separately.[20] Table 7 summarize these results. Each panel is based on such a proxy for electoral uncertainty, as

[20] Please note that non-election years are included in in the subsample analysis as a benchmark group.

indicated by the panel header: Panel A considers *close election*, Panel B uses *absence of incumbent* and Panel C examines *party change*. Each odd-number column, where the dummy is equal to zero, uses the subsample with high uncertainty elections. Each even-number column, where the dummy is equal to one, uses the subsample with low uncertainty elections. Across all three measures, I find that the coefficient estimates of *election* indicators are more negative in the high uncertainty subsample than those in the low uncertainty subsample, especially for the first two measures, *close election* and *absence of incumbent*. In unreported *Wald* test, I show that the differences in coefficient estimates between subsamples are statistically significant for the first two measures and are in fact insignificant for the third measure.

To summarize, I find that the dampening effect of political uncertainty on innovation is mainly driven by elections in which the electoral uncertainty and competition are likely to be high. Although my main identification strategies are less vulnerable to potential reverse causality, the findings in this subsection help strengthen the causality that indeed runs from political uncertainty to innovation, further confirming my main hypothesis.

1.3.6. Republican vs. Democrat

Prior literature has shown that financial markets behave differently under different political regimes (i.e., Republican vs. Democrat). For example, Santa-Clara and Valkanov (2003) find that excess stock returns are higher and real interest rates are lower under Democratic than Republican presidencies after controlling for business-cycle variables and risk factors. Belo et al. (2013) report that during Democratic presidencies, firms with high government exposure experience higher cash flows and stock returns, and that the opposite is true during Republican presidencies. In a recent study, Giuli and Kostovetsky (2014) further show that Democratic-leaning firms are more socially responsible than Republican-leaning firms. A natural question that arises is whether and to what extent the innovation sensitivity to electoral uncertainty varies with political regimes. As the univariate tests in Table 4 and Panel D of Figure 1 show, there are important differences in innovation behaviors between Republican regime and Democrat regime over the election cycle. To this end, I investigate whether and how incumbent party affiliation alters the patterns of innovation around elections.

The party identification of the governor is the party of the governor who held office for the majority of the year. Since gubernatorial elections usually take place at the beginning of November followed by inaugurations of the new governors in the following January or February, the party of the election year will be the party of the incumbent, while the party of the following year will be the newly elected governor's party. The indicator variable, $regime_{j,t}$, takes a value of one if the incumbent governor is Republican in year t and zero if he/she is Democrat. To investigate the role of political regime, following Julio and Yook (2012), I estimate an augmented version of the baseline innovation regression:

$$\begin{aligned} \text{Innovation}_{ijt} = \; & \alpha_i + \gamma_t + \beta_0 \times \text{Election}_{j,t} + \beta_1 \times \text{Election}_{j,t} \times \text{Regime}_{j,t} \\ & + \beta_2 \times \text{Post-election}_{j,t+1} + \beta_3 \times \text{Post-election}_{j,t+1} \times \text{Regime}_{j,t} \\ & + \beta_4 \times \text{Regime}_{j,t} + \sum \boldsymbol{\varphi_i X_i} + \sum \boldsymbol{\delta_j S_{jt}} + \varepsilon_{ijt} \end{aligned} \tag{1.2}$$

where $\text{Regime}_{j,t}$ is the indicator variable set equal to one if the party affiliation of the incumbent governor of state j in year t is Republican and zero if he/she is Democrat. The coefficient estimates of the interaction terms, β_1 and β_3, should pick up the added effects of Republican regime on the magnitude of innovation sensitivity to political uncertainty in *election years* and *post-election years* respectively. The coefficient estimate on the indicator variable $\text{Regime}_{j,t}$ alone should capture any underlying differences in innovation between Republican regime and Democrat regime over the *full sample peiod*.

[Insert Table 8 about here]

Table 8 summarizes the estimation results. The dependent variable in columns (1) to (4) is the natural logarithm of one plus patent counts, which measures innovation quantity. In columns (5) to (8), the dependent variable is the natural logarithm of one plus citation counts, which measures innovation quality. In column (1) of Table 8, I add to my baseline innovation regression the *regime* indicator to assess the differential effects of political regime on innovation across the full sample years. In column (2), I interact the *regime* indicator with the two election event timing indicators, *election* and *post-election*, to investigate the role of political regime around election years. To facilitate comparison, in columns (3) and (4), I split my full sample based on political regime and reexamine the elections' impact on innovation under the Republican regime and the Democrat regime separately. Columns (5) to (8) replicate the analysis in the first four columns but using citation based innovation measure as the independent variable.

I first note that the coefficient estimate on the *regime* indicator is negative and statistically significant in column (1), but is insignificant in column (4). The results indicate that on average, firms are less innovative under Republican regime over the full sample period. Importantly, I find that the coefficient estimates for the interaction term, *election* $\times$ *regime*, are large, negative and statistically significant for both innovation measures. In addition, when the interaction terms are added, the coefficient estimates on the *election* indicator not only become insignificant but also reverse in sign. These results imply that all of the election-year reduction in innovation can be explained by states where the incumbent governor is a Republican. On the other hand, innovation experiences a slight increase in election years where the incumbent is a Democrat. The magnitude of the interaction term *election* $\times$ *regime* in column (2) suggests a 9.2% reduction in the election-year innovation under the Republican regime, which is more than double the average 3.8% reduction in election-year innovation across all elections, previously estimated using the baseline regression specification reported in column (3) of Table 5. Across all specifications, I find a significant positive relationship between the *post-election* indicators and innovation, consistent with the baseline findings. In column (2), the coefficient estimate on the interaction term, *post-election* $\times$ *regime*, is negative and statistically significant but has a small magnitude about half that of the *post-election* indicator. These results indicate that while there is a weakly significant decline in post-election innovation quantity under Republican regime, this is dominated by the overall increase in post-election years. However, Republican regime does not have a significant effect on the post-election increase in innovation quality, as indicated by the insignificant coefficient of the interaction term *post-election* $\times$ *regime* in column (6). The subsample analyses reported in columns (3)-(4) and columns (7)-(8) compare the impact of an election on innovation when the incumbent is a Republican or Democrat. Results confirms that the election year decline in innovation only exists in years where the incumbent is a Republican. I also include in the final two rows a test of whether the net change in innovation surrounding elections is significantly different from zero under Republican regime. This is simply a test of whether the sum of the coefficients on the election event timing indicators and the interaction terms are zero. The table shows that under Republican regime, the average post-election increase in innovation cannot offset the reduction in innovation in election years, representing a real distortion (net reduction) of innovation activity.

[Insert Figure 2 about here]

To visualize and to better understand the role of political regime, I construct Figure 2 based on the estimates in column (2) of Table 8 and those in column (3) of Table 5. Panel A of Figure 2 illustrates the magnitude of innovation sensitivity to electoral uncertainty around the election cycle for Republican regime, Democrat regime and all elections respectively. The red long-dashed (blue short-dashed) line shows the changes in innovation under the Republican (Democrat) regime, while the dark solid line illustrates the changes in innovation for all elections. A contrasting pattern emerges from Panel A of Figure 2: the reduction in election-year innovation only exists in Republican regime, while there is a weak increase in innovation in election years under Democrat regime; In post-election years, Republican regime experience an increase in innovation, but the increasing pattern is more noticeable for Democrat regime. It is also interesting to note that under Republican regime, the post-election increase in innovation is much smaller in magnitude than the election-year reduction, suggesting a *net reduction* in innovation due to the electoral uncertainty. In contrast, Democrat regime is associated with a *net increase* in innovation around election years. Panel B of Figure 2 summarizes the *net change* in innovation around the election cycle for Republican regime (red bar), Democrat regime (blue bar) and all elections (dark bar) respectively.

[Insert Table 9 about here]

Having shown that political regime (i.e., Republican vs. Democrat) affects innovation sensitivity to electoral uncertainty around the election cycle, I next extend the scope of the analysis one step further by examining the role of regime transition (i.e., D→D, D→R, R→R, or R→D). I expect that the effect of political regime on innovation in election years should also depend on the post-election political regime, and vice versa. To do this, for each of the four regime transition indicators, I add to the baseline regression interaction terms between the indicator and the two election event timing dummies (*election* and *post-election*). Table 9 summarize the results from this analysis with the type of regime transition indicated by the column header.

I first consider election years. The main variables of interest are the interaction terms between *election* dummy and *regime transition* indicators. First, I note that the interaction term *election* × *D2D dummy* in column (1) has a significant positive coefficient, while the coefficient of the interaction term *election* × *D2R dummy* in column (2) is significant negative. These results imply that the previously observed weak increase in election-year innovation under Democrat regime is driven by *D2D* regime transition. On the contrary, there is indeed a decline in election-year innovation under Democrat regime, if Republican party wins the subsequent election (*D2R* transition). Following a similar logic, interaction terms in columns (3) and (4) of Table 9 suggest that the election-year reduction in innovation under Republican regime only exists in *R2R* regime transition, while Republican regime experiences a spike in election-year innovation on condition that Democrat party wins the subsequent election (*R2D* transition).

I then examine post-election years. The main variables of interest are the interaction terms between *post-election* dummy and *regime transition* indicators. The interaction term *post-election* $\times$ *D2D dummy* in column (1) has a significant positive coefficient, while the coefficient of the interaction term *post-election* $\times$ *R2D dummy* in column (4) is weakly significant and negative. These results indicate that the spike in post-election year innovation under Democrat regime only exists in *D2D* regime transition. In contrast, there is a weak decline in post-election innovation under Democrat regime, if Republican party is defeated in the previous election (*R2D* transition). Similarly, interaction terms in columns (2) and (3) indicate that the marginal increase in post-election year innovation under Republican regime only occurs in *D2R* regime transition, while Republican regime shows a slight decrease in post-election year innovation if Republican party is reelected (*R2R* transition).

To summarize, I find that on average, republican regime is associated with less innovation. I further show that the magnitude of innovation sensitivity to electoral uncertainty varies with political regimes. Specifically, the reduction in election-year innovation only exists in Republican regime, while there is a weak increase in innovation in election years under Democrat regime; In post-election years, Republican regime experience an increase in innovation, but the increasing pattern is more noticeable for Democrat regime. In a more in-depth analysis, I find that the change in innovation surrounding elections is also highly dependent on whether and how the political regime is transferred. Collectively, my findings appear to support the view that Republican party stifles innovation (Khanna (2014)). This is consistent with economics literature showing that on average, annual GDP is higher under Democratic term (e.g., Alesina and Rosenthal (1995) and Alesina et al. (1997)). The contrasting evidence of the differential impact of Republican and Democratic regimes on the innovation dynamics around the election cycle is likely to be driven by the differences in innovation strategies and polices between the two parties. Democrats tend to engage in specific, identifiable national goals, such as safe and clean energy, exploring and learning about space, or wiring the nation. They are also willing to create new programs that provide targeted resources to the private sector to directly subsidize early-stage commercial innovation. In contrast, Republicans prefer to create the general conditions for, and incentives to encourage, innovation in many areas. For example, they prefer low corporate taxes, tax incentives for R&D performance, and free trade regimes to encourage innovation, while eschewing subsidies for specific technologies and sectors (Kahin and Hill (2013)).

1.3.7. Industry Characteristics

To better understand the mechanism through which political uncertainty induced by gubernatorial elections affects innovation, in this subsection, I examine whether my results vary across industry characteristics. Specifically, I examine whether and to which extent politically sensitive industries and heavily regulated industries exacerbate or attenuate the impact of political uncertainty on firm innovation around the election cycle.

Firms are likely to differ from each other with respect to their sensitivity to electoral uncertainty. For example, Julio and Yook (2012) and Jens (2013) find that the dampening effect of political uncertainty on corporate investment is stronger in politically sensitive industries. I therefore expect that the election year reduction in innovation should be more pronounced for firms operating in politically sensitive industries, because these firms are more likely to face regulatory changes that affect their business operations and corporate decisions (Kostovetsky

(2009)). Following the identification and classification of Hong and Kostovetsky (2011), I classify firms operating in Beer (4), Smoke (5), Guns (26), Gold (27), Mines (28), Coal (29), and Oil (30) industries as politically sensitive, where Fama French 48 industries is used as the industry classification. The indicator variable, *political sensitive industry (PSI)*, takes on a value of one if firms belong to one of these politically sensitive industries. I split the sample into firms in politically sensitive industries and firms in other industries, and re-estimate the baseline regression model. Panel A of Table 10 summarize the results from this analysis. Consistent with my expectation, I find that politically sensitive industries have approximately a 9.2% (16.0%) decline in innovation measured as patent quantity (quality) in election years, while is figure is only 3.7% (5.1%) in other industries. It is also interesting to note that politically sensitive industries do not experience an increase in post-election innovation. These results support the view that firms operating in politically sensitive industries are particularly sensitive to increases in political uncertainty.

[Insert Table 10 about here]

Firms operating in *heavily regulated industries* have strong incentives to manage and mitigate political risks via lobbying activities and/or through the capture of regulators and politicians. For example, Liu and Ngo (2013) study the political incentives to delay bank failure around elections and show that the likelihood of bank failure declines significantly in the year leading up to a gubernatorial election. They attribute their finding to incumbent politicians interfering in bank failure policy to appease local interest groups such as bank owners, bank employees, uninsured depositors, and small business borrowers relying on local bank financing. Following a similar argument, Dai and Ngo (2014) find that the election year increase in accounting conservatism does not exist in heavily regulated industries. Motivated by these observations, I hypothesize that the increases in political uncertainty around elections should be less a concern for regulated industries. Following Dai and Ngo (2014), I create an indicator variable, *Regulated*, which is set equal to one for firms operating in Utility (31), Banks

(44), Insurance (45), Real Estate (46) and Trading (47) industries. To test the hypothesis, I split the sample into two subsamples according to the classification of regulated industries, and perform subsample analysis using the baseline regression model. Panel B of Table 10 reports the estimation results from this analysis. Not surprisingly, I find that regulated industries are generally immune to political uncertainty: the election year decline in innovation and the post-election spike in innovation do not exist in regulated industries. These results indicate that while firms operating in regulated industries face possible legislation and policy changes induced by elections, this electoral uncertainty may be largely mitigated through their lobbying activities.

1.3.8. Financial Constraints

Prior literature documents that binding financing constraints discourage innovation. The idea is that firms facing tighter financial constraints spend less for their innovative projects than unconstrained ones. For example, Canepa and Stoneman (2008) find that firms from high tech industries and small firms in the U.K. were more likely to report an innovative project being abandoned or delayed due to financial constraints. Hajivassiliou and Savignac (2007) make a similar observation based on French survey data. Using survey data from 27 non-OECD countries, Gorodnichenko and Schnitzer (2013) further find evidence that financial constraints restrain the ability of domestic firms to innovate and hence to catch up to the technological frontier. Following this stream of research, I hypothesize that there is a more pronounced impact of political uncertainty on innovation productivity for less financially constrained firms, to the extent that these firms are on average more innovative and thus should be more negatively affected.

I employ three proxies for financial constraints: the Kaplan and Zingales (1997) index, firm size, and dividend payer, with the last two proxies popularized by Almeida et al. (2004).

The indicator variable, *KZ dummy*, takes a value of one for firms with below median KZ index of Kaplan and Zingales (1997) in year t. Smaller firms generally face higher hurdles to access capital markets and therefore are on average more financially constrained than larger firms. *Firm size* is an indicator variable set equal to one for firms with above median book value of total assets in year t. Finally, firms that are more financially constrained are expected to have lower payout ratios to preserve cash for future investments. I thus create an indicator variable *Dividend payer* that takes a value of one for firms with dividend payment in year t. Small firms (*Firm size* = 0), firms without dividend payment (*Dividend payer* = 0) and firms with above median KZ index (*KZ dummy* =0) are considered as being more financially constrained.

[Insert Table 11 about here]

To test the hypothesis, I split the sample into two groups according to the proxies for financial constraints, and run the baseline regression model on each group. By comparing the difference in the effects of political uncertainty between the subgroups, I identify how financial constraints amplify the effect of political uncertainty. Table 11 summarize the estimation results from this analysis. Each panel is based on such a proxy for financial constraints, as indicated by the panel header. Across all the three measures of financial constraints, I find that the negative coefficient estimate of the election year indicators are larger and more significant in the less financially constrained subsample than in the more constrained subsample, suggesting that the election year reduction in innovation is concentrated in firms subject to less binding financial constraints. In terms of economic magnitude, column (2) of Panel A reveals that the election year indicator has a coefficient estimate of -0.054, while this figure is only -0.022 for the election year indicator reported in column (1). Again, this represents an economically large difference. In unreported *Wald* test, I show that the differences in coefficient estimates between subsamples are statistically significant at the 1% level for all the three measures of financial constraints.

1.4. Conclusion

Although there is a growing interest in the effect of politics on firm performance and corporate decisions, little attention has been paid to how these political factors affect firm innovation. Politics and political factors paly a key role in determining firm innovation because politicians make public policies and regulatory decisions that frequently alter the economic environments and external boundaries in which firms operate.

In this paper, I investigate the link between political uncertainty and firm innovation, using U.S. gubernatorial elections as a source of plausibly exogenous variation in uncertainty. I find that firm innovation, captured by patent counts and citations, declines 3.8% and 5.5% respectively in the year leading up to an election and quickly reverses afterward. The increase in post-election innovation is roughly equal to the reduction in innovation in election years, representing a temporary reallocation in innovation activity. These results are robust to alternative specifications and various subsamples. Further investigation reveals that Republican regime is associated with less innovation, and that the magnitude of innovation sensitivity to electoral uncertainty is highly dependent on which party is in power and how the power is transferred surrounding elections. Specifically, the reduction in election-year innovation only exists in Republican regime, while there is a weak increase in innovation in election years under Democrat regime; In post-election years, Republican regime experience an increase in innovation, but the increasing pattern is more noticeable for Democrat regime. Finally, I find that the election-year reduction in innovation is concentrated in elections with high levels of uncertainty, in politically sensitive and non-regulated industries, and in firms subject to less binding financing constraints.

Overall, my findings in this paper support an important view that politics and political factors do appear to matter for firms' real innovation decisions. The normal political transition process and the possibility of policy changes around elections influence the way firms make innovation decisions. My findings thus contribute to a growing literature on the role of politics in determining firm performance and corporate policies, by analyzing how political forces shape firms' innovation behavior.

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Table 1
Summary Statistics

Panel A reports the frequency distribution of gubernatorial elections by state. Election data from 1976 to 2006 are collected from CQ Electronic (CQE) Library. I identify *close* elections, elections with *absence* of incumbent or elections with party *change* as high uncertain elections. I further classify elections by party change before and after elections (D→D, D→R, R→R or R→D). Panel B reports summary statistics of selected variables used in subsequent regressions. Panel C presents correlation coefficients of selected variables, where * indicates significance at the 1% level. Appendix A provides detailed definitions of the variables.

Panel A: Summary statistics of gubernatorial elections

State	Abb.	Election	Close	Absence	Change	D→D	D→R	R→R	R→D
Alabama	AL	8	3	2	4	2	1	2	3
Alaska	AK	7	2	3	3	2	2	2	1
Arizona	AZ	8	2	2	3	3	1	2	2
Arkansas	AR	6	0	0	1	3	1	2	0
California	CA	8	3	2	2	2	1	4	1
Colorado	CO	8	1	1	2	5	1	1	1
Connecticut	CT	6	0	0	0	3	0	3	0
Delaware	DE	8	0	0	2	3	1	3	1
Florida	FL	8	1	0	3	3	1	2	2
Georgia	GA	8	1	0	1	6	0	1	1
Hawaii	HI	8	3	0	1	6	0	1	1
Idaho	ID	8	2	4	1	4	0	3	1
Illinois	IL	8	3	3	1	1	1	6	0
Indiana	IN	8	1	0	2	3	1	3	1
Iowa	IA	8	1	3	1	2	1	5	0
Kansas	KS	8	2	1	5	2	3	1	2
Kentucky	KY	7	1	0	1	6	0	0	1
Louisiana	LA	7	2	3	5	1	3	1	2
Maine	ME	4	1	0	1	2	0	1	1
Maryland	MD	8	3	0	2	6	1	0	1
Massachusetts	MA	8	3	5	2	3	1	3	1
Michigan	MI	8	2	1	3	2	2	3	1
Minnesota	MN	6	2	1	3	1	1	2	2
Mississippi	MS	7	2	1	3	3	1	1	2
Missouri	MO	8	3	3	4	2	2	2	2
Montana	MT	8	3	2	2	3	1	3	1
Nebraska	NE	8	2	1	5	1	2	2	3

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State	Abb.	Election	Close	Absence	Change	D→D	D→R	R→R	R→D
Nevada	NV	8	1	0	3	3	1	2	2
New Jersey	NJ	8	3	2	4	2	2	2	2
New Mexico	NM	8	1	0	4	3	2	1	2
New York	NY	8	2	2	2	4	1	2	1
North Carolina	NC	8	0	0	3	4	2	1	1
North Dakota	ND	8	0	2	3	2	1	3	2
Ohio	OH	8	1	0	3	1	2	4	1
Oklahoma	OK	8	3	2	4	3	2	1	2
Oregon	OR	8	2	2	2	5	1	1	1
Pennsylvania	PA	8	2	1	4	2	2	2	2
Rhode Island	RI	4	2	1	1	0	0	3	1
South Carolina	SC	8	2	0	4	1	2	3	2
South Dakota	SD	8	1	2	1	0	0	7	1
Tennessee	TN	8	1	1	4	2	2	2	2
Texas	TX	8	2	2	5	0	2	3	3
Utah	UT	8	1	4	1	2	0	5	1
Virginia	VA	8	1	0	3	3	2	2	1
Washington	WA	8	2	5	3	5	2	0	1
West Virginia	WV	8	1	1	5	3	3	0	2
Wisconsin	WI	8	1	1	4	1	2	3	2
Wyoming	WY	8	2	1	2	5	1	1	1
Total		366	80	67	128	131	61	107	67

Panel B: Summary statistics of key variables								
Variable	N	Mean	STD	Min	P25	P50	P75	Max
Innovation output measures								
# Patent	95134	0.67	1.14	0.00	0.00	0.00	1.10	8.38
# Citation	95134	1.40	2.14	0.00	0.00	0.00	2.91	11.56
Gubernatorial elections								
Election (0)	95134	0.26	0.44	0.00	0.00	0.00	1.00	1.00
Post-election (+1)	95134	0.25	0.43	0.00	0.00	0.00	0.00	1.00
Republican (R)	95134	0.53	0.50	0.00	0.00	1.00	1.00	1.00
Democrat (D)	95134	0.47	0.50	0.00	0.00	0.00	1.00	1.00
D2D dummy	95134	0.08	0.26	0.00	0.00	0.00	0.00	1.00
D2R dummy	95134	0.05	0.22	0.00	0.00	0.00	0.00	1.00
R2R dummy	95134	0.09	0.29	0.00	0.00	0.00	0.00	1.00
R2D dummy	95134	0.04	0.20	0.00	0.00	0.00	0.00	1.00
Close dummy	95134	0.07	0.26	0.00	0.00	0.00	0.00	1.00
Incumbent absence	95134	0.05	0.23	0.00	0.00	0.00	0.00	1.00
Party change	95134	0.10	0.29	0.00	0.00	0.00	0.00	1.00
Control variables								
Asset	90870	4.58	2.41	-0.83	2.91	4.43	6.18	10.45
Age	95134	15.46	12.66	1.00	5.00	11.00	23.00	52.00
ROA	90870	0.02	0.36	-2.17	0.02	0.12	0.18	0.42
Tobin's Q	90870	2.07	2.58	0.22	0.92	1.26	2.10	18.37
Cash	90870	-0.04	0.40	-2.57	0.00	0.08	0.12	0.30
Leverage	90870	0.25	0.26	0.00	0.05	0.20	0.35	1.63
CAPEX	90870	0.06	0.06	0.00	0.02	0.05	0.08	0.32
R&D expense	90870	0.07	0.15	0.00	0.00	0.02	0.08	0.97
Herfindahl Index (HHI)	95131	0.17	0.14	0.02	0.07	0.13	0.22	0.77
GDP growth %	95134	7.19	3.42	-8.60	5.00	6.90	9.10	30.20
PSI	95134	0.04	0.19	0.00	0.00	0.00	0.00	1.00
Regulated	95134	0.07	0.26	0.00	0.00	0.00	0.00	1.00
KZ dummy	90009	0.50	0.50	0.00	0.00	1.00	1.00	1.00
Dividend payer	95134	0.39	0.49	0.00	0.00	0.00	1.00	1.00
Innovation easiness	95134	0.22	0.41	0.00	0.00	0.00	0.00	1.00
High-tech	95134	0.38	0.48	0.00	0.00	0.00	1.00	1.00

Panel C: Correlation coefficients of key variables

Variable	ID	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
# Patent	(1)	1													
# Citation	(2)	0.91*	1												
Election	(3)	-0.02*	-0.02*	1											
Post-election	(4)	0.01*	0.02*	-0.34*	1										
Asset	(5)	0.40*	0.31*	-0.00	-0.01*	1									
Age	(6)	0.28*	0.20*	0.00	-0.01*	0.57*	1								
ROA	(7)	0.09*	0.08*	-0.01*	-0.00	0.39*	0.21*	1							
Tobin's Q	(8)	0.00	0.01*	-0.03*	0.02*	-0.26*	-0.16*	-0.51*	1						
Cash	(9)	0.08*	0.07*	-0.01*	0.00	0.37*	0.20*	0.95*	-0.51*	1					
Leverage	(10)	-0.08*	-0.09*	0.00	0.01	-0.04*	0.02*	-0.22*	0.08*	-0.28*	1				
CAPEX	(11)	0.06*	0.08*	0.01	-0.02*	0.02*	-0.07*	0.04*	0.00	0.03*	0.03*	1			
R&D expense	(12)	0.03*	0.05*	0.01*	-0.00	-0.33*	-0.25*	-0.68*	0.43*	-0.66*	0.02*	0.01*	1		
HHI	(13)	-0.02*	-0.03*	0.00	0.01	-0.01*	0.07*	0.14*	-0.12*	0.12*	0.06*	0.02*	-0.20*	1	
GDP growth %	(14)	-0.04*	-0.02*	-0.11*	-0.07*	-0.09*	-0.09*	0.11*	-0.08*	0.10*	0.02*	0.14*	-0.11*	0.09*	1

Table 2
State Decomposition

This table compares innovation output during election periods and non-election periods by state. Innovation output is measured by patent counts (Panel A) and citation counts (Panel B) respectively. Boldfaced numbers denote significance of mean difference at the 10% level.

Panel A: # Patent									
State	Election	Non-election	Mean diff	Sign	State	Election	Non-election	Mean diff	Sign
AL	0.3327	0.3756	-0.0429	–	MT	0.7729	0.8066	-0.0336	–
AR	0.3660	0.2337	**0.1323**	+	NC	0.4944	0.5155	-0.0211	–
AZ	0.3535	0.4100	-0.0565	–	ND	1.1860	0.9129	0.2731	+
CA	0.6845	0.7481	**-0.0636**	–	NE	0.6264	0.5796	0.0468	+
CO	0.4607	0.4869	-0.0262	–	NJ	0.6433	0.6823	-0.0390	–
CT	0.8382	0.9247	-0.0865	–	NM	0.2639	0.2841	-0.0202	–
DE	1.1370	1.0798	0.0572	+	NV	0.3331	0.3011	0.0320	+
FL	0.3453	0.4103	**-0.0651**	–	NY	0.5583	0.5989	-0.0406	–
GA	0.4780	0.5138	-0.0358	–	OH	0.7209	0.7776	-0.0567	–
HI	0.0866	0.0871	-0.0005	–	OK	0.3662	0.3964	-0.0302	–
IA	0.4263	0.4927	-0.0664	–	OR	0.6180	0.6491	-0.0312	–
ID	0.5109	0.5507	-0.0397	–	PA	0.7460	0.7946	-0.0486	–
IL	0.9499	0.9751	-0.0252	–	RI	0.8806	1.1462	-0.2656	–
IN	0.8614	0.9157	-0.0544	–	SC	0.4430	0.4733	-0.0303	–
KS	0.3537	0.4010	-0.0473	–	SD	0.2556	0.2983	-0.0427	–
KY	0.7485	0.7605	-0.0120	–	TN	0.5076	0.5622	-0.0546	–
LA	0.3608	0.3735	-0.0126	–	TX	0.6091	0.6530	-0.0440	–
MA	0.6640	0.7127	-0.0487	–	UT	0.3867	0.3986	-0.0119	–
MD	0.6450	0.6849	-0.0400	–	VA	0.4999	0.5370	-0.0371	–
ME	0.1419	0.1218	0.0201	+	WA	0.6310	0.6152	0.0159	+
MI	0.8914	0.9700	-0.0786	–	WI	0.7083	0.7650	-0.0567	–
MN	0.5314	0.6192	**-0.0878**	–	WV	0.5111	0.5108	0.0003	+
MO	0.7020	0.6878	0.0142	+	WY	0.7324	0.5346	0.1978	+
MS	0.5148	0.3942	0.1206	+	Total	0.6396	0.6849	**-0.0453**	36

Panel B: # Citation									
State	Election	Non-election	Mean diff	Sign	State	Election	Non-election	Mean diff	Sign
AL	0.7966	0.9285	-0.1319	+	MT	1.7324	1.9058	-0.1734	+
AR	0.8029	0.5528	0.2501	–	NC	0.9976	1.0500	-0.0524	+
AZ	0.8397	0.9423	-0.1026	+	ND	2.7930	2.5180	0.2750	–
CA	1.5404	1.6330	**-0.0926**	+	NE	1.3190	1.1534	0.1656	–
CO	1.0852	1.1826	-0.0974	+	NJ	1.2645	1.3432	-0.0786	+
CT	1.6109	1.7791	**-0.1682**	+	NM	0.6640	0.6093	0.0548	–
DE	1.9755	1.7101	0.2654	–	NV	0.7270	0.7488	-0.0217	+
FL	0.8224	0.9663	**-0.1440**	+	NY	1.1343	1.2102	-0.0759	+
GA	1.0698	1.1201	-0.0503	+	OH	1.4638	1.5925	**-0.1288**	+
HI	0.2853	0.0899	0.1954	–	OK	0.8595	0.8479	0.0116	–
IA	0.9061	0.9852	-0.0791	+	OR	1.3008	1.3718	-0.0710	+
ID	0.7438	0.8983	-0.1545	+	PA	1.4357	1.5291	-0.0935	+
IL	1.8104	1.8622	-0.0519	+	RI	1.7757	1.9557	-0.1799	+
IN	1.7998	1.8828	-0.0830	+	SC	1.1256	1.1291	-0.0035	+
KS	0.8608	0.9723	-0.1115	+	SD	0.6074	0.7977	-0.1902	+
KY	1.3776	1.4116	-0.0340	+	TN	1.0298	1.1517	-0.1220	+
LA	0.6356	0.6967	-0.0610	+	TX	1.2585	1.3118	-0.0533	+
MA	1.4930	1.5765	-0.0835	+	UT	0.9734	1.0255	-0.0521	+
MD	1.2920	1.3927	-0.1006	+	VA	0.9990	1.0998	-0.1008	+
ME	0.4195	0.3009	0.1187	–	WA	1.3268	1.3525	-0.0257	+
MI	1.7259	1.8503	-0.1244	+	WI	1.4528	1.5723	-0.1196	+
MN	1.3393	1.3932	-0.0539	+	WV	1.0789	0.8528	0.2261	–
MO	1.3865	1.3103	0.0762	–	WY	1.6199	1.6729	-0.0530	+
MS	0.9450	0.8027	0.1423	–	Total	1.3429	1.4208	**-0.0780**	36

Table 3
Industry Decomposition

This table compares innovation output during election periods and non-election periods by industry, where Fama French 48 industries is used as the industry classification. Innovation output is measured by patent counts (Panel A) and citation counts (Panel B) respectively. Boldfaced numbers denote significance of mean difference at the 10% level.

Panel A: # Patent

Ind.	Election	Non-election	Mean diff	Sign	Ind.	Election	Non-election	Mean diff	Sign
Agric	0.6113	0.5535	0.0579	–	Ships	0.7764	0.9359	-0.1594	+
Food	0.4945	0.5367	-0.0422	+	Guns	1.2044	1.4359	-0.2315	+
Soda	0.0330	0.0558	-0.0227	+	Gold	0.1173	0.1381	-0.0208	+
Beer	0.9820	1.1539	-0.1720	+	Mines	0.3304	0.4144	-0.0840	+
Smoke	0.8278	0.9292	-0.1014	+	Coal	0.3975	0.2735	0.1240	–
Toys	0.5269	0.5817	-0.0548	+	Oil	0.9689	1.0343	-0.0654	+
Fun	0.3498	0.3327	0.0171	–	Util	0.1388	0.1375	0.0013	–
Books	0.2386	0.2008	0.0378	–	Telcm	0.6003	0.6366	-0.0363	+
Hshld	0.7173	0.7740	-0.0567	+	PerSv	0.2936	0.2620	0.0316	–
Clths	0.2715	0.3029	-0.0313	+	BusSv	0.3803	0.3965	-0.0162	+
Hlth	0.2199	0.2577	-0.0378	+	Comps	0.7411	0.8021	-0.0610	+
MedEq	0.6868	0.7200	-0.0331	+	Chips	0.8171	0.9064	**-0.0893**	+
Drugs	0.8554	0.9107	-0.0553	+	LabEq	0.6633	0.6874	-0.0241	+
Chems	1.2472	1.3534	-0.1062	+	Paper	0.8308	0.8999	-0.0691	+
Rubbr	0.4883	0.4885	-0.0003	+	Boxes	0.7822	0.8912	-0.1090	+
Txtls	0.3296	0.3473	-0.0177	+	Trans	0.1709	0.2364	**-0.0655**	+
BldMt	0.6108	0.6484	-0.0376	+	Whlsl	0.2354	0.2516	-0.0162	+
Cnstr	0.2356	0.2573	-0.0217	+	Rtail	0.2213	0.2182	0.0031	–
Steel	0.5784	0.6320	-0.0536	+	Meals	0.1441	0.0908	0.0533	–
FabPr	0.2613	0.2832	-0.0219	+	Banks	0.1601	0.1863	-0.0262	+
Mach	0.8265	0.8852	**-0.0588**	+	Insur	0.1151	0.1437	-0.0287	+
ElcEq	0.7608	0.8299	-0.0691	+	RlEst	0.0806	0.0716	0.0090	–
Autos	1.0116	1.0868	-0.0752	+	Fin	0.3156	0.3492	-0.0337	+
Aero	1.2539	1.3534	-0.0995	+	Other	0.3286	0.3432	-0.0147	+
Total	0.6408	0.6862	**-0.0455**	39					

Panel B: # Citation									
Ind.	Election	Non-election	Mean diff	Sign	Ind.	Election	Non-election	Mean diff	Sign
Agric	1.1732	1.0271	0.1461	–	Ships	1.3248	1.6699	-0.3451	+
Food	1.0368	1.1274	-0.0906	+	Guns	2.2943	2.6580	-0.3637	+
Soda	0.0871	0.0909	-0.0037	+	Gold	0.2560	0.3636	-0.1076	+
Beer	1.8960	2.1936	-0.2976	+	Mines	0.6826	0.8384	-0.1558	+
Smoke	1.5088	1.7101	-0.2012	+	Coal	0.8814	0.6388	0.2426	–
Toys	1.0908	1.2461	-0.1553	+	Oil	1.6176	1.7212	-0.1036	+
Fun	0.8354	0.8324	0.0031	–	Util	0.3222	0.3250	-0.0028	+
Books	0.6320	0.5026	0.1294	–	Telcm	1.2757	1.3210	-0.0453	+
Hshld	1.4439	1.5760	-0.1321	+	PerSv	0.7231	0.7550	-0.0319	+
Clths	0.6708	0.7355	-0.0647	+	BusSv	0.9377	0.9624	-0.0247	+
Hlth	0.6665	0.7253	-0.0588	+	Comps	1.6669	1.7780	-0.1111	+
MedEq	1.7941	1.8440	-0.0499	+	Chips	1.7742	1.9075	**-0.1333**	+
Drugs	1.5066	1.6154	**-0.1088**	+	LabEq	1.5080	1.5247	-0.0167	+
Chems	2.1801	2.3189	-0.1387	+	Paper	1.7419	1.8624	-0.1206	+
Rubbr	1.1646	1.1388	0.0258	–	Boxes	1.6279	1.8420	-0.2141	+
Txtls	0.7026	0.8117	-0.1091	+	Trans	0.3953	0.5494	**-0.1541**	+
BldMt	1.2825	1.3563	-0.0738	+	Whlsl	0.5624	0.6056	-0.0432	+
Cnstr	0.5456	0.6209	-0.0753	+	Rtail	0.5294	0.5387	-0.0093	+
Steel	1.1399	1.2453	-0.1054	+	Meals	0.4049	0.2378	**0.1670**	–
FabPr	0.6478	0.6216	0.0262	–	Banks	0.3823	0.4596	-0.0773	+
Mach	1.7249	1.7994	-0.0745	+	Insur	0.3010	0.3828	-0.0818	+
ElcEq	1.5222	1.6170	-0.0948	+	RlEst	0.2456	0.2192	0.0264	–
Autos	1.9495	2.0753	-0.1258	+	Fin	0.7819	0.8613	-0.0794	+
Aero	2.1281	2.2973	-0.1692	+	Other	0.6489	0.7060	-0.0571	+
Total	1.3442	1.4226	**-0.0784**	40					

Table 4
Univariate Test: Innovation around Elections

This table summarizes the mean innovation rates measured by # patents (proxy for innovation quantity) and # citations (proxy for innovation quality) around election event years for the full sample (Panel A), the Republican subsample (Panel B) and the Democrat subsample (Panel C) respectively. For each event year in the [-1, 0, +1, +2] election cycle, the table further reports the mean innovation rates in that year (denoted by *Mean*) along with the mean innovation rates in the rest of sample years (denoted by $Mean^R$), where year 0 indicates the gubernatorial election year. The mean difference in innovation rates ($Mean\ diff \doteq Mean - Mean^R$) and the associated t-statistics of the difference are also reported. ***, ** and * denote significance at the 1%, 5% and 10% levels respectively. See Appendix A for variable descriptions as well as the variable sources.

Panel A: Full sample (N = 95134)								
	# Patent				# Citation			
	-1	0	1	2	-1	0	1	2
N	23142	25048	23554	23371	23142	25048	23554	23371
Mean	0.6739	0.6396	0.7009	0.6803	1.4001	1.3429	1.4637	1.3994
$Mean^R$	0.6727	0.6849	0.6638	0.6706	1.4004	1.4208	1.3795	1.4006
Mean diff	0.0012	-0.0453	0.0371	0.0098	-0.0003	-0.0780	0.0843	-0.0013
t-statistics	0.14	-5.41***	4.34***	1.14	-0.02	-4.94***	5.24***	-0.08
Panel B: Republican subsample (N = 50278)								
	# Patent				# Citation			
	-1	0	1	2	-1	0	1	2
N	12072	13906	12113	12178	12072	13906	12113	12178
Mean	0.6979	0.6400	0.7145	0.6854	1.4809	1.3492	1.5244	1.4462
$Mean^R$	0.6779	0.6991	0.6726	0.6819	1.4353	1.4834	1.4215	1.4463
Mean diff	0.0200	-0.0591	0.0418	0.0035	0.0456	-0.1342	0.1029	-0.0002
t-statistics	1.67*	-5.16***	3.49***	0.29	2.00**	-6.15***	4.51***	-0.01
Panel C: Democrat subsample (N = 44856)								
	# Patent				# Citation			
	-1	0	1	2	-1	0	1	2
N	11070	11142	11441	11193	11070	11142	11441	11193
Mean	0.6477	0.6390	0.6864	0.6749	1.3119	1.3350	1.3995	1.3485
$Mean^R$	0.6668	0.6697	0.6537	0.6578	1.3609	1.3534	1.3314	1.3489
Mean diff	-0.0191	-0.0306	0.0327	0.0171	-0.0490	-0.0184	0.0680	-0.0004
t-statistics	-1.55	-2.49**	2.68***	1.39	-2.14**	-0.80	3.01***	-0.02

Table 5
Political Uncertainty and Innovation: Baseline Results

The unit of observation is at firm-year level. The dependent variable in columns (1) to (3) is the natural logarithm of one plus patent counts (# patents), which measures innovation quantity. In columns (4) to (6), the dependent variable is the natural logarithm of one plus citation counts (# citations), which measures innovation quality. Independent variables include firm size, age, ROA, Tobin's Q, cash, leverage, capex, R&D expense, HHI, state level GDP growth rate and the election year (0) and post-election year (+1) dummies, with year 0 being the year the election occurred. See Appendix A for variable descriptions as well as the variable sources. Variables of interests are the two election dummies. I use baseline regression specification and control for firm and year fixed effects. Standard errors are clustered at the firm level and corrected for heteroskedasticity. The last two rows presents the sum of the coefficients on the election and post-election indicators along with the corresponding t-statistics for the null hypothesis that the coefficients of election and post-election indicators sum to zero. T-statistics are reported in square brackets below coefficient estimates. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	# Patent			# Citation		
	(1)	(2)	(3)	(4)	(5)	(6)
Election	-0.050***		-0.037***	-0.080***		-0.054***
	(-14.80)		(-10.26)	(-9.79)		(-6.08)
Post-election		0.050***	0.036***		0.093***	0.073***
		(14.96)	(10.21)		(11.12)	(8.08)
Asset	0.195***	0.195***	0.195***	0.307***	0.308***	0.308***
	(20.53)	(20.58)	(20.55)	(18.83)	(18.89)	(18.87)
Age	-0.006***	-0.005***	-0.006***	-0.023***	-0.022***	-0.022***
	(-5.51)	(-5.14)	(-5.31)	(-11.49)	(-11.15)	(-11.28)
ROA	-0.042*	-0.039*	-0.041*	-0.089	-0.085	-0.086
	(-1.79)	(-1.69)	(-1.73)	(-1.61)	(-1.53)	(-1.55)
Tobin's Q	0.005***	0.005***	0.005***	0.015***	0.015***	0.014***
	(3.22)	(3.21)	(3.04)	(3.82)	(3.78)	(3.68)
Cash	-0.022	-0.025	-0.024	0.046	0.041	0.042
	(-1.22)	(-1.37)	(-1.32)	(1.07)	(0.95)	(0.98)
Leverage	-0.052***	-0.054***	-0.053***	-0.148***	-0.152***	-0.150***
	(-3.05)	(-3.17)	(-3.11)	(-3.86)	(-3.95)	(-3.90)
CAPEX	0.352***	0.356***	0.358***	1.416***	1.425***	1.429***
	(5.85)	(5.90)	(5.95)	(10.46)	(10.51)	(10.54)
R&D expense	0.534***	0.530***	0.533***	1.216***	1.208***	1.213***
	(12.77)	(12.67)	(12.75)	(12.40)	(12.33)	(12.37)
HHI	-0.159**	-0.159**	-0.159**	-0.529***	-0.528***	-0.528***
	(-2.04)	(-2.03)	(-2.03)	(-3.59)	(-3.58)	(-3.58)
GDP growth	-0.000	0.002*	0.001	-0.002	0.001	-0.000
	(-0.28)	(1.78)	(0.65)	(-0.92)	(0.71)	(-0.05)
Constant	-0.129***	-0.176***	-0.151***	0.328***	0.247***	0.282***
	(-3.22)	(-4.34)	(-3.74)	(4.62)	(3.47)	(3.94)
N	90870	90870	90870	90870	90870	90870
R^2	0.058	0.057	0.058	0.027	0.027	0.027
Firm/Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Tests for linear combinations of coefficients						
Election + Post-election			-0.002			0.019
t-statistics			-0.07			1.31

Table 6

Political Uncertainty and Innovation: Robustness Tests

This table presents robustness tests for the baseline results shown in Table 5. The unit of observation is at firm-year level. The dependent variable in columns (1) to (3) is the natural logarithm of one plus patent counts (# patents), which measures innovation quantity. In columns (4) to (6), the dependent variable is the natural logarithm of one plus citation counts (# citations), which measures innovation quality. Key variables are the election year (0) and post-election year (+1) dummies, with year 0 being the year the election occurred. In Panel A, I use presidential election cycle, instead of gubernatorial election cycle, to proxy for political uncertainty. In Panel B, I present regression results from a placebo test, where election events are randomly generated every four years for each state. Panel C excludes firms headquartered in California, Florida and Minnesota and Panel D excludes firms operating in machinery, electronic equipment and transportation industries, where industry classification is based on Fama French 48 industries. In Panel E, I further remove observations in the dot-com bubble period (i.e., 1999 and 2000). I use baseline regression specification and control for firm and year fixed effects. Standard errors are clustered at the firm level and corrected for heteroskedasticity. To save space, I suppress the estimates of firm and state economy control variables. T-statistics are reported in square brackets below coefficient estimates. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: President election cycles

	# Patent			# Citation		
	(1)	(2)	(3)	(4)	(5)	(6)
President election	-0.047***		-0.042***	-0.119***		-0.106***
	(-13.40)		(-11.40)	(-12.92)		(-10.91)
Post-president election		0.029***	0.013***		0.078***	0.040***
		(8.52)	(3.76)		(8.40)	(4.07)
N	90870	90870	90870	90870	90870	90870
R^2	0.073	0.073	0.074	0.032	0.031	0.032
Constant	Yes	Yes	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Placebo test

	# Patent			# Citation		
	(1)	(2)	(3)	(4)	(5)	(6)
Election^{P}	-0.001		0.007	-0.009		-0.003
	(-0.22)		(1.28)	(-1.03)		(-0.34)
Post-election^{P}		0.009	0.008		0.016	0.012
		(1.31)	(1.09)		(1.56)	(1.19)
N	90870	90870	90870	90870	90870	90870
R^2	0.043	0.044	0.044	0.038	0.039	0.039
Constant	Yes	Yes	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Drop California, Florida and Minnesota						
	# Patent			# Citation		
	(1)	(2)	(3)	(4)	(5)	(6)
Election	-0.045***		-0.036***	-0.078***		-0.062***
	(-12.00)		(-9.29)	(-8.93)		(-6.63)
Post-election		0.037***	0.024***		0.071***	0.049***
		(10.15)	(6.28)		(7.66)	(4.97)
N	68418	68418	68418	68418	68418	68418
R^2	0.047	0.047	0.047	0.023	0.023	0.024
Constant	Yes	Yes	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel D: Drop machinery, electronic equipment and transportation industries						
	# Patent			# Citation		
	(1)	(2)	(3)	(4)	(5)	(6)
Election	-0.044***		-0.032***	-0.073***		-0.049***
	(-12.64)		(-8.82)	(-8.55)		(-5.44)
Post-election		0.046***	0.035***		0.087***	0.070***
		(13.06)	(9.37)		(9.88)	(7.46)
N	75634	75634	75634	75634	75634	75634
R^2	0.047	0.047	0.047	0.023	0.023	0.023
Constant	Yes	Yes	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel E: Remove dot-com bubble period (1999 and 2000)						
	# Patent			# Citation		
	(1)	(2)	(3)	(4)	(5)	(6)
Election	-0.038***		-0.029***	-0.046***		-0.035***
	(-11.46)		(-8.30)	(-5.60)		(-4.04)
Post-election		0.038***	0.028***		0.045***	0.033***
		(11.46)	(7.91)		(5.37)	(3.62)
N	84450	84450	84450	84450	84450	84450
R^2	0.054	0.054	0.054	0.025	0.025	0.025
Constant	Yes	Yes	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 7
Subsample Analysis: Degree of Electoral Uncertainty and Innovation

This table examines whether the degree of electoral uncertainty amplifies the dampening effect of political uncertainty on innovation productivity. I split the full sample into two subsamples according to the three proxies for the degree of ex ante electoral uncertainty and perform subsample analysis. *Close* election is an indicator variable set equal to one if the victory margin, defined as the vote difference between the first place candidate and the second place candidate, is less than 5%. *Absence* of incumbent is an indicator variable set equal to one if the incumbent governor does not seek re-election due to reasons other than term-limit expiration (such as retired or defeated in primary). *Party change* is an indicator variable set equal to one if the incumbent governor and the successor have different party affiliations. I identify close elections, elections with absence of incumbent and elections with party change as high uncertainty elections. Each panel is based on such a proxy for electoral uncertainty, as indicated by the panel header. The unit of observation is at firm-year level. The dependent variable in columns (1) and (2) is the natural logarithm of one plus patent counts (# patents), which measures innovation quantity. In columns (3) and (4), the dependent variable is the natural logarithm of one plus citation counts (# citations), which measures innovation quality. Key variables are the election year (0) and post-election year (+1) dummies, with year 0 being the year the election occurred. I use baseline regression specification and control for firm and year fixed effects. Standard errors are clustered at the firm level and corrected for heteroskedasticity. To save space, I suppress the estimates of firm and state economy control variables. T-statistics are reported in square brackets below coefficient estimates. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Election closeness				
	# Patent		# Citation	
	Close dummy = 0	Close dummy = 1	Close dummy = 0	Close dummy = 1
	(1)	(2)	(3)	(4)
Election	-0.037***	-0.077***	-0.053***	-0.110***
	(-5.83)	(-15.62)	(-3.58)	(-10.02)
Post-election	0.038***	0.035***	0.075***	0.073***
	(10.91)	(10.30)	(8.50)	(8.30)
N	84451	73261	84451	73261
R^2	0.068	0.059	0.029	0.028
Constant	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes

Panel B: Absence of incumbent (other than term limit)				
	# Patent		# Citation	
	Absence = 0	Absence = 1	Absence = 0	Absence = 1
	(1)	(2)	(3)	(4)
Election	-0.027***	-0.074***	-0.027***	-0.150***
	(-6.81)	(-8.16)	(-2.82)	(-7.43)
Post-election	0.036***	0.037***	0.073***	0.075***
	(-10.45)	(-10.77)	(-8.30)	(-8.50)
N	85951	71761	85951	71761
R^2	0.058	0.068	0.026	0.031
Constant	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes

Panel C: Change party				
	# Patent		# Citation	
	Party change = 0	Party change = 1	Party change = 0	Party change = 1
	(1)	(2)	(3)	(4)
Election	-0.040***	-0.046***	-0.058***	-0.065***
	(-6.20)	(-9.55)	(-3.86)	(-5.98)
Post-election	0.038***	0.036***	0.076***	0.072***
	(10.84)	(10.39)	(8.61)	(8.20)
N	82137	75575	82137	75575
R^2	0.066	0.059	0.029	0.028
Constant	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes

Table 8
Political Uncertainty and Innovation: Political Regime

This table examines whether incumbent party affiliation (i.e., Republican vs. Democrat) affects the pattern of innovation productivity around elections. The unit of observation is at firm-year level. The dependent variable in columns (1) to (4) is the natural logarithm of one plus patent counts (# patents), which measures innovation quantity. In columns (5) to (8), the dependent variable is the natural logarithm of one plus citation counts (# citations), which measures innovation quality. *Regime* is an indicator variable set equal to one if the governor is a Republican in year t. In columns (1) and (5), I first include the regime dummy in the baseline regression. To investigate the cross-sectional heterogeneity in party affiliation, in columns (2) and (6), I further add to the baseline regression interaction terms between regime dummy and election dummy, and between regime dummy and post-election dummy. Finally, I split the full sample based on the affiliation of the governor and perform subsample analysis, as indicated by the column header. Variables of interests are the two interaction terms, election $\times$ regime and post-election $\times$ regime, along with the election year (0) and post-election year (+1) dummies, with year 0 being the year the election occurred. I use baseline regression specification and control for firm and year fixed effects. Standard errors are clustered at the firm level and corrected for heteroskedasticity. To save space, I suppress the estimates of firm and state economy control variables. T-statistics are reported in square brackets below coefficient estimates. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	# Patent				# Citation			
	Baseline	Interacted	Republican	Democrat	Baseline	Interacted	Republican	Democrat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Election	-0.036***	0.012*	-0.055***	-0.000	-0.053***	0.019	-0.123***	0.013
	(-10.31)	(1.91)	(-10.08)	(-0.04)	(-6.23)	(1.39)	(-9.53)	(0.93)
Post-election	0.036***	0.048***	0.043***	0.037***	0.073***	0.061***	0.091***	0.082***
	(10.40)	(7.70)	(9.01)	(7.07)	(8.29)	(4.37)	(7.33)	(6.27)
Republican	-0.047***				-0.010			
	(-5.02)				(-0.54)			
Election		-0.088***				-0.131***		
× Regime		(-9.15)				(-6.52)		
Post-election		-0.024**				0.023		
× Regime		(-2.39)				(1.08)		
N	90870	90870	47958	42912	90870	90870	47958	42912
R^2	0.059	0.059	0.063	0.043	0.027	0.027	0.032	0.022
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Tests for linear combinations of coefficients								
Election + Post-election		-.052				-.027		
t-statistics		-5.54				-1.28		

Table 9
Political Uncertainty and Innovation: Regime Transition

This table further evaluates the cross sectional variations of regime transition (i.e., D→D, D→R, R→R, R→D) on innovation productivity around elections. The unit of observation is at firm-year level. The dependent variable in columns (1) to (4) is the natural logarithm of one plus patent counts (# patents), which measures innovation quantity. In columns (5) to (8), the dependent variable is the natural logarithm of one plus citation counts (# citations), which measures innovation quality. *D2D dummy* is an indicator variable set equal to one if both the incumbent governor and the successor are Democrats. *D2R dummy* is an indicator variable set equal to one if the incumbent governor is a Republican and the successor is a Democrat. *R2R dummy* is an indicator variable set equal to one if both the incumbent governor and the successor are Republicans. *R2D dummy* is an indicator variable set equal to one if the incumbent governor is a Democrat and the successor is a Republican. To investigate the cross-sectional heterogeneity in regime transition, for each of the four regime transition indicators, I add to the baseline regression interaction terms between the indicator and election dummy, and between the indicator and post-election dummy as indicated by the column header. Variables of interests are the two interaction terms, election × regime transition indicator and post-election × regime transition indicator, along with the election year (0) and post-election year (+1) dummies, with year 0 being the year the election occurred. I use baseline regression specification and control for firm and year fixed effects. Standard errors are clustered at the firm level and corrected for heteroskedasticity. To save space, I suppress the estimates of firm and state economy control variables. T-statistics are reported in square brackets below coefficient estimates. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	# Patent				# Citation			
	D→D	D→R	R→R	R→D	D→D	D→R	R→R	R→D
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Election	-0.055***	-0.031***	-0.010*	-0.047***	-0.068***	-0.032***	-0.025**	-0.077***
	(-11.46)	(-7.52)	(-1.87)	(-12.07)	(-6.32)	(-3.30)	(-2.11)	(-8.22)
Post-election	0.029***	0.035***	0.042***	0.039***	0.073***	0.082***	0.069***	0.070***
	(6.56)	(8.48)	(8.48)	(9.94)	(6.53)	(8.16)	(5.85)	(7.15)
Election	0.060***				0.051**			
× D2D dummy	(5.40)				(2.22)			
Post-election	0.022**				0.001			
× D2D dummy	(2.12)				(0.06)			
Election		-0.030**				-0.106***		
× D2R dummy		(-2.44)				(-4.13)		
Post-election		0.018				-0.047*		
× D2R dummy		(1.57)				(-1.85)		
Election			-0.079***				-0.082***	
× R2R dummy			(-6.76)				(-3.51)	
Post-election			-0.019*				0.014	
× R2R dummy			(-1.78)				(0.59)	
Election				0.066***				0.153***
× R2D dummy				(6.14)				(6.15)
Post-election				-0.021*				0.018
× R2D dummy				(-1.92)				(0.72)
N	90870	90870	90870	90870	90870	90870	90870	90870
R^2	0.058	0.058	0.059	0.058	0.027	0.027	0.027	0.027
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 10
Subsample Analysis: Industry Characteristics

This table examines the cross-sectional variations of industry characteristics on innovation productivity around elections. I split the full sample into two subsamples according to politically sensitive industries and heavily regulated industries and perform subsample analysis. *Politically sensitive industries (PSI)* is an indicator variable set equal to one if firms fall into the following industries: Beer (4), Smoke (5), Guns (26), Gold (27), Mines (28), Coal (29), and Oil (30) as defined in Hong and Kostovetsky (2011). *Regulated industries* is an indicator variable set equal to one for firms that belong to finance and utility industries: Utility (31), Banks (44), Insurance (45), Real Estate (46) and Trading (47), as defined in Dai and Ngo (2014). Fama French 48 industries is used as the industry classification. Each Panel is based on such a subsample, as indicated by the panel header. The unit of observation is at firm-year level. The dependent variable in columns (1) and (2) is the natural logarithm of one plus patent counts (# patents), which measures innovation quantity. In columns (3) and (4), the dependent variable is the natural logarithm of one plus citation counts (# citations), which measures innovation quality. Variables of interests are the election year (0) and post-election year (+1) dummies, with year 0 being the year the election occurred. I use baseline regression specification and control for firm and year fixed effects. Standard errors are clustered at the firm level and corrected for heteroskedasticity. To save space, I suppress the estimates of firm and state economy control variables. T-statistics are reported in square brackets below coefficient estimates. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Politically sensitive industries (PSI)				
	# Patent		# Citation	
	PSI = 0	PSI = 1	PSI = 0	PSI = 1
	(1)	(2)	(3)	(4)
Election	-0.036***	-0.088***	-0.050***	-0.148***
	(-10.13)	(-4.95)	(-5.71)	(-4.57)
Post-election	0.036***	0.015	0.074***	0.036
	(10.41)	(0.88)	(8.24)	(0.97)
N	87416	3454	87416	3454
R^2	0.059	0.057	0.027	0.041
Constant	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes

Panel B: Heavily regulated industries				
	# Patent		# Citation	
	Regulated = 0	Regulated = 1	Regulated = 0	Regulated = 1
	(1)	(2)	(3)	(4)
Election	-0.040***	-0.008	-0.056***	-0.018
	(-10.95)	(-0.92)	(-6.33)	(-0.74)
Post-election	0.038***	0.011	0.077***	0.033
	(10.44)	(1.26)	(8.22)	(1.49)
N	84321	6549	84321	6549
R^2	0.062	0.007	0.029	0.009
Constant	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes

Table 11
Subsample Analysis: Financial Constraints

This table examine whether financial constraints amplify or alleviate the effect of political uncertainty on the innovation productivity around elections. I split the full sample into two subsamples according to the proxies for financial constraints and perform subsample analysis. The proxies for financial constraints include: the Kaplan and Zingales (1997) index, firm size, and dividend payer. *KZ dummy* is an indicator variable set equal to one for firms with below median KZ index of Kaplan and Zingales (1997) in year t. *Firm size* is an indicator variable set equal to one for firms with above median book value of total assets in year t. *Dividend payer* is an indicator variable set equal to one for firms with dividend payment in year t. Small firms (Firm size = 0), firms without dividend payment (Dividend payer = 0) and firms with above median KZ index (KZ dummy = 0) are considered as being more financially constrained. Each panel is based on such a proxy for financial constraints, as indicated by the panel header. The unit of observation is at firm-year level. The dependent variable in columns (1) and (2) is the natural logarithm of one plus patent counts (# patents), which measures innovation quantity. In columns (3) and (4), the dependent variable is the natural logarithm of one plus citation counts (# citations), which measures innovation quality. Variables of interests are the election year (0) and post-election year (+1) dummies, with year 0 being the year the election occurred. I use baseline regression specification and control for firm and year fixed effects. Standard errors are clustered at the firm level and corrected for heteroskedasticity. To save space, I suppress the estimates of firm and state economy control variables. T-statistics are reported in square brackets below coefficient estimates. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Kaplan and Zingales (1997) Index				
	# Patent		# Citation	
	KZ dummy = 0	KZ dummy = 1	KZ dummy = 0	KZ dummy = 1
	(1)	(2)	(3)	(4)
Election	-0.022***	-0.054***	-0.029**	-0.080***
	(-4.18)	(-9.39)	(-2.27)	(-5.96)
Post-election	0.034***	0.038***	0.072***	0.074***
	(6.49)	(6.88)	(5.53)	(5.32)
N	45083	44926	45083	44926
R^2	0.047	0.066	0.028	0.029
Constant	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes

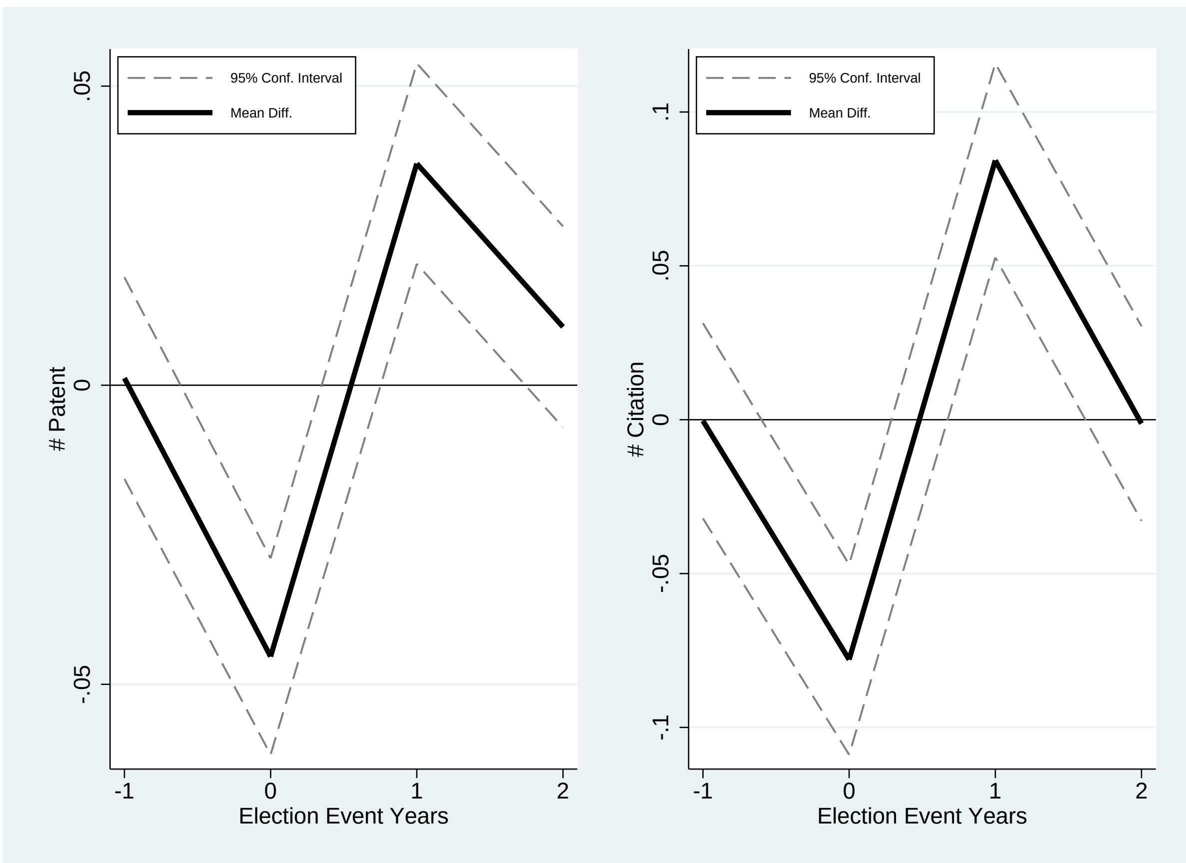
Panel B: Firm size				
	# Patent		# Citation	
	Firm size = 0	Firm size = 1	Firm size = 0	Firm size = 1
	(1)	(2)	(3)	(4)
Election	-0.009**	-0.065***	-0.019	-0.085***
	(-2.08)	(-11.95)	(-1.49)	(-7.31)
Post-election	0.027***	0.040***	0.052***	0.081***
	(5.85)	(7.55)	(3.97)	(6.55)
N	45434	45436	45434	45436
R^2	0.024	0.061	0.018	0.027
Constant	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes

Dividend Payer				
	# Patent		# Citation	
	Dividend = 0	Dividend = 1	Dividend = 0	Dividend = 1
	(1)	(2)	(3)	(4)
Election	-0.017***	-0.059***	-0.008	-0.094***
	(-3.81)	(-10.49)	(-0.64)	(-7.46)
Post-election	0.046***	0.022***	0.090***	0.053***
	(9.87)	(4.17)	(7.38)	(4.13)
N	54150	36720	54150	36720
R^2	0.061	0.063	0.032	0.039
Constant	Yes	Yes	Yes	Yes
Firm/Economy Control	Yes	Yes	Yes	Yes
Firm/Year FE	Yes	Yes	Yes	Yes

Figure 1. Innovation around Elections: Univariate Evidence

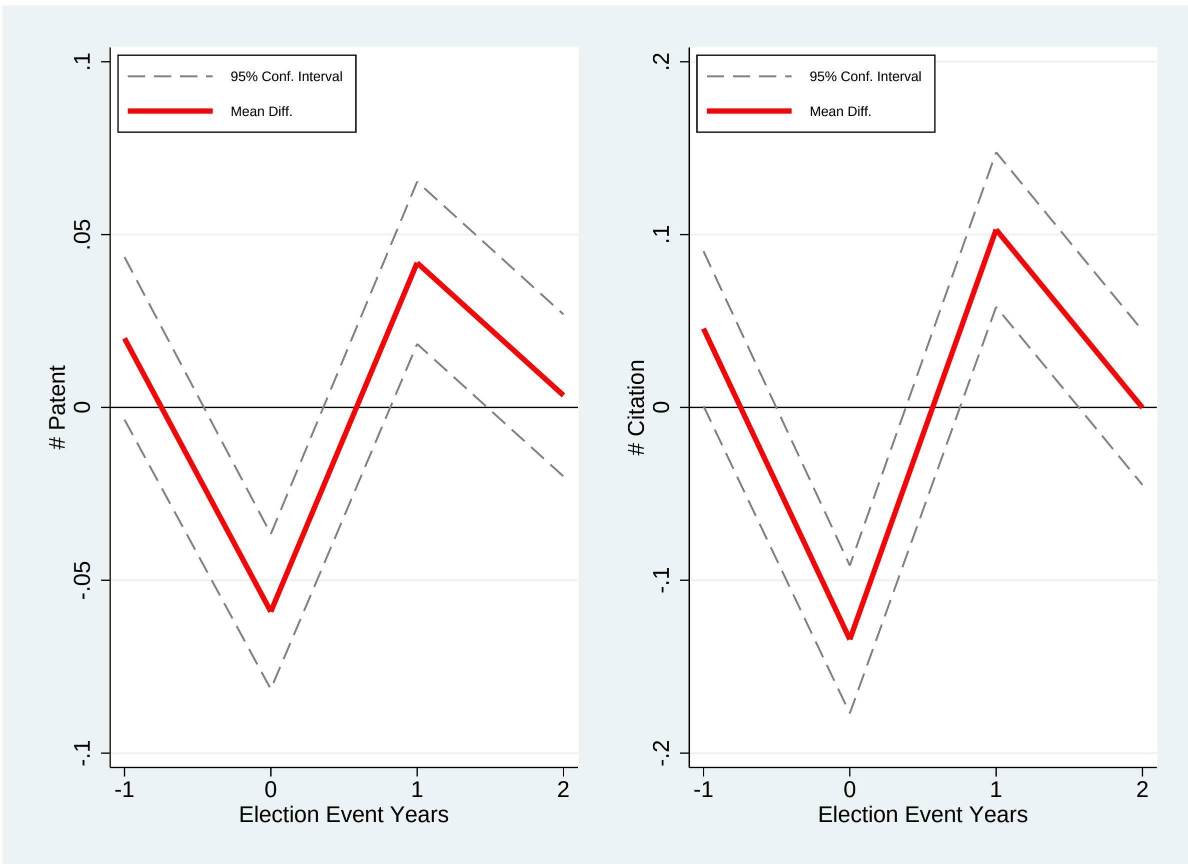
Panel A: Full Sample

This figure depicts the pooled mean difference (dark solid line) and 95% confidential intervals (gray dashed line) in innovation productivity for a given year in the [-1, 0, +1, +2] election time event period and the rest of sample years. Innovation productivity is measured as # patents (proxy for innovation quantity) in the left panel and as # citations (proxy for innovation quality) in the right panel respectively. Year 0 indicates the actual calendar year when a gubernatorial election event occurs. I use *full sample* with sample year t from 1976 to 2006. See Appendix A for descriptions of the two innovation productivity measures as well as the variable sources.



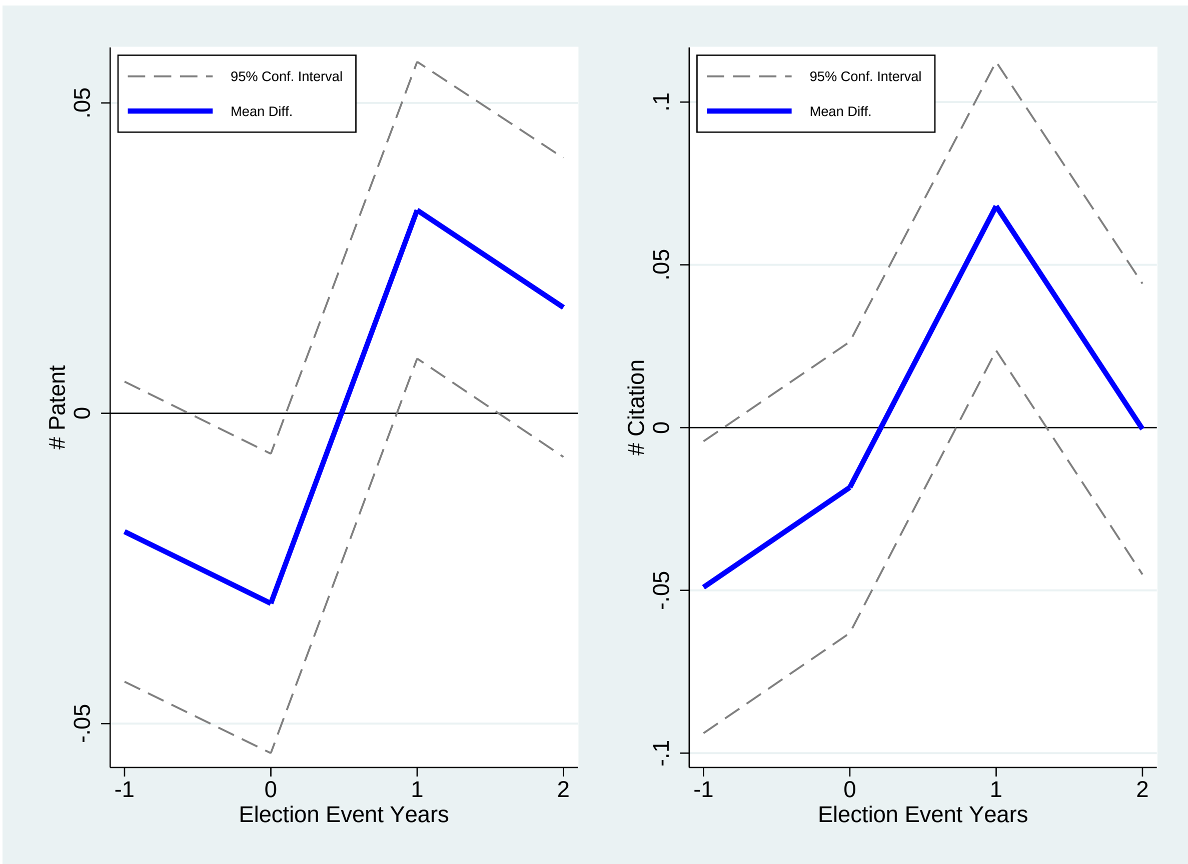
Panel B: Republican

This figure depicts the pooled mean difference (red solid line) and 95% confidential intervals (gray dashed line) in innovation productivity for a given year in the [-1, 0, +1, +2] election time event period and the rest of sample years. Innovation productivity is measured as # patents (proxy for innovation quantity) in the left panel and as # citations (proxy for innovation quality) in the right panel respectively. Year 0 indicates the actual calendar year when a gubernatorial election event occurs. I use *Republican subsample* with sample year t from 1976 to 2006. See Appendix A for descriptions of the two innovation productivity measures as well as the variable sources.



Panel C: Democrat

This figure depicts the pooled mean difference (blue solid line) and 95% confidential intervals (gray dashed line) in innovation productivity for a given year in the [-1, 0, +1, +2] election time event period and the rest of sample years. Innovation productivity is measured as # patents (proxy for innovation quantity) in the left panel and as # citations (proxy for innovation quality) in the right panel respectively. Year 0 indicates the actual calendar year when a gubernatorial election event occurs. I use *Democrat subsample* with sample year t from 1976 to 2006. See Appendix A for descriptions of the two innovation productivity measures as well as the variable sources.



Panel D: Republican vs. Democrat

This figure depicts the pooled mean difference (green solid line) and 95% confidential intervals (gray dashed line) in innovation productivity between *Republican regime vs. Democrat regime* for a given year in the [-1, 0, +1, +2] election time event period and the rest of sample years. Innovation productivity is measured as # patents (proxy for innovation quantity) in the left panel and as # citations (proxy for innovation quality) in the right panel respectively. Year 0 indicates the actual calendar year when a gubernatorial election event occurs. I use *full sample* with sample year *t* from 1976 to 2006. See Appendix A for descriptions of the two innovation productivity measures as well as the variable sources.

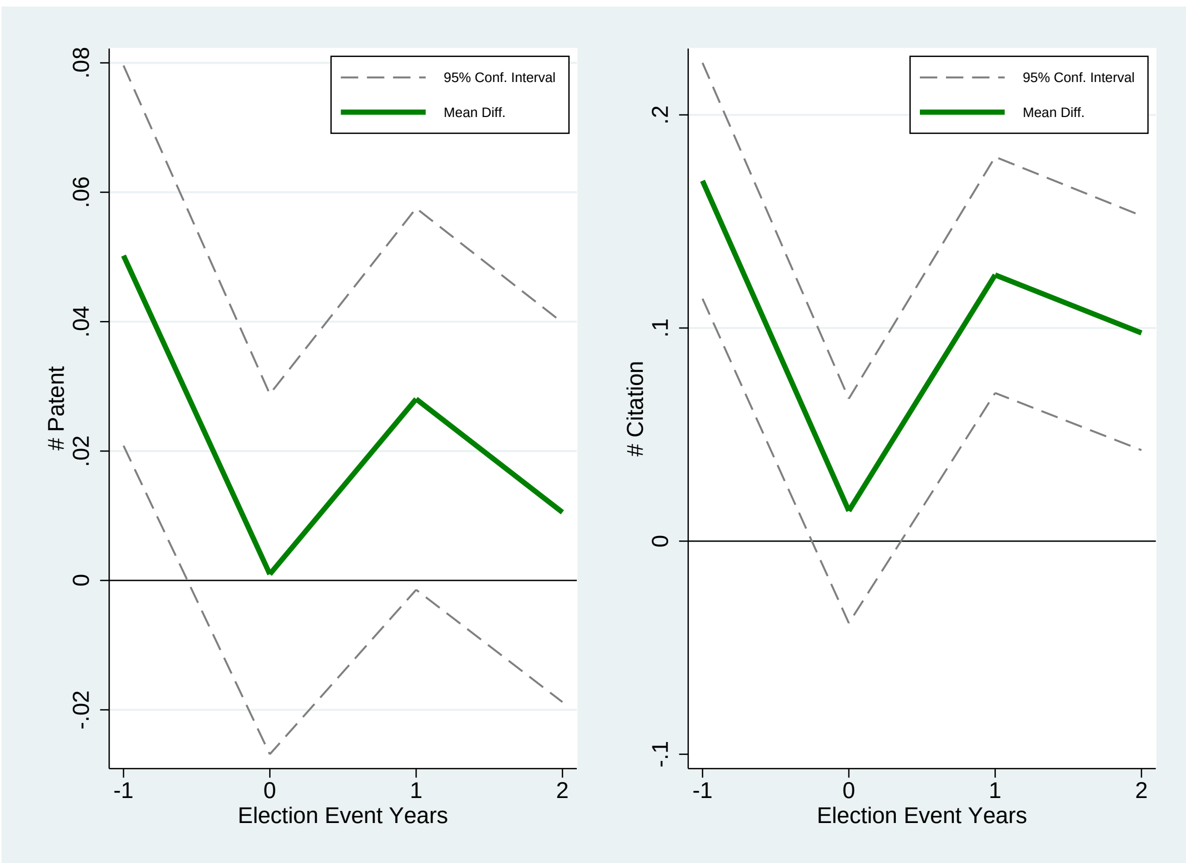
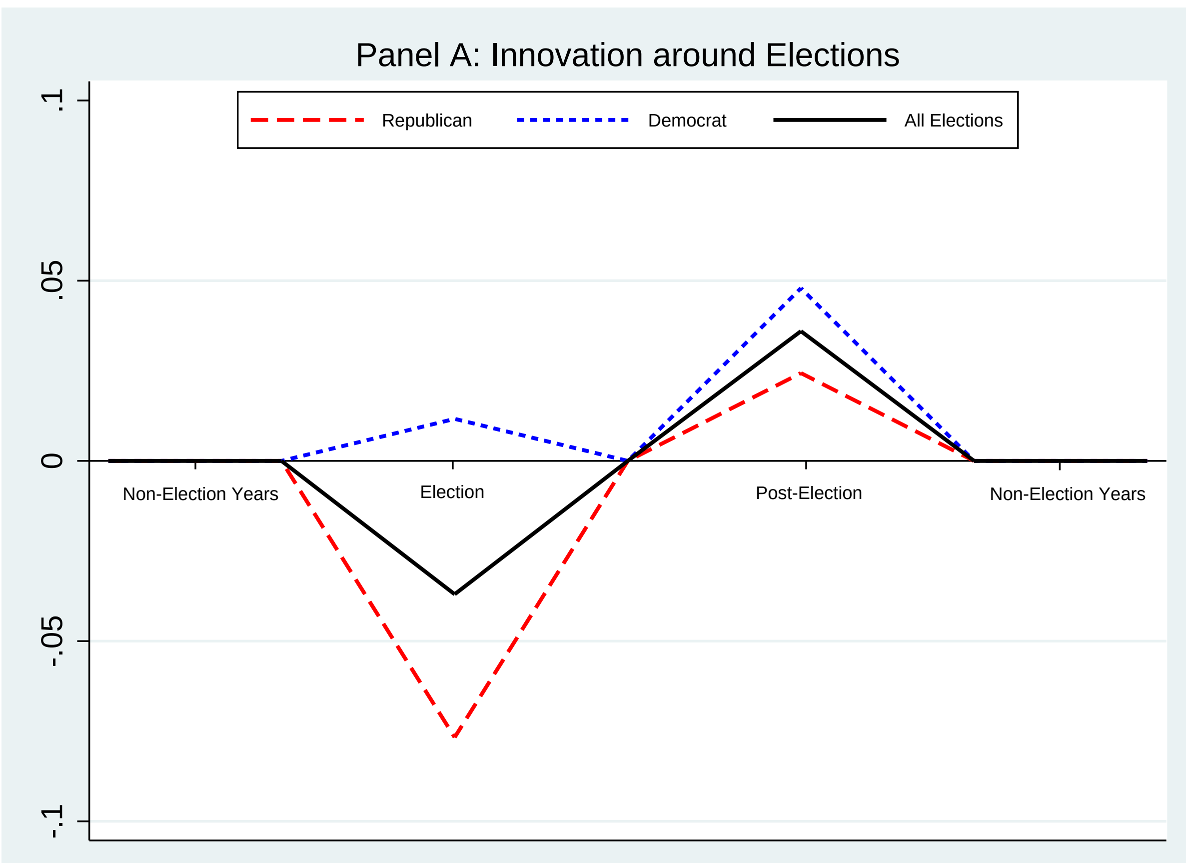


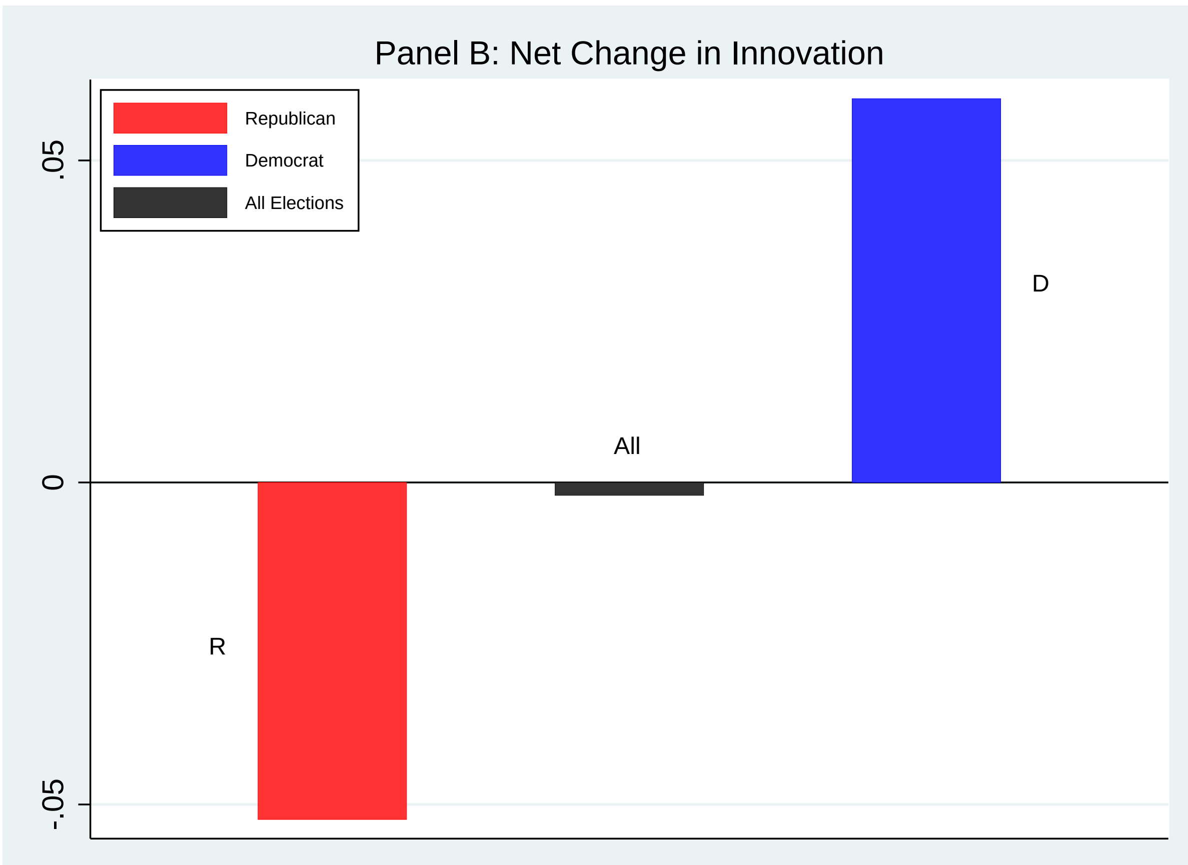
Figure 2. Innovation around Elections: Multivariate Evidence

This figure displays estimates from the baseline regression results in column (3) of Table 5 along with the results reported in column (2) of Table 8 of the following specification:

$$\begin{aligned}\text{Innovation}_{ijt} &= \alpha_i + \gamma_t + \beta_0 \times \text{Election}_{j,t} + \beta_1 \times \text{Election}_{j,t} \times \text{Regime}_{j,t} + \beta_2 \times \text{Post-election}_{j,t+1} \\ &\quad + \beta_3 \times \text{Post-election}_{j,t+1} \times \text{Regime}_{j,t} + \beta_4 \times \text{Regime}_{j,t} + \sum \varphi_\mathbf{i} \mathbf{X_i} + \sum \delta_\mathbf{j} \mathbf{S_{jt}} + \varepsilon_{ijt}\end{aligned}$$

where $\text{Regime}_{j,t}$ is an indicator variable which takes on a value of one if the incumbent governor is Republican in state j in year t. Panel A of Figure 2 illustrates the magnitude of innovation sensitivity to electoral uncertainty around the election cycle for Republican regime, Democrat regime and all elections respectively. The red long-dashed (blue short-dashed) line shows the changes in innovation under the Republican (Democrat) regime, while the dark solid line illustrates the changes in innovation for all elections. Panel B of Figure 2 further reports the *net change* in innovation around the election cycle (election year [0] and post-election year [+1]) for Republican regime (red bar), Democrat regime (blue bar) and all elections (dark bar) respectively.




Panel B: Net Change in Innovation
Republican
Democrat
All Elections
.05
0
-.05
R
All
D

Chapter 2

Political Turnover, Ownership, and Corporate Investment

2.1. Introduction

We exploit a unique feature of political transition in China to examine how the personal incentives of politicians influence real investment. Certain types of politicians are promoted within the Communist party based on the economic performance in the region in which they govern. Since politicians in China exert a great deal of power over state-owned enterprises (SOEs), it is possible that the real investment of SOEs will vary with the political turnover cycle across the provinces in China. The incentives to report strong growth over the political tenure of office combined with time-to-build considerations suggest that investment by SOEs should be highest when a new politician is appointed as a provincial governor. We examine whether these dynamics of investment for SOEs are present in China around political turnovers. We also examine the effects of political influence over SOE investment on the investment choices of private listed firms (non-SOEs) in the Chinese economy. We find that political incentives do influence investment behavior and that these effects appear to represent a misallocation of capital over time as the investment of privately owned firms appears to be crowded out by the politically controlled investment of SOEs.

China, as the world's largest emerging economy, is unique both politically and economically in several ways. First, the connection between economic activities and political influence/interference in China is extremely close. Political agendas often lead economic activities. Government leaders at both the central and provincial levels have enormous power in the economy to promote growth through investment projects. Second, political leaders are appointed rather than elected. National top leaders are changed every ten years[21] and provincial leaders are typically replaced every five years. Finally, investment in China is very high relative to other countries in the world. In 2013, the investment-to-GDP ratio in China was 47.1%. China's investment rate compares to 16.8% in the US and 27.4% and 21.2% in South Korea and Japan, respectively. The vast amount of resources devoted to investment along with the influence of political leaders make China an interesting and important setting to study how politics influence investment and whether the quality of investment is affected by political involvement.

There are two ways that the system of provincial governor turnover may affect corporate investment. First, China is unique in the way politicians move from one post to another. In contrast to the use of competitive elections to select leaders, the promotion of politicians in China follows more of a tournament system where politicians are rewarded for stimulating economic growth in the region in which they govern. The appointment and evaluation of provincial leaders is done through a process in which the central government has absolute power and discretion towards personnel choices. Similarly at the firm level, CEOs of central and local SOEs are appointed by the central and local government, respectively. The government[22]

[21] Since Mao's death, central level leadership transition becomes a regular phenomenon.

[22] In 2006, SOEs accounted for more than 30% of the China's GDP and approximately 90% of all publicly listed firms. SOEs play a central role in pivotal industries such as energy, steel, machinery and national defence

in most cases remains, directly or indirectly, the largest and controlling shareholder in SOEs. In this sense, corporate decisions of SOEs are often sensitive to political influences. For example, political leaders can influence SOEs directly through arranging preferential treatment in bank credit, government subsidies, and market entry compared to private enterprises (Faccio, Masulis and McConnell (2006), Claessens, Feijen and Laeven (2008), Li, Meng, Wang and Zhou (2008)). Political leaders can also cast their influence on SOEs through indirect channels such as affecting personnel decisions. Various levels of governments in China thus often seek to affect/direct investment in order to achieve policy goals, especially in SOEs. Second, firms in China face similar issues related to uncertainty about government policy post-turnover as firms in other countries face around elections. Political uncertainty has been shown to have real economic impact on corporate investment in other countries. Julio and Yook (2012) examine the firm-level corporate investment corresponding with the event of national elections across 48 countries, and they find that corporate investment temporarily decreases prior to the election outcome as firms become more cautious anticipating the election outcome. While our focus is on post-turnover investment dynamics, we also examine firm-level investment in China just prior to the timing of top leader transition to check for uncertainty effects.

Top provincial leaders include both governors and party secretaries. Since the dual leadership system is unique in China, it is important distinguish their functional roles. Governors are responsible for and put more effort on presiding over resource allocation and promoting provincial economic development, while party secretaries represent the communist party's interests and ensure the implementation of party policies from higher levels. Given the different types of power exerted by these two types of provincial leaders, we expect that turnovers of governors will be more relevant for corporate investment than those of party secretaries. As such, we focus on the turnover of provincial governors rather than that of party secretaries in our empirical analysis.

(Li & Putterman, 2008). The public sector is often dominated by large SOEs, that provide key inputs to facilitate private sector growth and investment, and is regarded as a foundation of national growth.

We hypothesize that the incentives of provincial political turnovers can have a significant impact on local firm-level investment. When the government has great influence on corporate decisions, firm level investment may vary around the timing of political leader changes. Corporate investment policy change may be due to political uncertainty ex ante and political influence ex post. Fan, Wong and Zhang (2007) show that newly privatized firms with political connected CEOs often have poor governance and performance. Since SOEs make decisions not only to maximize shareholder value but also to serve political interests, we furthermore hypothesize that SOEs differ from non-SOE firms and exhibit different investment patterns corresponding with the local top leader turnovers. For SOEs, managers often are appointed by government, which means they want to serve the interest of the politicians more than that of shareholders, e.g., helping political leaders to improve economic performance by expansions or increasing capital expenditures. In contrast, non-SOEs, those firms with private investors as controlling shareholders, are not directly influenced by provincial governors and are hence unlikely to invest based on the wishes of the provincial governor.

We find evidence consistent with the hypothesis that political turnover and the incentives of the new provincial governors influence real corporate decisions through their influence on state-owned firms. The main finding and the primary contribution of this paper is that there is a divergence in investment rates between SOEs and non-SOEs in the period just following the turnover. The investment rates of SOEs increase significantly early in the new term of a provincial governor, consistent with the view that politicians exert their influence on investment in an effort to boost economic growth in the province and increase the likelihood of future political promotion. At the same time, the investment rates of non-SOEs decline significantly after the turnover. The wedge in investment rates between SOEs and non-SOEs is estimated on a within-industry basis, suggesting that the political boost in investment for SOEs acts to crowd out private investment. We also find that corporate investment becomes significantly sensitive to measures of investment opportunities, suggesting that political influence is a source of capital misallocation around the turnover cycle. Finally, we also find that, similar to other studies studying political turnover in democratic countries (Julio and Yook (2012), Durnev (2012)), that firms tend to be cautious just before political turnover in a given province and decrease investment.

To further tighten the identification of political influence effects on investment and to rule out concerns that the results are driven by regional economic variation, we employ a neighboring-province difference-in-differences estimation procedure whereby we compare corporate investment for firms in a given province that is experiencing a political turnover to corporate investment by firms in a bordering province where no turnover event is taking place. We also exploit heterogeneity in the strength of the incentive to boost investment across politicians. Provincial governors' characteristics such as age, education, birthplace and previous working experience provide variation in the degree to which career concerns affect economic decisions. For example, we exploit the fact that due to retirement rules, governors between the age of 55 to 60 are most concerned about their political careers and thus have the strongest incentives to manipulate investment through SOEs, relatively to either younger ones or older ones out of the age bracket. Furthermore, the increase in investment among SOEs mainly takes place in provinces where the political turnovers involve normal transitions (the timing can be predicted), less-educated immediate successors and when successors are born locally (more political influence ex post). These findings are consistent with the cross-sectional identification predictions.

Our paper contributes to the literature on the effects of political change on corporate investment in general and in China specifically. At the Macro level, Li and Zhou (2005) present empirical evidence on the link between political turnover of top provincial leaders and provincial economic performance (measured by GDP growth). Maskin, Qian and Xu (2000) and Chen, Li and Zhou (2005) show that the economic performance is an important predictor of political promotion of top provincial leaders in China, while Cao et al., (2013) study CEO political promotions as incentive mechanisms in SOEs since they have concerns about future political careers. Both Maskin, Qian and Xu (2000) and Cao et al., (2013) show that the probability of promotion increases with the average economic performance during the tenure term. Our study on firm-level investment behaviors around political turnovers sheds lights on the channels through which top provincial leaders attempt to prop up provincial economic performance by affecting the investment policies of SOEs.

The paper also provides new supporting evidence for what Shleifer and Vishny (2002) term the "grabbing hand" view of government. Shleifer and Vishny (2002) argue that privatization of state firms is controlled by politicians who act to maximize their private benefits. Our research also contributes to the strand of literature examining potential over-investment of firms in China. For example, Ding et al. (2010) find that firms in China over-invest in almost all sectors. Liu and Siu (2011) find that SOEs compared to private controlled firms are more severe in over-investment problems. Our paper also contributes to the literature on political connections in China. Chen et al. (2011) find that political connections significantly reduce investment efficiency in SOEs but not in non-SOEs. Geng and N'Diaye (2012) show that investment in China is artificially propped up by low interest rates and an undervalued currency. Our paper highlights that political connections and ownership contribute to over-investment by Chinese firms.

Following the 2008 financial crisis, many investment projects have been announced as part of Chinese government's initiatives to stimulate the economy. Provincial and municipal governments unveiled plans to invest more than \$1.6 trillion, according to the National Development and Reform Commission, a central planning agency. According to Barnett and Brooks (2006), SOEs accounted for two-thirds of total Chinese investment in 1990, while their share remained over one-third by 2004. Given the size of investment in China and its link to economic growth, an understanding of the quality of capital allocation is central to the welfare benefits of China's industrial policy. Our paper suggests that significant investment distortions are present in China and these distortions are caused to some extent by the high degree of influence provincial governors hold over state-owned firms.

2.2. Political Turnover and Incentives in China

In China, the market has observed an alarming trend of increasing government policies favoring the state sector. An article in Financial Times (2008)[23] reports that in many industries such as natural resources, civil aviation, real estate, and finance, SOEs crowded the private firms out. State ownership and government politics continue to influence Chinese SOEs' corporate policies both directly and indirectly. For example, the government maintains its control on listed SOEs by appointing top executives, many of whom possess political connections as current or former government officials/bureaucrats. Thus, state ownership and government politics are likely to continue to influence Chinese SOEs' corporate policies. Under China's current political system, government bureaucrats have great control over the allocation of resources such as capital (loans through state-owned banks), land supply and government concessions, contracts as well as appointment of executives in SOEs. Research on China's economic and financial issues must take into consideration the relationship between economic and political institutions (Parish and Michelson (1996)).

Alesina and Perotti (1996) and Jones and Olken (2005) show that new political leaders cast different impact on the economy. We therefore focus on provincial leadership turnovers in China. Provincial governors are held mainly responsible for promoting local economic development. Under China's current political systems, GDP is considered as the main examination index for the performance appraisal of local government officials. Those provincial governors who can deliver the best GDP growth figure during their tenure will have a higher chance of

[23]This phenomenon known as "Guo Jin Min Tui" in Mandarin Chinese describes that the state advances and private sector recedes. This question got serious attention during the 2010 annual meeting of National People's Congress, as illustrated in China Economic Weekly, 2010. Is Guo Jin Min Tui true or false? March 26.

promotion later on. Such performance-based promotion scheme creates tournament-like incentives for local officials in China. Therefore, career concerns of newly appointed provincial governors create strong incentives for them to promote economic growth, which in turn can be used to enhance their reputation and credibility for future promotion. For example, local government reports or provincial yearbooks often contain detailed information on the relative rankings of the economic performance, ranging from GDP growth rate to miles of roads constructed. The combination of promotion incentives and time-to-build considerations suggests that increases in investment are likely to be concentrated in the early years of a new leader's term.

China changes its top leaders every ten years and replaces other top-ranking local governors or party secretaries every five years, at about the same time of the national Communist party congress. A politics-fuelled investment boom accompanied with virtually every new congress is anticipated when a new slate of officials takes over at both central and local levels. Due to career concerns, when new governors take local offices, they plan well their term and hope to make major achievements through capital intensive infrastructure or industrial projects that can be completed during their tenures. It is not uncommon for new local governors to start to announce ambitious investment plans right after their appointments. For example, the Financial Times (2012) observed that "The investment projects that have been announced in recent weeks have been described as 'stimulus' initiatives to prop up the economy. Among others, Guizhou province wants to spend RMB 3 Trillion on boosting tourism, while the city of Chongqing is aiming for an RMB 1.5 Trillion investment in seven strategic industries such as telecommunications". Chen et al. (2011) show that SOEs invest less efficiently than non-SOEs in China due to government interventions such as state ownership or appointment of top executives.

If fundamental political institutions in China do not change, the politics-fuelled investment cycle will keep on repeating itself. Political turnovers will cause undesirable economic consequences by disrupting the local firms' political connections and corporate investment decisions. When a provincial governor assumes a new office in a different province, he usually doesn't have connections with local private firms. However, he can easily exert significant control over local SOE firms through affecting firms' personnel appointment and corporate decisions. Such indirect intervention enables new governors to affect corporate investment activities.

Provincial governors' personal attributes such as age, education, birthplace and previous working experience might matter on the way how politicians influence local economic entities. Since the economic reform in the late 1970s, an important change in the evaluation criteria for government officials is the declining role of family class origin and the increasing emphasis on the educational credentials and expertise of applicants (Bian (2002)). Political conformity and loyalty, which used to be the most important pre-reform criteria for promotion, now gave way to economic performance/ranking among peers and other competence-related indicators such as good education background and demonstrated expertise in administrative management. As a result of this adjustment, top provincial governors are now better educated than in the past. For example, in our provincial governor turnover sample over a 15-year period from 1998 to 2012, 59 out of 113 (approximate 52%) immediate successors have higher education background (either Master or PhD) at the time of appointment.

Due to career concerns, the easiest way for local governors to prop up GDP figures is to implement capital intensive infrastructure projects through introducing ambitious fiscal stimulus plans. Education measures provincial governor's human capital and field of vision, and thus we expect it to have a negative effect on governor's investment impulsion. We argue that well-educated new governors don't tend to increase SOE firms corporate investment after the governor turnovers, while such increase is mainly driven by less-educated immediate successors. The rationale behind the argument is that governors with better education are more rational and less likely to stimulate GDP growth. In other words, better educated governors may not only concern their own political careers but also consider the possible negative and irreversible effects brought by short-term government schemes, which often results in inefficiency, misallocation of resources and corruption.

Boom of SOEs' investment in post-turnover periods is mainly caused by locally born governors. One primary reason is that locally born governors might know better the constituents of local economy of his home province and share the same inherited cultural traits and backgrounds, which help them to speed up the transition process and shorten adaptation period. Such local advantage means that local-born governors are better in mobilizing local economic forces. Alternatively one can argue that locally born governors may be subject to the "homeland bias" so that they have stronger incentive to boost economic growth, to benefit local people and to improve their living standards. Previous working experience of governors especially in the central government can help improve local economic growth due to the connections with central government, which help local governors gain access to resources and alleviate political constraints.

Finally, governors' age matters. Due to the implement of mandatory retirement systems in the early 1980s, most bureaucrats have to retire at the age of 65. In view of this, if a new governor's age is above 60 at the time of appointment, his political career concern might not be as strong as younger ones (Li and Zhou (2005)) as retirement is imminent and the politician will have no further promotions. On the other hand, relatively young bureaucrats may also have low chance of promotion due to their junior status and the lack of political capital in the party. Given these considerations and the average tenure period being 4 to 5 years, there is likely to be an inverse U-shaped relationship between governors' career concerns and their ages, with such concerns peaking around 55 to 60 years old. We thus expect to see that the increase in investment is mainly driven by provincial governors of ages in between 55 and 60, since governors in this age bracket will likely be motivated to take more risk and influence economic performance in the local economy in order to increase their (probably the last) chance of political promotion.

2.3. Data Description, Variable Definition and Summary Statistics

Our turnover data contains 113 turnovers of top provincial governors that occurred in mainland China's 31 provinces between 1998 and 2012. The data, compiled from a variety of internet sources[24], contain detailed personnel information regarding each governors age, education, birth place, previous working experience and most importantly the timing and nature of the appointment. Macroeconomic data and firm characteristics are obtained from the Chinese Stock Market Accounting Research (CSMAR) and Wind databases for the period 1998 to 2012. The sample period is chosen to match the availability of listed firms' financial statements (especially the cash flow statements) in the CSMAR database, as CSMAR starts collecting cash flow data from 1998. We further drop delisted firms, financial firms and firms with less than three observations (i.e. IPO year $\geq$ 2010) in the sample. Finally, we winsorize all firm characteristics at the 1st and 99th percentiles in order to minimize the impact of data errors and outliers.

We obtain the list of private listed firms from CSMAR's China Listed Private Enterprise database and divide the full sample into two subsamples according to the firms' ownership type (i.e., state-owned listed versus private listed). By applying these selection criteria, we

[24] They include Who's Who in the CCP database of http://xinhuanet.com/, China institutions and leaders' database of http://people.com.cn/ and the Central Peoples' Government of the Peoples' Republic of China website www.gov.cn. In addition, two governor turnovers (Xuenong Meng, governor of Beijing from 2003/01 to 2003/04 and Jinping Xi, acting governor of Zhejiang from 2002/10 to 2003/01) are excluded in our sample as their tenure durations are less than one year.

end up with a sample of 2,578 firms spanning 15 years for a total of 21,552 unique firm-year observations, of which 1,159 firms with 12,823 observations are state-owned listed.

Appendix A lists the definitions of all variables used in our analysis, including both dependent variables and control variables. The key variable is the firm-level investment rate, defined as capital expenditure divided by beginning-of-year book value of total assets (lagged total assets). The key control variables include Tobin's Q, calculated as the book value of total assets minus book value of equity plus market value of equity scaled by book value of total assets[25]. Cash flow is measured as EBIT plus depreciation and amortization minus interest expense and taxes scaled by beginning-of-year book value of total assets. State-owned enterprise (SOE) dummy is an indicator variable set equal to one if the ownership type of the listed firm is state-owned. We define four turnover event time dummy variables: the pre-turnover year [-1] dummy, the turnover year [0] dummy, the one-year post-turnover [+1] dummy and the two-year post-turnover [+2] dummy, where year zero is the actual turnover year. The timing of the dummy variables is set to capture the firms' investment dynamics during the full political turnover cycle.

Table 12 reports summary statistics of the number of turnovers and the classification of turnover types for each of the 31 provinces in mainland China for the sample period from 1998 to 2012.

[Insert Table 12 here]

In Table 12, we categorize turnovers into normal and abnormal types according to the nature of the turnover. We define normal turnovers as the cases when top provincial leaders

[25]Chen and Xiong (2002) point out that non-tradable shares in China are generally associated with an illiquidity discount of between 70% to 80% of their market value. Following Bai et al. (2004), we construct three measures for market value (MV) of equity: (i) MV of tradable (common) shares; (ii) MV of tradable shares plus 80% discount of MV of non-tradable shares; (iii) MV of tradable shares plus 70% discount of MV of non-tradable shares. Throughout the paper, we use the third measure for MV of equity. We obtain similar results using the two alternative measures.

are parallel-moved or promoted. On the other hand, we define abnormal turnovers as the cases when a top leader is dead, demoted, resigned, retired or indicted. Our categorization of normal versus abnormal turnovers follows the identification and classification of Chen et al. (2005) and Li and Zhou (2005). There are 113 political turnovers in total, distributed quite evenly across the 31 provinces. Among the turnovers, 83 are classified as normal type and the rest are classified as abnormal. The distribution of turnovers offers a great deal of cross-sectional variation to test their effects on firm investment. The sample of SOEs consists of 12,667 unique firm-year observations, while the non-SOE sample contains 7,923 firm-year observations. In general, Beijing, Guangdong, Jiangsu, Shandong, Shanghai and Zhejiang have more listed firms than other provinces but a comparable number of political turnovers. Table 13 reports the distribution of firm-year observations and the turnovers of provincial governors by each calendar year from 1998 to 2012.

[Insert Table 13 here]

Table 13 shows that turnovers of provincial governors occur every 4.11 years on average and the average length of tenure for governors is 4.14 years. Peak of turnovers happens in 1998, 2003, and 2007. Firm observations increase over time reflecting increased IPO volume over the sample period. Table 13 also indicates that governor turnovers are centered on the past Third Plenary Session of the Central Committee of the Communist Party, a key event that often marks new reforming policies for economic and social development. Table 14 summarizes our full sample.

[Insert Table 14 here]

Panel A of Table 14 summarizes firm characteristics used in our analysis. In the full sample, the mean firm investment rate, defined as capital expenditure divided by lagged total assets, is 0.0655 with a median of 0.0387. Tobin's Q has mean of 2.14 and median of 1.70.

Cash flow deflated by lagged total asset has mean value of 0.0657 and median value of 0.0608. Firms' sales grow at mean rate of 0.2305 and median rate of 0.1320. These summary statistics are consistent with earlier literature on Chinese firms such as Chen et al. (2011). SOEs have slightly higher investment rates compared to the whole sample. Non-SOEs have slightly lower investment rates compared to SOEs but have a significantly larger average Tobin's Q and experience higher rates of sales growth.

Panel B of Table 14 reports the mean investment rates for the full sample, SOEs and non-SOEs separately during the turnover event time [-2, +2] period, with year 0 being the year the turnover occurred. We first consider the full sample. Unconditionally, firm investment rates are slightly lower in pre-turnover years than in other years. On the other hand, investment rates increase over the turnover event time period and keep rising up to one year post-turnover. The investment rate one year post-turnover has a mean value higher than any other turnover years. For example, the average investment rate is 0.0679 one year post-turnover, representing a 4.6% increase relative to the mean investment rate of 0.0649 in other sample years. On average, firms' investment rates in the full sample show an increasing trend over the turnover event time [-2, +1] period as depicted in Panel B of Table 14. For SOEs, the mean investment rate one year pre-turnover is not different much from other years, but SOE investment rates are significantly higher one year post-turnover. The difference amounts to 0.0070, representing approximately a 10% increase from the mean investment rate of 0.0666 in other years. The table also shows that non-SOEs exhibit different patterns in investment from SOEs. Although on average, non-SOEs have lower investment rate than SOEs, the mean investment rate of non-SOEs does not experience significant decrease or increase over the [-1, +1] turnover event time period. In addition, Panel B of Table 14 indicates that investment rates of non-SOEs drop significantly two year post-turnover, compared with other sample years.

[Insert Figure 3 here]

Figure 3 compares mean investment rates over the turnover event time [-2, +2] period for the full sample, SOEs and non-SOEs separately. A clear pattern emerges from Figure 3: investment rates for the full sample shows an increasing trend and the trend pattern is much more noticeable for SOEs. For SOEs, investment peaks one year post-turnover; while for non-SOEs, investment generally peaks one year pre-turnover and then deteriorates quickly. The wedge between investment rates of SOEs and non-SOEs increase sharply over the [-1, +2] turnover period.

2.4. Empirical Results

2.4.1. Regression Specification

In this paper, we empirically examine whether the political incentives of politicians around the turnover of provincial governors has an impact on corporate investment decisions of firms in China. To test for changes in the investment dynamics of firms across the turnover cycle, we employ an augmented investment-Q specification and estimate the following baseline panel regression model:

$$\begin{aligned} I_{ijt} &= \alpha_i + \gamma_t + \beta_1 \text{Pre-Turnover}_{j,t-1} + \beta_2 \text{Turnover}_{j,t} + \beta_3 \text{Post-Turnover}_{j,t+1} \\ &\quad + \beta_4 \text{Post-Turnover}_{j,t+2} + \beta_5 Q_{i,t-1} + \beta_6 CF_{i,t} + \beta_7 \%\Delta GDP_{j,t-1} + \beta_8 \%\Delta Sales_{i,t} + \varepsilon_{ijt} \end{aligned} \tag{2.1}$$

where i stands for the firm, j indexes the province, and t denotes the year. The dependent variable, the firm-level investment rate, is defined as capital expenditures scaled by lagged total assets. The primary explanatory variables of interest are time-province dummies measuring the periods before and after the turnover event. First is the turnover year dummy, which is the calendar year when the actual turnover occurs. The pre-turnover period is defined as the one year period immediately before the turnover year. The one (two) year post-turnover year dummy takes on a value of one if the firm-year-province pair falls in the one (two) year immediately after the turnover year period. Other explanatory variables include Tobin's Q, cash flow and provincial-level real GDP growth rates, which are used to control for firm investment opportunities and provincial economic conditions. In addition, we include firm sales growth as an addition control for expected future demand (Bloom, Bond and Van Reenen (2007)). To control for time-variant unobservable variation, we include both firm and year fixed effects in the baseline investment regression. This specification captures the within-firm variation in corporate investment around turnover event years. Standard errors are clustered at the firm level in all specifications.

There are potential concerns with the one-way clustering of regression standard errors used in our analysis. However, as pointed out by Thompson (2011) and Petersen (2009), two-way clustering is only valid provided: (i) Both N and T are "large"; and (ii) The aggregate shocks must dissipate over time. In such cases, clustering by two dimensions will likely produce unbiased standard errors. Our sample fits neither of these two requirements. First, in our sample N exceeds 2,500 firms but the average T is around 11.6 years with a maximum of 15 years. Second, the turnovers are centered around the Third Plenary Session of the Central Committee of the Communist Party as tabulated in Table 13. In view of this, we first report our baseline results with standard errors clustered at firm level only. For robustness, we repeat our analysis with standard errors clustered at both economic region and year levels and find similar results[26]. Following strategies promulgated by the Central People's Government, we categorize the 31 provinces/municipalities into eight economic regions according to the similarities in their economic conditions and industrial structures. The information on the eight economic regions is reported in Appendix B.

[26]To save space, the robustness with alternative standard error estimates are not reported here. Results are available upon request.

2.4.2. Investment around Turnover Years

In Table 15, we report the empirical results for our baseline specification separately for the the full sample, the sample of SOEs only, and the non-SOE sample. We estimate panel regressions and include firm and year fixed effects in all specifications. Standard errors are clustered at firm level.

[Insert Table 15 here]

Table 15 reports the estimation results for all three samples. The first two columns report the estimates for the full sample, the third and fourth columns report results for SOEs, and the final two columns report the estimates of the investment regressions for the non-SOE sample. We estimate two specifications for each sample that differ only in whether a two year post-turnover dummy is included.

For the full sample (first two columns of Table 15), we find a negative relationship between the pre-turnover dummy and corporate investment rates, consistent with the prior literature documenting pre-election declines in investment rates (Julio and Yook (2012) and Jens (2013)). Investment rates are not significantly different from other periods in the turnover year nor in the post-turnover period. The coefficients on the control variables are consistent with the literature. Corporate investment is positively related to Q, cash flow, sales growth, and regional economic growth.

As the univariate tests in Table 14 and as Figure 3 shows, there are important differences in investment behaviors between SOEs and non-SOEs over the provincial turnover cycle. SOEs show a noticeable increasing pattern following political turnovers while non-SOEs exhibit a clear decreasing trend around political turnovers. Given these difference, we divide the full sample into two groups by their ownership type, i.e., SOEs versus non-SOEs, and estimate the baseline regression on these two subsamples separately.

Specifications (3) and (4) of Table 15 report the regression results for the sample of SOEs. Corporate investment rates for SOEs are negative but not statistically significantly different in the pre-turnover year and the turnover year. However, we see a large increase in investment rates in the post-turnover year. The negative coefficients of pre-turnover dummy and the positive significant coefficients on the one-year post-turnover dummy together suggest that SOEs exhibit a tendency to first slightly decrease investment immediately before change in governors but scale up investment right after a new provincial governor takes office. The estimates in specification (3) show that investment rates first decrease by 0.0022 in pre-turnover years and then increase right away by 0.0040 on average in the one-year post-turnover period, after controlling for growth opportunities and macroeconomic conditions. In terms of economic magnitude, the coefficients in specification (3) translates into a 3.2% decrease and a 6.0% increase in investment rates in the one-year pre- and post-turnover years respectively, relative to mean investment rates in other years.

Specifications (5) and (6) of Table 15 report the regression results for the sample of non-SOE firms. We find that non-SOEs generally invest less in the pre-turnover period. We also find, in contrast to the behavior of SOEs, investment rates for non-SOEs decline in the post-turnover period. In terms of economic magnitude, these coefficient estimates in Specification (5) and (6) translate into an 4.8% to 8.2% drop in investment rates during the one-year post-turnover period, compared with mean investment rates in other sample years. As before, the other coefficient estimates are consistent with the literature in terms of signs and magnitudes. For robustness, we also estimate panel regressions with standard errors double-clustered at both economic region and year levels for non-SOEs and find similar results.

Overall, the regression results highlight an interesting pattern in corporate investment activity around the turnover of provincial governors in China. First, we find a negative relationship between investment and the pre-turnover period for non-SOEs. This is consistent with the findings of Julio and Yook (2012) in the sense that non-SOEs face political uncertainty prior to political leadership changes. We also find a robust increase in investment rates for SOEs following the appointment of a new provincial governor. The investment-to-assets ratio for SOEs increases by approximately 6.0% to 6.9%. The post-turnover increase in investment is a novel finding in the literature. The evidence is consistent with the view that the incentives of provincial governors lead them to exert influence on the investment policy of SOEs very early in their new term. In China, SOEs often follow political leadership and through SOEs, newly appointed bureaucrats stimulate investment activities to showcase their economic agenda for regional development.

The contrasting evidence of the effect of political turnovers on investment between SOEs and non-SOEs is consistent with the unique political institutions in China. Non-SOEs are more immune from political influence as they are more likely to maximize their private shareholders value. Therefore, political turnovers of provincial governors do not necessarily directly interfere in firm decisions or investment activities. Provincial-level SOEs, on the other hand, are sensitive to political interference and political agendas as provincial governors exert a great degree of influence on firm decisions. SOE investment therefore is subject to political uncertainty ex ante and political influence ex post. The results suggest the possibility that the increasing investment rates of SOEs post-turnover crowd out the investment of non-SOEs. The next section examines this hypothesis in detail.

2.4.3. Post-turnover Crowding Out Effects

The prior literature focusing on political turnover and investment has largely ignored the widespread concern that investment policies of SOEs may crowd out the investment of private firms in the post-turnover period. As discussed in Section 2, a politically fuelled investment boom accompanied with both central and local level governor turnover is highly expected and visible. Ambitious government-led investments and expenditure projects are normally announced right after new governors' appointments as stimulus initiatives to prop up the local economy. Given time-to-build considerations, new provincial governors tend to stimulate investment through SOEs at the beginning of their term. Most of the new investment projects are initiated through SOEs to reinforce their dominant role in the market. As a consequence, non-SOEs rarely participate in post-turnover politically motivated investment projects. Many large SOEs are given government subsidies and possess great advantages in resources, personnel, tax advantages and access to relatively low cost financing compared to non-SOEs. Hence non-SOEs have a disadvantage compared to SOEs in participating in these investment projects. In addition, the surge of investment by SOEs may have a crowding-out effect on private investment.

To empirically test for a post-turnover crowding out effect, we include a SOE dummy as well as interaction terms between the SOE dummy and post-turnover indicators in our baseline investment regressions on the full sample. We include industry fixed effect to effectively compare the investment rates of SOEs and non-SOEs within the same industry across the turnover cycle. We use the industry classifications issued by the China Securities Index (CSI) company[27]. The estimation results are reported in Table 16.

[Insert Table 16 here]

The first five specifications of Table 16 report estimates for each turnover period separately. We first note that on average, SOEs tend to invest less than non-SOEs, as demonstrated by the negative coefficients on the SOE dummy variable. Specification (1) compares the pre-turnover investment activity between SOEs and non-SOEs. The interaction term between the pre-turnover dummy and the SOE dummy is insignificant, suggesting that the pre-turnover behavior of the two types of firms is not significantly different. The same is true of the turnover year itself, as reported in Specification (2). The real difference in investment behavior become

[27] In unreported analysis, we also try industry classifications compiled by the China Securities Regulatory Commission (CSRC) and obtain similar results.

apparent in the post-turnover period. Specification (3) through (5) report positive and significant interaction terms between the SOE dummy and the post-turnover indicator variables. Specifications (6) and (7) include the full set of turnover indicator variables in the regression. Specification (6) defines the post-turnover period as two separate years, while Specification (7) combines the two years together. The results are similar to those reported in the earlier regressions. SOE investment increases significantly relative to that of the private firms in various post-turnover periods. For example, in the total post-turnover period, SOEs increase investment significantly relative to non-SOEs.

To summarize, the absolute decline of non-SOE post-turnover investment reported in Table 15 and the relative post-turnover decrease in investment for non-SOEs reported in Table 16 provide evidence that SOE investment crowds out private investment following political turnovers. We now turn to investigate whether the post-turnover patterns in investment reported above are consistent with the common view that political influence in China acts to distort capital allocation.

2.4.4. Investment Efficiency around Political Turnover

In the previous subsection, we have documented the fact that the investment of SOEs in the post-turnover period has a crowding-out effect on the investment of non-SOEs. A natural question that arises is whether and to what extent crowding out represents a misallocation of resources. The previous results suggest the possibility that political incentives lead to over-investment by SOEs and under-investment by private firms. Given that investment makes up close to 50% of GDP in China, the degree to which investment is efficient is an important consideration. In this subsection, we measure changes in investment efficiency in the post-turnover period.

We measure investment efficiency as the sensitivity of investment to Tobin's Q. The basic idea is that an efficient investment policy is one in which investment rises when growth opportunities are high and declines when investment opportunities diminish. This metric has been used extensively in the literature. For example, Gertner, Powers and Scharfstein (2002) use this measure to analyze changes in investment efficiency around corporate spinoffs, Ozbas and Scharfstein (2010) investigate the investment efficiency of diversified firms, and Desai and Goolsbee (2005) examine the relationship between taxes and investment efficiency. Chen et al. (2011) use the sensitivity of investment to Tobin's Q to assess difference in average investment efficiency between SOEs and non-SOEs in China.

To measure changes in efficiency, we add to our baseline investment regression an interaction between the post-turnover dummy variable and Tobin's Q. We conduct separate tests for the whole sample, the subsample of SOEs, and the non-SOEs in order to investigate investment inefficiency after political turnovers. Table 17 reports the results from the post-turnover investment inefficiency tests.

[Insert Table 17 here]

The coefficients on the interaction terms between the two-year post-turnover dummy and Q are negative and significant in Specifications (1) and (2) of Table 17, while the interaction terms between the one-year post-turnover dummy and Q are not significant. The negative interaction term suggests that investment efficiency declines in the post turnover period in that investment expenditures are less correlated with growth opportunities, consistent with a potential capital misallocation.

The last four specifications in Table 17 compare post-turnover investment efficiency for SOEs and non-SOEs separately. In Specifications (3) and (4), the interaction terms for the SOE sample are negative. The magnitude of the interaction terms in Specification (3) suggests a reduction of nearly 50%, dropping by -0.0022 compared to the non-turnover sensitivity to Q of 0.0046. The last two columns report the results for the non-SOE sample. We also see for the private firms that investment efficiency declines significantly in the post-turnover period. We also include for all specifications a test of whether the post-investment investment efficiency is significantly different from zero. This is simply a test of whether the sum of the coefficients on Tobin's Q and the interaction term are zero. The table shows that investment efficiency, while significantly different from zero in all samples in other periods, is only marginally significant in the post-turnover period and insignificant for the private firms. These results imply that investment expenditures are not responding to signals about investment opportunities when incentives to invest for politicians are high, resulting in a loss of efficiency.

2.4.5. Additional Tests

In this section, we exploit heterogeneity in the degree to which political incentives are expected to influence the investment decisions of SOEs and non-SOEs around political turnover events. In some cases, the incentives of politicians to boost investment at the beginning of their term are very high, while in other cases the incentives are relatively muted. Incentives vary across the type of office the politician holds, the type of turnover, the education and the age of the politician, and whether or not the politician was born in the region of interest.

We first look at the difference in investment behavior between the appointment of provincial governors and that of party secretaries. Given the different economic and political roles of the two types of provincial leaders as discussed earlier, we expect that turnovers of party secretaries do not have an impact on firm investment post-turnover. As a placebo test, we reestimate the baseline investment regression using the turnovers of party secretaries.

The regression results are reported in Table 18. We find that across all samples that the post-turnover investment behavior of Chinese firms does not change significantly after the turnover of a party secretary. We do find a slight decline in investment in the turnover year for

SOEs, but we do not see the divergence in investment activity between SOEs and non-SOEs that is present following the turnover of a provincial governor. The lack of a post-turnover effect is consistent with the fact that in Chinese institutions the party secretary is typically in charge of the Chinese Communist Party (CCP) personnel decisions but is not directly involved with economic affairs.

[Insert Table 18 here]

We have so far established the fact that SOEs decrease their investment prior to governor's turnover but scale up after the turnover, while non-SOEs reduce investment after the turnover. We now investigate the cross-sectional heterogeneity in turnover types (normal turnover vs. abnormal turnover) and characteristics of the immediate successors. Normal turnovers include promotions or parallel turnovers while abnormal turnovers include retirements and terminations due to death or indictment. 80 out of the 113 (around 71%) turnovers in our sample are classified as normal turnovers (promoted or laterally moved), and the remaining 29% are classified as abnormal. We manually collect education, birth place and age information of immediate successors for governors. We define an education dummy that takes on a value of one if the immediate successor holds a masters or PhD degree, and zero otherwise. 59 out of the 113 (approximate 52%) successors have a high education level (Masters or PhD) at the time of appointment. The remaining 54 of 113 have only bachelors degrees or less. We define a birth place dummy set equal to one if the immediate successor is born in the same province as he will assume office, and zero if his birth province is from a different region. 28 out of the 113 (approximate 25%) successors are born locally and assume offices in their birth provinces. Finally, we define a governor age dummy that takes on a value of one if the age of the new governor is in between 55 and 60 at the time of appointment. On average, provincial governors are 55.6 years old when they assume office and 64 out of the 113 (around 56%) governors are in between 55 and 60 years old at the time of appointment.

[Insert Table 19 here]

Table 19 presents the investment regression estimates for the sample of SOEs. The interaction terms between the post-turnover dummy and turnover type, between post-turnover dummy and education, between post-turnover dummy and birth place, and between post-turnover dummy and governor age are included in the analysis. We find that the interaction terms have great explanatory power. First, when interaction term between post-turnover dummy and normal turnover type dummy is included, the post-turnover dummy is not significant in the regression. This finding suggests that most of increase in investment after turnover is caused by normal turnovers. One explanation is that, compared to abnormal turnovers, immediate successors in normal turnovers have stronger incentives to promote economic development to increase their chances for future promotion. Second, we include the education dummy and interact it with post-turnover indicator. The interaction term is negative and significant while the post-turnover dummy itself is positive and significant, with similar magnitudes. This result suggests that well-educated new governors do not abnormally stimulate corporate investment, and thus the average increase is mainly caused by the less-educated immediate successors following the governor turnovers. Third, we add an interaction term between the post-turnover dummy and the same birth place dummy (whether the immediate successor of governor comes from the same province for the new position). The interaction term is significant and positive while the post-turnover dummy does not have a significant coefficient. This result indicates that most of the investment increase following political turnover is caused by politicians who return to govern their home provinces. In the last two columns, we further add to our baseline investment regression the interaction term between the post-turnover dummy and the governor age dummy. We find that the interaction term is positive and statistically significant while the post-turnover dummy is not significant. This finding suggests that the post-turnover investment boom for SOEs is driven primarily by governors within the 55- to 60-year-old age bracket at the time of appointment. The intuition is that, as discussed in Section 2, governors within this age bracket are most concerned about their political careers and thus have the strongest incentives to stimulate investment through SOEs, relative to younger or older politicians due to mandatory retirement rules.

Overall, the results in this section suggest that the degree to which political incentives for stimulating investment are present and can explain differences in the post-turnover investment patterns we see for SOEs in China. Specifically, post-turnover effects on investment are mainly caused by normal turnovers of governors, and by turnovers with less educated immediate successors, and by turnovers in which the new governor was born in the same province, and by turnovers where the new governor is between 55 and 60 years old.

2.4.6. Neighboring Province Difference-in-Differences Estimator

We now examine the post-turnover effect on firms' corporate investment by employing a "neighboring province" difference-in-difference (DD) estimation methodology. The previous results implicitly use all 31 provinces in China unaffected by political turnover in a given year as the "control" group. Since firms in neighboring provinces are more likely to be subject to similar unobserved economic shocks, we can tighten the identification by comparing changes in investment for firms in a province with a turnover event to changes in investment for firms in the neighboring provinces without a turnover event. Using the neighboring provinces as controls addresses the concern that our earlier results may be picking up regional variation in economic activity that are not absorbed by year fixed effects and supports the conclusion that the turnover effects documented above are caused by the incentives surrounding political turnover periods.

Following the identification of Dube, Lester and Reich (2010), we implement the neighboring province difference-in-differences estimator as follows: We define the "treatment" indicator to be set equal to one in the year just following the political turnover. For every province, we consider all firms in bordering provinces that are not currently in a post-turnover period as being control firms. Provinces that match the same turnover period as a treatment province is excluded from being a control province. The estimator is intended to measure differences in investment around the turnover period between firms in the treatment province and "untreated" firms in neighboring provinces in the same year.

We summarize the geographical distribution of provinces and their neighbors in Table 20. On average, a province has 4.48 neighboring provinces. Inner Mongolia and Shaanxi have the largest number of neighboring provinces at 8 each. While neighboring provinces are likely to have more correlated economic performance than more distant provinces, there are some cases where neighboring provinces can be quite different. For example, the provinces of Anhui and Zhejiang are considered to be quite different with respect to economic conditions and industry representation. To mitigate this concern, we categorize the 31 provinces into eight economic regions according to the similarities in their economic conditions and industrial structures and repeat the difference-in-difference estimator within these economic regions. The definitions of the eight economic regions is reported in Appendix B. We therefore tighten the definition of neighboring provinces by further requiring that they are located within the same economic region (*boldfaced* ones in Table 20). Figure 4 illustrates the frequency distribution of the number of turnovers by province.

[Insert Table 20 here]

To illustrate how we construct the treatment and control provinces, consider the example of Shanghai with post-turnover years in 2002 and 2004 as the *initial* treatment province. From Table 20, Shanghai has two geographically neighboring provinces, Jiangsu and Zhejiang, which are used as the *initial* neighboring control provinces. To obtain the *unaffected* control group, we first drop firm-year observations in 1999, 2003, 2009 and 2011 from Jiangsu province and firm-year observations in 2003 and 2012 from Zhejiang province respectively, as these sample years represent the post-turnover years [+1] of respective provinces. To avoid *asymmetric* comparison between the treatment province and its neighboring control provinces, we further drop firm-year observations in 2003 from Shanghai as this year coincides with the post-turnover years (i.e., 2003) of *both* Jiangsu province and Zhejiang province. In this example, *Treatment indicator* takes on a value of one if firms are located in Shanghai (i.e., the treatment province) and zero if they are in Jiangsu and Zhejiang (i.e., the neighboring control provinces). The *Post-turnover dummy* is set to equal to one if firm-year observations fall in 2002 or 2004 (i.e., the post-turnover years of Shanghai) and zero otherwise.

With the treatment and control firms properly assigned, we estimate the following "neighboring province" difference-in-differences (DD) model

$$
\begin{aligned}
I_{ijt} \quad = \quad & \beta_0 + \beta_1 \text{Treatment dummy}_j + \beta_2 \text{Post-Turnover}_{j,t+1} \\
& + \beta_3 \text{Treatment dummy}_j \times \text{Post-Turnover}_{j,t+1} \\
& + \beta_4 Q_{i,t-1} + \beta_5 CF_{i,t} + \beta_6 \%\Delta GDP_{j,t-1} + \beta_7 \%\Delta Sales_{i,t} + \mu,
\end{aligned}
\tag{2.2}
$$

where i indexes firms, j indexes provinces, and t indexes time. *Treatment dummy* is an indicator variable that takes on a value of one if the firm-year observations belong to the treatment province and zero if they belong to the neighboring control provinces. *Post-turnover dummy* is an indicator variable that is set to one if firm-year observations fall in the post-turnover years of the treatment province; Firm characteristics and provincial GDP growth rate are included. μ is the error term. We control for province, industry and year fixed effects. Standard errors are clustered at the firm level. The variable of interest is the coefficient estimate of the interaction term, which measures the added effect of political turnovers on firms' investment rates in the post-turnover years. Panel A and Panel B of Table 21 report the "neighboring province" DD estimation results based on the original neighboring province definition (in normal font) and the refined definition (in boldface font) as given in Table 20 respectively.

[Insert Table 21 here]

The results further confirm that our previous findings are not likely caused by unobservable common factors that affect both the treatment and the control group in a similar manner. Columns (3) and (4) of Table 21 show that coefficient estimates of the interaction terms are positive and statistically significant for the sample of SOEs. This suggests that relative to other sample years, the post-turnover years experience an significant investment increase for SOEs, consistent with the baseline results. In terms of economic magnitude, the interaction term *Treatment dummy* $\times$ *Post-turnover dummy* in column (4) of Panel A has a coefficient estimate of 0.0032, representing a 4.6% increase in investment rates for SOEs in the post-turnover years relative to the mean investment rates of SOEs in other sample years. As before, we don't observe the post-turnover investment boom for the full sample and the sample of non-SOEs.

2.5. Conclusion

This paper studies how state ownership and political incentives influence corporate investment in China. Using manually collected information on the transition of top provincial leaders in China for both governors and party secretaries, we find that turnovers of governors has a significant but divergent impact on SOEs and non-SOEs. Post-turnover, we find that there is a large wedge between the investment rates of SOEs and private firms. Investment rates for SOEs are abnormally high while investment rates of of non-SOEs are lower than normal. The results are consistent with the view that the incentives of new provincial governors influence the investment rates of SOEs in an effort to boost provincial economic growth and increase the chance of personal promotion. Furthermore, we find that the investment behavior of SOEs post-turnover has a crowding-out effect on the investment rates of private firms. These divergent patterns of investment reflect a misallocation of capital as measures of investment efficiency decline significantly following the turnover of a provincial governor.

Our research sheds lights on the interaction between political economy and corporate finance in an emerging economy. China, as the largest emerging economy with a unique political system, provides an interesting laboratory for studying how corporations react to both political uncertainty associated with leadership turnovers and the incentives politicians face to boost investment. Our empirical findings show that in China corporate decisions of SOEs often follow the political lead, while non-SOEs face great political uncertainty and diminished capacity for investment. Non-SOEs are not equipped with safeguards against political interference from the government, while SOEs are more likely to serve the interest of political leaders since their personnel decisions are controlled by these leaders and not by the shareholders. Our paper shows how political systems interact with ownership structures in China. It suggests that SOEs, though partially privatized through share issuance, are still subject to the heavy influence of politicians. The government still plays an important role in firm investment decisions, especially those that are state-owned.

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Table 12
Summary of Firm-Level Observations and Turnovers by Province

The first three columns report the distribution of firm observations for SOEs and non-SOEs jointly and separately across provinces. The last three columns report the distribution of provincial governors' turnovers. We split turnovers into normal and abnormal types by nature of the turnover. Normal turnovers include promotions or parallel turnovers while abnormal turnovers include retirements and terminations due to death or indictment.

Province	Observations (Total)	Observations (SOEs)	Observations (Non-SOEs)	Turnovers (Total)	Turnovers (Normal)	Turnovers (Abnormal)
Anhui	647	447	200	5	5	0
Beijing	1,378	1,028	350	4	4	0
Chongqing	381	282	99	3	1	2
Fujian	644	387	257	4	3	1
Gansu	238	150	88	4	4	0
Guangdong	2,710	1,599	1,111	2	0	2
Guangxi	306	169	137	3	2	1
Guizhou	211	160	51	4	1	3
Hainan	328	142	186	4	3	1
Hebei	421	266	155	6	6	0
Heilongjiang	362	262	100	4	4	0
Henan	449	301	148	3	3	0
Hubei	884	499	385	4	2	2
Hunan	535	382	153	5	5	0
Inner Mongolia	267	118	149	4	2	2
Jiangsu	1,337	649	688	4	4	0
Jiangxi	319	264	55	3	1	2
Jilin	439	235	204	5	4	1
Liaoning	706	450	256	4	3	1
Ningxia	148	64	84	1	0	1
Qinghai	118	65	53	4	4	0
Shaanxi	362	240	122	5	4	1
Shandong	1,135	687	448	3	1	2
Shanghai	2,565	1,971	594	3	3	0
Shanxi	351	256	95	6	3	3
Sichuan	813	403	410	2	1	1
Tianjin	374	301	73	3	3	0
Tibet	114	26	88	3	2	1
Xinjiang	374	244	130	2	2	0
Yunnan	288	196	92	3	1	2
Zhejiang	1,395	424	971	3	2	1
Total	20,599	12,667	7,932	113	83	30

Table 13
Summary of Firm-Level Observations and Turnovers by Year

The first three columns report the distribution of firm observations for SOEs and non-SOEs jointly and separately across years. The last three columns report the distribution of provincial governors' turnovers. We split turnovers into normal and abnormal types by nature of the turnover. Normal turnovers include promotions or parallel turnovers while abnormal turnovers include retirements and terminations due to death or indictment.

Year	Observations (Total)	Observations (SOEs)	Observations (Non-SOEs)	Turnovers (Total)	Turnovers (Normal)	Turnovers (Abnormal)
1998	751	489	262	13	8	5
1999	854	559	295	7	7	0
2000	950	621	329	4	3	1
2001	1,094	704	390	8	3	5
2002	1,180	765	415	9	6	3
2003	1,248	810	438	13	12	1
2004	1,316	852	464	6	4	2
2005	1,413	899	514	1	1	0
2006	1,424	901	523	8	5	3
2007	1,511	943	568	13	9	4
2008	1,634	992	642	5	4	1
2009	1,692	1,006	686	3	2	1
2010	1,844	1,042	802	9	8	1
2011	1,844	1,042	802	8	5	3
2012	1,844	1,042	802	6	6	0
Total	20,599	12,667	7,932	113	83	30

Table 14
Summary Statistics

Panel A shows summary statistics for the firm characteristics used in our analysis jointly and separately for SOEs and non-SOEs between 1998 and 2012. Panel B depicts the mean investment rates around turnover event years. Panel B also reports the significance of the difference in mean investment rates for a given year in the [-2, +2] turnover period and the rest of sample years. ***, ** and * denote significance at the 1%, 5% and 10% levels respectively. Year 0 indicates the actual calendar year when turnover event occurs. See Appendix A for variable descriptions as well as the variable sources.

Panel A: Firm Characteristics				
Full Sample				
	N	Mean	Median	Std. Dev.
Investment Rate	20,599	0.0655	0.0387	0.0824
Q	20,385	2.1358	1.7010	1.4708
Cash Flow	19,466	0.0657	0.0608	0.0800
Sales Growth	20,324	0.2305	0.1320	0.6532
SOEs				
	N	Mean	Median	Std. Dev.
Investment Rate	12,667	0.0681	0.0417	0.0815
Q	12,612	1.8742	1.5224	1.2030
Cash Flow	12,073	0.0696	0.0626	0.0746
Sales Growth	12,501	0.2152	0.1349	0.5599
Non-SOEs				
	N	Mean	Median	Std. Dev.
Investment Rate	7,932	0.0615	0.0338	0.0838
Q	7,773	2.5604	2.0164	1.7418
Cash Flow	7,393	0.0593	0.0575	0.0877
Sales Growth	7,823	0.2551	0.1260	0.7789

Panel B: Mean Investment Rates around Turnover Years					
Full Sample					
Year	-2	-1	0	+1	+2
N	3,896	4,291	4,706	4,336	3,690
Investment Rate	0.0615	0.0654	0.0673	0.0679	0.0630
Mean Diff	-0.0050***	-0.0001	0.0023	0.0030**	-0.0031**
SOEs					
Year	-2	-1	0	+1	+2
N	2,414	2,630	2,882	2,674	2,368
Investment Rate	0.0662	0.0675	0.0708	0.0736	0.0698
Mean Diff	-0.0023	-0.0007	0.0036**	0.0070***	0.0021
Non-SOEs					
Year	-2	-1	0	+1	+2
N	1,482	1,661	1,824	1,662	1,322
Investment Rate	0.0539	0.0622	0.0617	0.0587	0.0510
Mean Diff	-0.0094***	0.0009	0.0002	-0.0035	-0.0127***

Table 15
Baseline Investment Regressions

The unit of observation is at firm-year level. The dependent variable is the firm-level investment rate defined as CAPX/Lagged Assets. Independent variables include the lagged Tobin's Q, cash flow, province-level real GDP growth rate, sales growth and the turnover period [-1, 0, +1, +2] dummies, with year 0 being the year the actual turnover occurred. See Appendix A for the definition of variables. The first two columns report results for the full sample. The last four columns present results for SOEs and non-SOEs separately. Variables of interests are the four turnover period dummies. We use baseline investment regression and control for firm and year fixed effects. Standard errors are clustered at firm level. T-statistics are reported in square brackets below coefficient estimates. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Full Sample		SOEs		Non-SOEs	
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-turnover year (-1)	-0.0027	-0.0029	-0.0022	-0.0019	-0.0035	-0.0044
	[-2.26]**	[-2.39]**	[-1.51]	[-1.30]	[-1.71]*	[-2.14]**
Turnover year (0)	-0.0001	-0.0003	0.0014	0.0017	-0.0023	-0.0034
	[-0.07]	[-0.23]	[0.83]	[0.99]	[-1.04]	[-1.50]
Post-turnover year (+1)	0.0014	0.0009	0.0040	0.0046	-0.0030	-0.0051
	[1.05]	[0.65]	[2.51]**	[2.51]**	[-1.38]	[-2.11]**
Post-turnover year (+2)		-0.0014		0.0019		-0.0077
		[-1.05]		[1.11]		[-3.35]***
Q	0.0044	0.0044	0.0042	0.0042	0.0046	0.0045
	[7.10]***	[7.07]***	[4.38]***	[4.41]***	[5.67]***	[5.56]***
Cash Flow	0.2631	0.2629	0.2563	0.2564	0.2697	0.2684
	[18.88]***	[18.88]***	[13.62]***	[13.62]***	[12.96]***	[12.96]***
GDP Growth	0.0273	0.0280	0.0264	0.0251	0.0256	0.0254
	[2.17]**	[2.22]**	[1.75]*	[1.67]*	[1.11]	[1.10]
Sales Growth	0.0067	0.0067	0.0083	0.0083	0.0052	0.0052
	[5.32]***	[5.32]***	[4.77]***	[4.77]***	[2.94]***	[2.93]***
Constant	0.0363	0.0367	0.0383	0.0378	0.0337	0.0362
	[16.29]***	[16.02]***	[13.79]***	[13.21]***	[8.81]***	[9.23]***
Observations	19,163	19,163	11,982	11,982	7,181	7,181
Between R^2	20.10%	20.11%	25.23%	25.28%	15.34%	15.69%
Fixed Effects	Firm	Firm	Firm	Firm	Firm	Firm
	Year	Year	Year	Year	Year	Year

Table 16
Post-turnover Crowding Out Effects

The unit of observation is at firm-year level. The dependent variable is the firm-level investment rate defined as CAPX/Lagged Assets. Independent variables include the lagged Tobin's Q, cash flow, province-level real GDP growth rate, sales growth and the turnover period [-1, 0, +1, +2] dummies, with year 0 being the year the actual turnover occurred. See Appendix A for the definition of variables. To test for the post-turnover crowding out effect, we further include a SOE dummy as well as interaction terms between the SOE dummy and post-turnover indicators in our baseline investment regression on the full sample. Variables of interests are the interaction terms. We control for industry and year fixed effects. Standard errors are clustered at firm level. T-statistics are reported in square brackets below coefficient estimates. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pre-turnover year (-1)	-0.0028					-0.0048	-0.0049
	[-1.49]					[-2.29]**	[-2.34]**
Pre-turnover (-1) × SOE dummy	-0.0012					0.0007	0.0008
	[-0.54]					[0.30]	[0.31]
Turnover year (0)		-0.0014				-0.0041	-0.0044
		[-0.77]				[-1.87]*	[-2.03]**
Turnover year (0) × SOE dummy		0.0014				0.0035	0.0034
		[0.59]				[1.26]	[1.24]
Post-turnover year (+1)			-0.0028			-0.0063	
			[-1.47]			[-2.71]***	
Post-turnover (+1) × SOE dummy			0.0053			0.0081	
			[2.23]**			[2.82]***	
Post-turnover year (+2)				-0.0083		-0.0104	
				[-4.17]***		[-4.65]***	
Post-turnover (+2) × SOE dummy				0.0071		0.0096	
				[2.92]***		[3.49]***	
Post-turnover year (+1,+2)					-0.0072		-0.0083
					[-3.81]***		[-4.08]***
Post-turnover (+1,+2) × SOE dummy					0.0080		0.0088
					[3.53]***		[3.53]***
SOE dummy	-0.0043	-0.0049	-0.0057	-0.0057	-0.0075	-0.0088	-0.0088
	[-2.04]**	[-2.27]**	[-2.65]***	[-2.66]***	[-3.31]***	[-3.29]***	[-3.29]***
Firm/Economy Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19,163	19,163	19,163	19,163	19,163	19,163	19,163
Between R^2	33.01%	33.00%	33.04%	33.23%	33.21%	33.35%	33.26%
Fixed Effects	Industry	Industry	Industry	Industry	Industry	Industry	Industry
	Year	Year	Year	Year	Year	Year	Year

Table 17
Investment Efficiency Tests

The unit of observation is at firm-year level. The dependent variable is the firm-level investment rate defined as CAPX/Lagged Assets. Independent variables include the lagged Tobin's Q, cash flow, province-level real GDP growth rate, sales growth and the post-turnover period [+1, +2] dummies, with year 0 being the year the actual turnover occurred. See Appendix A for the definition of variables. To measure changes in efficiency, we add to our baseline investment regression an interaction between the post-turnover dummy variable and Tobin's Q. Variables of interests are the interaction terms. The first two columns report results for the full sample. The last four columns present results for SOEs and non-SOEs separately. We control for firm and year fixed effects. Standard errors are clustered at firm level. T-statistics are reported in square brackets below coefficient estimates. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Dependent Variable: Firm-level Investment					
	All Firms		SOEs		Non-SOEs	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-turnover year (+1)		0.0019		0.0039		-0.0045
		[0.81]		[1.29]		[-1.19]
Post-turnover (+1) $\times Q$		-0.0003		0.0001		0.0004
		[-0.27]		[0.09]		[0.25]
Post-turnover year (+2)	0.0047	0.0052	0.0045	0.0053	0.0024	0.0012
	[2.11]**	[2.19]**	[1.64]	[1.80]*	[0.65]	[0.31]
Post-turnover (+2) $\times Q$	-0.0032	-0.0032	-0.0022	-0.0020	-0.0035	-0.0034
	[-3.06]***	[-3.04]***	[-1.57]	[-1.37]	[-2.26]**	[-2.18]**
Q	0.0048	0.0049	0.0046	0.0045	0.0049	0.0048
	[7.37]***	[6.94]***	[4.58]***	[4.05]***	[5.75]***	[5.28]***
Cash Flow	0.2625	0.2627	0.2553	0.256	0.2703	0.2693
	[18.89]***	[18.89]***	[13.61]***	[13.64]***	[12.99]***	[12.96]***
GDP Growth	0.0299	0.0302	0.0268	0.0265	0.0317	0.0285
	[2.38]**	[2.40]**	[1.77]*	[1.76]*	[1.40]	[1.25]
Sales Growth	0.0067	0.0067	0.0083	0.0083	0.0051	0.0052
	[5.22]***	[5.33]***	[4.78]***	[4.79]***	[2.89]***	[2.91]***
Constant	0.035	0.0345	0.038	0.0371	0.031	0.0327
	[15.89]***	[14.87]***	[13.50]***	[12.46]***	[8.44]***	[8.36]***
Test: $\beta_Q + \beta_{Int} = 0$	0.0017	0.0017	0.0025	0.0025	0.0014	0.0014
t-statistic	[1.65]*	[1.66]*	[1.83]*	[1.87]*	[0.98]	[0.97]
Observations	19,163	19,163	11,982	11,982	7,181	7,181
Between R^2	0.2012	0.2019	0.2485	0.2523	0.1588	0.1580
Fixed Effects	Firm	Firm	Firm	Firm	Firm	Firm
	Year	Year	Year	Year	Year	Year

Table 18
Baseline Investment Regressions: Party Secretary Turnover

This table presents estimation results of the baseline specification for *party secretary* turnovers. The unit of observation is at firm-year level. The dependent variable is the firm-level investment rate defined as CAPX/Lagged Assets. Independent variables include the lagged Tobin's Q, cash flow, province-level real GDP growth rate, sales growth and the *party secretary* turnover period [-1, 0, +1, +2] dummies, with year 0 being the year the actual turnover occurred. See Appendix A for the definition of variables. The first two columns report results for the full sample. The last four columns present results for SOEs and non-SOEs separately. Variables of interests are the four *party secretary* turnover period dummies. We use baseline investment regression and control for firm and year fixed effects. Standard errors are clustered at firm level. T-statistics are reported in square brackets below coefficient estimates. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Full Sample		SOEs		Non-SOEs	
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-turnover year (-1)	-0.0022	-0.0026	-0.0019	-0.0021	-0.0027	-0.0035
	[-1.79]*	[-2.10]**	[-1.23]	[-1.34]	[-1.34]	[-1.68]*
Turnover year (0)	-0.0035	-0.0043	-0.0048	-0.0051	-0.0015	-0.0029
	[-2.79]***	[-3.00]***	[-3.04]***	[-2.94]***	[-0.70]	[-1.18]
Post-turnover year (+1)	0.0003	-0.0005	0.0008	0.0004	-0.0006	-0.0020
	[0.22]	[-0.32]	[0.50]	[0.22]	[-0.25]	[-0.77]
Post-turnover year (+2)		-0.0025		-0.0012		-0.0045
		[-1.74]*		[-0.69]		[-1.91]*
Q	0.0043	0.0043	0.0042	0.0041	0.0045	0.0045
	[6.94]***	[6.88]***	[4.38]***	[4.36]***	[5.51]***	[5.43]***
Cash Flow	0.2625	0.2622	0.2559	0.2558	0.2699	0.2694
	[18.88]***	[18.85]***	[13.64]***	[13.61]***	[12.97]***	[12.98]***
GDP Growth	0.0257	0.0254	0.0230	0.0232	0.0299	0.0272
	[2.04]**	[2.01]**	[1.53]	[1.54]	[1.31]	[1.18]
Sales Growth	0.0067	0.0067	0.0083	0.0083	0.0052	0.0052
	[5.34]***	[5.34]***	[4.78]***	[4.78]***	[2.91]***	[2.91]***
Constant	0.0376	0.0386	0.0408	0.0412	0.0325	0.0347
	[16.83]***	[16.08]***	[14.61]***	[13.85]***	[8.49]***	[8.35]***
Observations	19,163	19,163	11,982	11,982	7,181	7,181
Between R^2	20.09%	20.05%	24.95%	24.90%	15.40%	15.45%
Fixed Effects	Firm	Firm	Firm	Firm	Firm	Firm
	Year	Year	Year	Year	Year	Year

Table 19
Baseline Investment Regressions: Heterogeneity in Type and Politician

The unit of observation is at firm-year level. The dependent variable is the firm-level investment rate defined as CAPX/Lagged Assets. Independent variables include the lagged Tobin's Q, cash flow, province-level real GDP growth rate, sales growth and the turnover period [-1, 0, +1] dummies, with year 0 being the year the actual turnover occurred. To investigate the cross-sectional heterogeneity in turnover types and governor characteristics, we add to our baseline investment regression an interaction term between post-turnover dummy and turnover type, as well as interaction terms between the post-turnover dummy and various governor characteristics such as education level, birth place and age. Variables of interests are the interaction terms. Turnover type is an indicator variable that takes on a value of one if the provincial governor is promoted or moves laterally after his tenure of service. Education is set to one if the provincial governor holds a Master or PhD degree. Birth Place is an indicator variable that takes on a value of one if the new governor will assume office in his home province. Governor Age is set to one if the governor age is in between 55 and 60 at the time of appointment. See Appendix A for the definition of variables. We use the sample of SOEs and control for firm and year fixed effects. Standard errors are clustered at firm level. T-statistics are reported in square brackets below coefficient estimates. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Turnover Type		Education		Birth Place		Governor Age	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-turnover year(-1)		-0.0023		-0.0025		-0.0019		-0.0024
		[-1.54]		[-1.69]*		[-1.32]		[-1.61]
Turnover year (0)		0.0017		0.0013		0.0016		0.0013
		[0.99]		[0.79]		[0.93]		[0.79]
Post-turnover year (+1)	-0.0005	-0.0005	0.0082	0.0085	0.0018	0.0021	-0.0004	-0.0003
	[-0.18]	[-0.20]	[3.73]***	[3.68]***	[1.11]	[1.20]	[-0.17]	[-0.13]
Post-turnover year (+1)	0.0061	0.0066						
× Turnover Type	[1.99]**	[2.15]**						
Post-turnover year (+1)			-0.0082	-0.0084				
× Education			[-2.87]***	[-2.96]***				
Post-turnover year (+1)					0.0096	0.0094		
× Birth Place					[2.82]***	[2.77]***		
Post-turnover year (+1)							0.0063	0.0064
× Governor Age							[2.26]**	[2.31]**
Q	0.0042	0.0041	0.0041	0.0040	0.0042	0.0042	0.0042	0.0042
	[4.43]***	[4.38]***	[4.30]***	[4.26]***	[4.45]***	[4.40]***	[4.42]***	[4.38]***
Cash Flow	0.2562	0.2564	0.256	0.2562	0.2555	0.2557	0.2565	0.2568
	[13.67]***	[13.65]***	[13.65]***	[13.62]***	[13.66]***	[13.63]***	[13.69]***	[13.66]***
GDP Growth	0.0237	0.0234	0.0277	0.0274	0.0263	0.0264	0.0285	0.0282
	[1.57]	[1.54]	[1.86]*	[1.83]*	[1.76]*	[1.75]*	[1.90]*	[1.87]*
Sales Growth	0.0083	0.0083	0.0082	0.0083	0.0083	0.0083	0.0082	0.0082
	[4.78]***	[4.78]***	[4.76]***	[4.77]***	[4.78]***	[4.78]***	[4.73]***	[4.73]***
Constant	0.0385	0.0386	0.0382	0.0385	0.0382	0.0382	0.0379	0.0381
	[14.17]***	[13.87]***	[14.13]***	[13.84]***	[14.13]***	[13.77]***	[14.00]***	[13.72]***
Observations	11,982	11,982	11,982	11,982	11,982	11,982	11,982	11,982
Between R^2	25.02%	25.09%	25.10%	25.16%	25.22%	25.30%	25.02%	25.09%
Fixed Effects	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
	Year	Year	Year	Year	Year	Year	Year	Year

Table 20
Neighboring Provinces

This table tabulates the geographically neighboring provinces for each of the 31 provinces used in our analysis. "# of NP"stands for the number of neighboring provinces and "NP1 - NP8"are the neighboring provinces. The geographically neighboring provinces within the same economic regions as defined in Appendix B are boldfaced.

Province Name	Abbreviation	# of NP	NP1	NP2	NP3	NP4	NP5	NP6	NP7	NP8
Anhui	AH	7 (**4**)	**HEN**	**HB**	**HN**	JS	**JX**	SD	ZJ	
Beijing	BJ	2 (**2**)	**HEB**	**TJ**						
Chongqing	CQ	5 (**3**)	**GZ**	HB	HN	**SN**	**SC**			
Fujian	FJ	3 (**2**)	**GD**	JX	**ZJ**					
Gansu	GS	6 (**6**)	**NM**	**NX**	**QH**	**SN**	**SC**	**XJ**		
Guangdong	GD	5 (**2**)	**FJ**	GX	**HI**	HN	JX			
Guangxi	GX	4 (**2**)	GD	**GZ**	HN	**YN**				
Guizhou	GZ	5 (**4**)	**CQ**	**GX**	HN	**SC**	**YN**			
Hainan	HI	1 (**1**)	**GD**							
Hebei	HEB	7 (**3**)	**BJ**	HEN	NM	LN	**SD**	SX	**TJ**	
Heilongjiang	HL	2 (**1**)	NM	**JL**						
Henan	HEN	6 (**3**)	**AH**	HEB	**HB**	SN	SD	**SX**		
Hubei	HB	6 (**4**)	**AH**	CQ	**HEN**	**HN**	**JX**	SN		
Hunan	HN	6 (**2**)	CQ	GD	GX	GZ	**HB**	**JX**		
Inner Mongolia	NM	8 (**3**)	**GS**	HEB	HL	JL	LN	**NX**	**SN**	SX
Jiangsu	JS	4 (**3**)	AH	**SD**	**SH**	**ZJ**				
Jiangxi	JX	6 (**3**)	**AH**	FJ	GD	**HB**	**HN**	ZJ		
Jilin	JL	3 (**2**)	**HL**	NM	**LN**					
Liaoning	LN	3 (**1**)	HEB	NM	**JL**					
Ningxia	NX	3 (**3**)	**GS**	**NM**	**SN**					
Qinghai	QH	4 (**4**)	**GS**	**SC**	**XZ**	**XJ**				
Shaanxi	SN	8 (**5**)	**CQ**	**GS**	HEN	HB	**NM**	**NX**	SX	**SC**
Shandong	SD	4 (**2**)	AH	**HEB**	HEN	**JS**				
Shanghai	SH	2 (**2**)	**JS**	**ZJ**						
Shanxi	SX	4 (**1**)	HEB	**HEN**	NM	SN				
Sichuan	SC	7 (**7**)	**CQ**	**GS**	**GZ**	**QH**	**SN**	**XZ**	**YN**	
Tianjin	TJ	2 (**2**)	**BJ**	**HEB**						
Tibet	XZ	4 (**4**)	**QH**	**SC**	**XJ**	**YN**				
Xinjiang	XJ	3 (**3**)	**GS**	**QH**	**XZ**					
Yunnan	YN	4 (**4**)	**GX**	**GZ**	**SC**	**XZ**				
Zhejiang	ZJ	5 (**3**)	AH	**FJ**	**JS**	JX	**SH**			

Table 21
Neighboring Province Difference-in-Differences Estimates

This table presents the results of regressions using the neighboring province difference-in-difference methodology. Results reported in Panel A are based on the geographically neighboring provinces defined in Table 20. Panel B further reports estimates using geographically neighboring provinces within the same economic regions (boldfaced regions in Table 20). The dependent variable is firm-level investment defined as CAPX/Lagged Assets. *Treatment dummy* is an indicator variable that takes on a value of one if the firm belongs to the treatment province and zero if it belongs to the control province. *Post-turnover year* is also an indicator variable that is set to one if the firm-year observation falls in the post-turnover year [+1] period, with year 0 being the year the turnover occurred. The variable of interest is the interaction term *Treatment dummy* × *Post-turnover dummy*. The first two columns report results for the full sample. The last four columns present results for SOEs and non-SOEs separately. To save space, we suppress firm and economy control variables. We control for province, industry and year fixed effects. Standard errors are clustered at the firm level and t-statistics are reported in square brackets below coefficient estimates. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Geographically neighboring provinces						
	Full sample		SOEs		Non-SOEs	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment dummy ×	0.0021	0.0017	0.0035	0.0032	-0.0011	-0.0009
Post-turnover year (+1)	[1.61]	[1.27]	[2.07]**	[1.86]*	[-0.51]	[-0.44]
Observations	88159	81786	52783	49786	35376	32000
Adjusted R^2	12.10%	22.85%	17.01%	25.90%	11.03%	22.27%
Firm/Economy Controls	No	Yes	No	Yes	No	Yes
Province/Industry/Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster by firm	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Geographically neighboring provinces within same economic regions						
	Full sample		SOEs		Non-SOEs	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment dummy ×	0.0019	0.0018	0.0036	0.0034	-0.0017	-0.0009
Post-turnover year (+1)	[1.33]	[1.28]	[1.94]*	[1.89]*	[-0.73]	[-0.38]
Observations	62,496	57,904	37,562	35,398	24,934	22,506
Adjusted R^2	11.59%	22.46%	16.50%	25.55%	10.06%	21.44%
Firm/Economy Controls	No	Yes	No	Yes	No	Yes
Province/Industry/Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster by firm	Yes	Yes	Yes	Yes	Yes	Yes

Figure 3. Investment Rates around Turnover Years

The figure depicts average investment rates around turnover event years for all listed firms (red line), SOEs (blue line) and non-SOEs (green line) respectively. Year 0 indicates the calendar year in which governor turnover event occurs.

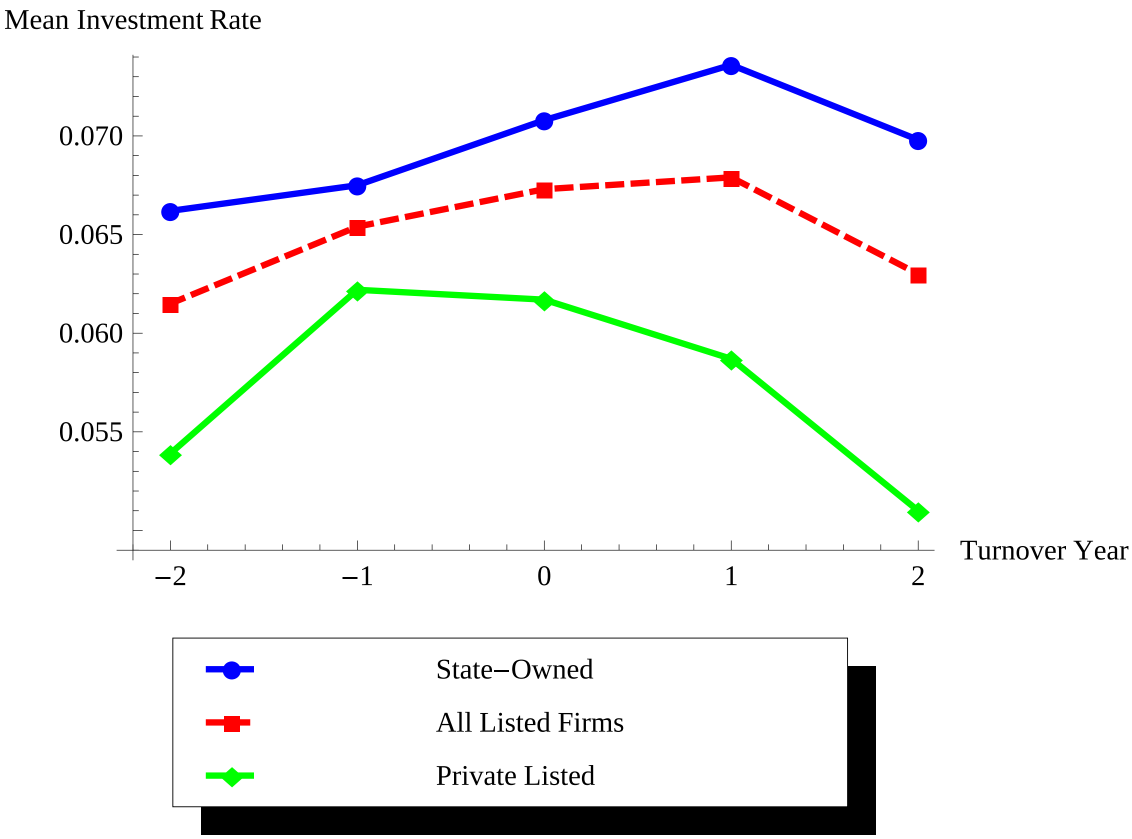

Figure 4. Frequency Distribution of Provincial Governor Turnovers by Province

Appendix A: Variable Descriptions for Chapter 1

Variable	Definition	Source
Innovation output measures		
# Patent	Natural logarithm of one plus the patent count. Patent count is defined as number of patent applications filed in year t of each firm. Only patents that are later granted are included. This variable measures innovation quantity and is corrected for truncation bias following Hall, Jaffe, and Trajtenberg (2001). The patent count is set to zero for companies that have no patent information available from the NBER patent database.	NBER Patent Database
# Citation	Natural logarithm of one plus the citation count. Citation count is defined as number of citations received by patent applications filed in year t of each firm. The citation number is corrected for the truncation bias in citation counts using the Hall, Jaffe, and Trajtenberg (2001) adjustment factor. This variable measures patent quality. Only patents that are later granted are included. The citation count is set to zero for companies that have no citation information available from the NBER patent database.	NBER Patent Database
Gubernatorial elections		
Pre-election (-1)	Indicator variable takes on a value of one for the year prior to a gubernatorial election occurred in that state.	CQE Library
Election (0)	Indicator variable takes on a value of one if a gubernatorial election occurred in that state in that year.	CQE Library
Post-election (+1)	Indicator variable takes on a value of one for the one-year period after a gubernatorial election occurred in that state.	CQE Library
Post-election (+2)	Indicator variable takes on a value of one for the two-year period after a gubernatorial election occurred in that state.	CQE Library
Republican (R)	Indicator variable set equal to one if the governor is a Republican in year t.	CQE Library
Democrat (D)	Indicator variable set equal to one if the governor is a Democrat in year t.	CQE Library
D2D dummy	Indicator variable set equal to one if both the incumbent governor and the successor are Democrats.	CQE Library
D2R dummy	Indicator variable set equal to one if the incumbent governor is a Republican and the successor is a Democrat.	CQE Library
R2R dummy	Indicator variable set equal to one if both the incumbent governor and the successor are Republicans.	CQE Library
R2D dummy	Indicator variable set equal to one if the incumbent governor is a Democrat and the successor is a Republican.	CQE Library
Close election	Indicator variable set equal to one if the victory margin, defined as the vote difference between the first place candidate and the second place candidate, is less than 5%. I classify this type of elections as high uncertainty elections.	CQE Library
Incumbent absence	Indicator variable set equal to one if the incumbent governor does not seek re-election due to reasons other than term-limit expiration (such as retired or defeated in primary or simply not running for re-election). I identify elections with absence of incumbent as high uncertainty elections.	CQE Library
Party change	Indicator variable set equal to one if the incumbent governor and the successor have different party affiliations. I identify elections with party change as high uncertainty elections.	CQE Library
Control variables		
Asset	Defined as natural logarithm of the book value of total assets (AT from COMPUSTAT) measured at the end of fiscal year t.	COMPUSTAT
Continued on next page		

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Variable	Definition	Source
Age	Defined as natural logarithm of one plus the number of years of the corporation has existed from the IPO year to year t.	COMPUSTAT
ROA	Defined as operating income before depreciation (OIBDP from COMUSTAT) divided by book value of total asset (AT), measured at the end of fiscal year t.	COMPUSTAT
Tobin's Q	Defined as [the market value of equity (PRCC_F $\times$ CSHO from COMUSTAT) plus book value of assets (AT) minus book value of equity (CEQ from COMUSTAT) minus balance sheet deferred taxes (TXDB from COMUSTAT)] divided by book value of asset (AT), measured at the end of fiscal year t.	COMPUSTAT
Cash	Firm's cash flows. It is defined as income before extraordinary items (IB from COMUSTAT) plus depreciation and amortization (DP from COMUSTAT) divided by lagged PPE (PPENT from COMUSTAT), measured at the end of fiscal year t.	COMPUSTAT
Leverage	Firm's leverage ratio. It is defined as book value of debt (DLTT+DLC from COMUSTAT) divided by book value of total assets (AT) measured at the end of fiscal year t.	COMPUSTAT
R&D expense	Firm's research and development expenditure. It is defined as research and develop expenditure (XRD from COMUSTAT) divided by book value of lagged PPE (PPENT), measured at the end of fiscal year t.	COMPUSTAT
CAPEX	Firm's capital expenditure. It is defined as capital expenditure (CAPX from COMUSTAT) divided by book value of lagged PPE (PPENT), measured at the end of fiscal year t.	COMPUSTAT
Herfindahl Index (HHI)	Herfindahl index of 3-digit SIC industry of each firm measured at the end of fiscal year t based on sales.	COMPUSTAT
GDP Growth	State level annual GDP growth rate, obtained from Bureau of Economic Analysis (BEA).	BEA
Politically sensitive industries (PSI)	Indicator variable set equal to one if firms fall into the following industries: Beer (4), Smoke (5), Guns (26), Gold (27), Mines (28), Coal (29), and Oil (30), where industry classifications are based on Fama French 48 industries.	Kostovetsky (2009)
Regulated industries	Indicator variable set equal to one for firms that belong to finance and utility industries: Utility (31), Banks (44), Insurance (45), Real Estate (46), and Trading (47), where industry classifications are based on Fama French 48 industries.	Dai and Ngo (2014)
KZ dummy	Indicator variable set equal to one for firms with above median KZ index of Kaplan and Zingales (1997) in year t. This variable is used as a proxy for the extent of financial constraint. Firms with above median KZ index are considered as financially constrained.	Kaplan and Zingales (1997)
Dividend payer	Indicator variable set equal to one for firms with dividend payment in year t.	COMPUSTAT
Difficult to innovate	Indicator variable set equal to one for firms that belong to pharmaceutical, medical instrumentation, chemicals, computers, communications, and electrical industries. The classification is based on 3-digit SIC codes (see Hall, Jaffe, and Trajtenberg (2005) and Tian and Wang (2011)).	Hall et al. (2005)
High-tech firm	Indicator variable set equal to one for firms that belong to biotech, computing, computer equipment, electronics, medical equipment, pharmaceuticals, and software industries. The classification is based on 3-digit SIC codes as defined in Hall and Lerner (2009).	Hall and Lerner (2009)

Appendix B: Variable Descriptions for Chapter 2

Variable	Definition	Source
Investment	Capital expenditure divided by beginning-of-year book value of total assets (lagged total assets).	CSMAR
Tobin's Q	Book value of total assets minus book value of equity plus market value of equity scaled by book value of total assets.	CSMAR
Cash Flow	EBIT plus depreciation and amortization minus interest expense and taxes scaled by beginning-of-year book value of total assets.	CSMAR
Sales Growth	Firm level annual sales growth rate.	CSMAR
GDP Growth	Annual provincial real GDP growth rate.	Wind
SOE Dummy	Indicator variable set equal to one if the ownership type of the listed firm is state-owned.	CSMAR
Pre-turnover Year (-1)	Indicator variable takes on a value of one if the firm-year-province pair falls in the period of one year immediately before the turnover year.	Hand collected
Turnover Year (0)	Indicator variable takes on a value of one if the firm-year-province pair falls in the period of the turnover year.	Hand collected
Post-turnover Year (+1)	Indicator variable takes on a value of one if the firm-year-province pair falls in the period of one year immediately after the turnover year.	Hand collected
Post-turnover Year (+2)	Indicator variable takes on a value of one if the firm-year-province pair falls in the period of two year immediately after the turnover year.	Hand collected
Turnover Type Dummy	Indicator variable set equal to one if the provincial governor is promoted or moves laterally after his tenure of service.	Hand collected
Education Dummy	Indicator variable set equal to one if the provincial governor holds a Master or PhD degree.	Hand collected
Birth Place Dummy	Indicator variable set equal to one if the immediate successor will assume office in his home province.	Hand collected
Governor Age Dummy	Indicator variable set equal to one if the governor age is in between 55 and 60 at the time of appointment.	Hand collected

Appendix B: Information on Eight Economic Regions

Economic Region	Number	Provinces/Municipalities/Autonomous regions
Northeast Economic Region	3	Heilongjiang, Jilin and Liaoning
Northwest Economic Region	5	Gansu, Qinhai, Ningxia, Tibet and Xinjiang
Southwest Economic Region	5	Guangxi, Yunnan, Guizhou, Sichuan, Chongqing
Central Economic Region	4	Hunan, Hubei, Jiangxi and Anhui
Southern Coastal Economic Region	3	Guangdong, Fujian and Hainan
Eastern Coastal Economic Region	3	Shanghai, Jiangsu and Zhejiang
Northern Coastal Economic Region	4	Shandong, Hebei, Beijing and Tianjin
Middle Reach of Yellow River Economic Region	4	Shaanxi, Henan, Shanxi and Inner Mongolia