Does political corruption impede firm innovation?

Evidence from the United States

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Abstract

We examine how local political corruption affects firm innovation in the United States. We find that firms located in more corrupt districts are less innovative, as measured by their patenting activities. We identify two possible economic channels through which corruption may affect innovation: a disincentive effect and a culture effect. We show that the negative impact of corruption on innovation is stronger for firms that have weaker bargaining power against corrupt officials and for firms that locate in areas with lower local religiosity. Overall, our results indicate that local political corruption impedes corporate innovation.

JEL Classification: G31, G38, D72

Key Words: political corruption, innovation, patent, citation, R&D, culture

1. Introduction

It is well acknowledged that entrepreneurship and innovation are key driving forces of economic development (Solow, 1957; Romer, 1990; Chang et al., 2015; Kogan et al., 2017). Though much effort has been devoted to exploring factors determining innovation, the impact of politics is less studied. In particular, political corruption has been shown to significantly affect economic growth (see Mauro, 1995; Jain, 2001), but relatively less is known about how it affects a firm's innovation, especially in developed countries like the United States. Given the prevalence of political corruption and the importance of innovation in a firm's long-term growth, in this paper, we examine the impact that local political corruption in the United States has on a firm's innovation.[1]

There is a substantial amount of research in economics examining the effect of corruption on the real economy; most focuses on macroeconomic outcomes. This literature has suggested two competing hypotheses. The *sanding wheel hypothesis* says that corruption is inefficient to the economy because it imposes an extortionary tax that can lead to distorted investments and misallocated resources (Shleifer and Vishny, 1993; Murphy et al., 1991, 1993). On the other hand, the *greasing wheel hypothesis* argues that corruption promotes efficiency by helping firms cut through bureaucratic ties (Leff, 1964; Leys, 1965). The *sanding wheel hypothesis* is particularly well supported by empirical evidence. For example, studies show that corruption hurts school enrollment and human capital accumulation (Reinikka and Svensson,

[1] In our sample, more than 14,000 US government officials were convicted of acts of official corruption between 1990 and 2005.

2005), reduces investments and economic growth (Murphy et al., 1991; Mauro, 1995; Wei, 1999), and increases inefficient public spending (Tanzi and Davoodi, 1997).[2]

We hypothesize that political corruption impedes firm innovation and propose two possible mechanisms by which it does so: the disincentive effect and the culture effect. Murphy, Shleifer, and Vishny (1993) give several reasons entrepreneurs and innovating firms are more likely to be targeted by corrupt officials. First, innovators typically have high and inelastic demand for government-supplied goods such as licenses and permits and thus have to engage in more interactions with government officials who may have the power to extort the firm. Second, politicians are more likely to target innovators because their innovations may hurt the interests of politically connected incumbent firms. Further, most innovative projects are long term, which provides corrupt government officials with more rent-seeking opportunities. Consistent with these arguments, Ayyagari, Demirguc-kunt, and Maksimovic (2014) show that innovators pay more bribes than non-innovators in developing countries but do not receive better services, suggesting that political corruption acts as a tax on innovating firms by increasing their costs. In addition, Murphy et al. (1993) emphasize that innovative projects have high tail risk, which makes them particularly vulnerable to ex post rent seeking. Entrepreneurs and innovators may need to share the rents with corrupt officials when the innovative projects succeed, but they have to bear the whole cost when the projects fail. For these reasons, we conjecture that higher innovation costs and higher risk of ex post rent seeking

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[2] In contrast, Egger and Winner (2005) and Levy (2007) show that corruption stimulates beneficial trades and improves efficiency. It has also been argued that corruption can lead to a more efficient allocation of licenses and government contracts (Lui, 1985; Lien, 1986). See Svensson (2005) for a review of the literature.

resulting from local political corruption would reduce innovators' ex ante incentives to innovate. We call this effect the disincentive effect.

Political corruption may also influence firm innovation through the culture effect. Literature in both sociology and political science has shown that quality of government affects people's perceptions of the trustworthiness of others and that public corruption decreases social trust (Brehm and Rahn, 1997; Uslaner, 2004; Rothstein and Stolle, 2008, Rothstein and Eek, 2009; Richey, 2010).[3] This means that corruption can form as a local culture that shapes individuals' behaviors.[4] Because innovation largely relies on collaboration among innovators (Dovey, 2009) and successful collaboration hinges on trust among all parties involved (Xie et al., 2017), we expect that lower social trust caused by a corrupt local culture would lead to lower innovation efficiency. In other words, even when firms have high incentives to innovate, lower social trust can lead to less or poor collaboration among innovators and consequently lower innovation.

To test our hypotheses, we use a large sample of US-listed firms over the period 1990–2006. Following prior literature (Glaeser and Saks, 2006; Butler et al., 2009; Smith, 2016; Brown et al., 2016), we use the number of corruption convictions of public officials per 1,000 capita at the location of a firm's headquarters as a proxy for local corruption. Our baseline results show that the level of local corruption is negatively related to local firms' innovation output, as measured by the number of patents and the average number of citations per patent.

[3] See Nannestad (2008) for a review on social trust.

[4] Several recent papers have found evidence consistent with this argument. See Fisman and Miguel (2007), Parsons et al.(2016), Liu (2016), and Dass et al. (2017).

This relation is both statistically and economically significant. For example, firms located in districts that are above the mean value of per capita corruption have 7% fewer patents and 7.8% fewer citations per patent than firms located in other areas. We find consistent results when using alternative innovation measures, including the dollar value of patents and new product announcements. Overall, the results are consistent with our hypothesis that public corruption impedes innovation.

We next take steps to address the endogeneity concern. The main challenge to our identification is that the level of local corruption can be correlated with some omitted variables that also affect a firm's innovation activities. In addition, our results might stem from reverse causality: in regions where firms lack innovation capabilities, they may actively engage in some rent-seeking activities to secure their economic rents, which leads to more public corruption in the local area. Finally, our results can be biased if there are nontrivial measurement errors in our variable of interest. To mitigate these concerns, we conduct several robustness tests. First, we include a variety of control variables that may covariate with both local corruption and firm innovation. Second, we employ a Two-Stage-Least-Square (2SLS) regression analysis where we take advantage of the fact that some states adopted strong Freedom of Information Act (FOIA) laws during our sample period. Finally, we use alternative measures of local political corruption. Our findings are all robust and consistent with our hypothesis, lending credence to the idea that corruption has a causal and negative impact on firm innovation.

In further analysis, we explore the underlying economic channels through which political corruption impedes firm innovation. Consistent with our disincentive-effect hypothesis, we find that corruption reduces a firm's investment in innovation input in the first place. Moreover, we find that the negative impact of political corruption on both innovation input and output is stronger for firms with more concentrated operations in the states where their headquarters are located. This is because these firms have less flexibility to allocate their resources to different places and thus less bargaining power against local corrupt officials (Bai et al., 2013). We also find that, due to lower innovation incentives, firms in more corrupt areas undertake less risky innovation projects. Finally, we show that the negative impact of corruption is weaker for firms located in areas with higher religiosity. We interpret this result as consistent with the culture effect channel. Because religion helps foster the social trust that is important for efficient collaboration among innovators, our findings suggest that corruption may lower social trust among people but to a lesser degree for people with high religiosity.

This paper is not the first to examine the relationship between political corruption and corporate innovation.[5] Some cross-country evidence exists showing that corruption impedes innovation and entrepreneurship in emerging countries (Paunov, 2016; Anokhin and Schulze, 2009). However, there is a large heterogeneity in the quality of political and legal institutions from one country to another. For instance, more corrupt countries tend to have lower-quality institutions that are likely to affect firms' innovation activities negatively (Jain, 2001; Hsu et

[5] A contemporaneous paper by Ellis et al. (2017) also examines the impact of corruption on innovation. Their main finding that political corruption hurts innovation is consistent with ours. However, we address the endogeneity issue further than Ellis et al. and explore the underlying mechanisms of how corruption affects innovation.

al., 2014; Brown et al., 2013). Unlike these studies, we focus on a single country and therefore provide a better setting to identify the real effect of corruption on innovation. Moreover, findings in emerging markets may not be applicable to the United States, which has the largest economy in the world, is a leading nation in technological innovation, and has a relatively uncorrupt government. Finally, compared with survey-based corruption measures used by Paunov (2016) and Anokhin and Schulze (2009), we use a proxy for corruption based on ex-post and traceable corruption-related convictions. Therefore, it has less subjective bias or measurement errors.

Our paper contributes to the literature in several ways. First, we contribute to the substantial literature on innovation. Prior studies have identified many determinants of corporate innovation productivity, including credit market conditions (Benfratello et al., 2008; Achaya et al., 2013; Cornaggia et al., 2015; Hombert and Matray, 2017), financial analyst coverage (He and Tian, 2013), venture capital (Chemmanur et al., 2014; Colombo et al., 2016), investors' attitudes toward failure (Tian and Wang, 2014), employee stock options (Chang et al., 2015), stock liquidity (Fang et al., 2014), financial constraint (Brown et al., 2012), product market competition (Aghion et al., 2005), and institutional investors (Aghion et al., 2013; Brav et al., 2016). However, only a few studies explore the effects of institutional features on firm innovation, such as tax rate (Mukherjee et al., 2017), bankruptcy and labor laws (Acharya and Subramanian, 2009; Acharya et al. 2014), financial development (Ayyagari et al., 2011; Hsu et al., 2014) and political uncertainty (Cumming et al., 2016; Bhattacharya et al., 2017). We add to this literature by considering another institutional feature as an important factor in the innovation-generating process.

Our paper also contributes to the large body of research on the effect of corruption.[6] Many studies in economics have shown that political corruption affects macroeconomic outcomes (Leff, 1964; Leys, 1965; Mauro, 1995; Wei, 1999; Globerman and Shapiro, 2003; Egger and Winner, 2005). Only a few studies have examined the impact of corruption on firm outcomes and policies. For example, Beck, Demirgûç-Kunt, and Maksimovic (2005) and Fisman and Svensson (2007) find that corruption constrains firm growth. More recently, Smith (2016) finds that US firms located in more corrupt districts tend to hold less cash and borrow more to shield themselves from extortion by rent-seeking politicians. Brown, Smith, White, and Zutter (2016) find that higher levels of local corruption are associated with lower firm value measured by Tobin's Q, and they argue that corruption destroys value by pulling down firm investment efficiency. We add to this literature by providing additional evidence for the effect of corruption on individual firms. Since innovation is critical to economic growth, our study sheds more light on how corruption influences economic development.

The rest of the paper is organized as follows. Section 2 describes our data and sample construction. Section 3 presents our empirical results. Section 4 concludes.

[6] The consequences of political corruption, political connections, and lobbying activities are distinct in the literature. Most studies show political corruption has a negative effect on an economy but political connection and lobbying a positive one. See Faccio et al. (2006), Claessens et al. (2008), Houston et al. (2014), Ovtchinnikov, et al. (2014), Feng et al. (2015), Borisov et al. (2016) and Pan and Tian (2017).

2. Data and variables

To construct our sample, we start with all US firms covered by COMPUSTAT during the period 1990–2006. This restriction is chosen due to the availability of patent, firm location, and corruption data. We exclude financial firms (SIC codes 6000–6999), utilities (SIC 4900–4999), public sectors (SIC 9000–9999), and firms with headquarters outside the United States from our sample.

We follow the literature and use the annual number of corruption convictions of public officials in each federal judicial district as our baseline measure of local corruption in the US (Glaeser and Saks, 2006; Butler et al., 2009; Campante and Do, 2014; Smith, 2016).[7] The data, obtained from the Report to Congress by the US Department of Justice's Public Integrity Section (PIN), has been public since 1990. Following earlier studies, we scale the raw number of convictions by the total population of each judicial district (per 100,000). For the very few cases in which the conviction number is missing from the raw data, we use the average conviction rate of the prior year and the following year to impute the missing values. Overall, a higher conviction rate of public officials per capita indicates a higher level of local corruption. Using the mapping file from the Department of Housing and Urban Development, we then merge this data with COMPUSTAT based on the FIPS code of each federal judicial district[8] and the zip code of each firm's historical headquarters location.[9] Since COMPUSTAT only

[7] The corruption convictions mainly include bribery, extortion, election crimes, and criminal conflicts of interest. Most are prosecuted in the federal judicial system, and the data should suffer less from the enforcement concern. For more detail, see Glaeser and Saks (2006).

[8] Half of the US states (25) have more than one federal judicial district. For these states, we manually check each district's official website and assign each county in each state to its corresponding district.

[9] Since two counties may share a common zip code, we require *tot_ratio*=1 (the fraction of the county residence or business institutions that use the zip code) to ensure a unique match between zip and FIPS.

provides a firm's current headquarters location, we manually collect the firm's historical business address from its 10-K filings to identify its historical headquarters location. For firms with no electronic filings before 1994, we set the location equal to the earliest available location.[10]

To measure innovation productivity at the firm level, we use firm-year patent data from the latest version of the National Bureau of Economics Research (NBER) Patent database created by Hall, Jaffe, and Trajtenberg (2001). This dataset provides annual information on patent assignee names, number of patents, number of citations received by each patent, patent application year, and patent grant year during the period 1976–2006. Following the innovation literature, we construct two measures of a firm's innovation productivity: the number of patents a firm applied for in a year that are eventually granted, which measures the quantity of a firm's innovation activity, and the average number of citations a patent received in subsequent years, which captures the quality of a firm's innovations. For firms without any patent or citation information from the NBER dataset, we set the patent and citation counts to zero.

As described in Hall, Jaffe, and Trajtenberg (2001, 2005), there are two truncation problems associated with the raw patent data. First, on average there is a two-year time gap between application date and grant date, so patents applied for but not yet granted may not be included in the data. Second, we only observe patent citations through 2006, even though patents may continue to receive citations afterwards. To address these truncation issues, we follow Hall, Jaffe, and Trajtenberg (2001, 2005) to adjust the patent numbers using the

[10] Prior literature suggests that headquarters relocation is very rare.

empirical application-grant distribution and to adjust the citation numbers using the citation-lag distribution.[11]

We obtain firm characteristics from COMPUSTAT and CRSP and region characteristics from the US Census Bureau, Bureau of Labor Statistics, Bureau of Economic Analysis, and Association of Religion Data Archives. Following prior literature, we keep firms that have nonzero sales, nonzero tangible assets, and total assets. Table 1 provides the summary statistics of our main sample. Detailed definitions of the variables are given in the Appendix. All financial variables are adjusted to the dollar value in 2000 using CPI data from the Bureau of Labor Statistics. To mitigate the influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. On average, a firm in our sample has 3.75 granted patents per year, and each patent has 3.09 citations. An average firm has log size of 5.4, R&D-to-assets ratio of 5%, ROA of 5%, PPE-to-assets ratio of 0.27, book leverage of 0.23, and Tobin's Q of 2.02.

In Table 2, we present the Top 10 districts by corruption and innovation, respectively.[12] As shown in Panel A, the most corrupt district is the District of Columbia (DC), which accords with the fact that the District is the US capital city with the largest number of politicians but a small population base. Turning to Panel B, we find that Northern California and Massachusetts are the two most innovative districts, consistent with the patent regional heterogeneity

[11] We also try ending the sample period before 2004 to alleviate the application-grant lag issue. The results are the same and are available upon request. The NBER patent dataset provides the adjustment weights (*hjtwt*) for the citations.
[12] To construct the innovation ranking, we use the aggregate number of patents in a district. The ranking is similar if we use patent citations.

described in Lerner and Seru (2015). To ensure that our results are not driven by these outlier districts, we will exclude them in our robustness tests later.

3. Empirical results

In this section, we first present the results from our baseline specification. We find a strong negative association between local political corruption and corporate innovation. We then address some potential concerns with our identification. All results support our argument that local political corruption impedes firm innovation.

3.1 Baseline results

To examine whether local political corruption affects US firms' innovation activities, we run the following OLS regressions:

$$Innovation_{i,j,t} = a + \beta\ Corruption_{j,t-1} + \gamma Controls_{t-1} + Ind + year + \varepsilon_{i,t}$$

The dependent variable in this model is our measure of innovation productivity for firm i in district *j* in given year *t*. All time-varying independent variables are lagged by one year.[13] The explanatory variable of interest is $Corruption_{j,t-1}$, the scaled conviction rate of public officials in district *j* in year *t* – 1. If political corruption impedes firm innovation, β should be negatively significant. We follow the existing literature to control for a number of known determinants of a firm's innovation activities, including firm size, R&D expense (R&D)[14], firm profitability (ROA), asset tangibility (PPE), leverage, capital expenditure, growth opportunity (Tobin's Q), financial constraint (KZ index), product market competition (HHI and the square

[13] The results are similar if we use three-year lagged values for the independent variables.
[14] Following prior studies, we code the missing R&D as zero. The results are similar if we exclude these observations.

of HHI) and firm age (Brown et al., 2012; Aghion et al., 2005; Fang et al., 2014; Tian and Wang, 2014). We also include both industry fixed effects to control for unobserved, time-invariant heterogeneity across industries and year dummies to control for aggregate time trends.[15]

Table 3 presents our baseline regression results. Since local corruption is measured at the district level, we cluster standard errors by district (Petersen, 2009).[16] The dependent variable is the natural logarithm of one plus the number of patents in the first two columns. Results in column (1) show that the coefficient estimate for our corruption measure is negative and statistically different from zero at the 1% level. To interpret the economic magnitude more intuitively, we define a dummy variable, *Highcorruption*, which equals one if a firm locates in a district with a corruption conviction rate above the average value for the year and zero otherwise. From results in column (2), we find that firms located in these more corrupt districts are 7.1% less innovative in terms of patent number. When examining patent citations in columns (3) and (4), we find consistent results. Patents generated by firms located in the more corrupt districts have 8.4% fewer citations than those generated by other firms. Unreported tests show similar results when we examine citations using only firm-years with at least one granted patent or excluding all self-citations.

The estimated coefficients of other control variables are largely consistent with prior literature. For instance, larger and older firms, firms with more R&D expense, higher

[15] We use Fama and French 12-industry classification.
[16] District-level clustering renders ours a relatively conservative inference. Our results are the same, if not stronger, when we use firm-level clustering.

profitability, more capital expenditures, and more growth opportunities are more innovative. Meanwhile, firms with high leverage and more tangible assets are less innovative.

3.2 Alternative innovation measures

3.2.1 Innovation input

Results in Table 3 show that firms located in more corrupt districts have lower innovation productivity after we control for innovation input—that is, R&D expenditure. If political corruption has a disincentive effect on firm innovation, we should also observe a significant negative relation between corruption and innovation input.[17]

Table 4 shows the regression results exploring the relation between corruption and R&D investment. Here R&D is measured at one-year lead value. In column (1), we use the full sample. As expected, the estimated coefficient on corruption is negative and highly significant. Since Koh and Reed (2015) find that nonreporting R&D firms have different patenting activities from firms that report zero R&D, in column (2), we exclude all observations with missing R&D. The sample size is reduced, but the results remain the same. In column (3), we further exclude observations with zero R&D and find the estimated effect virtually unchanged. Unreported tests show similar results when we scale R&D investments by total sales. Overall, the results are consistent with our argument that corruption reduces firms' incentive to innovate in the first place.

[17] The culture effect on innovation input is not obvious. Poor collaboration resulting from corrupt local culture may lead to fewer innovative ideas among innovators and thus less funding needs. On the other hand, inefficient collaboration may lead to more R&D expenses for the same innovation project.

3.2.2 Alternative innovation output measures

We also repeat the analysis of Table 3 using alternative measures of innovation output. The results are presented in Table 5. First, we construct two innovation efficiency measures following Hirshleifer, Hsu, and Li (2013). These variables allow us to conduct a preliminary test for the proposed culture-effect hypothesis. If corruption indeed deteriorates social trust and reduces a firm's innovation efficiency, then the innovation output per unit of R&D should be lower for firms located in more corrupt areas.[18] To measure innovation efficiency, we use patents granted, scaled by accumulated R&D capital in the previous five years and assuming an annual depreciation rate of 20% (*IE_Patents*) and adjusted patent citations over the previous five years, scaled by total R&D expense (*IE_Citations*). Results are reported in columns (1) and (2), where the dependent variable is the natural logarithm of one plus the innovation efficiency measure. The sample size is reduced because we exclude all firm-year observations where the firm has no R&D expenditures over the previous five years. As we can see, the coefficient estimates on the corruption measure again are negative and statistically significant, suggesting that local political corruption negatively affects a firm's innovation efficiency.

We also examine whether employees in firms located in different districts have different innovation productivity. If innovators have less incentive to collaborate and innovate, they will have lower innovation productivity. To test this hypothesis, we follow Acharya et al. (2014) and estimate the number of patents per 1,000 firm employees (*Log (1+Patents/Employees)*).[19]

[18] This may also be consistent with the disincentive channel. When firms have low R&D spending, productive innovators may have fewer incentives to innovate because the project funding is too limited or they believe it is difficult to request more R&D funding in the future.

[19] Because not all employees in a firm are involved in the innovation process, this is an imperfect innovation

Column (3) of Table 5 shows that for firms located in more corrupt districts, their employees have lower innovation productivity inside the firm.

In addition to the number of citations, another frequently used measure of patent quality is the patent's originality and generality (Hall, Jaffe, and Trajtenberg, 2001). Patents are considered as having higher originality if they cite more unique technology classes of patents, while patents that are cited by a wider set of patent technology classes are viewed as having greater generality. Following the literature, we calculate a patent's originality score as one minus the Herfindahl index of patents it cites across different three-digit technology classes. A patent's generality score is calculated as one minus the Herfindahl index of the technology classes of all the patents that cite it. Columns (4) and (5) show that both the originality and generality scores of patents are lower for patents generated by firms in more corrupt districts.

Kogan, Seru, and Stoffman (2017) argue that patent citations may not fully capture the economic importance of each innovation and propose a quality measure using the stock market responses to news about patent grants.[20] They find their measure is a good predictor of further forward citations and contains additional information relative to citations-weighted patent counts. In Table 5 column (6), we use their market-value-based proxy (*Log (1+Dollar value)*) as an alternative measure of innovation quality. As expected, firms located in more corrupt districts generate patents with lower dollar value.

Finally, in addition to patents we use a firm's new product development as another innovation proxy. As suggested by Hall, Helmers, Rogers, and Sena (2014), a firm may face a

productivity measure.

[20] We thank the authors for sharing the data.

trade-off between protecting intellectual property and trade secrets; it thus may partially refrain from formally filing some patents. In such a case, innovation measures derived from observed patent grants may not accurately reflect the degree of a firm's innovativeness. Mukherjee, Singh, and Žaldokas (2017) thus construct a proxy for innovation using stock market reaction to the news of new product development. In column (7), we use the number of new product announcements with three-day cumulative abnormal returns above the 75th percentile (*Log (1+New product)*) from Mukherjee et al. (2017) as the dependent variable. We find that the number of major new product introductions is significantly lower for firms located in more corrupt districts. Taken together, all results from Table 5 suggest that firms in more corrupt areas are less innovative.

3.3 Potential identification problems and robustness tests

3.3.1. Endogeneity concerns

Like most empirical research into corporate finance, our study is subject to endogeneity concerns. We now discuss these potential identification problems and propose some remedies. First, our results may be driven by omitted underlying local conditions that are correlated with both corruption and corporate innovation. For instance, areas with a better local economy or higher education level may have lower public corruption and more innovators (Glaeser and Saks, 2006). Another type of endogeneity issue that our analysis is subject to is reverse causality. In particular, it is possible that firms with a lack of innovation may actively engage in rent-seeking activities to secure economic rents. Such activities can lead to more local public corruption. Moreover, our results might also suffer from nontrivial measurement errors because data on federal corruption convictions is aggregated and sparse (Cordis and Milyo, 2016). To mitigate these endogeneity concerns, we conduct more tests in the following subsections.

3.3.2. Additional controls

In Table 6, we add a set of fixed effects to mitigate the concern that unobservable geographical characteristics may drive our results. For instance, certain states may have greater support for innovation-related investment and at the same time have lower corruption. To mitigate such concern, in columns (1) and (3) we add state-year fixed effects to control for unobserved and time-varying heterogeneity across states that may affect corruption and innovation simultaneously. Our results remain the same. In columns (2) and (4) we include industry-state-year dummies and compare firms in the same industry, state, and year but different judicial districts. The results are also similar. This helps alleviate the concern that our findings are purely driven by unobservable state-level factors.

In Table 7, we control for a set of region-level factors that may affect both local corruption and innovation. These controls include local income level, unemployment rate, education attainment and government size.[21] In addition, Campante and Do (2014) find that isolated capital cities are associated with more state corruption due to less oversight by the electorate. Because geographic proximity is important for knowledge spillovers, we control for the isolation of the state capital city in the regressions as well (Glaeser et al, 1992). Results in columns (1) and (3) show that the estimated coefficients on corruption are both negative and

[21] Education attainment is measured at the state level because the US Census Bureau doesn't provide the data at the county level.

statistically significant. In columns (2) and (4) we add more firm-level controls that have been shown to be related to innovation, including institutional ownership (Aghion et al., 2013), stock liquidity (Fang et al., 2014), and marginal tax rate (Mukherjee et al., 2017).[22] Though the sample size is diminished due to data availability, the estimated coefficients on corruption are persistently negative and statistically significant. Overall, our results are robust to the inclusion of a large set of controls.

3.3.3 The instrumental variable approach

We next adopt an instrumental variable approach to further address the endogeneity concern. One source of valid instruments is theories on the causes of corruption. For example, Mauro (1995) argues that ethnic fragmentation spurs local political corruption because political officials are more likely to extort groups that are of different races to maintain the controlling power of their own race group. Glaeser and Saks (2006) later use data on convictions and find consistent results. Following that, several papers have used ethnic diversity to instrument local corruption. Unfortunately, because it is likely to violate the exclusion restriction, this instrument cannot be employed here. For instance, many studies have shown that ethnic diversity brings firms new ideas and perspectives from different cultural backgrounds and therefore facilitates innovation (Nathan and Lee, 2013; Parrotta, et al., 2014; Lee, 2014).[23]

We construct our instrumental variable based on the fact that different states have different state-level FOIA laws. Since FOIA laws allow the pubic to access information about

[22] We use the simulated marginal tax rate before interest expense at the firm level, which was developed by Graham and Mills (2008). We thank the authors for generously sharing their data.

[23] Our results are quantitatively similar if we control for ethnic fragmentation in our regressions.

government activities more easily, it is argued that these laws make it more difficult for corrupt public officials to escape public scrutiny. Cordis and Warren (2014) find evidence consistent with this argument. Specifically, they assign a FOIA score to each state in each year to quantify the degree of the freedom of information from 1986 to 2009. A state is classified as a weak FOIA state if its FOIA score is six or less and as a strong FOIA state if its FOIA score is above six.[24] They show that for states that transition from weak to strong FOIA laws, the number of corruption convictions increases in the short term, due to the higher probability that corruption is detected, and then decreases in the long term—seven years after the FOIA law's enactment—when the reduction in corruption offsets the detection effect. We thus argue that for these states, the law's transition can be used as an instrument for corruption. Based on Cordis and Warren (2014), we use seven years as the cutoff and define our instrumental variable as a dummy variable that takes the value of one if a firm is headquartered in a state that has transitioned from weak to strong FOIA laws at least seven years ago.[25] We expect this instrument to meet the relevance condition in that it captures the negative impact of FOIA laws on corruption (which is reflected in the lower conviction rate). In the meantime, this variable is unlikely related to firm innovation other than through its effect on the local government, especially after we control for the aggregate time trend. We then use the sample firms locating in the FOIA transition states and run the 2SLS analysis.

The 2SLS regression results are presented in Table 8. In column (1), we present the results

[24] The detailed definition and value of the FOIA score for each state is available in Cordis and Warren (2014).
[25] As argued by Cordis and Warren (2014), since the FOIA laws can affect both the real corruption level and the probability of corrupt behavior to be detected, the relation between FOIA score and corruption does not show strong patterns. Our results are also not affected by including FOIA score in the regressions.

of the first-stage regression. As expected, our instrument is negatively and significantly associated with local public corruption. First-stage F-statistics is above 10, and the Sanderson-Windmeijer F-statistic rejects the null of weak instrument.

In column (2), we report the second-stage results for the number of patents. After instrumentation, the coefficient on our corruption measure is still negative and statistically significant. When examining patent citations in column (3), we find the coefficient is negative, though fell just short of statistical significance. Because patent citations may not fully capture the economic importance of each innovation (Kogan et al., 2017), we then examine the alternative patent quality measure *Log (1+Dollar value)* in column (4), and find a negative and significant effect of corruption on the dollar value of patents. Taken together, the 2SLS results provide us with greater confidence that corruption indeed affects innovation.

3.3.4 More robustness tests

The number of federal corruption convictions is the most commonly used measure of corruption in the United States because it is considered more objective than other survey-based measures. In addition, focusing on federal corruption convictions can help alleviate the concern that local political bias might affect the conviction rates across districts. However, as discussed by Boylan and Long (2003) and Cordis and Milyo (2016), there are still many limitations regarding the PIN data. For instance, the PIN data is collected from US district attorneys, and its accuracy is not unquestionable. Moreover, the measure is noisy in terms of its annual fluctuations, partially resulting from the time delay from corruption engagement to observed conviction. Hence, for robustness, we redo our analysis using the average number of convictions in the past five years. Unreported results are very similar to our main results.

In Table 9, we conduct more analysis using several other corruption measures that have appeared in the literature. First, to adjust for the impact of government size, we use the number of convictions in a state, scaled by the number of governmental employees of the state. Second, to account for the fact that firms may operate outside their headquarters, following Garcia and Norli (2012) we calculate the weighted average conviction rate for each firm based on the firm's fraction of operating activities in each state. Garcia and Norli determine this fraction by counting how many times each state is mentioned in a firm's 10-K report and dividing the number of the state's mentions by the total mentions of all states.[26] Third, we use a survey-based corruption proxy obtained from the Center for Public Integrity's State Integrity Investigation. In the investigation, 100 government integrity experts were asked to calculate a State Integrity Index according to 330 corruption-risk indicators in 14 categories. Each state is graded in term of the overall integrity of its government. We measure corruption using the negative value of the integrity grades so that a higher value corresponds to a more corrupt state. Although the survey was conducted in 2012, we believe it is a reasonable proxy for our sample period because local corruption levels tend to be stable over time. Finally, we construct a corruption measure using data from the TRACfed database maintained by the Transactional Records Access Clearinghouse (TRAC). TRAC data is generated from FOIA requests from 1986 onward (Cordis and Milyo, 2016). Results in Table 9 show that, for both innovation measures, the coefficients on the four corruption proxies are all negatively significant,

[26]Garcia and Norli (2012) provide data from 1993 to 2008. Our sample size is therefore smaller when using this adjusted measure. We thank the authors for generously sharing their data.

suggesting that measurement errors are unlikely to significantly bias our results.

We also conduct subsample analyses to ensure that our results are not driven by any specific group of firms. Since the corruption level in the District of Columbia is much higher than that of the other districts, we rerun our baseline regression using a subsample that excludes firms located there. Similarly, because of a high cluster of high-tech companies in California and Massachusetts (Lerner and Seru, 2015), we also do a subsample test by further dropping firms located in these two states. In addition, innovation output peaked during the Internet bubble period. Therefore, following Nanda and Rhodes-Kropf (2013), we examine whether the negative relation is purely driven by that period by excluding all firm-year observations between 1998 and 2000. Finally, because most of our firm-year observations count zero patents (as shown in Table 1), we repeat our baseline tests by focusing on innovators—that is, firms that have at least one granted patent during our sample period. As shown in Table 10, the coefficients on our corruption measure are consistently negative and statistically significant across subsamples, suggesting that our main findings are not driven by a particular group of firms.

3.4 Economic channel

Thus far we have shown that local political corruption has a negative impact on firm innovation. In this section we investigate the economic channels that are likely to be behind our results. In particular, we examine whether our disincentive-effect and culture-effect hypotheses hold.

3.4.1 Disincentive effect

As discussed above, innovators are more likely to be targeted by corrupt officials and therefore have less incentive to innovate when operating in a corrupt political environment. Consistent with this argument, we show that firms in more corrupt districts have lower R&D investment in the first place. If the disincentive effect indeed takes place, we would expect the impact to be greater for firms that have less bargaining power against rent-seeking political officials—that is, when their ability to avoid these officials is constrained. Bai et al. (2013) argue that firms operating in a single state are more easily extorted by rent-seeking political officials because they cannot shift their operations across state lines. In contrast, firms with less concentrated operations should have greater bargaining power against corrupt local officials. We therefore expect that the negative impact on innovation should be stronger for more geographically concentrated firms.

Second, we expect the disincentive impact to be greater for highly risky innovation projects. Mukherjeee et al. (2017) show that high corporate taxes force firms to forgo such highly risky projects. Similarly, ex-post rent-seeking risk will lower firms' risk-taking incentives, implying that firms located in more corrupt areas should have projects with less dispersed values or have fewer projects with extremely high or low values.

To test the first hypothesis, we follow Garcia and Norli (2012) and define *Concentration* as the number of mentions of the headquarters state over the mentions of all states in a firm's 10-K report. This variable ranges from zero to one, and a higher value indicates a higher degree of operations concentration. We also construct a dummy variable, *Highconcentration*, equal to one if the value of concentration is above the sample mean in a year. We then construct an interaction term between *Corruption* and *Concentration* (or *Highconcentration*) and add it to our regression. Table 11 presents the results. The dependent variable is a firm's innovation input (R&D) in columns (1) and (2), and innovation output in columns (3) – (6). We find that the coefficients on the interaction term are negative in all regressions and statistically significant for R&D and patent citations. These results are consistent with our conjecture that corruption affects innovation through the disincentive effect and that firms with lower bargaining power are more affected by local corruption.

To test the second hypothesis, we follow Mukherjeee et al. (2017) and use the distribution of citations to measure the riskiness of a firm's innovation projects. In particular, we examine the volatility of a firm's patent citations *(σ(Citations))* and the number of patents with extremely high (i.e., top 10%) or low (i.e., zero) citations. As predicted, results in Table 12 show that patents generated by firms in more corrupt districts have lower standard deviation of citations. These firms also have fewer highly cited patents or zero-cite patents, implying that these firms have fewer projects with extremely high value or no value at all. Overall, the results are consistent with our argument that corruption reduces a firm's innovation incentive.

3.4.2 Culture effect

Besides the disincentive effect, corruption may also affect firm innovation through the culture effect. Prior literature suggests that political corruption can serve as a culture factor (Parsons et al., 2014; Liu, 2016; Dass et al., 2016) and reduce the level of local social trust (Anokhin and Schulze, 2009), while higher social trust is typically associated with higher innovation efficiency (Xie et al., 2017). Thus, holding incentive to innovate the same, a firm operating in a more corrupt culture would have less innovation output. This is consistent with our earlier results on innovation efficiency.

To test the culture-effect hypothesis more directly, we consider another important culture factor that has been examined in the literature: religiosity. Psychology and ethics research suggests that religion fosters social trust (Longenecker et al., 2004; McCullough and Willoughby, 2009; Vitell, 2009). Similar evidence has been found in economics literature. Using the World Values Survey, Guiso, Sapienza, and Zingales (2003) find that more religious people tend to be more trusting and more trustworthy. Theoretical models by Bénabou and Tirole (2011) and Levy and Razin (2012) also suggest that religion can reduce opportunists among individuals and spur more cooperation.[27] We thus conjecture that, if local political corruption affects people's perceptions of the trustworthiness of others, there should be less deterioration of trust among more religious people. In other words, if the culture effect serves as one channel through which political corruption affects firm innovation, the negative impact will be weaker in areas with higher religiosity.

Following Hilary and Hui (2009) and Callen and Fang (2015), we measure religiosity (*Religiosity*) using the number of churches per 1,000-capita in the county of a firm's headquarters location.[28] We also construct a dummy variable, *Highreligorsity*, which equals one if the level of religiosity is above the mean value in a year. We then conduct the cross-

[27] There is also a literature examining the relation between religion and corruption. However, the results are mixed. Many studies fail to find any significant association between these two (Shadabi, 2013).
[28] The data is available on the website of the Association of Religion Data Archives: http://www.thearda.com/. They collect the survey data every decade (1990, 2000, 2010). Following the literature, we linearly interpolate the data to obtain the values for the missing years in our sample.

sectional test in Table 13. As it shows, the coefficients on the *Religiosity* and *Highreligiosity* dummy are both negative and significant. This is consistent with the evidence documented by Bénabou, Ticchi, and Vindigni (2015, 2016), who show that higher religiosity is associated with lower innovation. The authors argue that, although religiosity has a positive impact on trust and social norms, it can have negative impact on attitudes toward science and innovative change. More interestingly, when turning to the interaction term, we find the coefficients to be positively significant in all regressions. These results imply that the negative impact of corruption on innovation is mitigated by higher local religiosity. This is consistent with our notion that religion may reduce the negative impact of corruption on social norms, lending support to our culture-effect hypothesis. Meantime, our results also paint a nuanced picture of how religion affects innovation.

4. Conclusion

In this paper, we investigate how local political corruption affects firm innovation in the United States. Consistent with findings in a cross-country setting, we find that firms located in more corrupt districts are much less innovative. We take several approaches to address the endogeneity concern. First, we control for a broad set of time-variant firm and region characteristics as well as unobserved characteristics that may covariate with both corruption and innovation. We find that the negative relation holds and that the results are the same when we conduct a 2SLS analysis. Further, our results do not change when we use different corruption measures.

Next, we explore two possible economic channels through which political corruption may affect firm innovation. We find that the negative impact of corruption is greater for firms with operations more concentrated around their headquarters, which is consistent with the idea that firms with concentrated operations have less bargaining power against rent-seeking political officials and are more sensitive to local political corruption. We also find that firms in more corrupt areas undertake less risky innovation projects. Finally, we find evidence that higher local religiosity mitigates the negative impact of corruption on innovation, supporting the culture-effect hypothesis.

Overall, our paper contributes to the innovation literature by showing that corruption is an important institutional determinant of firm innovation, even in developed countries like the United States. Our study also contributes to the literature on the impact of political corruption on firm-level outcomes. Since innovation is an essential ingredient to economic growth, our findings can help us better understand how corruption affects the real economy.

Appendix. Variable definitions

This table lists the definitions and sources of all the variables used in this paper.

Variable	Definition	Data source
	Corruption measures	
Corruption	Annual number of public corruption convictions in a district, scaled by total population.	DOJ
Highcorruption	Dummy variable equal to one if a firm is located in a district where the corruption rate above the mean value for the year and zero otherwise.	DOJ
Corruption_employee	Annual number of public corruption convictions in a state, scaled by total governmental employees.	DOJ
Corruption_operation	Weighted average conviction rate based on a firm's fraction of operations in each state.	DOJ and Garcia and Norli (2012)
Corruption_survey	Negative one times the score of integrity from the 2012 survey of State Integrity Investigation.	The Center for Public Integrity
Corruption_TRAC	Annual number of public corruption convictions in a district, collected from TRACfed, scaled by total population.	TRACfed
	Innovation measures	
Patents	Annual number of patents applied for and eventually granted.	NBER patent database
Citations	Annual number of citations per patent received.	NBER patent database
IE_Patents	Innovation efficiency measure constructed following Hirshleifer et al. (2013). Patent counts, scaled by cumulative R&D expense over the previous five years, assuming an annual depreciation rate of 20%.	NBER patent database
IE_Citations	Innovation efficiency measure constructed following Hirshleifer et al. (2013). Adjusted patent citations over the previous five years, scaled by total R&D expense.	NBER patent database
Patents/Employees	Annual number of patents applied for and eventually granted per 1,000 firm employees	NBER patent database
Originality	The sum of originality score of patents, where originality equals one minus the Herfindahl index of patents it cites across different three-digit technology classes.	NBER patent database
Generality	The sum of generality score of patents, where generality equals one minus the Herfindahl index of patents that cite it across different three-digit technology classes.	NBER patent database
Dollar value	Annual dollar value of patents granted.	Kogan et al. (2017)
New product	Number of new product announcements with three-day cumulative abnormal returns above the 75th percentile.	Mukherjee et al. (2017)
σ(Citations)	Standard deviation of a firm's patent citations over subsequent 5 years.	NBER patent database
Highly cited patents	Number of top 10% most cited patents in a tech-class in a year applied.	NBER patent database
Zero-cite patents	Number of patents applied with zero citations.	NBER patent database
	Firm characteristics	

Firm size	Natural log of book value of total assets.	COMPUSTAT
R&D	R&D expense, scaled by total assets.	COMPUSTAT
ROA	Operating income before depreciation, scaled by total assets.	COMPUSTAT
PPE	Property, plant and equipment, scaled by total assets.	COMPUSTAT
Leverage	Book value of total debt, scaled by total assets.	COMPUSTAT
Capex	Capital expenditure, scaled by total assets.	COMPUSTAT
HHI	Herfindahl Hirschman index of four-digit SIC industry.	COMPUSTAT
HHI^2	The square of HHI.	COMPUSTAT
Tobin's Q	Market value of equity + book value of total assets − book value of equity minus deferred taxes, divided by total assets.	COMPUSTAT
KZ index	$-1.002 \times$ cash flow + $0.283 \times$ Tobin's Q + $3.139 \times$leverage $- 39.368 \times$ dividends $- 1.315 \times$ cash holdings, based on Kaplan and Zingales (1997).	COMPUSTAT
Firm age	Natural log of one plus the number of years listed on COMPUSTAT.	COMPUSTAT
Institutional ownership	Average quarterly institutional ownership in each year.	Thompson Reuters
Amihud illiquidity	Annual Amihud illiquidity value based on Amihud (2002).	CRSP
Marginal tax rate	A firm's simulated marginal tax rate before interest expense.	Graham and Mills (2008)
Concentration	Number of mentions of the headquarters state over the mentions of all states in each year.	Garcia and Norli (2012)
Highconcentration	Dummy variable equal to one if the value of concentration is above the sample mean in a year and zero otherwise.	Garcia and Norli (2012)
	Region characteristics	
Income level	Annual per capita income in a county.	Bureau of Economic Analysis
Unemployment rate	Annual unemployment rate in a county.	Bureau of Labor Statistics
Education level	Percentage of population over age 25 with bachelor degree or above in a state.	US Census Bureau
Government size	Natural log of one plus the number of governmental employees in a state.	US Census Bureau
Capital isolation	Gravity-based Centered Index of Spatial concentration, which measures the state size and shape adjusted average distance to capital city.	Campante and Do (2009)
Religiosity	Number of churches per 1000 capita in a county.	Association of Religion Data Archives
Highreligiosity	Dummy variable equal to one if a county's religiosity is above the mean in a year.	Association of Religion Data Archives
	Instrumental variable	
FOIA7YR	Dummy variable equal to one if a firm is headquartered in a state that has transitioned from weak to strong FOIA laws at least seven years ago.	Cordis and Warren (2014)

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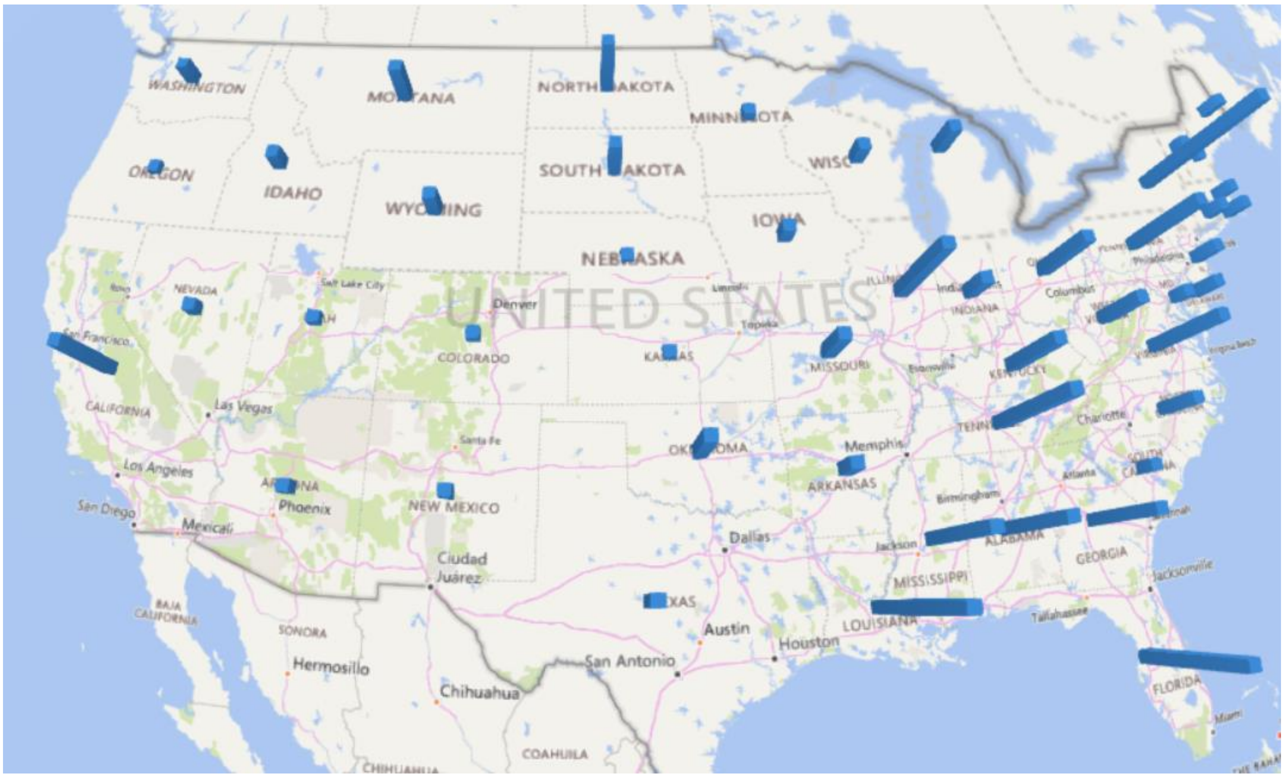
Figure 1 The Geography of political corruption and innovation

Panel A: The geography of political corruption (aggregated across all years)



Panel B: The geography of innovation (aggregated across all years)

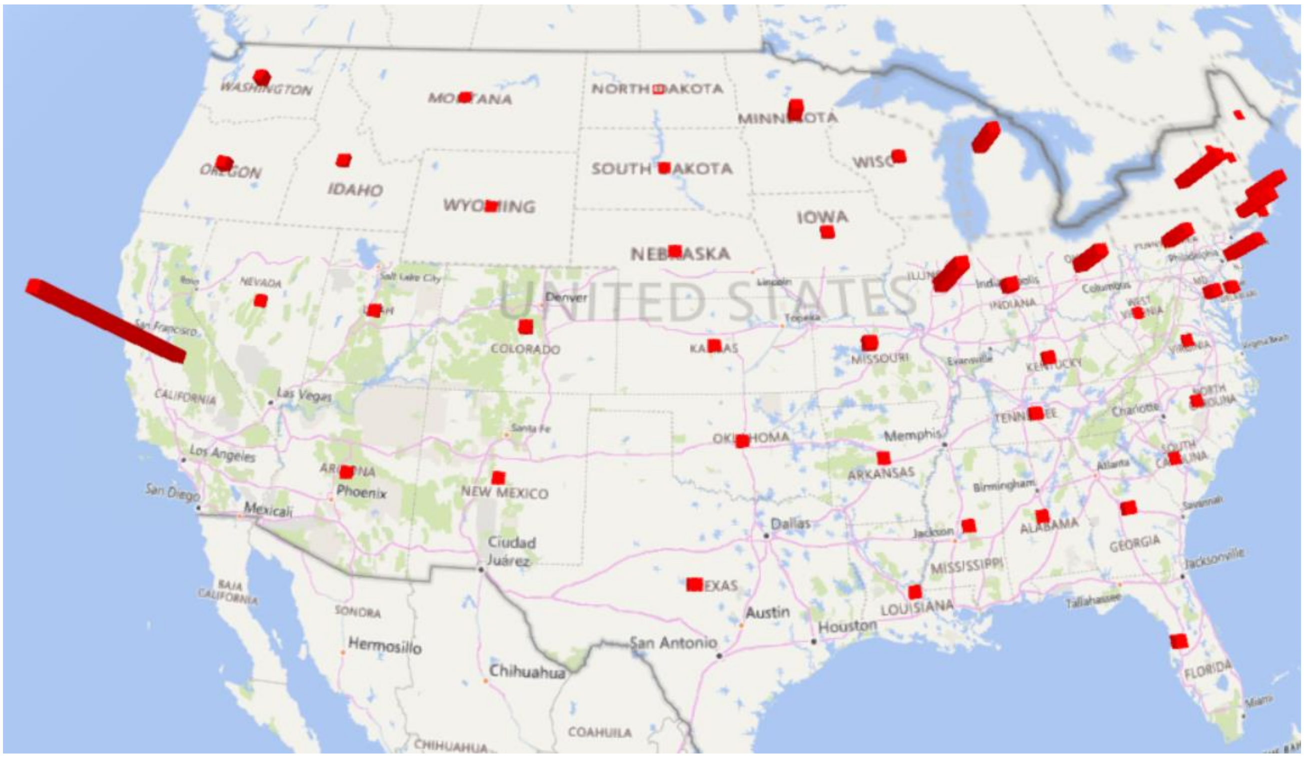


Table 1. Summary statistics

This table reports descriptive statistics for our main sample. The sample comprises public firms in COMPUSTAT from 1990 to 2006. Financial firms (SIC codes 6000–6999), utilities (SIC 4900–4999), public sectors (SIC 9000–9999) and firms with headquarters outside the United States are excluded from the sample. All continuous variables are winsorized at the 1% and 99% levels. Detailed variable definitions are provided in the Appendix.

Variables	Mean	S.D.	25%	Median	75%	*N*
Patents	3.75	15.06	0.00	0.00	0.00	56,565
Citations	3.09	8.53	0.00	0.00	0.00	56,565
Corruption	0.33	0.29	0.13	0.24	0.45	56,565
Firm size	5.36	1.99	3.91	5.24	6.68	56,565
R&D	0.05	0.11	0.00	0.00	0.06	56,565
ROA	0.05	0.24	0.02	0.11	0.17	56,565
PPE	0.27	0.21	0.10	0.21	0.38	56,565
Leverage	0.23	0.21	0.03	0.19	0.35	56,565
Capex	0.06	0.06	0.02	0.04	0.08	56,565
Tobin’s Q	2.02	1.69	1.06	1.44	2.24	56,565
KZ index	−6.29	22.54	−4.23	-0.63	0.81	56,565
Firm age	2.35	0.86	1.61	2.40	3.09	56,565
HHI	0.27	0.20	0.13	0.21	0.36	56,565

Table 2. The geography of political corruption and innovation

This table presents the top 10 districts by corruption and innovation, respectively. Panel A shows the summary statistic for the top 10 corrupt districts by conviction rate per 100,000, where the districts are ranked according to the median values. Panel B shows the top 10 innovative districts by aggregate innovation intensity, where districts are ranked according to the total number of patents.

Panel A: Top 10 corrupt districts by conviction rate per 100,000

US Federal Judicial District	Corruption conviction (aggregated)
District of Columbia	126.3049
Louisiana, Eastern	19.76539
Mississippi, Northern	15.35308
Tennessee, Western	15.20004
Florida, Southern	14.70904
New York, Southern	14.01636
North Dakota	13.34088
Louisiana, Middle	13.01472
Virginia, Eastern	12.49257
Kentucky, Eastern	12.02779

Panel B: Top 10 innovative districts by aggregate innovation intensity

US federal judicial district	Patents (aggregated)
California, Northern	4,843.2
Massachusetts	1,975.4
Illinois, Northern	1,418.7
California, Central	1,412.5
New Jersey	1,282.0
Connecticut	1,093.8
Minnesota	957.6
New York, Southern	876.5
Ohio, Northern	837.0
California, Southern	835.6

Table 3. Baseline regression: US political corruption and firm innovation

This table reports the results from our baseline regressions. The dependent variable is *Log* (*1+Patents*) in columns (1) and (2) and *Log (1+Citations)* in columns (3) and (4). Detailed variable definitions are provided in the Appendix. Robust *t*-statistics, adjusted for district-level clustering, are reported in parentheses. *******, ******, and ***** indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Log (1+Patents)		Log (1+Citations)	
	(1)	(2)	(3)	(4)
Corruption	−0.122***		−0.116***	
	(−3.086)		(−3.292)	
Highcorruption		−0.071***		−0.084***
		(−2.711)		(−3.162)
Firm size	0.236***	0.236***	0.156***	0.156***
	(11.845)	(11.815)	(13.795)	(13.759)
R&D	1.479***	1.485***	1.650***	1.649***
	(10.357)	(10.392)	(14.129)	(14.297)
ROA	0.036	0.038	0.224***	0.223***
	(0.518)	(0.553)	(3.157)	(3.211)
PPE	−0.198***	−0.197***	−0.159***	−0.160***
	(−3.033)	(−3.007)	(−2.811)	(−2.810)
Leverage	−0.300***	−0.301***	−0.358***	−0.357***
	(−7.463)	(−7.474)	(−8.807)	(−8.860)
Capex	0.639***	0.644***	0.598***	0.602***
	(2.775)	(2.802)	(3.091)	(3.131)
Tobin’s Q	0.056***	0.056***	0.051***	0.051***
	(16.133)	(16.091)	(12.673)	(12.615)
KZ index	0.001***	0.001***	0.001***	0.001***
	(7.170)	(7.246)	(3.364)	(3.388)
Firm age	0.101***	0.101***	0.073***	0.073***
	(8.323)	(8.356)	(4.813)	(4.898)
HHI	0.308*	0.307*	0.129	0.130
	(1.730)	(1.726)	(0.722)	(0.731)
HHI^2	−0.035	−0.034	0.008	0.007
	(−0.180)	(−0.173)	(0.046)	(0.042)
Industry FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Observations	56,565	56,565	56,565	56,565
R-squared	0.315	0.315	0.240	0.240

Table 4. The impact of corruption on innovation input

This table examines the impact of corruption on innovation input. The dependent variable is one-year lead R&D investment. Column (1) uses the full sample, column (2) excludes observations with missing R&D, and column (3) excludes observations with missing or zero R&D. All controls from Table 3 are included in the regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in the Appendix. Robust *t*-statistics, adjusted for district-level clustering, are reported in parentheses. *******, ******, and ***** indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Lead R&D		
	Full	Exclude missing R&D	Exclude missing or zero R&D
	(1)	(2)	(3)
Corruption	−0.017***	−0.016**	−0.016**
	(−3.118)	(−2.527)	(−2.037)
Baseline controls	yes	yes	yes
Industry FE	yes	yes	yes
Year FE	yes	yes	yes
Observations	51,494	31,790	25,908
R-squared	0.483	0.491	0.464

Table 5. Alternative innovation output measures

This table reports the regression results for alternative innovation output measures. The dependent variable is *Log (1+IE_Patents)* in column (1), *Log (1+IE_Citations)* in column (2), *Log (1+Patents/Employees)* in column (3), *Log (1+Originality)* in column (4), *Log (1+Generality)* in column (5), *Log (1+Dollar value)* in column (6), and *Log (1+New product)* in column (7). All controls from Table 3 are included in the regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in the Appendix. Robust *t*-statistics, adjusted for district-level clustering, are reported in parentheses. *******, ******, and ***** indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Log (1+IE_Patents)	Log (1+IE_Citations)	Log (1+Patents/Employees)	Log (1+Originality)	Log (1+Generality)	Log (1+Dollar value)	Log (1+New product)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Corruption	−0.031***	−0.035***	−0.151***	−0.088***	−0.052**	−0.207***	−0.016*
	(−3.858)	(−4.430)	(−3.809)	(−3.028)	(−2.427)	(−2.803)	(−1.784)
Baseline controls	yes	yes	yes	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes
Observations	22,339	22,339	55,725	56,565	56,565	56,565	56,565
R-squared	0.066	0.076	0.209	0.302	0.271	0.334	0.084

Table 6. Fixed effect analysis

This table reports the regression results when more fixed effects are added to our baseline regressions. State-year fixed effects are controlled in columns (1) and (3), and industry-state-year fixed effects are controlled in columns (2) and (4). The dependent variable is *Log (1+Patents)* in columns (1) and (2) and *Log (1+Citations)* in columns (3) and (4). All baseline controls from Table 3 are included in the regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in the Appendix. Robust *t*-statistics, adjusted for district-level clustering, are reported in parentheses. *******, ******, and ***** indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Log (1+Patents)		Log (1+Citations)	
	(1)	(2)	(3)	(4)
Corruption	−0.110**	−0.133**	−0.120**	−0.116**
	(−2.410)	(−2.602)	(−2.249)	(−2.056)
Baseline controls	yes	yes	yes	yes
Industry FE	yes	no	yes	no
State-year FE	yes	no	yes	no
Industry-state-year FE	no	yes	no	yes
Observations	56,565	56,565	56,565	56,565
R-squared	0.332	0.399	0.261	0.334

Table 7. Adding more controls

This table reports the regression results when more control variables are added to the baseline regressions. The dependent variable is *Log (1+Patents)* in columns (1) and (2) and *Log (1+Citations)* in columns (3) and (4). All controls from Table 3 are included in the regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in the Appendix. Robust *t*-statistics, adjusted for district-level clustering, are reported in parentheses. *******, ******, and ***** indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Log (1+Patents)		Log (1+Citations)	
	(1)	(2)	(3)	(4)
Corruption	−0.133**	−0.163**	−0.080**	−0.104*
	(−2.308)	(−2.050)	(−2.079)	(−1.693)
Income level	−0.028***	−0.033**	−0.033***	−0.037***
	(−2.838)	(−2.443)	(−4.109)	(−3.916)
Unemployment rate	0.004	0.010	−0.023**	−0.018
	(0.428)	(0.923)	(−2.232)	(−1.498)
Education attainment	1.380***	1.520***	1.099***	1.094***
	(3.863)	(3.498)	(3.418)	(2.644)
Government size	−0.229*	−0.269*	−0.130	−0.113
	(−1.956)	(−1.863)	(−1.300)	(−0.839)
Capital isolation	0.026	0.016	0.036	0.020
	(0.968)	(0.446)	(1.247)	(0.544)
Institutional ownership		−0.290***		0.006
		(−3.917)		(0.117)
Amihud illiquidity		0.027***		−0.021***
		(5.276)		(−3.345)
Marginal tax rate		−0.320***		−0.144
		(−3.063)		(−1.518)
Baseline controls	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Observations	49,707	21,853	49,707	21,853
R-squared	0.316	0.393	0.242	0.304

Table 8. Two-Stage-Least-Square Analysis

This table reports the regression results of the 2SLS analysis. Column (1) reports the first-stage regression results, with *Corruption* as the dependent variable. The instrumental variable is a dummy that takes the value of one if a firm is headquartered in a state that transitioned from weak to strong FOIA laws at least seven years ago. Results from the second-stage regressions are reported in columns (2) through (4), with *Log (1+Patents), Log (1+Citations)* and *Log (1+Dollar value)* as the dependent variables, respectively. The independent variable of interest in the second stage is the predicted values of *Corruption* from the first-stage regression. All baseline controls from Table 3 are included in all regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in the Appendix. Robust *t*-statistics, adjusted for district-level clustering, are reported in parentheses. *******, ******, and ***** indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	First stage	Second stage		
	Corruption	Log (1+Patents)	Log (1+Citations)	Log (1+Dollar value)
	(1)	(2)	(3)	(4)
FOIA7YR	−0.195***			
	(−25.05)			
Corruption		−0.371**	−0.383	−0.791***
		(−2.013)	(−1.617)	(−2.787)
Baseline controls	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
F-statistics	12.185			
Prob > F	0.003			
Observations	12,399	12,399	12,399	12,399
R-squared	0.181	0.300	0.226	0.328

Table 9. Alternative corruption measures

This table reports the regression results when alternative corruption measures are used as the main independent variable. *Corruption_employee* is the number of convictions in a state, scaled by the number of governmental employees of the state. *Corruption_operation* is the weighted average state conviction rate based on a firm's fraction of operations in each state. *Corruption_survey* is the negative value of state scores of integrity obtained from the survey of State Integrity Investigation in 2012. *Corruption_TRAC* is constructed using corruption conviction data from TRACfed. The dependent variable is *Log (1+Patents)* in Panel A and *Log (1+Citations)* in Panel B. All baseline controls from Table 3 are included in the regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in the Appendix. Robust *t*-statistics, adjusted for district-level clustering, are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Dependent variable: *Log (1+Patents)*

	(1)	(2)	(3)	(4)
Corruption_employee	−0.011***			
	(−4.577)			
Corruption_operation		−0.271***		
		(−3.067)		
Corruption_survey			−0.006**	
			(−2.629)	
Corruption_TRAC				−0.115**
				(−2.432)
Baseline controls	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Observations	50,498	32,558	56,440	56,565
R-squared	0.312	0.335	0.316	0.314

Panel B: Dependent variable: *Log (1+Citations)*

	(1)	(2)	(3)	(4)
Corruption_employee	−0.007***			
	(−3.668)			
Corruption_operation		−0.246***		
		(−4.086)		
Corruption_survey			−0.004*	
			(−1.829)	
Corruption_TRAC				−0.120***
				(−2.857)
Baseline controls	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Observations	50,498	32,558	56,440	56,565
R-squared	0.237	0.258	0.240	0.239

Table 10. Robustness tests

This table shows the regression results of the robustness tests. We replicate the baseline regression by excluding firms in Washington, DC, in column (1), further excluding firms in California and Massachusetts in column (2), excluding the Internet bubble period (1998–2000) in column (3), and using a subsample of only innovators in column (4). The dependent variable is *Log (1+Patents)* in Panel A and *Log (1+Citations)* in Panel B. All baseline controls from Table 3 are included in the regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in the Appendix. Robust *t*-statistics, adjusted for district-level clustering, are reported in parentheses. *******, ******, and ***** indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Dependent variable: *Log (1+Patents)*

	Excluding DC	Excluding DC, CA & MA	Excluding bubble period	Innovator subsample
	(1)	(2)	(3)	(4)
Corruption	−0.116***	−0.070**	−0.131***	−0.162***
	(−2.935)	(−2.209)	(−3.112)	(−3.214)
Baseline controls	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Observations	56,431	46,388	44,908	15,598
R-squared	0.315	0.312	0.315	0.210

Panel B: Dependent variable: *Log (1+Citations)*

	Excluding DC	Excluding DC, CA & MA	Excluding bubble period	Innovator subsample
	(1)	(2)	(3)	(4)
Corruption	−0.114***	−0.083**	−0.122***	−0.102**
	(−2.806)	(−2.355)	(−3.683)	(−2.294)
Baseline controls	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Observations	56,431	46,388	44,908	15,598
R-squared	0.240	0.232	0.249	0.200

Table 11. The asymmetric impact on firms with concentrated operations

This table examines how operations concentration affects the relation between corruption and innovation. The dependent variable is *R&D expenditure*, scaled by total assets in columns (1) and (2), *Log (1+Patents)* in columns (3) and (4), and *Log (1+Citations)* in columns (5) and (6). The explanatory variable of interest is the interaction term between *Corruption* and *Concentration* (*Highconcentration*). All controls from Table 4 are included in the regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in the Appendix. Robust *t*-statistics, adjusted for district-level clustering, are reported in parentheses. *******, ******, and ***** indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	R&D		Log (1+Patents)		Log (1+Citations)	
	(1)	(2)	(3)	(4)	(5)	(6)
Corruption × Concentration	−0.043**		−0.098		−0.235**	
	(−2.615)		(−1.003)		(−2.155)	
Corruption × Highconcentration		−0.020**		−0.078		−0.086*
		(−2.430)		(−1.490)		(−1.880)
Corruption	−0.005	−0.015***	−0.117*	−0.127**	−0.043	−0.070
	(−1.464)	(−2.816)	(−1.896)	(−2.231)	(−0.873)	(−1.401)
Concentration	0.034***		0.344***		0.309***	
	(3.530)		(3.944)		(3.943)	
Highconcentration		0.015***		0.184***		0.127***
		(3.305)		(3.611)		(3.538)
Baseline controls	yes	yes	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Observations	18,338	18,338	20,348	20,348	20,348	20,348
R-squared	0.518	0.517	0.330	0.330	0.257	0.256

Table 12. Riskiness of innovation

This table examines the relation between corruption and the riskiness of innovation. The dependent variable is the standard deviation of a firm's patent citations (*σ(Citations)*) in column (1), *Log (1+Highly cited patents)* in column (2), and *Log (1+Zero-cite patents)* in column (3). All controls from Table 3 are included in the regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in the Appendix. Robust *t*-statistics, adjusted for district-level clustering, are reported in parentheses. *******, ******, and ***** indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	σ(Citations)	Log (1+Highly cited patents)	Log (1+Zero-cite patents)
	(1)		(2)
Corruption	−1.384**	−0.057**	−0.041**
	(−2.181)	(−2.333)	(−2.523)
Baseline controls	yes	yes	yes
Industry FE	yes	yes	yes
Year FE	yes	yes	yes
Observations	7,350	56,565	56,565
R-squared	0.225	0.252	0.209

Table 13. Religiosity and culture effect

This table examines how local religiosity affects the relation between corruption and innovation. The dependent variable is *Log (1+Patents)* in columns (1) and (2) and *Log (1+Citations)* in columns (3) and (4). The explanatory variable of interest is the interaction term between *Corruption* and *Religion* (*Highreligion*). All controls from Table 3 are included in the regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in the Appendix. Robust *t*-statistics, adjusted for district-level clustering, are reported in parentheses. *******, ******, and ***** indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Log (1+Patents)		Log (1+Citations)	
	(1)	(2)	(3)	(4)
Corruption × Religion	0.194**		0.193**	
	(2.173)		(1.990)	
Corruption × Highreligion		0.169**		0.148**
		(2.505)		(2.211)
Corruption	−0.279***	−0.165***	−0.276***	−0.154***
	(−3.203)	(−3.453)	(−3.322)	(−3.904)
Religion	−0.142*		−0.147**	
	(−1.985)		(−2.059)	
Highreligion		−0.094*		−0.082*
		(−1.969)		(−1.853)
Baseline controls	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Observations	56,394	56,394	56,394	56,394
R-squared	0.305	0.316	0.229	0.240