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ARTICLE

Fiscal monitoring and corporate investment

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Abstract

Does state fiscal monitoring of local governments impact firms? Exploiting the staggered adoption of state fiscal monitoring policies, our results show that state fiscal monitoring of local governments increases corporate investment. Affected firms increase their investment by increasing capital expenditures as well as research and development expenditures. Additional analyses reveal that firms fund this increase in investment by decreasing share repurchases and issuing debt. We also provide evidence that the increase in corporate investment is driven by a reduction in local corruption.

KEYWORDS
corporate investment, fiscal monitoring, institutions, local governments

JEL CLASSIFICATION
G30, G31, G38

1 | INTRODUCTION

Corporate investment decisions vary significantly depending on where a firm is headquartered. This holds across countries (e.g., S. Johnson et al., 2002; Julio & Yook, 2012; McLean et al., 2012) and within individual countries such as the United States. In the year 2010, for example, firms headquartered in cities such as Phoenix or Dallas invested at around twice the rate of firms headquartered in cities such as Indianapolis or Cleveland (Dougal et al., 2015). Relatively little is known about the determinants of such regional differences in corporate investment. The goal of our paper is to assess the role of a previously unexplored determinant of investment through which such geographic differences could manifest: fiscal monitoring.

We examine the impact of state fiscal monitoring of local governments (hereafter: fiscal monitoring or state monitoring) on corporate investment by exploiting the staggered adoption of state fiscal monitoring policies of local governments in the United States. Fiscal monitoring policies involve state governments actively reviewing and assessing fiscal conditions of their local governments (i.e., governments of cities, towns, villages and counties; Urahn et al., 2016). As of 2019, 23 states have adopted fiscal monitoring policies.

Ex-ante, the effect of state fiscal monitoring on corporate investment is unclear. On the one hand, local governments decrease their spending following the adoption of state fiscal monitoring policies (Nakhmurina, 2020), and local spending cuts could depress corporate investment (Brückner & Tuladhar, 2014; Hebous & Zimmermann, 2021). Moreover, state monitoring lowers local corruption (Nakhmurina, 2020), and lower corruption could result in firms decreasing their investment (Leff, 1964; Lui, 1985). On the other hand, lower local corruption could also lead to higher corporate investment (Du & Heo, 2022; N. D. Johnson et al., 2011). In addition, fiscal monitoring reduces local government debt (Nakhmurina, 2020), and lower local government debt could increase corporate investment (Y. Huang et al., 2020). Finally, fiscal monitoring could also have no impact on corporate investment if the previous effects offset each other.

For a sample of 95,935 firm-year observations from 1995 to 2018, we find that the introduction of fiscal monitoring policies increases corporate investment. The change in investment is economically significant and plausible. This result is robust to changes to the definition of corporate investment, the treatment of outliers, the clustering of standard errors and the set of control variables. Moreover, the result holds in a stacked regression in the spirit of Cengiz et al. (2019).

Further analyses show that the increase in investment is driven by higher capital expenditures as well as by higher research and development expenditures. We also document that firms change their financing policies by decreasing their share repurchases and by issuing more debt. Both financing policies increase the funds available to firms. These results give further context and credibility to our main findings since the increase in funds allows firms to undertake additional investments.

A key challenge in reliably estimating the impact of state fiscal monitoring on corporate investment is the potential endogeneity of state monitoring. To overcome this empirical challenge, our identification strategy exploits the staggered adoption of fiscal monitoring policies across states and over time, in the spirit of Nakhmurina (2020). We run difference-in-differences regressions that compare the investment of firms headquartered in states that have adopted fiscal monitoring policies to the investment of firms headquartered in states that have not adopted fiscal monitoring policies.

Two features of our identification strategy alleviate the risk of omitted variables confounding our results. First, our treatment is plausibly exogenous to firms. Fiscal monitoring policies are implemented by state governments and target local governments with the goal of increasing the state governments' understanding of the fiscal conditions of local governments (Nakhmurina, 2020). It is thus unlikely that the introduction of state monitoring policies is endogenously driven by firm-specific conditions. Second, the adoption of fiscal monitoring policies is staggered over time and states. As a consequence, confounding factors would have to be staggered over time and states concurrently with the fiscal monitoring policies introduced by states.

Although our identification strategy substantially raises the bar that concurrent events drive our results, one concern could be that changing economic conditions can determine both the adoption of fiscal monitoring policies by states and the investment decisions of firms. We address this concern by running a neighboring-state test (e.g., Holmes, 1998; Ljungqvist et al., 2017; Nakhmurina, 2020; Qiu, 2019). Our main findings hold in the neighboring-state test as firms headquartered in counties of states that have adopted fiscal monitoring policies increase their investment compared to firms headquartered in counties of bordering states that have not adopted fiscal monitoring policies. We therefore gain confidence in a causal interpretation of our main result that fiscal monitoring increases corporate investment.

Examining potential channels that explain how state monitoring increases corporate investment, we present evidence consistent with a reduction in local corruption boosting firm investment. Corruption could depress corporate investment by decreasing economic growth and investment opportunities (N. D. Johnson et al., 2011) or because corruption increases the risk of expropriation (Ellis et al., 2020; Murphy et al., 1993). Moreover, local corruption could lead to firms substituting investment with rent-seeking activities (Q. Huang & Yuan, 2021). We first confirm the findings of Nakhmurina (2020) that the adoption of fiscal monitoring policies results in lower local corruption. We then show that firms benefitting more from a decrease in local corruption (i.e., more politically vulnerable firms) experience a higher increase in investment following the introduction of fiscal monitoring policies compared to firms benefitting less from a decrease in local corruption (i.e., less politically vulnerable firms).

We also investigate an alternative channel that could explain our main results. In particular, we test whether firms increase their investment due to a reduction in financing constraints. However, we find evidence that is inconsistent with this channel as more financially constrained firms do not experience a different change in investment following the introduction of fiscal monitoring policies compared to less financially constrained firms.

We contribute to multiple streams of research. We extend the corporate investment literature as we are, to the best of our knowledge, the first to document the influence of state fiscal monitoring on corporate investment. There is substantial literature on the determinants of corporate investment decisions (e.g., Almeida & Campello, 2007; Bai et al., 2020; Balakrishnan et al., 2016; Dang, 2011; Fazzari et al., 1988; Lamont, 1997; Leahy & Whited, 1996; Stein, 2003) with a number of recent studies examining the impact of governments on corporate investment. Julio and Yook (2012), Gulen and Ion (2016) and Jens (2017) show that governments can increase uncertainty resulting in firms delaying their investment. Y. Huang et al. (2020) find that local government debt can crowd out corporate investment in China. We document in the US context the relevance of fiscal monitoring, a factor that has not been explored by existing research, as a determinant of corporate investment. In doing so, our study also contributes to the literature that analyzes how national institutions (e.g., Acemoglu et al., 2005; Porta et al., 1998) and local institutions (e.g., Acemoglu & Dell, 2010; Banerjee & Iyer, 2005; Dell, 2010; Dell et al., 2018; Laeven & Woodruff, 2007; Rajan & Ramcharan, 2001) affect economic outcomes.

Our paper also contributes to the literature that examines the relationship between the geographic location of firms and firm outcomes. Specifically, we demonstrate that governments at firm locations play a meaningful role in affecting firm outcomes. Prior studies establish the importance of corporate location on firm outcomes such as asset prices (Coval & Moskowitz, 2001; Pirinsky & Wang, 2006), payout (John et al., 2011) and corporate misconduct (Dyreng et al., 2012; Parsons et al., 2018) as well as liquidity (Bernile et al., 2015). Earlier work shows that local investors (e.g., John et al., 2011) and interactions between local firms (e.g., Dougal et al., 2015) lead to regional differences in corporate outcomes. Our paper differs from these papers as we provide evidence of local governments as a location-related factor that helps explain regional differences in firm outcomes. Thereby, we aim to contribute to a better understanding of how the geographic location of firms impacts important corporate outcomes.

2 | BACKGROUND AND RESEARCH DESIGN

2.1 | Background on state fiscal monitoring of local governments

While many states take a hands-off approach to the financial situation of their local governments, some states have adopted fiscal monitoring policies. Fiscal monitoring policies involve a state process to actively and regularly review financial information from local governments (Urahn et al., 2016). Nakhmurina (2020) concludes that the motivation of these policies is to detect fiscal distress or more generally understand the fiscal conditions of local governments.[1]

[1] To achieve this goal, the department monitoring the local government uses a wide range of financial reports, including audited financial statements and unaudited budgets, and indicators (e.g., cash flows; Urahn et al., 2016). The specific department that conducts the monitoring varies from state to state. Most states choose the state auditor's office, the state comptroller's office or a separate state unit established for local government services (Urahn et al., 2016).

Nakhmurina (2020) shows that fiscal monitoring policies discipline local governments and improve the financial decisions made by local governments. The policies discipline local governments through an increase in the likelihood that mismanagement is detected. Local governments make better financial decisions as a result of guidance and feedback from the department monitoring the local government. Moreover, better financial decisions result as fiscal monitoring policies improve the financial reporting of the underlying economic positions of the local governments. As a consequence of disciplining and better financial decision making, fiscal monitoring results in lower local government spending, lower local corruption and lower local government debt (Nakhmurina, 2020).

We examine the spill-over effects of fiscal monitoring policies on the private sector. We focus on corporate investment decisions as these are a primary means through which firms create value for their investors and stakeholders (Roychowdhury et al., 2019).[2] Ex-ante, the effect of state fiscal monitoring on corporate investment is unclear. On the one hand, corporate investment could decrease as a result of two non-mutually exclusive channels. First, fiscal monitoring policies reduce local government spending (Nakhmurina, 2020). In turn, local government spending cuts could negatively impact corporate investment by reducing economic growth and investment opportunities (Adelino et al., 2017; Guo et al., 2016). Further, lower local government spending could result in local governments cutting transfers to firms. By receiving less government transfers, firms' financial positions could deteriorate, and firms' financial constraints could increase. Thus, firms could decrease their investment as a result of local governments cutting transfers to firms (Brückner & Tuladhar, 2014; Hebous & Zimmermann, 2021).

Second, fiscal monitoring policies also result in a reduction in local corruption. In a corrupt environment, firms can decrease investment uncertainty by building political connections to decrease political risk (Leff, 1964) or by bribing public officials to cut through bureaucratic red tape (Lui, 1985). Therefore, lower corruption could increase investment uncertainty and lead to lower corporate investment.

On the other hand, corporate investment could increase as a result of two non-mutually exclusive channels. First, lower local corruption might not decrease corporate investment but could increase corporate investment. Lower corruption could increase corporate investment by increasing economic growth and investment opportunities (N. D. Johnson et al., 2011). In addition, lower corruption could incentivize firms to increase their investment by decreasing the risk of expropriation (Du & Heo, 2022; Murphy et al., 1993). Moreover, lower local corruption could result in firms reducing their rent-seeking activities, which could free up resources for additional investment activities (Q. Huang & Yuan, 2021). Therefore, as a consequence of lower local corruption, firms could increase their investment.

Second, state monitoring decreases local government debt (Nakhmurina, 2020). In turn, lower local government debt could increase the credit supply to firms and thereby loosen financial constraints for firms (Y. Huang et al., 2020). Consequently, firms could increase their investment due to lower government debt loosening financial constraints for firms.

State fiscal monitoring could also have no impact on corporate investment. This could be the case if the previous effects offset each other. For example, a decrease in investment due to lower local government spending could be offset by an increase in investment due to lower local government debt. Our goal in this paper is to assess empirically whether fiscal monitoring increases, decreases or has no impact on corporate investment.

2.2 | Research design

A key challenge in estimating the impact of state fiscal monitoring on corporate investment is the potential endogeneity of fiscal monitoring. Most notably, correlated omitted variables could make any inferences about the causal relationship between fiscal monitoring and corporate investment unreliable (Roberts & Whited, 2013). To overcome

[2] We also examine the impact of fiscal monitoring policies on employment and find inconclusive evidence (see Online Appendix). When investigating potential channels for spill-over effects of fiscal monitoring policies on the private sector (Section 4.5), we document a reduction in local corruption. As prior empirical studies find no evidence for corruption affecting employment (see e.g., E. L. Glaeser & Saks, 2006), we consider our inconclusive evidence on the relationship between fiscal monitoring policies and employment consistent with prior research.

this empirical challenge, our identification strategy exploits the staggered adoption of state-level fiscal monitoring policies (Nakhmurina, 2020).

We assess the effect of state fiscal monitoring on investment by comparing the investment of firms headquartered in states where fiscal monitoring has increased (the "treatment" group) to the investment of firms headquartered in states where no such increase has occurred (the "control" group). In particular, we estimate the following difference-in-differences regression model:

$$Investment_{i,t} = \beta_0 + \beta_1 FMP\ Post_{s,t} + Controls_{i,t} + \alpha_i + \theta_t + \varepsilon_{i,s,t}, \tag{1}$$

where i denotes firm, s denotes state and t denotes year. Our variable of interest is $FMP\ Post_{s,t}$, and our dependent variable is $Investment_{i,t}$. *FMP Post* is a dummy equal to 1 if state s has adopted fiscal monitoring policies in year t.[3] We include a vector of firm-level controls ($Controls_{i,t}$) as well as firm fixed effects (α_i) and year fixed effects (θ_t). We cluster standard errors at the state level since our treatment occurs at the state level. Clustering at the state level takes into account that residuals are serially correlated within a firm and also correlated across firms within the same state (Bertrand et al., 2004).

The coefficient β_1 of *FMP Post* captures the average changes in *Investment* of firms headquartered in states that adopt fiscal monitoring policies ("treatment" group) relative to the changes in *Investment* of firms headquartered in unaffected states ("control" group). Therefore, the coefficient captures the effect of state fiscal monitoring on *Investment*.

A key assumption for a reliable causal interpretation of β_1 is that in the absence of the introduction of fiscal monitoring policies, the average change in *Investment* would have been the same for the treatment group and control group (Roberts & Whited, 2013). A potential threat to this assumption is the presence of omitted confounding variables that correlate with both the adoption of fiscal monitoring policies and *Investment*. Two features of our identification strategy alleviate this risk.

First, our treatment is plausibly exogenous to firms. Fiscal monitoring policies are implemented by state governments and target local governments with the goal of increasing the state governments' understanding of the fiscal conditions of local governments (Nakhmurina, 2020).[4] It is thus unlikely that the adoption of state fiscal monitoring policies is endogenously driven by firm-specific conditions. Second, the adoption of fiscal monitoring policies is staggered over time and states. As a consequence, confounding factors would have to be staggered over time and states concurrently with the fiscal monitoring policies introduced by states.

We also include a comprehensive set of fixed effects and control variables to further mitigate the risk of an omitted variable bias. In particular, we include firm fixed effects to control for time-invariant firm characteristics, year fixed effects to absorb time trends and *Tobin's Q*, *Cash Flow* and *Firm Size* as control variables to account for time-varying changes in the primary determinants of investment decisions (Foucault & Fresard, 2014).

We graphically inspect trends in corporate investment around fiscal monitoring policies in Figure 1. Following Barrios (2021), we run event-study estimates from a two-way fixed effects regression of the effect of fiscal monitoring policies on *Investment*. The graph shows that prior to the adoption of fiscal monitoring policies, *Investment* is stable for treated firms and control firms. *Investment* only differs between both groups after the adoption of fiscal monitoring policies. These trends indicate that in the absence of the introduction of fiscal monitoring policies, the average change in *Investment* would have been the same for the treatment group and control group. Consequently, a key assumption for a reliable causal interpretation of the effect of fiscal monitoring policies on firm investment does not seem to be violated.

[3] Our main results hold if we require the state to adopt fiscal monitoring policies in year $t-1$ to account for a possible time lag between the adoption of fiscal monitoring policies and corporate outcomes (untabulated).

[4] Consistent with this, Nakhmurina (2020) shows that time-varying local government characteristics, aggregated at the state level, do not predict the adoption of fiscal monitoring policies by states.

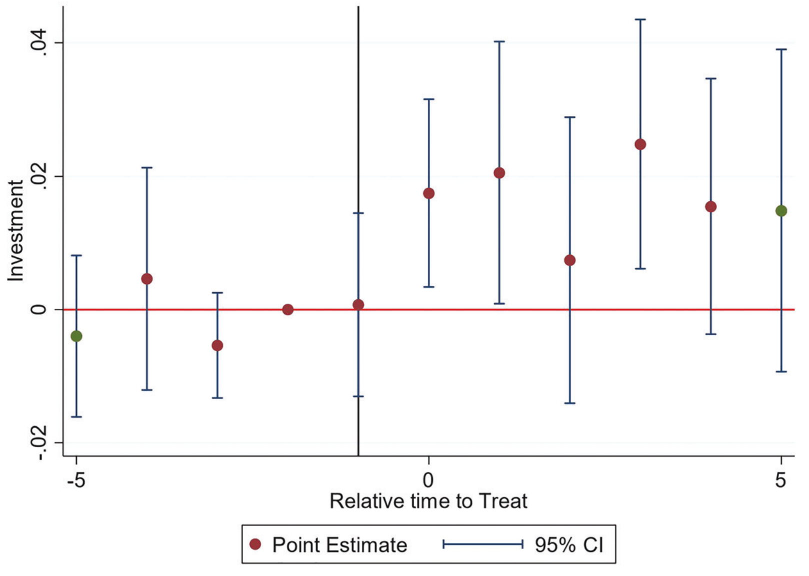

FIGURE 1 Firm investment around the adoption of fiscal monitoring policies. This figure presents event-study estimates from a two-way fixed effects regression of the effect of fiscal monitoring policies on *Investment*. *Investment* equals capital expenditures plus research and development expenditures, all divided by lagged total assets. The indicator for period $t = -2$ serves as the benchmark with both a coefficient and standard error of zero. The sample includes all firms that are located in states that adopt fiscal monitoring policies over our sample period (1995–2018). The regression includes year and firm fixed effects. Standard errors are clustered at the state level.

Overall, our identification strategy substantially raises the hurdle for alternative explanations of our results. Nevertheless, one remaining concern could be that changing economic conditions determine both the adoption of fiscal monitoring policies by states and the investment decisions of firms. To mitigate this concern, we run a neighboring-state test in an additional analysis (e.g., Holmes, 1998; Ljungqvist et al., 2017; Nakhmurina, 2020; Qiu, 2019). In this test, we compare firms headquartered close to the border of states that have adopted fiscal monitoring policies with firms headquartered close to the border of states that have not adopted fiscal monitoring policies. This research design mitigates the threat that confounding economic events impact our results since firms headquartered close to state borders are subject to the same set of economic conditions.

3 | DATA

3.1 | Sample construction

Using the package "edgar" from the Comprehensive R Archive Network (CRAN) (Lonare et al., 2021), we obtain all firms that file annual reports in the US Securities and Exchange Commission's Electronic Data Gathering Analysis and Retrieval (EDGAR) system between 1995 and 2018. We then merge our list of firms with firm-level accounting data from Compustat downloaded via the University of Pennsylvania's Wharton Research Data Services (WRDS). We only keep firms headquartered in the United States. Following the corporate investment literature, we drop observations with negative assets or negative sales. Moreover, we drop observations from the financial industry (Standard Industrial Classification [SIC] codes between 6000 and 6999) or utilities industry (SIC codes between 4000 and 4999). After excluding observations without sufficient data to calculate the variables used in the regressions and after dropping singleton observations (i.e., fixed effect groups with only one observation in the available sample; Correia, 2015), our sample comprises 95,935 firm-year observations.

3.2 | Main variables and descriptive statistics

We obtain the data for computing our variable of interest, *FMP Post*, from various sources. We collect 22 states that have adopted fiscal monitoring policies from Urahn et al. (2016) and one additional state from Nakhmurina (2020). Urahn et al. (2016) identify the states that implemented fiscal monitoring policies by reviewing state government websites, analyzing state statutes as well as conducting telephone interviews with state and local officials. The additional state identified by Nakhmurina (2020) adopted fiscal monitoring policies after the publication of Urahn et al.'s (2016) report on fiscal monitoring.

Nakhmurina (2020) provides the adoption years for 10 states that have introduced fiscal monitoring policies between 2009 and 2017. As our sample period is from 1995 until 2018, we collect the adoption years of the remaining states by researching the public resources of the state auditor or state comptroller (see Nakhmurina, 2020). Following Qiu (2019), we assign firms to states by using the state reported in the business address in the annual filings.[5]

Table 1, panel A, lists the states that have adopted fiscal monitoring policies as well as the respective adoption years. As of 2019, 23 states have adopted fiscal monitoring policies. The adoption of fiscal monitoring policies is staggered over time. Over our sample period from 1995 until 2018, 13 states have adopted fiscal monitoring policies. In addition, 10 states have adopted fiscal monitoring policies before our sample period.

Table 1, panel B, presents the sample composition by state. Around 32% of all firms are headquartered in states that adopt fiscal monitoring policies over our sample period (*FMP states*). In addition, around 17% of all firms are headquartered in states that adopt fiscal monitoring policies before our sample period (*Always FMP states*). Therefore, a substantial number of firms are located in states that adopt fiscal monitoring policies.

From Compustat, we obtain data to calculate our dependent variable *Investment*. Following the large existing literature on investment (e.g., M. Baker et al., 2003; Chen et al., 2007), we define *Investment* as capital expenditures (Compustat item CAPX) plus research and development expenditures (XRD) divided by lagged total assets (AT).[6]

We also obtain data to compute our control variables from Compustat. Our main model includes the control variables *Tobin's Q* (AT minus CEQ plus CSHO multiplied by PRCC_F divided by AT), *Cash Flow* (IB plus DP divided by AT) and *Firm Size*, that is, the logarithm of AT. We compute these variables following Foucault and Fresard (2014).

In a robustness test, we use an alternative proxy for our dependent variable by computing *Capital, R&D and Acquisition Expenditures* (the sum of CAPX and XRD and ACQ divided by lagged AT). In an additional test, we split *Investment* into its components: *Capital Expenditures* (CAPX divided by lagged AT) and *R&D Expenditures* (XRD divided by lagged AT).

Dependent variables related to financing policies are also calculated using data from Compustat. Following Dessaint et al. (2019), we calculate *Total Payout* as dividend payout (DIVC) plus share repurchases (PRSTKC) divided by total assets (AT) and *Security Issue* as equity issue (SSTK) plus debt issue (DLTIS) divided by total assets (AT). In addition, we split *Total Payout* and *Security Issue* into their components. We split *Total Payout* into *Dividend Payout* (DIVC divided by AT) and *Share Repurchases* (PRSTKC divided by AT), and we split *Security Issue* into *Equity Issue* (SSTK divided by AT) and *Debt Issue* (DLTIS divided by AT).

For our channel analysis, we hand-collect data for the dependent variable *Corruption* from the annual Report to Congress on the Activities and Operations of the Public Integrity Section from the US Department of Justice (following

[5] We replace missing values for business addresses with addresses from the Loughran and McDonald 10X File Summaries obtained from the Notre Dame Software Repository for Accounting and Finance (Loughran & McDonald, 2016) and from Compustat (Compustat item STATE).

[6] Throughout our paper, research and development expenditures is set equal to zero if the item XRD is missing in Compustat. Our main results hold if we additionally include in our regressions a dummy variable to indicate firms with missing research and development expenditures (untabulated). Our main results also hold if we do not replace missing values for research and development expenditures with 0 (untabulated).

TABLE 1 Fiscal monitoring policies adoption

Panel A: Adoption years	
State	**FMP introduced**
Colorado	2013
Connecticut	(Before sample period)
Florida	2001
Iowa	(Before sample period)
Kentucky	(Before sample period)
Louisiana	2014
Maryland	(Before sample period)
Michigan	2002
Minnesota	(Before sample period)
Nevada	2015
New Hampshire	(Before sample period)
New Jersey	(Before sample period)
New Mexico	2012
New York	2013
North Carolina	(Before sample period)
Ohio	2016
Oregon	2015
Pennsylvania	2014
Rhode Island	2016
South Dakota	(Before sample period)
Tennessee	2014
Virginia	2017
Washington	(Before sample period)

Panel B: Sample composition		
	Firm years	
State	**Number**	**% of total**
Colorado	2,963	3.1%
Florida	4,807	5.0%
Louisiana	507	0.5%
Michigan	1,612	1.7%
Nevada	140	0.1%
New Mexico	1,104	1.2%
New York	8,138	8.5%
Ohio	2,888	3.0%
Oregon	1,035	1.1%
Pennsylvania	3,605	3.8%
Rhode Island	261	0.3%

(Continues)

TABLE 1 (Continued)

Panel B: Sample composition		
	Firm years	
State	**Number**	**% of total**
Tennessee	1,169	1.2%
Virginia	2,087	2.2%
FMP states	**30,316**	**31.6%**
Always FMP states	15,893	16.6%
Never FMP states	49,726	51.8%
Total	**95,935**	**100.0%**

Note: These tables provide additional information on states that adopt fiscal monitoring policies (FMP). Panel A reports the adoption years of FMP. Panel B presents the sample composition by state. The sample period is 1995–2018. We collect the 23 states that adopted fiscal monitoring policies from Nakhmurina (2020). Nakhmurina (2020) provides the adoption years for 10 states that adopted these policies between 2009 and 2017. In the spirit of Nakhmurina (2020), we collect the adoption years of the remaining states by researching the public resources of the state auditor or state comptroller. *FMP states* are states that adopt FMP over our sample period. *Always FMP states* are states that adopt FMP before our sample period. *Never FMP states* are states that do not adopt FMP.

Nakhmurina, 2020). These reports contain the annual number of corruption convictions per US Attorney's Office and are frequently used in the academic literature (E. L. Glaeser & Saks, 2006). We match the corruption data on the US Attorney's Office level to our firm-level dataset via counties. We obtain the counties of each US Attorney's Office by researching the websites of each US Attorney's Office. We identify the counties where firms are headquartered by first collecting the zip code from annual reports (obtained via EDGAR) and then using a zip code to county crosswalk provided by the Missouri Census Data Center. For the dependent variable *Scaled Corruption* (i.e., *Corruption* divided by population), we collect the population size of each county from the 2000 Census and aggregate the county population on US Attorney's Office level.

We provide definitions of all our variables in the Appendix. Table 2, panel A, presents descriptive statistics for the variables used in our main regressions and Table 2, panel B, presents descriptive statistics for dependent variables used in additional tests. We winsorize all continuous, non-logarithmic variables at the 1% and 99% levels. The average value of our dependent variable, *Investment*, is equal to 0.17. This value is comparable to the one reported by Chen et al. (2007).

3.3 | Additional independent variables

In robustness tests, we expand our set of controls by including the variables *Leverage* (DLTT plus DLC divided by AT), *Cash Holdings* (CHE divided by AT), *Sales Growth* (SALE minus lagged SALE divided by lagged SALE) and *Tangibility* (PPENT divided by AT). We compute *Cash Holdings* and *Tangibility* following Bustamante and Fresard (2021), and we calculate *Leverage* following Mitton (2022). We obtain the data to calculate these variables from Compustat.

For our channel analysis, we calculate geographic dispersion by counting the number of distinct states mentioned in the annual report (following Garcia & Norli, 2012). We obtain annual reports from EDGAR and define the dummy variable *Not Dispersed* as equal to 1 if the observation's geographic dispersion is below the median geographic dispersion of all observations in the same state and the same year.

We convert a firm location (zip code) to a latitude and longitude using data from OpenDataSoft. We obtain the latitude and longitude of state capitals from John Burkardt's website. We use the command "geodist" in Stata to calculate the distance between the location of a firm and the capital of the state in which the firm is headquartered. Following

TABLE 2 Descriptive statistics

Panel A: Main variables						
	n	mean	p25	median	p75	s.d.
Investment	95,935	0.17	0.03	0.08	0.18	0.27
FMP Post	95,935	0.24	0.00	0.00	0.00	0.43
Tobin's Q	95,935	3.08	1.15	1.63	2.74	5.46
Cash Flow	95,935	−0.20	−0.10	0.06	0.11	0.99
Firm Size	95,935	4.92	3.28	4.96	6.64	2.50
Panel B: Other dependent variables						
	n	mean	p25	median	p75	s.d.
Capital, R&D and Acquisition Expenditures	92,585	0.21	0.04	0.11	0.23	0.32
R&D Expenditures	95,935	0.09	0.00	0.01	0.09	0.20
Capital Expenditures	95,935	0.07	0.01	0.03	0.07	0.11
Total Payout	87,210	0.02	0.00	0.00	0.02	0.05
Dividend Payout	95,775	0.01	0.00	0.00	0.00	0.02
Share Repurchases	87,357	0.02	0.00	0.00	0.01	0.04
Security Issue	91,352	0.24	0.01	0.06	0.30	0.44
Equity Issue	94,471	0.11	0.00	0.01	0.04	0.28
Debt Issue	92,712	0.12	0.00	0.00	0.12	0.27
Corruption	95,652	18.86	6.00	13.00	28.00	17.21
Scaled Corruption	95,652	3.14	1.34	2.46	4.25	2.65

Note: These tables report descriptive statistics of the main variables used in our regressions. Panel A displays the variables used in the main regressions, while Panel B shows dependent variables used in other regressions. The sample period is 1995–2018. *Investment* equals capital expenditures plus research and development expenditures, all divided by lagged total assets. *FMP Post* is a dummy equal to 1 if the observation is headquartered in a state that has adopted fiscal monitoring policies and otherwise 0. *Tobin's Q* is total assets minus common/ordinary equity plus common shares outstanding multiplied by the closing share price, all divided by total assets. *Cash Flow* is income before extraordinary items plus depreciation and amortization, all divided by total assets. *Firm Size* is the logarithm of total assets. *Capital, R&D and Acquisition Expenditures* is capital expenditures plus research and development expenditures plus acquisition expenditures, all divided by lagged total assets. *R&D Expenditures* is research and development expenditures divided by lagged total assets, and *Capital Expenditures* is capital expenditures divided by lagged total assets. *Total Payout* is dividend payout plus share repurchases, all divided by total assets. *Dividend Payout* is dividend payout divided by total assets, and *Share Repurchases* is share repurchases divided by total assets. *Security Issue* is equity issue plus debt issue, all divided by total assets. *Equity Issue* is equity issue divided by total assets, and *Debt Issue* is debt issue divided by total assets. *Corruption* equals the annual number of corruption convictions per US Attorney's Office. *Scaled Corruption* is *Corruption* divided by the 2000 Census population (in millions) of the Attorney's Office district. All continuous, non-logarithm variables are winsorized at the 1% and 99% levels.

Du and Heo (2022), we generate the dummy *Close* that is equal to 1 if the observation's distance to the state capital is below the median distance to the state capital of all observations in the same state and the same year.

We follow Farre-Mensa and Ljungqvist (2016) to calculate independent variables related to financial constraints, specifically the Kaplan and Zingales (1997) index, the Whited and Wu (2006) index and the Hadlock and Pierce (2010) index. We create the dummy variable *Fin. Constr. 1*, which is equal to 1 if the observation's Kaplan and Zingales index is above the median Kaplan and Zingales index of all observations in the same state and the same year. Similarly, *Fin. Constr. 2* is equal to 1 if the observation's Whited and Wu index is above the median Whited and Wu index of all observations in the same state and the same year, and *Fin. Constr. 3* is equal to 1 if the observation's Hadlock and Pierce index

TABLE 3 Impact of fiscal monitoring policies on firm investment

	Dependent variable: Investment				
	(1)	(2)	(3)	(4)	(5)
FMP Post	0.014**	0.013**	0.014**	0.014**	0.014**
	(2.436)	(2.399)	(2.402)	(2.446)	(2.470)
Tobin's Q		0.010***			0.011***
		(9.594)			(13.618)
Cash Flow			−0.016***		0.012***
			(−3.980)		(3.960)
Firm Size				−0.009**	0.009***
				(−2.021)	(2.993)
Observations	95,935	95,935	95,935	95,935	95,935
Adj. *R*-squared	0.456	0.473	0.457	0.457	0.474
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
SE Cluster	State	State	State	State	State

Note: This table presents difference-in-differences regressions to test for the effects of state fiscal monitoring policies (*FMP Post*) on corporate investment. The dependent variable in all regressions is *Investment*. The sample period is 1995–2018. *Investment* equals capital expenditures plus research and development expenditures, all divided by lagged total assets. *FMP Post* is a dummy equal to 1 if the observation is headquartered in a state that has adopted fiscal monitoring policies and otherwise 0. *Tobin's Q* is total assets minus common/ordinary equity plus common shares outstanding multiplied by the closing share price, all divided by total assets. *Cash Flow* is income before extraordinary items plus depreciation and amortization, all divided by total assets. *Firm Size* is the logarithm of total assets. All continuous, non-logarithm variables are winsorized at the 1% and 99% levels. *t*-statistics are presented in parentheses. Standard errors (SE) are clustered at the state level.
***, ** and * indicate the significance at the 1%, 5% and 10% levels, respectively.

is above the median Hadlock and Pierce index of all observations in the same state and the same year. We obtain the data to calculate these variables from Compustat.

4 | RESULTS

4.1 | Impact of fiscal monitoring policies on corporate investment

We run difference-in-differences regressions using the specification as defined in Section 2.2 (Equation 1). The dependent variable in our regressions is *Investment* (i.e., capital expenditures plus research and development expenditures divided by lagged assets). The variable of interest is *FMP Post*, a dummy equal to 1 if the firm is headquartered in a state that has already adopted fiscal monitoring policies (otherwise the dummy is equal to 0).

We present our main results in Table 3. We run five different regressions. In column 1, we run a model with firm fixed effects and year fixed effects. The variable of interest (*FMP Post*) is statistically significant at the 5% level and has a positive coefficient of 0.014. From columns 2 to 4, we add, step by step, our three main control variables: *Tobin's Q*, *Cash Flow* and *Firm Size*. We include control variables that are primary determinants of investment decisions to reduce the risk of an omitted variable bias and to increase the precision of our estimates (Angrist & Pischke, 2008). *FMP Post* is statistically significant at the 5% level and has a positive coefficient in all three regressions. Throughout all three regressions, the coefficient of *FMP Post* is similar to the coefficient obtained in column 1. The coefficient is equal to 0.013 in column 2 and equal to 0.014 in columns 3 and 4. Adding control variables increases the precision of our estimates as each control variable is statistically significant and the adjusted *R*-squared of each regression with additional control variables (columns 2 to 4) is higher than the adjusted *R*-squared reported in our regression with firm and year fixed effects but no control variables (column 1). In column 5, we run our model with firm fixed effects and year fixed effects as well as all the three control variables. Consistent with the previous regressions, *FMP Post* is statistically significant at the 5% level. The coefficient of *FMP Post* is positive and equal to 0.014.

Having established a statistically significant relationship between the adoption of fiscal monitoring policies and corporate investment, we turn our attention to the economic significance. Mitton (2022) presents two measures for benchmarking the economic significance if the variable of interest is a dummy variable. *E1y* is the change in the dependent variable, as a percentage of its mean, associated with a change from 0 to 1 in the explanatory variable. *E1s* is the change in the dependent variable, as a percentage of its standard deviation, associated with a change from 0 to 1 in the explanatory variable. For our main result (Table 3, column 5), *E1y* equals 0.08, and *E1s* equals 0.05. These numbers are comparable to results in prior studies. For example, Becker and Strömberg (2012) find that following a legal ruling that limits managers of distressed firms to take actions that favor equity holders over debt holders, affected firms increase their investment by around 0.06 (*E1s*). Therefore, we can conclude that our results are plausible and economically significant.[7]

Overall, our results show that firms headquartered in states that adopt fiscal monitoring policies increase their investment relative to firms headquartered in unaffected states. This indicates that fiscal monitoring significantly impacts the investment of firms. Specifically, state fiscal monitoring increases the investment of firms.

State monitoring can manifest itself in lower local corruption and lower local government debt (Nakhmurina, 2020). In turn, as outlined in Section 2.1, lower local corruption or lower local government debt could lead to an increase in corporate investment. We explore these possible channels for our results in Section 4.5.

4.2 | Border analysis

Our identification strategy exploits the adoption of fiscal monitoring policies. A key feature of our identification strategy is that fiscal monitoring policies are passed by states and target local governments with the goal of increasing the state governments' understanding of the fiscal conditions of local governments (Nakhmurina, 2020). It is thus unlikely that the adoption of fiscal monitoring policies is endogenously driven by firm-specific conditions. Another important feature is that the adoption of fiscal monitoring is staggered over time and states. The staggered adoption of policies further alleviates concerns that results are driven by concurrent events. Moreover, our regression models include a comprehensive set of fixed effects and control variables to further mitigate the risk that factors that correlate with both the adoption of fiscal monitoring policies and corporate investment affect our results.

Although our identification strategy substantially raises the bar that concurrent events impact our findings, it is still possible that unrelated events could confound our results. This could be the case if such events were staggered over time concurrently with the fiscal monitoring policies introduced by states. In particular, one concern could be that changing economic conditions can determine both the adoption of fiscal monitoring policies by states and the investment decisions of firms.

To address this threat to identification, we run a neighboring-state test that exploits that firms headquartered close to state borders are subject to the same set of economic conditions (Ljungqvist et al., 2017). In this test, we compare observations headquartered at the border of states that have adopted fiscal monitoring policies with observations headquartered at the border of states that have not adopted fiscal monitoring policies (Nakhmurina, 2020).

In Table 4, we report the results of this border analysis. In the spirit of Qiu (2019), we restrict the sample to firms headquartered in counties whose centroid is located within 25 miles (column 1), 50 miles (column 2), 75 miles (column

[7] Mitton (2022) provides benchmarks for assessing the economic significance if corporate investment is proxied by capital expenditures. In Section 4.4, we also use capital expenditures as a proxy for corporate investment and further discuss the economic significance of our results.

TABLE 4 Border analysis

	Dependent variable: Investment			
Border:	**25 miles**	**50 miles**	**75 miles**	**100 miles**
	(1)	**(2)**	**(3)**	**(4)**
FMP Post	0.036***	0.025**	0.026**	0.026***
	(3.030)	(2.546)	(2.627)	(3.248)
Tobin's Q	0.010***	0.011***	0.010***	0.010***
	(6.255)	(7.740)	(6.806)	(8.672)
Cash Flow	0.001	0.008	–0.001	0.009*
	(0.093)	(1.338)	(–0.238)	(1.894)
Firm Size	0.014***	0.011***	0.009*	0.010**
	(2.753)	(2.854)	(2.004)	(2.537)
Observations	15,017	24,193	32,225	50,523
Adj. *R*-squared	0.512	0.515	0.515	0.483
Firm FE	Yes	Yes	Yes	Yes
State-Border FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
SE Cluster	State	State	State	State

Note: This table presents difference-in-differences regressions to test for the effects of state fiscal monitoring policies (*FMP Post*) on corporate investment of firms headquartered close to the border of monitoring and non-monitoring states. The dependent variable in all regressions is *Investment*. In column 1, we restrict the sample to firms headquartered in counties within 25 miles of the state border. In column 2, we restrict the sample to firms headquartered in counties within 50 miles of the state border. In column 3, we restrict the sample to firms headquartered in counties within 75 miles of the state border. In column 4, we restrict the sample to firms headquartered in counties within 100 miles of the state border. The sample period is 1995–2018. *Investment* equals capital expenditures plus research and development expenditures, all divided by lagged total assets. *FMP Post* is a dummy equal to 1 if the observation is headquartered in a state that has adopted fiscal monitoring policies and otherwise 0. *Tobin's Q* is total assets minus common/ordinary equity plus common shares outstanding multiplied by the closing share price, all divided by total assets. *Cash Flow* is income before extraordinary items plus depreciation and amortization, all divided by total assets. *Firm Size* is the logarithm of total assets. All continuous, non-logarithm variables are winsorized at the 1% and 99% levels. *t*-statistics are presented in parentheses. SE are clustered at the state level.
***, ** and * indicate the significance at the 1%, 5% and 10% levels, respectively.

3) and 100 miles (column 4) of state borders.[8] Following Nakhmurina (2020), we add state-border fixed effects to our regression model.

Across all four regressions in Table 4, the variable of interest, *FMP Post*, stays positive and statistically significant. Compared to our main results, the coefficient of our variable of interest increases to 0.025 (column 2), 0.026 (columns 3 and 4) or 0.036 (column 1). In addition, the statistical significance of *FMP Post* increases to 1% if we restrict our sample to firms headquartered within 25 miles (column 1) or 100 miles (column 4) of the state borders. The results show that firms headquartered in counties of states that have adopted fiscal monitoring policies increase their investment compared to firms headquartered in counties of bordering states that have not adopted fiscal monitoring policies. Since firms headquartered close to state borders are subject to the same set of economic conditions, we can rule out economic events as a confound of our finding that state fiscal monitoring increases corporate investment. We therefore gain confidence in a causal interpretation of our main result that state monitoring increases corporate investment.

[8] We retrieve the distance of counties to state borders from The State Border Data Set from Holmes (1998).

4.3 | Robustness tests

In the next section, we run several tests to ensure that our results are robust. We present our results in Table 5, panels A and B. The dependent variable in all regressions, except in panel A, column 4, is *Investment*.

First, we alter our treatment of outliers. In our main results, we winsorize all continuous, non-logarithmic variables at the 1% and 99% levels to reduce the influence of extreme values on our results. In Table 5, panel A, column 1, we trim all continuous, non-logarithmic variables at the 1% and 99% levels. Similar to our main results, the variable of interest has a positive coefficient and is statistically significant at the 5% level. In further steps to eliminate potential outliers in our sample that could influence our results, we winsorize all continuous, non-logarithmic variables at the 5% and 95% levels in column 2, and we winsorize all continuous, non-logarithmic variables at the 10% and 90% levels in column 3. Our results hold as the *FMP Post* dummy has a positive coefficient and is statistically significant at the 5% level in both regressions. Based on these tests, we can conclude that our results are robust to changing our treatment of outliers.

Second, to make sure that our results do not depend on the definition of our investment variable, in Table 5, panel A, column 4, we run a regression with a different proxy for firm investment as we sum capital expenditures, research and development expenditures and acquisition expenditures (e.g., Derrien & Kecskes, 2013). Our results hold as *FMP Post* has a positive coefficient and is statistically significant.

In our third set of robustness tests, we change our clustering of standard errors. In our main results, we cluster standard errors by state since our treatment occurs at the state level (e.g., Bai et al., 2020; Garmaise, 2011). Clustering at the state level takes into account that residuals are serially correlated within a firm and also correlated across firms within the same state (Bertrand et al., 2004). When clustering by state, however, the number of clusters might not be sufficient to derive consistent estimates of standard errors (MacKinnon & Webb, 2017). Therefore, in Table 5, panel B, column 1, we cluster standard errors by firm (e.g., Chy et al., 2021). Another concern could be that residuals are also serially correlated over time (Thompson, 2011). Hence, in column 2, we cluster standard errors by year and by state (e.g., S. Glaeser, 2018). The coefficient of our variable of interest (*FMP Post*) is positive and statistically significant (at the 1% level in column 1 and at the 5% level in column 2). Therefore, our results do not seem to critically depend on the method for clustering standard errors.

Next, we enhance our set of control variables to further decrease the threat of an omitted variable bias. In Table 5, panel B, column 3, we add state fixed effects (e.g., Li et al., 2018). In our data, state fixed effects are not perfectly collinear with firm fixed effects since firms can change their headquarters to a different state during our sample period. Adding state fixed effects to our model further decreases the threat of an omitted variable bias as this controls for state-specific and time-invariant differences in investment.[9] If we include state fixed effects, *FMP Post* remains statistically significant at the 5% level and the coefficient increases to 0.021.

In Table 5, panel B, column 4, we add state-level control variables, and in Table 5, panel B, column 5, we add additional firm-level control variables.[10] In the spirit of Nakhmurina (2020), we control for time-varying changes in state characteristics by adding variables that capture socio-demographic characteristics of states (*Population*, *Working Age Population* and *Educational Attainment*) as well as economic characteristics of states (*Personal Income*, *House Price Index* and *Unemployment Rate*). We control for additional time-varying changes in the determinants of investment decisions by adding the variables *Leverage*, *Cash Holdings*, *Sales Growth* and *Tangibility* (e.g., Bustamante & Fresard, 2021; Foucault & Fresard, 2014). If we add state-level control variables (panel B, column 4), *FMP Post* is statistically significant at the 1% level and has a positive coefficient. In our regression with additional firm-level control variables (panel B, column 5), *FMP Post* is also statistically significant (at the 1% level) and has a positive coefficient. These results indicate that time-varying changes in state characteristics as well as firm characteristics do not drive our results.

[9] At the same time, the state fixed effects might not accurately capture these time-invariant differences in investment across states as they are identified using a relatively small number of firms that have moved their headquarters to a different state (see also the discussion on the "limited mobility bias" by Andrews et al., 2008, 2012).

[10] While the inclusion of additional control variables can further decrease the threat of an omitted variable bias, as well as increase the precision of our estimates, "bad controls" (e.g., control variables that are themselves outcome variables) could potentially lead to biased estimates (Angrist & Pischke, 2008).

TABLE 5 Robustness tests

Panel A: Winsorizing and definition of investment				
Test:	**Trim: 1%/99%**	**Wins.: 5%/95%**	**Wins.: 10%/90%**	**Capital, R&D and Acquisition Expenditures**
	(1)	**(2)**	**(3)**	**(4)**
FMP Post	0.010**	0.008**	0.006**	0.014*
	(2.171)	(2.421)	(2.066)	(1.810)
Tobin's Q	0.012***	0.019***	0.019***	0.013***
	(16.418)	(22.077)	(31.244)	(15.813)
Cash Flow	0.002	−0.001	−0.000	0.005
	(0.515)	(−0.226)	(−0.036)	(1.353)
Firm Size	−0.000	0.007***	0.004*	0.039***
	(−0.125)	(2.844)	(1.898)	(7.875)
Observations	91,776	95,935	95,935	92,585
Adj. *R*-squared	0.534	0.624	0.666	0.350
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
SE Cluster	State	State	State	State

Panel B: Standard errors, controls and stacked regression model						
Test:	**Clustering of SE: Firm**	**Clustering of SE: State and Year**	**Additional state fixed effects**	**Additional state-level controls**	**Additional firm-level controls**	**Stacked regression model**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
FMP Post	0.014***	0.014**	0.021**	0.012***	0.012***	
	(2.831)	(2.593)	(2.508)	(3.109)	(2.686)	

(*Continued*)

TABLE 5 Robustness tests

Panel B: Standard errors, controls and stacked regression model						
Test:	Clustering of SE: Firm	Clustering of SE: State and Year	Additional state fixed effects	Additional state-level controls	Additional firm-level controls	Stacked regression model
	(1)	(2)	(3)	(4)	(5)	(6)
Tobin's Q	0.011***	0.011***	0.011***	0.011***	0.009***	0.011***
	(17.866)	(7.912)	(13.685)	(13.776)	(12.582)	(6.744)
Cash Flow	0.012***	0.012**	0.012***	0.012***	−0.001	0.014***
	(3.607)	(2.695)	(3.927)	(3.871)	(−0.432)	(3.892)
Firm Size	0.009***	0.009*	0.009***	0.009***	0.002	0.031***
	(3.902)	(1.817)	(2.934)	(3.021)	(0.493)	(6.350)
$FMP[t=-3]$						−0.005
						(−0.928)
$FMP[t=-1]$						−0.001
						(−0.104)
$FMP[t=0]$						0.015
						(1.552)
$FMP[t=1]$						0.019*
						(1.671)
$FMP[t=2]$						0.011
						(0.966)
$FMP[t=3]$						0.033***
						(3.066)
$FMP[t=4]$						0.026*
						(1.949)

(Continues)

TABLE 5 (Continued)

Panel B: Standard errors, controls and stacked regression model						
Test:	Clustering of SE: Firm	Clustering of SE: State and Year	Additional state fixed effects	Additional state-level controls	Additional firm-level controls	Stacked regression model
	(1)	(2)	(3)	(4)	(5)	(6)
State-level controls	No	No	No	Yes	No	No
Additional firm-level controls	No	No	No	No	Yes	No
Observations	95,935	95,935	95,935	95,630	94,000	112,441
Adj. *R*-squared	0.474	0.474	0.475	0.476	0.528	0.583
Firm FE	Yes	Yes	Yes	Yes	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	No
State FE	No	No	Yes	No	No	No
Firm-FMP Event FE	No	No	No	No	No	Yes
Year-FMP Event FE	No	No	No	No	No	Yes
SE Cluster	Firm	State and year	State	State	State	State event

Note: These tables present difference-in-differences regressions to test for the effects of state fiscal monitoring policies (*FMP Post*) on corporate investment. The dependent variable in all regressions, except in column 4 of panel A, is *Investment*. In our first set of robustness tests in panel A, we change our treatment of outliers. In column 1, we trim all continuous, non-logarithm variables at the 1% and 99% levels. In column 2, we winsorize (wins.) all continuous, non-logarithm variables at the 5% and 95% levels. In column 3, we winsorize (wins.) all continuous, non-logarithm variables at the 10% and 90% levels. In our second set of robustness tests in panel A, we change our definition of *Investment* as our dependent variable in column 4 is the sum of *Capital, R&D and Acquisition Expenditures*. In our third set of robustness tests in panel B, we change our approach to clustering standard errors (SE). In column 1, we cluster by firm. In column 2, we cluster by state and year. In our fourth set of robustness tests in panel B, we change our control variables. In column 3, we add state fixed effects. In column 4, we add the state-level control variables *Population*, *Working Age Population*, *Educational Attainment*, *Personal Income*, *House Price Index* and *Unemployment Rate*. In column 5, we add the firm-level control variables *Leverage*, *Cash Holdings*, *Sales Growth* and *Tangibility*. In our final robustness test in panel B, we change our estimation model. In column 6, we run a stacked regression in the spirit of Cengiz et al. (2019). The sample period is 1995–2018. *Investment* equals capital expenditures plus research and development expenditures, all divided by lagged total assets. *FMP Post* is a dummy equal to 1 if the observation is headquartered in a state that has adopted fiscal monitoring policies and otherwise 0. *Tobin's Q* is total assets minus common/ordinary equity plus common shares outstanding multiplied by the closing share price, all divided by total assets. *Cash Flow* is income before extraordinary items plus depreciation and amortization, all divided by total assets. *Firm Size* is the logarithm of total assets. *Capital, R&D and Acquisition Expenditures* is capital expenditures plus research and development expenditures plus acquisition expenditures, all divided by lagged total assets. *Population* equals the logarithm of the annual state population. *Working Age Population* is the percentage of the annual state population between the ages of 25 and 64. *Educational Attainment* is the percentage of the annual state population that has a high school diploma or higher. *Personal Income* is the annual personal income per state. *House Price Index* is the annual housing price index (purchase-only index) per state. *Unemployment Rate* is the annual unemployment rate per state. *Leverage* is long-term debt plus debt in current liabilities, all divided by total assets. *Cash Holdings* is cash holdings divided by total assets. *Sales Growth* is sales minus lagged sales, all divided by lagged sales. *Tangibility* is total net property, plant and equipment divided by assets. All continuous, non-logarithm variables are winsorized at the 1% and 99% levels (with the exception of columns 1, 2 and 3 in panel A). *t*-statistics are presented in parentheses. Standard errors (SE) are clustered at the state level (with the exception of columns 1, 2 and 6 in panel B).

***, ** and * indicate the significance at the 1%, 5% and 10% levels, respectively.

Estimates in staggered difference-in-differences regressions could potentially be biased if treatment effects evolve over time (e.g., see A. Baker et al., 2022). We address this concern by running a stacked regression in the spirit of Cengiz et al. (2019). For the stacked regression, we create separate datasets for each "FMP event" (i.e., a year in which one or more states introduce fiscal monitoring policies). The sample of each dataset includes firms located in states that adopt fiscal monitoring policies at the FMP event (treated observations) and firms headquartered in states that do not adopt fiscal monitoring policies between three years before and four years after the FMP event (clean control observations). We then stack each dataset and run an OLS regression. The OLS regression is based on the specification as defined in Equation (1), but we replace the *FMP Post* dummy with lead and lag time dummies (e.g., $FMP[t = -1]$). We also replace firm and year fixed effects with firm-FMP event and year-FMP event fixed effects.

Our results in Table 5, panel B, column 6, show that treated firms only increase their investment compared to clean control firms, in the years following the adoption of fiscal monitoring policies. Hence, our results hold in an alternative model that mitigates concerns related to biased estimates in staggered difference-in-differences regressions.

Finally, in unreported robustness tests, we address the concern that a few treated states drive our results. To this end, we drop the observations from each treated state one at a time and re-estimate our main model without these observations. Across all our regression models, we continue to find a significantly positive effect of fiscal monitoring policies on corporate investment. This indicates that our main finding is not due to observations from one treated state only. Overall, our analyses in this section show that our results are robust to a substantial number of research design choices.

4.4 | Components of investment and financing policies

Our results show that firms increase their investment following the adoption of fiscal monitoring policies. To understand this result better, we assess two questions related to this finding. First, we examine which type of investment increases (Table 6). Second, we explore how firms fund their increase in investment (Table 7).

To examine which type of investment increases, in Table 6, we split our dependent variable, *Investment*, into its components: *Capital Expenditures* (column 1) and *R&D Expenditures* (column 2), and estimate regressions using the same control variables and fixed effects as in Equation (1). If the dependent variable is *Capital Expenditures*, the variable of interest, *FMP Post*, is statistically significant and has a positive coefficient (column 1). *FMP Post* is also statistically significant and has a positive coefficient if the dependent variable is *R&D Expenditures* (column 2). Consequently, firms increase both capital expenditures and research and development expenditures following the introduction of fiscal monitoring policies.

Mitton (2022) provides benchmarks for assessing the economic significance for regressions with capital expenditures as the dependent variable. These benchmarks are based on papers published in the *Journal of Finance*, *Journal of Financial Economics* and *Review of Financial Studies* from 2000 to 2018. Specifically, Mitton (2022) computes *E1y* as the change in the dependent variable, as a percentage of its mean, associated with a change from 0 to 1 in the explanatory variable and *E1s* as the change in the dependent variable, as a percentage of its standard deviation, associated with a change from 0 to 1 in the explanatory variable. For our results with *Capital Expenditures* as our dependent variable (Table 6, column 1), *E1y* equals 0.08 and *E1s* equals 0.05. These results lie between the 25th percentile ($E1y = 0.07$; $E1s = 0.04$) and median ($E1y = 0.18$; $E1s = 0.08$) of the benchmarks provided by Mitton (2022). This further confirms that our results are plausible and economically significant.

Next, we examine whether firms change their financing policies to fund their increase in investment. In Table 7, we run our main regression model (Equation 1 in Section 2.2) but replace the dependent variable, *Investment*, with different financing variables. In the spirit of Dessaint et al. (2019), we look at financing decisions related to payout (from columns 1 to 3) and new security issuances (from columns 4 to 6).

In Table 7, column 1, the dependent variable is *Total Payout* (i.e., the sum of dividend payout and share repurchases), and *FMP Post* has a negative coefficient (statistically significant at the 1% level). This implies that firms decrease their

TABLE 6 Components of firm investment

Dependent variable:	Capital Expenditures (1)	R&D Expenditures (2)
FMP Post	0.006*	0.008*
	(1.842)	(1.986)
Tobin's Q	0.004***	0.006***
	(15.549)	(8.561)
Cash Flow	0.013***	−0.002
	(9.458)	(−1.196)
Firm Size	0.011***	−0.003
	(9.309)	(−1.404)
Observations	95,935	95,935
Adjusted *R*-squared	0.423	0.633
Firm FE	Yes	Yes
Year FE	Yes	Yes
SE Cluster	State	State

Note: This table presents difference-in-differences regressions to test for the effects of state fiscal monitoring policies (*FMP Post*) on corporate investment. The dependent variable is *Capital Expenditures* in column 1 and *R&D Expenditures* in column 2. The sample period is 1995–2018. *Capital Expenditures* is capital expenditures divided by lagged total assets, and *R&D Expenditures* is research and development expenditures divided by lagged total assets. *FMP Post* is a dummy equal to 1 if the observation is headquartered in a state that has adopted fiscal monitoring policies and otherwise 0. *Tobin's Q* is total assets minus common/ordinary equity plus common shares outstanding multiplied by the closing share price, all divided by total assets. *Cash Flow* is income before extraordinary items plus depreciation and amortization, all divided by total assets. *Firm Size* is the logarithm of total assets. All continuous, non-logarithm variables are winsorized at the 1% and 99% levels. *t*-statistics are presented in parentheses. Standard errors (SE) are clustered at the state level.
***, ** and * indicate the significance at the 1%, 5% and 10% levels, respectively.

Total Payout following the introduction of fiscal monitoring policies as one source to finance their increased investment. To better understand this effect, we split *Total Payout* into its components: *Dividend Payout* (column 2) and *Share Repurchases* (column 3). While the variable of interest is insignificant in the regression with *Dividend Payout* as the dependent variable, the coefficient of *FMP Post* is negative and statistically significant at the 1% level if the dependent variable is *Share Repurchases*. Following better fiscal monitoring, firms decrease their total payout by decreasing their share repurchases. A decrease in share repurchases increases the funds available to firms, thereby helping firms to finance their increased investment. It seems plausible that firms change their policies regarding share repurchases, but make no changes to their dividend policies, since management has more discretion in changing the amount of shares repurchased. In contrast, dividends are more sticky (DeAngelo et al., 2009).

In Table 7, column 4, the dependent variable is *Security Issue*, and the coefficient of *FMP Post* is insignificant. *Security Issue* is composed of *Equity Issue* and *Debt Issue*. There is no statistically significant relationship between *FMP Post* and *Equity Issue* (column 5). Thus, there is no evidence that firms issue equity after the adoption of fiscal monitoring policies. In contrast, the coefficient of *FMP Post* is positive and statistically significant at the 5% level if the dependent variable is *Debt Issue* (column 6), suggesting that firms raise capital by issuing additional debt after the introduction of fiscal monitoring policies. Raising capital helps firms to finance their increased investment needs. The increase in debt, rather than equity, is plausible since debt can be considered a less expensive source of capital than equity.

To summarize these findings, following the adoption of fiscal monitoring policies, affected firms decrease share repurchases and increase debt issuance. Both of these financing policies increase the funds available to firms, thereby

TABLE 7 Financing policies

Dependent variable:	Total Payout	Dividend Payout	Share Repurchases	Security Issue	Equity Issue	Debt Issue
	(1)	(2)	(3)	(4)	(5)	(6)
FMP Post	−0.004***	−0.000	−0.003***	0.012	0.000	0.013**
	(−3.038)	(−0.986)	(−3.415)	(1.450)	(0.005)	(2.112)
Tobin's Q	0.001***	0.000***	0.000***	0.014***	0.012***	0.000
	(4.967)	(5.453)	(4.047)	(19.611)	(22.738)	(0.595)
Cash Flow	0.003***	0.001***	0.001**	−0.115***	−0.032***	−0.056***
	(4.364)	(5.632)	(2.630)	(−23.054)	(−12.525)	(−12.719)
Firm Size	−0.000	−0.000*	0.001*	0.016***	0.007***	0.010***
	(−0.314)	(−1.980)	(1.993)	(4.910)	(3.256)	(5.811)
Observations	87,210	95,775	87,357	91,352	94,471	92,712
Adj. *R*-squared	0.329	0.516	0.262	0.493	0.464	0.390
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
SE Cluster	State	State	State	State	State	State

Note: This table presents difference-in-differences regressions to test for the effects of state fiscal monitoring policies (*FMP Post*) on corporate financing policies. The dependent variable in column 1 is *Total Payout*. The dependent variable is *Dividend Payout* in column 2 and *Share Repurchases* in column 3. The dependent variable in column 4 is *Security Issue*. The dependent variable is *Equity Issue* in column 5 and *Debt Issue* in column 6. The sample period is 1995–2018. *Total Payout* is dividend payout plus share repurchases, all divided by total assets. *Dividend Payout* is dividend payout divided by total assets, and *Share Repurchases* is share repurchases divided by total assets. *Security Issue* is equity issue plus debt issue, all divided by total assets. *Equity Issue* is equity issue divided by total assets, and *Debt Issue* is debt issue divided by total assets. *FMP Post* is a dummy equal to 1 if the observation is headquartered in a state that has adopted fiscal monitoring policies and otherwise 0. *Tobin's Q* is total assets minus common/ordinary equity plus common shares outstanding multiplied by the closing share price, all divided by total assets. *Cash Flow* is income before extraordinary items plus depreciation and amortization, all divided by total assets. *Firm Size* is the logarithm of total assets. All continuous, non-logarithm variables are winsorized at the 1% and 99% levels. *t*-statistics are presented in parentheses. Standard errors (SE) are clustered at the state level.
***, ** and * indicate the significance at the 1%, 5% and 10% levels, respectively.

helping firms to finance their increased investment. These findings give further context and credibility to our main results.

4.5 | Channels

Having provided evidence that state fiscal monitoring increases corporate investment, we now investigate potential channels behind these results. In particular, following the arguments developed in Section 2.1, we investigate whether a reduction in local corruption or a decrease in local government debt is the main channel driving the positive effect of fiscal monitoring on firm investment.

Corruption could depress corporate investment by decreasing economic growth and investment opportunities (N. D. Johnson et al., 2011) or because corruption increases the risk of expropriation (Du & Heo, 2022; Ellis et al., 2020; Murphy et al., 1993). Moreover, local corruption could lead to firms substituting investment with rent-seeking activities (Q. Huang & Yuan, 2021). Consistent with this, firms headquartered in areas with higher local corruption have lower investment compared to firms headquartered in areas with lower local corruption (Smith, 2016).

State fiscal monitoring results in lower local corruption. We confirm this finding of Nakhmurina (2020) in Table 8, panel A, by running regressions where the dependent variable is *Corruption* (column 1) or *Scaled Corruption* (i.e.,

TABLE 8 Channels

Panel A: Corruption				
Dependent variable:	**Corruption**	**Scaled Corruption**	**Investment**	
	(1)	**(2)**	**(3)**	**(4)**
FMP Post	−3.678***	−0.860***	0.013*	0.003
	(−2.688)	(−4.184)	(1.747)	(0.570)
Tobin's Q	0.021	0.004	0.013***	0.010***
	(1.135)	(1.487)	(12.158)	(13.525)
Cash Flow	−0.008	−0.014	0.020***	0.010**
	(−0.072)	(−0.774)	(3.399)	(2.629)
Firm Size	0.303	0.058*	0.005*	0.012***
	(1.485)	(1.964)	(1.988)	(3.260)
*Not Dispersed * FMP Post*			0.015*	
			(1.820)	
*Close * FMP Post*				0.025***
				(2.784)
Observations	95,652	95,652	79,049	95,346
Adj. R-squared	0.596	0.469	0.506	0.490
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
SE Cluster	State	State	State	State

Panel B: Financing constraints			
Dependent variable:	**Investment**		
	(1)	**(2)**	**(3)**
FMP Post	0.019**	0.012**	0.013**
	(2.134)	(2.177)	(2.237)
Tobin's Q	0.018***	0.020***	0.020***
	(13.440)	(11.806)	(14.005)
Cash Flow	0.006	−0.065***	0.028**
	(0.720)	(−2.831)	(2.465)
Firm Size	−0.001	−0.008**	−0.004*
	(−0.342)	(−2.322)	(−1.809)
*Fin. Constr. 1 * FMP Post*	−0.007		
	(−0.682)		
*Fin. Constr. 2 * FMP Post*		0.004	
		(0.550)	
*Fin. Constr. 3 * FMP Post*			0.001
			(0.067)
Observations	88,394	92,048	94,801
Adj. R-squared	0.529	0.532	0.489

(Continues)

TABLE 8 (Continued)

Panel B: Financing constraints			
Dependent variable:	**Investment**		
	(1)	**(2)**	**(3)**
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
SE Cluster	State	State	State

Note: These tables present difference-in-differences regressions related to the corruption channel (panel A) and financing constraints channel (panel B). In panel A, the dependent variable in column 1 is *Corruption* and in column 2, *Scaled Corruption*. In columns 3 and 4, the dependent variable is *Investment*. In column 3, we define a dummy variable, *Not Dispersed*, that is equal to 1 if the observation's geographic dispersion is below the median geographic dispersion of all observations in the same state and same year. We interact *Not Dispersed* with all independent variables. In column 4, we generate the dummy *Close* that is equal to 1 if the observation's distance to the state capital is below the median distance to the state capital of all observations in the same state and year. We interact *Close* with all independent variables. In panel B, the dependent variable in all regressions is *Investment*. In column 1, we create a dummy variable, *Fin. Constr. 1*, which is equal to 1 if the observation's Kaplan and Zingales (1997) index is above the median Kaplan and Zingales index for all observations in the same state and same year. We interact *Fin. Constr. 1* with all independent variables. In column 2, we create a dummy variable, *Fin. Constr. 2*, which is equal to 1 if the observation's Whited and Wu (2006) index is above the median Whited and Wu index for all observations in the same state and same year. We interact *Fin. Constr. 2* with all independent variables. In column 3, we create a dummy variable, *Fin. Constr. 3*, which is equal to 1 if the observation's Hadlock and Pierce (2010) index is above the median Hadlock and Pierce index for all observations in the same state and same year. We interact *Fin. Constr. 3* with all independent variables. The sample period is 1995–2018. *Corruption* equals the annual number of corruption convictions per US Attorney's Office. *Scaled Corruption* is *Corruption* divided by the 2000 Census population (in millions) of the Attorney's Office district. *Investment* equals capital expenditures plus research and development expenditures, all divided by lagged total assets. *FMP Post* is a dummy equal to 1 if the observation is headquartered in a state that has adopted fiscal monitoring policies and otherwise 0. *Tobin's Q* is total assets minus common/ordinary equity plus common shares outstanding multiplied by the closing share price, all divided by total assets. *Cash Flow* is income before extraordinary items plus depreciation and amortization, all divided by total assets. *Firm Size* is the logarithm of total assets. All continuous, non-logarithm variables are winsorized at the 1% and 99% levels. *t*-statistics are presented in parentheses. Standard errors (SE) are clustered at the state level.
***, ** and * indicate the significance at the 1%, 5% and 10% levels, respectively.

Corruption divided by population; column 2). We document that fiscal monitoring policies decrease local corruption as the coefficient of *FMP Post* is negative and statistically significant at the 1% level in both regressions.

If this documented decrease in corruption drives our results, we expect firms that benefit more from a decrease in corruption to have a higher increase in investment compared to firms that benefit less from a decrease in corruption. Specifically, firms that are more vulnerable to corruption should have a higher increase in investment following the introduction of fiscal monitoring policies compared to firms that are less vulnerable to corruption.

We use two proxies for a firm's vulnerability to corruption. Prior literature (e.g., Bai et al., 2019; Q. Huang & Yuan, 2021; Smith, 2016) argues that firms that are not geographically dispersed are more vulnerable to corruption as these firms have less flexibility to allocate resources to different states and, thus, less bargaining power against local corrupt officials. In contrast, firms that are geographically dispersed have higher bargaining power against local corrupt officials as these firms have a credible threat to move their operations to a different state. Therefore, following prior literature (e.g., Q. Huang & Yuan, 2021), our first proxy for a firm's vulnerability to corruption is the geographic dispersion of a firm.

Du and Heo (2022) posit that firms that are geographically far from the state capital are less vulnerable to corruption as they are less politically visible. In contrast, firms that are geographically close to the state capital are more politically visible and thus more vulnerable to political corruption. Hence, following Du and Heo (2022), our second proxy for a firm's vulnerability to corruption is the distance of the headquarter of the firm to the state capital city.

We present the results of these subsample tests in columns 3 and 4 in Table 8, panel A. In column 3, we interact *Not Dispersed* with all independent variables.[11] The interaction of *Not Dispersed* with *FMP Post* is significant at the 10% level, and the coefficient of the interaction is positive. This indicates that firms that are comparatively less geographically dispersed (and thus more vulnerable to corruption) experience a higher increase in investment compared to firms that are more geographically dispersed (and thus less vulnerable to corruption). In column 4, we interact *Close* with all independent variables.[12] The interaction of *Close* and *FMP Post* is significant at the 1% level, and the coefficient is positive. This result reiterates that firms that are geographically close to the state capital (and are thus more vulnerable to corruption) have a higher increase in investment compared to firms that are geographically far from the state capital (i.e., less vulnerable to corruption).

The results in columns 3 and 4 in Table 8, panel A, provide evidence that, following the introduction of fiscal monitoring policies, firms that are more vulnerable to corruption experience a higher increase in investment compared to firms that are less vulnerable to corruption. As firms that are more vulnerable to corruption benefit more from a decrease in corruption, these findings are consistent with the notion that firms increase their investment due to a reduction in local corruption.

Another potential channel to explain our main result is local government debt. Lower government debt could increase corporate investment by increasing the credit supply to firms and thereby loosening financing constraints for firms (Y. Huang et al., 2018, 2020). Consistent with this, Y. Huang et al. (2020) document that corporate investment is negatively associated with local government debt in China and that this association is stronger for financially constrained firms. We therefore test in Table 8, panel A, whether firms with higher financing constraints experience a higher increase in investment following the introduction of fiscal monitoring policies compared to firms with lower financing constraints.

In our tests, we use our main regression model of Table 3, column 5, and split our sample into two groups: firms with high financing constraints and firms with low financing constraints. In Table 8, panel B, column 1, we interact the indicator variable *Fin. Constr. 1*, which is based on the Kaplan and Zingales (1997) index, with all independent variables. The interaction of *FMP Post* and *Fin. Constr. 1* is statistically insignificant. In column 2, we interact the dummy *Fin. Constr. 2*, which is based on the Whited and Wu (2006) index, with all independent variables. The interaction of *FMP Post* and *Fin. Constr. 2* is again not statistically significant. In column 3, we interact the dummy variable *Fin. Constr. 3*, which is based on the Hadlock and Pierce (2010) index, with all independent variables. Consistent with the previous results, the interaction of *FMP Post* and *Fin. Constr. 3* is statistically insignificant.[13]

These regressions show that more financially constrained firms do not experience a different change in investment following the introduction of fiscal monitoring policies compared to firms that have lower financing constraints. Consequently, we interpret this evidence as inconsistent with the notion that lower local government debt is a channel to explain the increase in investment following the adoption of fiscal monitoring policies.

5 | CONCLUSION

We study the effect of fiscal monitoring on corporate investment. Our identification strategy exploits the adoption of fiscal monitoring policies. A key feature of our identification strategy is that fiscal monitoring policies are passed by

[11] In the table, we only display the interaction of *Not Dispersed* with *FMP Post*. We do not show the interaction of the *Not Dispersed* dummy with the control variables. Note that as we interact the *Not Dispersed* dummy with all control variables, including fixed effects, we cannot include the *Not Dispersed* dummy as a separate variable in our regression.

[12] In the table, we only display the interaction of *Close* with *FMP Post*. We do not show the interaction of the *Close* dummy with the control variables. Note that as we interact the *Close* dummy with all control variables, including fixed effects, we cannot include the *Close* dummy as a separate variable in our regression.

[13] In all columns in Table 8, panel B, we only display the interaction of the financial constraints dummies with the variable of interest *FMP Post* (i.e., *Fin. Constr. 1 * FMP Post* in column 1, *Fin. Constr. 2 * FMP Post* in column 2 and *Fin. Constr. 3 * FMP Post* in column 3). We do not show the interaction of the financial constraints dummies with the control variables. Note that as we interact the financial constraints dummies with all control variables, including fixed effects, we cannot include the financial constraints dummies as separate variables in our regressions.

states and target local governments with the goal of increasing the state governments' understanding of the fiscal conditions of local governments (Nakhmurina, 2020). Another important feature is that the adoption of fiscal monitoring is staggered over time and states. The staggered adoption of policies that are plausibly exogenous to firms alleviates concerns that results are driven by concurrent events.

We find that the introduction of fiscal monitoring policies increases corporate investment. The increase in investment is economically significant and plausible. Our results hold in a neighboring-state test, where we compare firms that are headquartered at the border of states that have adopted fiscal monitoring policies with firms that are headquartered at the border of states that have not adopted fiscal monitoring policies. The neighboring-state test mitigates the threat that confounding economic events impact our results. Our results also stay significant in a battery of robustness tests, including changes to the definition of corporate investment, changes to the model specifications and changes to other research design choices, such as the treatment of outliers.

In an additional analysis, we find that the increase in investment is driven by an increase in capital expenditures as well as an increase in research and development expenditures. We also show that firms decrease their share repurchases and issue more debt. These findings give further context and credibility to our result since the increase in funds allows firms to undertake additional investments.

Finally, we examine plausible channels for our results. We present evidence that the increase in investment is driven by a reduction in local corruption. In contrast, we do not find evidence for firms increasing their investment due to less financing constraints. Taken together, our results provide evidence that fiscal monitoring is a meaningful determinant of corporate investment decisions.

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DECLARATION OF INTEREST

None.

DATA AVAILABILITY STATEMENT

Data are available from the sources cited in the manuscript.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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APPENDIX

Variable definitions

Variable	Definition (source)
Dependent variables	
Investment	(CAPX + XRD)/lagged AT (Compustat)
Capital, R&D and Acquisition Expenditures	(CAPX + XRD + AQC)/lagged AT (Compustat)
Capital Expenditures	CAPX/lagged AT (Compustat)
R&D Expenditures	XRD/lagged AT (Compustat)
Total Payout	(DIVC + PRSTKC)/AT (Compustat)
Security Issue	(SSTK + DLTIS)/AT (Compustat)
Dividend Payout	DIVC/AT (Compustat)
Share Repurchases	PRSTKC/AT (Compustat)
Equity Issue	SSTK/AT (Compustat)
Debt Issue	DLTIS/AT (Compustat)
Corruption	Annual number of corruption convictions per US Attorney's Office district (US Department of Justice)
Scaled Corruption	Corruption divided by the 2000 Census population (in millions) of the Attorney's Office district (US Department of Justice; 2000 Census)

(Continues)

Variable	Definition (source)
Independent variables	
FMP Post	Dummy equal to 1 if the observation is headquartered in a state that has adopted fiscal monitoring policies and otherwise 0 (Nakhmurina, 2020; Urahn et al., 2016; public resources of the state auditor or state comptroller)
Tobin's Q	(AT – CEQ + CSHO * PRCC_F)/AT (Compustat)
Cash Flow	(IB + DP)/AT (Compustat)
Firm Size	The logarithm of AT (Compustat)
Population	The logarithm of the annual state population (US Census Bureau, retrieved from Correlates of State Policy)
Working Age Population	The percentage of the annual state population between the ages of 25 and 64 (Morgan & Morgan (2016), retrieved from Correlates of State Policy Project)
Educational Attainment	The percentage of the annual state population that has a high school diploma or higher (Stateminder, retrieved from Correlates of State Policy Project)
Personal Income	The annual personal income per state (Bureau of Economic Analysis)
House Price Index	The annual housing price index (purchase-only index) per state (Federal Housing Finance Agency)
Unemployment Rate	The annual unemployment rate per state (Bureau of Labor Statistics)
Leverage	(DLTT + DLC)/AT (Compustat)
Cash Holdings	CHE / AT (Compustat)
Sales Growth	(SALE – lagged SALE)/lagged SALE (Compustat)
Tangibility	PPENT/AT (Compustat)
Not Dispersed	Dummy equal to 1 if the observation's geographic dispersion is below the median geographic dispersion of all observations in the same state and same year and otherwise 0. Geographic dispersion equals the number of (distinct) states mentioned in the annual report (EDGAR)
Close	Dummy equal to 1 if the observation's distance to the state capital is below the median distance to the state capital of all observations in the same state and year (OpenDataSoft; John Burkardt's website)
Fin. Constr. 1	Dummy equal to 1 if the observation's Kaplan and Zingales (1997) index is above the median Kaplan and Zingales index for all observations in the same state and same year and otherwise 0
Kaplan and Zingales (1997) index	−1.001909 * ((IB + DP)/lagged PPENT) + 0.2826389 * ((AT + PRCC_F * CSHO−CEQ−TXDB)/AT) + 3.139193 * ((DLTT + DLC)/(DLTT + DLC + SEQ)) −39.3678 * ((DVC + DVP)/lagged PPENT) −1.314759 * (CHE/lagged PPENT) (Compustat)
Fin. Constr. 2	Dummy equal to 1 if the observation's Whited and Wu (2006) index is above the median Whited and Wu (2006) index for all observations in the same state and same year and otherwise 0
Whited and Wu (2006) index	−0.091 * ((IB + DP) / AT) – 0.062 * (Div. indicator) + 0.021 * (DLTT / AT) −0.044 * Firm Size + 0.102 * Ind. Sales Growth – 0.035* Sales Growth; where Div. indicator is a dummy equal to 1 if DVC + DVP > 0 (and otherwise 0), and Ind. Sales Growth is the mean of the Sales Growth of the observation's two-digit SIC code and year (Compustat)

(Continues)

Variable	Definition (source)
Fin. Constr. 3	Dummy equal to 1 if the observation's Hadlock and Pierce (2010) index is above the median Hadlock and Pierce index for all observations in the same state and same year and otherwise 0
Hadlock and Pierce (2010) index	−0.737 * Size + 0.043 * Size ^ 2 – 0.040 * Firm Age; where Firm Age is the age of the firm in years (capped at 37 years), and Size is the logarithm of assets (capped at 4.5 billion USD) (Compustat)