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ORIGINAL RESEARCH

The impact of shareholder litigation risk on income smoothing

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Abstract
This paper investigates whether and how shareholder litigation influences income smoothing. Using the ruling of the Ninth Circuit Court of Appeals in 1999 as an exogenous shock to the threat of litigation, we find that the increasing difficulty of class action lawsuits decreases income smoothing. This finding is robust to different model specifications. We also show that such an effect is stronger for firms that are more likely to face greater pressure from the threat of shareholder litigation risk. Overall, our findings extend the literature on investigating how class action lawsuits can affect the motivation of income smoothing.

Keywords Shareholder litigation · Income smoothing · Earnings volatility · Class action lawsuits

JEL Classification K22 · M41

1 Introduction

The benefits and costs of shareholder litigation have attracted greater interest among scholars. Some studies show that shareholder litigation is an external corporate governance mechanism in which the interests between corporate managers and shareholders are better aligned (Bhagat and Romano 2002; Appel 2019). However, a growing body of research

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argues that many shareholder lawsuits are frivolous because attorneys may bring shareholders to file lawsuits to maximize their own interests rather than to plaintiff shareholders (Romano 1991; Bhagat et al. 1998; Graham et al. 2008; Gande and Lewis 2009; Badawi and Chen 2017). Such lawsuit files, with only limited evidence of fiduciary duty breaches, may put great pressures on companies as well as incur instability in the manager's career and result in possible suboptimal business decisions (Romano 1991; Aharony et al. 2015; Chu and Zhao 2021; Hassan et al. 2021; Lin et al. 2021; Obaydin et al. 2021). In this paper, we attempt to extend this line of research by investigating the association between shareholder litigation risk and income smoothing.

We focus on income smoothing for two main reasons. First, income smoothing is at the forefront of executives' minds (Gao and Zhang 2015). As noted in Loomis (1999), "The No. 1 job of management is to smooth out earnings". A survey on financial executives by Graham et al. (2005) indicates that an overwhelming 97% of interviewed financial executives show a preference for income smoothing. Second, from the shareholder's point of view, prior studies find that income smoothing can have significant drawbacks as it increases firm opacity and perceived riskiness (Bhattacharya et al. 2003; Lang et al. 2012; Chen et al. 2017; Yu et al. 2018). In this regard, exploring the variation in income smoothing following the change in shareholder litigation risk is of importance to enhance our knowledge of income smoothing motivation and of the role of shareholder litigation in influencing a common practice in financial reporting.

We notice that the impact of shareholder litigation risk on income smoothing is an empirical issue. On the one hand, shareholder litigation can be used to discipline the manipulation of financial information. Previous studies find that opportunistic disclosures and earnings manipulations are more likely to trigger shareholder litigation (DuCharme et al. 2004; Field et al. 2005; Rogers et al. 2011). Likewise, when income smoothing is used for fraudulent purposes, firms are more vulnerable to shareholder litigation, which in turn suggests a negative relationship between the threat of shareholder litigation risk and income smoothing. On the other hand, the threat of shareholder litigation can impose excessive pressure on managers. Investors usually attribute volatile earnings and failure to meet earnings expectations to poor management (Bushee 2001; Agarwal et al. 2018; Ghaly et al. 2020; Hassan et al. 2021). Shareholder litigation can incur not only direct legal costs to firms but also indirect reputational, job security, and opportunity costs to managers (Karpoff and Lott 1993; Strahan 1998; Brown et al. 2005). Consequently, high ex ante shareholder litigation risk may pressure management into engaging in income smoothing through which reported earnings become less fluctuated and legal exposure can be reduced (Fudenberg and Tirole 1995; Graham et al. 2005; Shaner 2014; Lin et al. 2021). This suggests a positive relationship between the threat of shareholder litigation risk and income smoothing.

It is empirically challenging to test the relationship between the threat of shareholder litigation and income smoothing since they are often endogenously determined. To circumvent the endogeneity problem, we exploit a plausibly exogenous variation of the threat of class action lawsuits created by the ruling of the Ninth Circuit Court of Appeals in 1999.[1] Following the adoption of the 1999 ruling, shareholders have encountered greater difficulty in filing class action lawsuits and it disproportionately impacts firms headquartered in the Ninth Circuit (Chu 2017). Pritchard and Sale (2005) observe a higher rate of case

[1] The Ninth Circuit includes these states: Alaska, Arizona, California, Hawaii, Idaho, Montana, Nevada, Oregon, and Washington.

dismissals due to the particularly strict pleading standards in the Ninth Circuit. Since the shock created by the 1999 ruling influences firms located in states belonging to the Ninth Circuit only (i.e., the treatment group), we estimate the effect of the ruling using the difference-in-differences method and compare the changes in income smoothing of the treatment group to those of the control group consists of firms located in states belonging to other circuits.

Similar to Huang et al. (2020), we use a sample of firm-years over the eight-year window (i.e., spanning four years before and four years after) around the ruling of the Ninth Circuit Court of Appeals in 1999. We find that the decline in the threat of class action lawsuits following the 1999 ruling significantly reduces income smoothing. In terms of economic magnitude, we find that firms headquartered in the Ninth Circuit experienced an average reduction in income smoothing of about 11.1% (as measured by the standard deviation of operating earnings divided by the standard deviation of cash flows from operations) and about 6.8% (as measured by the Spearman correlation between the change in cash flow from operations scaled by lagged total assets and the change in total accruals scaled by lagged assets), relative to the sample mean.

The key identification assumption of our difference-in-differences setting is that the treated and the control firms should be on parallel trends before the adoption of the 1999 ruling (Roberts and Whited 2012). We thus conduct the dynamic treatment analysis to ensure that the pre-treatment differences between the treatment and control groups are indistinguishable. We show that the ruling effects up to three years prior to the treatment are statistically insignificant, while the decrease in income smoothing occurs after the adoption of the ruling. These results also suggest that our main findings are unlikely to be driven by the reverse causality.

To ensure that our results on the association between the 1999 ruling and income smoothing are not driven by chance, we follow Arena et al. (2021) and conduct a placebo test by replacing the actual event year (i.e., 1999) with a pseudo-event year (i.e., 1996). The results show that the fictional 1996 ruling does not have any significant effect on income smoothing and, hence, our baseline findings are not affected by unobserved trend differences between the treated and control firms.

We next conduct the cross-sectional variation in firm characteristics to explore possible channels through which the reduced litigation threat due to the adoption of the ruling may decrease the propensity to smooth income. We find that the ruling effect is stronger for firms where shareholders are more short-term focused, for firms with higher idiosyncratic risk, for firms where managers have limited outside options, for firms in more competitive industries, and for firms that are more high-tech intensive. All these findings are in line with the view that firms that face greater pressure from the threat of shareholder litigation risk are associated with a greater decrease in income smoothing after the 1999 ruling.

Finally, we perform several additional robustness tests. We examine whether our baseline findings are driven by other confounding legal changes. Following Karpoff and Wittry (2018), Appel (2019), and Flammer and Kacperczyk (2019), we control for three state-level antitakeover laws, the Universal Demand laws, and laws related to trade secrets. We find the negative ruling effect on income smoothing to be robust. We next examine whether our main results remain consistent under different model specifications, such as alternative dependent variables, different standard errors clustering, technology bubble, firms incorporated in Nevada, and local economic conditions. All these robustness checks support the notion that the adoption of 1999 ruling decreases income smoothing.

Our study provides two main contributions to the extant literature. Our paper is related to a growing body of research that explores the association between shareholder litigation and corporate behaviour (Lowry and Shu 2002; Cao and Narayanamoorthy 2011; Gormley and Matsa 2011; Arena and Julio 2015, 2023; Abbott et al. 2017; Chu 2017; Arena 2018; Ni and Yin 2018; Houston et al. 2018; Appel 2019; Lin et al. 2021). More specifically, using the 1999 ruling of the Ninth Circuit Court of Appeals, previous studies show that, following the adoption of the ruling, firms have become more likely to experience decreased loan spreads (Chu 2017), increased financial restatements (Hopkins 2018), decreased voluntary disclosure (Houston et al. 2019), and increased real earnings management (Huang et al. 2020). Chung et al. (2020) find that firms in the Ninth Circuit states acquire larger targets. Arena et al. (2021) report that the adoption of the ruling significantly increases corporate tax avoidance. Hassan et al. (2021) find a significant increase in innovation output by firms headquartered in states that have adopted the 1999 ruling relative to firms elsewhere. Our paper contributes to this stream of literature by showing that the reduced threat of shareholder litigation risk after the 1999 ruling significantly decreases income smoothing.

Our paper also adds to the studies on the determinants of income smoothing. Previous studies suggest that income smoothing is positively related to managerial risk-taking incentives (Grant et al. 2009), managerial optimism (Bouwman 2014) and managerial ability (Baik et al. 2020). Other studies also examine the role of stakeholders in influencing income smoothing. For instance, Dou et al. (2013) find that firms operated in high relationship-specific environments smooth income more. Hamm et al. (2018) find that strong labor unions have better abilities to negotiate risk compensation for their employees when firm earnings are volatile, and hence, the strength of labor unions has a positive impact on income smoothing practices. Consistent with the findings of Hamm et al. (2018), Ng et al. (2019) find that a decrease in unemployment risk significantly moderates the firm's incentives of income smoothing. Chen et al. (2019) show that more socially responsible firms who also have a greater dependence on the supplier–buyer relationship are less likely to engage in income smoothing. Our study extends this line of research by showing whether an exogenous change in shareholder litigation risk can affect income smoothing activities.

The remainder of this paper is organised as follows. Section 2 discusses background and related literature. Section 3 describes our sample and empirical design. Section 4 presents empirical findings, and Sect. 5 concludes.

2 Background and related literature

2.1 Institutional background

According to US law, corporate managers/officers and directors have fiduciary duties to make business decisions that serve the best interests of shareholders, while failing to do so could eventually lead shareholders to file lawsuits against them for breaching such duties. Typically, shareholders can sue corporate insiders by initiating derivative lawsuits or by filing securities class action lawsuits. Derivative lawsuits allow shareholders to sue on behalf of the corporation, from which any financial reimbursement is distributed to the corporation. Shareholders who filed a derivative lawsuit are also required to first demand the corporate board to address their allegations for which the board may either accept or reject (Chung et al. 2020). Consequently, prior studies such as Romano (1991), Ferris et al. (2007), Erickson (2010), and Chung et al. (2020) indicate that derivative lawsuits are less likely to close with financial settlements, and shareholders often benefit from improved corporate governance mechanisms and enhanced managerial action.

In contrast, class action lawsuits are generally different with derivative lawsuits in terms of their motivations and objectives (Nguyen et al. 2018, 2020; Manchiraju et al. 2021). Specifically, unlike derivate lawsuits that are indirect in nature, class action lawsuits directly address harm to shareholders (Chung et al. 2020; Manchiraju et al. 2021). A class of allegedly harmed shareholders who files the lawsuit against firms and their management team members is the plaintiff. The primary reason for a class action lawsuit is that shareholders who traded shares at a price influenced by managerial misconduct or information manipulation are entitled to sue for compensation of resulting economic losses, and the financial recovery is paid directly to the plaintiff class of shareholders (Chung et al. 2020). Larcker and Tayan (2011) and Shi et al. (2016) suggest that class action lawsuits are directly against top managers as who are responsible to disclose information to shareholders.

The Securities Act of 1933 and the Security Exchange Act of 1934, passed by the US Congress, were designed to ensure broad and equal access to reliable information from securities issuers (Gibney 2001; Yang et al. 2021). In December 1995, Congress also passed the Private Securities Litigation Reform Act (PSLRA), through which the initiation of lawsuits became more difficult and, hence, corporations are protected from abusive, frivolous securities litigation (Chu 2017). However, although PSLRA requires plaintiffs in securities class action lawsuits to offer proof of scienter, the exact interpretation of the pleading standard is provided by various US circuit courts (Chu 2017; Huang et al. 2020). On July 2, 1999, the Ninth Circuit Court of Appeals issued a ruling (Re: Silicon Graphics Inc.), which resulted in a considerably stricter interpretation of pleading standards than other circuit courts (Johnson et al. 1999; Grundfest and Pritchard 2002). Compared with the mere "acting with recklessness" as required in other circuits, the Ninth Circuit requires plaintiffs to provide evidence that the defendants "acted with deliberate recklessness". Hence, the Ninth Circuit ruling adopted a high burden of proof since the evidence of intent is often obtained after a class action has been established (Huang et al. 2020). Crane and Koch (2018) document that the introduction of the Ninth Circuit ruling has led to a 43 percent reduction in the number of class action lawsuit filings when compared to an increase of 14 percent in other circuits.[2]

Prior studies on the Ninth Circuit ruling indicate that its enactment could not be anticipated and is unlikely to be related to firm characteristics, and thus the ruling appears to be an exogenous shock to the threat of shareholder litigation (Chu 2017; Huang et al. 2020; Yang et al. 2021). Given that the ruling was introduced to a subset of firms headquartered in the Ninth Circuit, we are able to allocate them into treated and control groups based on their locations. In particular, we employ a difference-in-differences approach to precisely compare post-ruling changes in income smoothing for firms located in the Ninth Circuit to similar changes for firms located in the other circuits.[3]

[2] Houston et al. (2019) similarly report that the number of lawsuit files initiated decreased significantly in the Ninth Circuit relative to other jurisdictions following the adoption of the ruling.

[3] Since most class action lawsuit filings are ultimately litigated in the state where a firm is headquartered, we use the firm's headquarter state as the determinant of the most likely location of litigation (Huang et al. 2020).

2.2 Prior studies on income smoothing

Beidleman (1973) describes income smoothing as the management's intentional dampening of fluctuations in reported earnings over time. As noted in Fudenberg and Tirole (1995), managers, who have concerns about their job securities, are likely to smooth income in consideration of both current and future relative performance. Specifically, when current income is low and future income is expected to be high, managers can take actions that shift future income into the current period, and when current income is high and future income is expected to be low, managers can take actions that shift current income into the future period (DeFond and Park 1997).

Previous studies point out that income smoothing and earnings management can be quite different (Khurana et al. 2018). First, the process of shifting income from the present to the future distinguishes income smoothing from earnings management that typically exaggerates current earnings to meet earnings benchmarks under all circumstances (Fudenberg and Tirole 1995). Second, unlike earnings management that aims to achieve a certain level of earnings (e.g., to avoid reporting a loss), the purpose of income smoothing is to achieve a less volatile earnings stream. Thus, although both earnings management and income smoothing affect investors' perceptions of firm earnings, the latter can also influence investors' perceptions of the riskiness of earnings (Cao et al. 2023). Third, according to Jung et al. (2013), Chen et al. (2017), and Hamm et al. (2018), whilst earnings management is often associated with activities such as boosting reported earnings to meet a short-term earnings target or to time it just before a specific event, income smoothing is usually to maintain stable earnings over multiple years. Hence, managers adopt income smoothing as an accounting strategy that sustains over the longer term and is not event driven, compared to earnings management. Finally, managers view income smoothing as more prevalent in practice than earnings management, as accounting policy is likely to constrain their ability to manage earnings upward for extended periods through earnings management (Khurana et al. 2018; Cao et al. 2023). Indeed, a survey by Graham et al. (2005) report an overwhelming 97% of around 400 financial executives to have a preference for income smoothing.

The extant literature offers mixed findings regarding the role of income smoothing. Earlier studies suggest that income smoothing can provide private information on future firm earnings and performance to uninformed outside investors and non-shareholding stakeholders (Beidleman 1973; Barnea et al. 1975; Ronen and Sadan 1981; Demski 1998; Sankar and Subramanyam 2001; Kirschenheiter and Melumad 2002; Tucker and Zarowin 2006). For instance, income smoothing can decrease the cost of debt (Trueman and Titman 1988) and increase the analyst following (Schipper 1991). Moreover, Bartov et al. (2002) and Myers et al. (2007) indicate that income smoothing can lead firms to meet analyst forecasts more frequently and enhance the firm value. However, there is a growing body of research that raises the concern of income smoothing. Studies such as Bhattacharya et al. (2003) and Leuz et al., (2003) argue that smoothing income artificially can hinder detection of managerial diversion of firm resources and undermine the information transparency of the firm. Jayaraman (2008) finds that income smoothing is linked to higher bid-ask spreads as well as the likelihood of informed trading. This result implies that income smoothing can be used to garble information about the firm's underlying true performance and increases information asymmetry between insiders and outsiders. In more recent studies, Chen et al. (2017) and Khurana et al. (2018) highlight the negative impact of income smoothing on shareholder wealth by documenting a positive relationship between income smoothing and stock price crash risk. Yu et al. (2018) find that income smoothing can result in higher information risk as it increases bid-ask spreads around unexpected loss announcement.

2.3 Hypothesis development

Following prior studies, there are two competing hypotheses related to the threat of shareholder litigation risk and income smoothing (Lin et al. 2021). First, the "disciplining hypothesis" indicates that shareholder litigation can deter income smoothing by discipling information manipulation in financial reporting and corporate misconduct. Theories and empirical evidence highlight the significant role that shareholder litigation plays in influencing accounting practices. For example, DuCharme et al. (2004) find that firms that manipulate earnings upward before stock issues are more vulnerable to litigation. Field et al. (2005) document a positive association between litigation risk and the likelihood of issuing earnings warnings, while the early disclosure can decrease the expected litigation risk. Peng and Röell (2008) show that a higher sensitivity of executive compensation to short-term stock price can lead to price manipulation and thus increases the probability of securities class action litigation. Using textual analysis to measure optimism, Rogers et al. (2011) show that the usage of more aggressive and optimistic language in earnings announcements is likely to be associated with a higher probability of shareholder litigation. Similar to Field et al. (2005) and Rogers et al. (2011), Billings and Cedergren (2015) report that firms are less likely to involve in strategic silence and are more likely to warn of the impending negative news when they face higher litigation risk. Likewise, as discussed in Sect. 2.2, income smoothing can be detrimental to shareholders and other stakeholders since it manipulates information and leads to information asymmetry between insiders and outsiders. In line with these arguments, illegal or aggressive forms of income smoothing can expose firms to litigation risk, and hence, firms that face a higher threat of shareholder litigation risk may not engage in income smoothing. Accordingly, when the threat of shareholder litigation risk declines, firms might perceive that income smoothing activities are less likely to trigger shareholder litigation. This leads to the following hypothesis:

Hypothesis 1a Following the adoption of the 1999 ruling, income smoothing activities may increase for firms headquartered in the Ninth Circuit states relative to other firms.

Second, the "pressure hypothesis" suggests that shareholder litigation can impose excessive pressures on management. Specifically, class action lawsuits have a direct cost on firms, as the total settlement costs for security class action lawsuits are about $107.30 billion over the period 1996–2019, with an average cost at $58.1million (Cornerstone Research 2020). Shareholder lawsuits also have an indirect cost to a manager's career (Karpoff and Lott 1993; Brown et al. 2005). Strahan (1998) shows that the likelihood of CEO turnover increases following class action lawsuits. In a similar vein, some studies demonstrate that shareholder litigation distracts managers' attention, undermines managers' reputation, and incurs instability of job tenure (Fich and Shivdasani 2007; Aharony et al. 2015). Further, Lin et al. (2021) document that although it is uncommon for every firm to experience shareholder lawsuits, shareholders do have the right to file a lawsuit whenever necessary and it does occur. Consequently, a higher threat of shareholder litigation can pressure managers into engaging in corporate activities that could enhance tenure stability by reducing their legal exposure (Shaner 2014; Lin et al. 2021).[4]

Indeed, investors usually associate volatile earnings or failure to meet earnings expectations with poor management (Bushee 2001; Agarwal et al. 2018; Ghaly et al. 2020; Hassan et al. 2021). As stated by US Congress senators, "Companies, particularly growth firms, say they are sued whenever their stock drops" (Seligman 1994, p.442). Accordingly, the prior literature suggests that managers may rationally reduce the investor's estimates of the earnings volatility and meet earnings expectations by income smoothing (Lambert 1984; Dye 1988; Trueman and Titman 1988; Michelson et al. 1995; Acharya and Lambrecht 2015). Lev and Kunitzky (1974) and Michelson et al. (1995) show that income smoothing lowers short-term price risk as it reduces earnings fluctuations. Grant et al. (2009) suggest that because earnings volatility can undermine a manger's tenure, income smoothing could potentially be a less costly method to mitigate such undesirable risk and boost share price. Similarly, Jung et al. (2013) document that since earnings volatility is an important factor in credit ratings, managers can use income smoothing to impact credit risk as perceived by both investors and rating agencies. Ng et al. (2019) find that firms have incentives to smooth income to diminish employees' concerns of unemployment risk due to volatile earnings. Collectively, these findings above are in line with the argument that managers are likely to please shareholders by reducing stock price volatility through income smoothing, as large fluctuations in firm performance are disfavored by institutional investors and can affect a manager's tenure (Badrinath et al. 1989; Carlson and Bathala 1997). This leads to the following hypothesis:

Hypothesis 1b Following the adoption of the 1999 ruling, income smoothing activities may decrease for firms headquartered in the Ninth Circuit states relative to other firms.

3 Data and methodology

3.1 Sample

Our sample consists of observations for all publicly listed firms from the Compustat/CRSP merged database with non-missing information on historical headquarters between 1995 and 2003.[5] To mitigate the potential concern that longer periods may contain effects from other confounding events, we compare the post-ruling period (i.e., 2000–2003) to the pre-ruling period (i.e., 1995–1998) (Huang et al. 2020). We also exclude the year of the ruling, 1999, from our analyses. Only firms with non-missing accounting data at least one year before and one year after the ruling year are included to the sample. The final sample comprises 15,953 firm-year observations. To reduce the potential impact of outliers, all accounting variables are winsorized at the 1st and 99th percentiles.

[4] For example, Cao and Narayanamoorthy (2011) find that litigation risk faced by managers is an important determinant in of management earnings forecast. Bourveau et al. (2018) indicate that higher litigation risk may decrease corporate disclosure since managers' private costs of disclosure increase with the higher risk of being involved in shareholder lawsuits. Chu and Zhao (2021) find that managers of acquiring firms may make suboptimal merger decisions to mitigate the pressure of being sued.

[5] Because Compustat only reports the most recent addresses of firms, we use the source of firms' headquarters location data from the yearly 10-K report by means of Jennings et al. (2017).

3.2 Empirical specification

We classify firms as treated firms if their headquarters are located in one of the Ninth Circuit states (i.e., treatment group) and firms as control firms if their headquarters are located in non-Ninth Circuit states (i.e., control group).[6] To test whether litigation risk affects income smoothing, we follow Bertrand and Mullainathan (2003), Chu (2017), Huang et al. (2020), and Yang et al. (2021) and employ a difference-in-differences design, through which we compare changes in income smoothing following the 1999 Ninth Circuit ruling for the treatment group to the corresponding changes for the control group. Specifically, we estimate the following regression specification:

$$y_{it} = \beta_0 + \beta_1 Ninth\,Circuit_i \times Post_t + \gamma X_{it} + D_i + Industry \cdot Year + \varepsilon_{i,t} \quad (1)$$

The dependent variable y_{it} is the measure of income smoothing, where i indexes firms and t indexes years. Following prior studies such as Leuz et al. (2003) and Dou et al. (2013), our first measure of income smoothing (*Smoothing1*) is the standard deviation of operating earnings divided by the standard deviation of cash flows from operations, where both the earnings and cash flows are scaled by lagged total assets. Standard deviations are calculated at the annual level, over rolling five-year windows ending in the current fiscal year. The rationale behind this measure is that earnings will be smoother than cash flows from operations if managers smooth reported earnings.

Our second measure of income smoothing (*Smoothing2*) is the Spearman correlation between the change in cash flow from operations scaled by lagged total assets and the change in total accruals scaled by lagged assets (Bhattacharya et al. 2003; Dechow et al. 2010). Similar to Jones (1991) and Kothari et al. (2005), we define total accruals as the change in non-cash current assets minus the sum value of the change in current liabilities excluding the current portion of long-term debt and the depreciation and amortization, scaled by lagged total assets. The intuition for *Smoothing2* is that managers are assumed to create accrual reserves in good times and use them to compensate for poor cash flows in bad times, leading to a negative correlation between changes in accruals and shocks to reported cash flows results (Burgstahler et al. 2006; Barth et al. 2008). To ensure larger values represent more income smoothing, both our income smoothing measures are multiplied by negative one.

Our main variable of interest is the interaction term $NinthCircuit \times Post$, in which *NinthCircuit* equals one if a firm's headquarter is located in one of the Ninth Circuit states, and zero otherwise, while *Post* equals one if in the 2000–2003 period, and zero in the 1995–1998 period. We expect the coefficient estimate of the interaction term $NinthCircuit \times Post$, β_1, to be negative and statistically significant. To further mitigate unobserved heterogeneity in our estimates of the litigation effect on income smoothing, we use two fixed effects. First, we control for firm fixed effects, denoted D_i, to remove unobserved time-invariant differences between Ninth Circuit firms and non-Ninth Circuit firms. In addition, we include industry-year fixed effects, denoted $Industry \cdot Year$, to ensure that we compare Ninth Circuit firms and non-Ninth Circuit firms within the same industry at the same period of time, removing unobserved changes in industry

[6] Firms are unlikely to change their headquarters location frequently. Moreover, Chu (2017) indicates that since the Ninth Circuit ruling could not be anticipated, firms are unlikely to change their headquarters in anticipating of the ruling. In our sample, about 2% firms changed their headquarters location from non-Ninth Circuit states to Ninth Circuit states. Our baseline results remain robust if we exclude these firms.

Table 1 Summary statistics

	N	Mean	Median	Std	P25	P75
Smoothing1	15,953	−0.722	−0.598	0.536	−0.967	−0.329
Smoothing2	15,953	0.755	0.915	0.368	0.705	0.980
Ninth Circuit	15,953	0.201	0.000	0.401	0.000	0.000
Size	15,953	5.367	5.331	0.916	4.662	6.004
ROA	15,953	0.084	0.120	0.238	0.060	0.178
Leverage	15,953	0.622	0.191	1.422	0.025	0.582
Market-to-book Ratio	15,953	1.819	1.327	1.570	0.992	1.987
Asset Tangibility	15,953	0.305	0.243	0.230	0.121	0.439
Cash Flow	15,953	0.149	0.066	0.190	0.017	0.208
Stock Return	15,953	−0.026	0.016	0.583	−0.318	0.295
Sales Growth	15,953	0.128	0.069	0.405	−0.034	0.199
R&D	15,953	0.044	0.000	0.091	0.000	0.046
CAPEX	15,953	0.070	0.046	0.076	0.024	0.087
Dividends	15,953	0.345	0.000	0.476	0.000	1.000
Institutional Ownership	15,953	0.293	0.206	0.296	0.000	0.552
Analysts Following	15,953	0.880	0.000	1.071	0.000	1.792
Big N Auditor	15,953	0.864	1.000	0.343	1.000	1.000
Debt Issue	15,953	0.010	0.000	0.091	−0.016	0.018
Equity Issue	15,953	0.067	0.008	0.285	0.000	0.031
Acquisition	15,953	0.343	0.000	0.475	0.000	1.000

This table presents descriptive statistics of the main variables used in this study. The sample period is from 1995 to 2003, while the year of the Ninth Circuit ruling, 1999, is excluded. Only firms with at least one year before and one year after the ruling year are included in the sample. The detailed definitions of these variables are provided in Appendix 1. All accounting variables are winsorized at the 1st and 99th percentiles

conditions. We do not include $NinthCircuit_i$ and $Post_t$ separately as these indicators are absorbed in the firm fixed effects and industry-year fixed effects. Standard errors are clustered by firm.

X_{it} in Eq. (1) refers to a vector of control variables. Following previous studies such as Pontiff and Schall (1998), Chen et al. (2002), Caprio et al. (2011), Custódio et al. (2013), Dou et al. (2013), Gao and Zhang (2015), Hovakimian and Hu (2016), Chen et al. (2017), Ham et al. (2017), Khurana et al. (2018), Hamm et al. (2018), Atanassov et al. (2020), and Huang et al. (2020), we control for firm characteristics, including the natural logarithm value of market capitalization (*Size*), firm performance (*ROA*), firm leverage (*Leverage*), firm growth opportunity (*Market-to-book Ratio*), firm tangibility (*Asset Tangibility*), firm cash (*Cash Flow*), firm stock return (*Stock Return*), firm sales growth (*Sales Growth*), firm research and development expenditures (*R&D*), firm capital expenditures (*CAPEX*), firm dividend payout (*Dividends*), firm institutional ownership (*Institutional Ownership*), the natural logarithm value of one plus the number of analysts following a firm (*Analysts Following*), the largest auditors (*Big N Auditor*), corporate debt issuance (*Debt Issue*), corporate equity issuance (*Equity Issue*), and corporate acquisitions (*Acquisitions*). All variables are defined in Appendix 1.

3.3 Summary statistics

Table 1 presents the descriptive statistics for the variables used in our baseline regression model. Mean (median) *Smoothing1* is −0.722 (−0.598) and mean (median) *Smoothing2* is 0.755 (0.915). About 20.1% of firms in our sample can be identified as Ninth Circuit firms. For firm-level characteristics, mean (median) *Size* is 5.367 (5.331), mean (median) *ROA* is 0.084 (0.120), mean (median) *Leverage* is 0.622 (0.191), mean (median) *Market-to-book Ratio* is 1.819 (1.327), mean (median) *Asset Tangibility* is 0.305 (0.243), mean (median) *Cash Flow* is 0.149 (0.066), mean (median) *Stock Return* is −0.026 (0.016), mean (median) *Sales Growth* is 0.128 (0.069), mean (median) *R&D* is 0.044 (0.000), mean (median) *CAPEX* is 0.070 (0.046), mean (median) *Dividends* is 0.345 (0.000), mean (median) *Debt Issue* is 0.010 (0.000), and mean (median) *Equity Issue* is 0.067 (0.008). In addition, the average percentage of institutional ownership (*Institutional Ownership*) is 29.3%, the average percentage of financial analyst coverage (*Analysts Following*) is 88%, the average percentage of Big N auditors (*Big N Auditor*) is 86.4%, and approximately 34.3% firms in our sample engage in acquisitions (*Acquisition*).

Panel A of Table 2 compares the characteristics of Ninth Circuit firms and non-Ninth Circuit firms at the firm-year level. On average, firms located in Ninth Circuit states have a lower income smoothing than those located in non-Ninth Circuit states. Also, Ninth Circuit firms are smaller, are less profitable, have lower leverage, have more growth opportunities, hold fewer tangible assets, have higher cash flow, have lower stock return, have higher sales growth, have higher R&D expenditure, pay lower dividends, have higher percentages of institutional ownership, have more analysts following them, are more likely to use Big N auditors, and have more equity issuance. Panel B compares the change in the mean value of income smoothing before and after the 1999 ruling, separately for firms located in Ninth Circuit and non-Ninth Circuit states. We find that the difference in the mean value of *Smoothing1* and *Smoothing2* before and after the adoption of ruling is 0.033 and 0.033, respectively, for non-Ninth Circuit firms, while such difference is 0.087 and 0.071, respectively, for Ninth Circuit firms. These differences are statistically significant at the 1% level. We find similar results for the difference in the median values of the two measures of income smoothing in Panel C. In sum, the results in panels B and C provide some preliminary evidence that a decrease in litigation risk may lead to a significant decrease in income smoothing.

4 Main results

4.1 Litigation risk and income smoothing

Table 3 reports the results of our main analysis. In columns (1)–(2), we present the estimates by including just the interaction term *NinthCircuit* × *Post*, firm and industry-year fixed effects, and no control variables. The coefficients on the interaction term, the main variable of interest, are negative (coefficient = − 0.072 for *Smoothing1*, and coefficient = − 0.048 for *Smoothing2*) and statistically significant at the 5% level. These results suggest that firms located in Ninth Circuit states experienced a decline in income smoothing following the ruling of the Ninth Circuit Court of Appeals. We add time-varying

Table 2 Univariate analysis

	Non-Ninth Circuit States (N = 12,740)		Ninth Circuit States (N = 3,213)		Differences	
	Mean	Median	Mean	Median	Mean	Median
Panel A. Summary Statistics						
Smoothing1	−0.702	−0.582	−0.803	−0.672	0.100***	0.089***
Smoothing2	0.768	0.921	0.702	0.889	0.065***	0.032***
Size	5.387	5.360	5.285	5.206	0.102***	0.154***
ROA	0.096	0.124	0.035	0.104	0.061***	0.020***
Leverage	0.681	0.219	0.389	0.093	0.292***	0.126***
Market-to-book Ratio	1.744	1.303	2.116	1.448	−0.372***	−0.145***
Asset Tangibility	0.316	0.259	0.259	0.180	0.057***	0.080***
Cash Flow	0.127	0.053	0.234	0.163	−0.107***	−0.110***
Stock Return	−0.014	0.025	−0.071	−0.014	0.057***	0.038***
Sales Growth	0.125	0.068	0.140	0.076	−0.014*	−0.008
R&D	0.032	0.000	0.089	0.040	−0.056***	−0.040***
CAPEX	0.071	0.046	0.069	0.046	0.002	0.000
Dividends	0.380	0.000	0.207	0.000	0.174***	0.000***
Institutional Ownership	0.290	0.204	0.302	0.211	−0.012**	−0.006
Analysts Following	0.854	0.000	0.983	0.693	−0.129***	−0.693***
Big N Auditor	0.854	1.000	0.903	1.000	−0.049***	0.000***
Debt Issue	0.009	0.000	0.010	0.000	−0.001	0.000
Equity Issue	0.059	0.007	0.098	0.018	−0.039***	−0.011***
Acquisition	0.343	0.000	0.343	0.000	0.001	0.000

	Before	After	Δmean	*p*-value	Before	After	Δmean	*p*-value
Panel B. Univariate Tests: variable difference before and after 1999 ruling (Mean Value)								
Smoothing1	−0.686	−0.718	0.033***	0.000	−0.757	−0.844	0.087***	0.000
Smoothing2	0.784	0.752	0.033***	0.000	0.739	0.669	0.071***	0.000

	Before	After	Δmedian	*p*-value	Before	After	Δmedian	*p*-value
Panel C. Univariate Tests: variable difference before and after 1999 ruling (Median Value)								
Smoothing1	−0.558	−0.609	0.052***	0.000	−0.617	−0.718	0.100***	0.000
Smoothing2	0.928	0.912	0.016***	0.000	0.907	0.870	0.038***	0.000

This table presents the univariate analysis of firms headquartered in states belonging to Ninth Circuit and firms located in other circuit states. The sample period is from 1995 to 2003, while the year of the Ninth Circuit ruling, 1999, is excluded. Only firms with at least one year before and one year after the ruling year are included to the sample. Panel A compares the characteristics of Ninth Circuit firms and non-Ninth Circuit firms at the firm-year level. Panel B compares the change in the mean value of income smoothing before and after the adoption of the 1999 ruling separately for firms located in Ninth Circuit states and those in other circuit states. Panel C compares the difference in the median value of income smoothing before and after the adoption of the 1999 ruling separately for firms located in Ninth Circuit states and those in other circuit states

***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. All accounting variables are winsorized at the 1st and 99th percentiles

control variables in columns (3)–(4) and find that it makes little difference to the significance of the income smoothing reduction, as the coefficients on the interaction term, *NinthCircuit* × *Post*, are −0.080 (*p-value* < 0.01) and −0.051 (*p-value* < 0.05), respectively.

Such findings are also economically meaningful. For example, the coefficient estimates in columns (3) and (4) demonstrate that, relative to the sample mean, the 1999 ruling decreases *Smoothing1* and *Smoothing2* by about 11.1% and 6.8%, respectively.[7]

To mitigate the concern that our baseline results might be driven by reverse causality, we follow Bertrand and Mullainathan (2003) and employ the dynamic treatment model, which tests the timing of income smoothing relating to the timing of the adoption of the ruling of the Ninth Circuit Court of Appeals. We estimate the dynamic treatment model as follows:

$$\begin{aligned} y_{it} = {} & \beta_1\ Year^{-3} \times Ninth\ Circuit_i + \beta_2\ Year^{-2} \times Ninth\ Circuit_i + \beta_3\ Year^{-1} \\ & \times Ninth\ Circuit_i + \beta_4\ Year^{+1} \times Ninth\ Circuit_i + \beta_5\ Year^{+2} \times Ninth\ Circuit_i + \\ & \beta_6\ Year^{+3} \times Ninth\ Circuit_i + \beta_7\ Year^{+4} \times Ninth\ Circuit_i + \gamma X_{it} + D_i + Industry \\ & Year + \varepsilon_{i,t} \end{aligned} \tag{2}$$

where we replace the interaction term $NinthCircuit \times Post$, the main variable of interest in Eq. (1), with a set of seven interaction terms: $Year^{-3} \times NinthCircuit$, $Year^{-2} \times NinthCircuit$, $Year^{-1} \times NinthCircuit$, $Year^{+1} \times NinthCircuit$, $Year^{+2} \times NinthCircuit$, $Year^{+3} \times NinthCircuit$, and $Year^{+4} \times NinthCircuit$, respectively. $Year^{-3}$ is a dummy variable equal to one for the third year prior to the year of the ruling, $Year^{-2}$ is a dummy variable equal to one for the second year prior to the year of the ruling, $Year^{-1}$ is a dummy variable equal to one for the year prior to the year of the ruling, $Year^{+1}$ is a dummy variable equal to one for the year after the year of the ruling, $Year^{+2}$ is a dummy variable equal to one for the second year after the year of the ruling, $Year^{+3}$ is a dummy variable equal to one for the third year after the year of the ruling, and $Year^{+4}$ is a dummy variable equal to one for fourth year after the year of the ruling.[8] $NinthCircuit_i$ is a dummy variable equal to one if a firm's headquarter is located in one of the Ninth Circuit states. The coefficient estimates of interaction terms,$Year^{-3} \times NinthCircuit$,$Year^{-2} \times NinthCircuit$, and $Year^{-1} \times NinthCircuit$, β_1, β_2, and β_3, are of particular interest since their magnitude and statistical significance demonstrate whether reverse causality is the potential issue, or whether the pre-trends in income smoothing are significantly different between the treatment and control groups.

Table 4 presents the estimation results of the dynamic treatment analysis as shown in Eq. (2). In columns (1) and (2), we find that the coefficient estimates of $Year^{-3} \times NinthCircuit$, $Year^{-2} \times NinthCircuit$, and $Year^{-1} \times NinthCircuit$ are relatively small and statistically insignificant for both measures of income smoothing. This result suggests that the parallel trend assumption is likely satisfied since there are no significant systematic differences in pretrends between the treatment and control groups (Roberts and Whited 2012). Moreover, compared to the pre-treatment year periods, we observe a decrease in income smoothing emerging only after the ruling year, as demonstrated by

[7] Jennings et al. (2023) argue that a greater number of dimensions of fixed effects may not ensure the robustness of the regression specification. This is because minimal measurement error can cause large biases and generate false positives when fixed effects absorb more than 90% of the variation in the main variable of interest. They therefore suggest scholars further assess the absorption rate by checking the R-squared from the regression of the main variable of interest on the fixed effect structure and be cautious if the value of R-squared is greater than 90%. In untabulated analysis, we perform the diagnostic test proposed by Jennings et al. (2023) and confirm that the combination of measurement error and high-dimensional fixed effects do not affect our results. We thank the anonymous referee for pointing out this issue.

[8] In Eq. (2), the benchmark year is four years before the year of the ruling, namely $Yeasr^{-4}$.

Table 3 Shareholder litigation and income smoothing

	Smoothing1 (1)	Smoothing2 (2)	Smoothing1 (3)	Smoothing2 (4)
Ninth Circuit × Post	−0.072**	−0.048**	−0.080***	−0.051**
	(0.029)	(0.022)	(0.029)	(0.022)
Size			0.091**	0.052*
			(0.038)	(0.028)
ROA			0.107***	−0.003
			(0.035)	(0.027)
Leverage			−0.005	−0.001
			(0.006)	(0.003)
Market-to-book Ratio			−0.002	0.002
			(0.005)	(0.004)
Asset Tangibility			0.068	0.022
			(0.087)	(0.066)
Cash Flow			0.234***	−0.005
			(0.057)	(0.042)
Stock Return			0.010	−0.015***
			(0.008)	(0.005)
Sales Growth			0.005	0.010
			(0.011)	(0.007)
R&D			−0.158	−0.089
			(0.150)	(0.112)
CAPEX			0.017	−0.023
			(0.089)	(0.061)
Dividends			0.124***	0.011
			(0.019)	(0.013)
Institutional Ownership			0.048	0.011
			(0.051)	(0.036)
Analysts Following			0.005	0.005
			(0.013)	(0.009)
Big N Auditor			−0.054	−0.015
			(0.033)	(0.026)
Debt Issue			0.059	−0.039
			(0.047)	(0.029)
Equity Issue			−0.004	−0.015
			(0.014)	(0.012)
Acquisition			0.026**	0.009
			(0.010)	(0.007)
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.046	0.060	0.061	0.063
Observations	15,953	15,953	15,953	15,953

In this table, we examine the impact of shareholder litigation on income smoothing. The main dependent variables are *Smoothing1* and *Smoothing2*, respectively. *Smoothing1* is the standard deviation of operating earnings divided by the standard deviation of cash flows from operations, where both of the earnings and

Table 3 (continued)

cash flows are scaled by lagged total assets. *Smoothing2* is the Spearman correlation between the change in cash flow from operations scaled by lagged total assets and the change in total accruals scaled by lagged assets. Our main variable of interest is the interaction term *NinthCircuit* × *Post*, in which *NinthCircuit* equals one if a firm's headquarter is located in one of the Ninth Circuit states, and zero otherwise, while *Post* equals one in the 2000–2003 period, and zero in the 1995–1998 period. In columns (1) and (2), we present the estimates by including just the interaction term *NinthCircuit* × *Post*, firm and industry-year fixed effects, and no control variables. We add time-varying control variables in columns (3)–(4). Detailed definitions of all control variables are provided in Appendix 1. Statistical significance is based on the heteroscedasticity-robust firm-clustered standard errors reported in parentheses

***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. All accounting variables are winsorized at the 1st and 99th percentiles

the considerably larger and significant coefficient estimates of $Year^{+2} \times NinthCircuit$, $Year^{+3} \times NinthCircuit$, and $Year^{+4} \times NinthCircuit$ for both *Smoothing1* and *Smoothing 2*. These findings lend further support for our baseline results not being driven by reverse causality.

One potential issue is that our baseline results could be driven by the systematic differences since the choice of headquarters in states that adopted the ruling of the Ninth Circuit Court of Appeals might be non-random and the Ninth Circuit firms might be fundamentally different from the non-Ninth Circuit firms. To mitigate such concern, we repeat the estimation of Eq. (1) using a sample with the treated and the matched control firms. To construct control firms, we first estimate a logit regression of whether a firm is likely to be located in one of the Ninth Circuit states based on the firm characteristics as used in Eq. (1) in year 1998, at least one year before the year of the ruling. The propensity score is then the probability estimated from the logit regression. Next, we use the nearest-neighbour method to ensure the treated firms are sufficiently similar to the matched control firms. In particular, each firm in the treatment group is matched to a firm in the control group that is from the same industry and with the closest propensity score (caliper = 0.005) in 1998. In Appendix 2, we perform a diagnostic test to verify whether the treatment and matched control firms are fundamentally indistinguishable. The results suggest that none of the differences in means for each observed firm-level characteristic between the treatment and matched control groups remains statistically significant. Therefore, it is evident that any difference in income smoothing between the two groups is due to the adoption of the ruling of the Ninth Circuit Court of Appeals.

Table 5 reports the estimation results using the matched sample, consists of 317 pairs of matched firms.[9] In columns (1)–(2), we repeat the regression analysis for income smoothing as shown in Eq. (1). We find that the coefficient estimates of the interaction term, *NinthCircuit* × *Post*, remain negative and statistically significant. Columns (3)–(4) report the estimation results for the dynamic treatment model as shown in Eq. (2). Again, we find that the results remain quantitatively similar.

We also conduct a placebo test to ensure that our main results are not driven by non-parallel trends before the ruling or by unobserved characteristics that affect income smoothing

9 The sample includes firms with at least one year of data in both the pre- and post-1999 periods. Moreover, in line with prior studies (Leuven and Sianesi 2003; Caliendo and Kopeinig 2008; Kubick et al. 2016; Ghaly et al. 2017; Florackis and Sainani 2018; Conyon et al. 2019), we further require that matched pairs should satisfy the common support condition and be appropriately weighted by the propensity score distribution of participants.

Table 4 Dynamic treatment analysis

	Smoothing1	Smoothing2
	(1)	(2)
$Before^{-3} \times$ Ninth Circuit	−0.017	−0.019
	(0.034)	(0.024)
Before − $^{2} \times$ Ninth Circuit	−0.033	−0.019
	(0.039)	(0.027)
Before − $^{1} \times$ Ninth Circuit	−0.052	−0.006
	(0.041)	(0.027)
$After^{+1} \times$ Ninth Circuit	−0.094**	−0.038
	(0.042)	(0.028)
$After^{+2} \times$ Ninth Circuit	−0.098**	−0.071**
	(0.045)	(0.030)
$After^{+3} \times$ Ninth Circuit	−0.121***	−0.067**
	(0.045)	(0.033)
$After^{+4} \times$ Ninth Circuit	−0.120***	−0.076**
	(0.044)	(0.034)
Firm Controls	Yes	Yes
Firm FE	Yes	Yes
Industry − year FE	Yes	Yes
Adjusted R^2	0.062	0.063
Observations	15,953	15,953

This table presents the estimation results of the dynamic treatment analysis. The main dependent variables are *Smoothing1* and *Smoothing2*, respectively. In column (1), *Smoothing1* is the standard deviation of operating earnings divided by the standard deviation of cash flows from operations, where both the earnings and cash flows are scaled by lagged total assets. In column (2), *Smoothing2* is the Spearman correlation between the change in cash flow from operations scaled by lagged total assets and the change in total accruals scaled by lagged assets. We replace the interaction term *NinthCircuit* × *Post*, the main variable of interest in our baseline regression model, with a set of seven interaction terms: $Year^{-3} \times NinthCircuit$, $Year^{-2} \times NinthCircuit$, $Year^{-1} \times NinthCircuit$, $Year^{+1} \times NinthCircuit$, $Year^{+2} \times NinthCircuit$, $Year^{+3} \times NinthCircuit$, and $Year^{+4} \times NinthCircuit$, respectively. $Year^{-3}$ is a dummy variable equal to one for the third year before the year of the ruling, $Year^{-2}$ is a dummy variable equal to one for the second year before the year of the ruling, $Year^{-1}$ is a dummy variable equal to one for the year before the year of the ruling, $Year^{+1}$ is a dummy variable equal to one for the year after the year of the ruling, $Year^{+2}$ is a dummy variable equal to one for the second year after the year of the ruling, $Year^{+3}$ is a dummy variable equal to one for the third year after the year of the ruling, and $Year^{+4}$ is a dummy variable equal to one for the fourth year after the year of the ruling. Detailed definitions of all control variables are provided in Appendix 1. Statistical significance is based on the heteroscedasticity-robust firm-clustered standard errors reported in parentheses

***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. All accounting variables are winsorized at the 1st and 99th percentiles

Table 5 Shareholder litigation and income smoothing: the matched sample

	Smoothing1 (1)	Smoothing2 (2)	Smoothing1 (3)	Smoothing2 (4)
Ninth Circuit × Post	−0.144***	−0.080**		
	(0.045)	(0.034)		
Year $-^{3}$ × Ninth Circuit			−0.036	0.001
			(0.054)	(0.037)
Year $-^{2}$ × Ninth Circuit			−0.027	−0.014
			(0.064)	(0.043)
Year $-^{1}$ × Ninth Circuit			−0.051	0.003
			(0.069)	(0.045)
Year^{+1} × Ninth Circuit			−0.143**	−0.019
			(0.072)	(0.047)
Year^{+2} × Ninth Circuit			−0.152**	−0.094*
			(0.073)	(0.050)
Year^{+3} × Ninth Circuit			−0.216***	−0.110**
			(0.073)	(0.054)
Year^{+4} × Ninth Circuit			−0.201***	−0.136**
			(0.072)	(0.055)
Firm Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.118	0.100	0.119	0.103
Observations	3,889	3,889	3,889	3,889

This table examines the impact of shareholder litigation on income smoothing with a sample consists of 317 pairs of matched firms. The main dependent variables are *Smoothing1* and *Smoothing2*, respectively. *Smoothing1* is the standard deviation of operating earnings divided by the standard deviation of cash flows from operations, where both the earnings and cash flows are scaled by lagged total assets. *Smoothing2* is the Spearman correlation between the change in cash flow from operations scaled by lagged total assets and the change in total accruals scaled by lagged assets. In columns (1) and (2), the main variable of interest is the interaction term *NinthCircuit* × *Post*, in which *NinthCircuit* equals one if a firm's headquarter is located in one of the Ninth Circuit states, and zero otherwise, while *Post* equals one in the 2000–2003 period, and zero in the 1995–1998 period. In columns (3) and (4), we replace the interaction term *NinthCircuit* × *Post* with a set of seven interaction terms: $Year^{-3}$ × *NinthCircuit*, $Year^{-2}$ × *NinthCircuit*,$Year^{-1}$ × *NinthCircuit*, $Year^{+1}$ × *NinthCircuit*, $Year^{+2}$ × *NinthCircuit*, $Year^{+3}$ × *NinthCircuit*, and $Year^{+4}$ × *NinthCircuit*. $Year^{-3}$ is a dummy variable equal to one for the third year before the year of the ruling, $Year^{-2}$ is a dummy variable equal to one for the second year before the year of the ruling, $Year^{-1}$ is a dummy variable equal to one for the year before the year of the ruling, $Year^{+1}$ is a dummy variable equal to one for the year after the year of the ruling, $Year^{+2}$ is a dummy variable equal to one for the second year after the year of the ruling, $Year^{+3}$ is a dummy variable equal to one for the third year after the year of the ruling, and $Year^{+4}$ is a dummy variable equal to one for the fourth year after the year of the ruling. Detailed definitions of all control variables are provided in Appendix 1. Statistical significance is based on the heteroscedasticity-robust firm-clustered standard errors reported in parentheses

***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. All accounting variables are winsorized at the 1st and 99th percentiles

differently for firms located in states belonging to the Ninth Circuit compared to firms in other circuit states. Arena et al. (2021) indicate that the test of the non-parallel trends may not work appropriately if the pseudo-event year is distant from the actual event year, while the sample

Table 6 Shareholder litigation and income smoothing: pseduo-ruling year

	Smoothing1 (1)	Smoothing2 (2)	Smoothing1 (3)	Smoothing2 (4)
Ninth Circuit × Post	−0.010	0.019	−0.008	0.017
	(0.031)	(0.020)	(0.031)	(0.020)
Firm Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.011	0.008	0.027	0.015
Observations	9,279	9,279	9,279	9,279

This table presents the placebo test results using 1996 as the pseudo-ruling year. The sample is between 1994 and 1998 (i.e., two years before and two years after the pseudo-ruling year). The main dependent variables are *Smoothing1* and *Smoothing2*, respectively. *Smoothing1* is the standard deviation of operating earnings divided by the standard deviation of cash flows from operations, where both of the earnings and cash flows are scaled by lagged total assets. *Smoothing2* is the Spearman correlation between the change in cash flow from operations scaled by lagged total assets and the change in total accruals scaled by lagged assets. Our main variable of interest is the interaction term *NinthCircuit* × *Post*, in which *NinthCircuit* equals one if a firm's headquarter is located in one of the Ninth Circuit states, and zero otherwise, while *Post* equals one in the 1997–1998 period, and zero in the 1994–1995 period. In columns (1) and (2), we present the estimates by including just the interaction term *NinthCircuit* × *Post*, firm and industry-year fixed effects, and no control variables. We add time-varying control variables in columns (3)–(4). Detailed definitions of all control variables are provided in Appendix 1. Statistical significance is based on the heteroscedasticity-robust firm-clustered standard errors reported in parentheses

***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. All accounting variables are winsorized at the 1st and 99th percentiles

period should end prior to the actual event year to ensure that there is no confounding effect from the actual event year. Following their study, we replace the actual event year (i.e., 1999) with a pseudo-event year (i.e., 1996) and reestimate the baseline regression using a four-year window (i.e., two years before and two years after the event). We report the placebo test results in Table 6. The results show that the coefficient estimates of *NinthCircuit* × *Post* are not statistically significant for all specifications, suggesting that the fictional 1996 ruling does not have any significant impact on income smoothing. Thus, our main results are unlikely to be driven by unobserved trend differences between the treated and control firms.

4.2 Further analyses and discussion

4.2.1 Cross-sectional variations in the effects of the Ninth Circuit ruling

In this subsection, we examine the cross-sectional variation in firm characteristics to explore possible channels through which the 1999 ruling can decrease income smoothing. Specifically, since the threat of shareholder litigation decreases following the ruling, we expect the association between the 1999 ruling and income smoothing to be more pronounced for firms that are more likely to experience the pressure from shareholder litigation risk.

Investors are not a homogeneous group. Different demographics, liquidity needs, or information sets can lead to different strategies of investment horizons (Hotchkiss and Strickland 2003). Investors that have a long-term orientation are less likely to be myopic as well as to pressure companies into maximizing short-term earnings growth and resell their stock at a profit compared to investors that have a short-term focus (Bushee 2001; Bolton et al. 2006; Gaspar et al. 2013). Hassan et al. (2021) indicate that myopic investors are likely to use shareholder litigation as a tool to pressure management into taking actions that can reduce short-term price risk. According to these arguments, we conjecture that institutions with short-term investment horizons (i.e., transient institutional investors) could be the main force in pressuring firms to reduce earnings volatility through income smoothing. To test this, we calculate the difference between the total amount of shares held by dedicated and quasi-index investors and the number of shares held by transient investors of a firm following Bushee's (2001) classification of institutional investor base, all divided by total shares (An and Zhang 2013; Brochet et al. 2015).[10] A larger (smaller) value of the difference means that firms have more (fewer) long-term institutional investors. We then partition the sample into firms with more and fewer long-term investors (i.e., *Long-term Shareholdings* and *Short-term Shareholdings*) based on the median of the distribution of the calculated differences in shareholdings. We repeat the baseline regression and report the estimated results in Panel A of Table 7. As expected, the results show that the coefficient estimates of *NinthCircuit* × *Post* are negative and significant for *Short-term Shareholdings* subgroup only. This suggests that the negative ruling effect on income smoothing is more pronounced for the firms where shareholders are likely to have a short-term investment horizon.

Grant et al. (2009) indicate that income smoothing can be viewed as an effective instrument to mitigate the idiosyncratic risk, through which undesirable risk consequences can be more likely avoided. While managers are more likely to be replaced when their firms' idiosyncratic risk increases (Bushman et al. 2010), we expect that managers under such conditions will have a higher propensity to stabilize their tenure by smoothing income, especially when the litigation risk is high. We follow Campbell et al. (2001) and employ the CAPM-based approach to measure the idiosyncratic risk of firms.[11] We construct two subsamples based on the above- and below-median idiosyncratic risk (i.e., *High IVol Risk* and *Low IVol Risk*) and report the estimation results in Panel B of Table 7. As expected, we find that the coefficients on the interaction term, *NinthCircuit* × *Post*, are negative and significant for *High IVol Risk*.

We further examine the impact of ruling on income smoothing in the presence of the manager's outside options. Previous studies show that managers with limited outside options care more about the stability of their tenures (Custódio et al. 2019). Consequently, these managers can be more sensitive to litigation pressure and are more likely to take activities that can stabilize their tenures. We therefore expect the association between the ruling and income smoothing to be more pronounced for firms where managers have fewer outside options. Similar to Custódio et al. (2019), we use local beta, which is the degree of comovement between a firm's stock return and stock returns of other firms within the same state, as the measure of the manager's outside options. The wage indexation theory of Oyer (2004) points out that relevant outside job opportunities for an employee are likely to

[10] Dedicated institutional investors are those who provide stable ownership and take large positions in portfolio companies. Quasi-index institutional investors are those who trade infrequently but own small stakes. Transient institutional investors are those who exhibit high portfolio turnover and own small stakes in individual firms (Bushee 1998; An and Zhang 2013). Both dedicated and quasi-index institutional investors are characterized by low turnover and have a long-term investment horizon.

[11] Our results remain robust if we measure firms' idiosyncratic risk based on the Fama–French three-factor model.

Table 7 Shareholder litigation and income smoothing: the cross-sectional analysis

	Smoothing1		Smoothing2	
	Long-term Shareholdings	Short-term Shareholdings	Long-term Shareholdings	Short-term Shareholdings
	(1)	(2)	(3)	(4)
Panel A. Investor Horizons				
Ninth Circuit × Post	−0.051	−0.116**	−0.045	−0.078**
	(0.043)	(0.045)	(0.033)	(0.033)
Firm Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.091	0.097	0.095	0.113
Observations	8,137	7,816	8,137	7,816
	Smoothing1		Smoothing2	
	Low *IVol* Risk	High *IVol* Risk	Low *IVol* Risk	High *IVol* Risk
	(1)	(2)	(3)	(4)
Panel B. IVol Risk				
Ninth Circuit × Post	−0.004	−0.107**	0.027	−0.067**
	(0.041)	(0.045)	(0.030)	(0.034)
Firm Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.120	0.088	0.104	0.105
Observations	6,774	6,868	6,774	6,868
	Smoothing1		Smoothing2	
	Low Local Beta	High Local Beta	Low Local Beta	High Local Beta
	(1)	(2)	(3)	(4)
Panel C. Outside Options				
Ninth Circuit × Post	−0.097**	−0.060	−0.060**	−0.004
	(0.043)	(0.040)	(0.030)	(0.028)
Firm Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.054	0.034	0.046	0.048
Observations	6,764	6,774	6,764	6,774
	Smoothing1		Smoothing2	
	Low Competitiveness	High Competitiveness	Low Competitiveness	High Competitiveness
	(1)	(2)	(3)	(4)
Panel D. Industry Competition				
Ninth Circuit × Post	−0.038	−0.112***	−0.014	−0.084***
	(0.042)	(0.039)	(0.030)	(0.030)
Firm Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes

Table 7 (continued)

	Smoothing1		Smoothing2	
	Low Competitiveness	High Competitiveness	Low Competitiveness	High Competitiveness
	(1)	(2)	(3)	(4)
Adjusted R^2	0.089	0.041	0.094	0.033
Observations	8,074	7,879	8,074	7,879
	Smoothing1		Smoothing2	
	Non-high-tech Intensity	High-tech Intensity	Non-high-tech Intensity	High-tech Intensity
	(1)	(2)	(3)	(4)
Panel E. Technology Intensity				
Ninth Circuit × Post	−0.012	−0.141**	−0.038	−0.085**
	(0.057)	(0.057)	(0.045)	(0.043)
Firm Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.049	0.075	0.053	0.063
Observations	3,612	3,595	3,612	3,595

This table presents the cross-sectional variation analysis of the effects of the Ninth Circuit ruling on income smoothing. The main dependent variables are *Smoothing1* and *Smoothing2*, respectively. *Smoothing1* is the standard deviation of operating earnings divided by the standard deviation of cash flows from operations, where both the earnings and cash flows are scaled by lagged total assets. *Smoothing2* is the Spearman correlation between the change in cash flow from operations scaled by lagged total assets and the change in total accruals scaled by lagged assets. Our main variable of interest is the interaction term *NinthCircuit* × *Post*, in which *NinthCircuit* equals one if a firm's headquarter is located in one of the Ninth Circuit states, and zero otherwise, while *Post* equals one in the 2000–2003 period, and zero in the 1995–1998 period. In panels A to E, we conduct subsample analyses for investor horizons, for a firm's idiosyncratic risk, for a firm's outside options, for the industry competitiveness, and for the level of a firm's high-tech intensity, respectively. We calculate the difference between the total amount of shares held by dedicated and quasi-index investors and the number of shares held by transient investors of a firm following Bushee's (2001) classification of institutional investor base, all divided by total shares (An and Zhang 2013; Brochet et al. 2015). A larger (smaller) value of the difference means that firms have more (fewer) long-term institutional investors. We then partition the sample into firms with more and fewer long-term investors (i.e., *Long-term Shareholdings* and *Short-term Shareholdings*) based on the median of the distribution of the calculated differences in shareholdings. We follow Campbell et al. (2001) and employ the CAPM-based approach to measure the idiosyncratic risk (*IVol Risk*) of firms. *High IVol Risk* and *Low IVol Risk* are firms with above- and below-median idiosyncratic risk. We measure a firm's outside options using local beta, which is the degree of comovement between a firm's stock return and stock returns of other firms within the same state. The local beta is estimated using a time-series regression of monthly stock return on the return of the stock's corresponding state index (exclude the particular stock), as well as the market portfolio return and the stock's industry (Fama-French 48 industry) return. *High* (*Low*) *Local Beta* is a dummy variable that equals one if the local beta is above (below) the median of the distribution, and zero otherwise. We measure the level of industry competitiveness by using the Herfindahl-Hirschman (HHI) index. *High* (*Low*) *Competitiveness* is a dummy variable that equals one if a firm's HHI is smaller (larger) than the median value of the sample. We follow Hsu et al. (2014) and Hassan et al. (2021) and first calculate the time-series median annual R&D expenditure growth in the state of the firm's headquarters. A firm is identified as *High-tech* (*Non-high-tech*) *Intensity* within a state if its annual R&D expenditure growth is higher (below) than the median annual R&D expenditure growth of that state. Detailed definitions of all control variables included in the regression analysis are provided in Appendix 1. Statistical significance is based on the heteroscedasticity-robust firm-clustered standard errors reported in parentheses

***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. All accounting variables are winsorized at the 1st and 99th percentiles.

be offered by firms in the same region rather than by firms that are farther away. The local beta is estimated using a time-series regression of monthly stock return on the return of the stock's corresponding state index (exclude the particular stock), as well as the market portfolio return and the stock's industry (Fama–French 48 industry) return.[12] *High* (*Low*) *Local Beta* is therefore a dummy variable that equals one if the local beta is above (below) the median of the distribution, and zero otherwise. We then partition the sample into *High Local Beta* and *Low Local Beta* groups. In Panel C of Table 7, we find that the coefficient estimates of *NinthCircuit* × *Post* are negative and significant for *Low Local Beta* only, suggesting that the negative ruling effect on income smoothing is stronger for firms where managers have limited outside options.

We also examine the relationship between the ruling and income smoothing in the presence of industry competition. The prior literature documents that managers experience greater pressure to cater to investor preferences when their firms face intense industry competition (DeFond and Park 1999; Brickley 2003; Javakhadze et al. 2014). Therefore, it is possible that higher litigation risk leads to managers in competitive industries having greater incentives to reduce earnings volatility through income smoothing, suggesting that the negative ruling effect on income smoothing can be stronger for firms in a more competitive industry. To test this, we follow Javakhadze et al. (2014) and Khurana et al. (2018) and measure the level of industry competitiveness by using the Herfindahl–Hirschman (HHI) index. We then construct an indicator variable, *High Competitiveness*, that equals one if a firm's HHI is smaller than the median value of the sample, and zero otherwise. We re-estimate our baseline model by constructing a subsample analysis based on the degree of industry competition. Panel D of Table 7 presents the test results for high and low levels of industry competition. Results show that the coefficients on the interaction term, *NinthCircuit* × *Post*, are significantly negative for *High Competitiveness*, indicating that the ruling effect is more pronounced for firms in a more competitive industry.

Finally, using the Securities Class Action Clearinghouse data, prior studies posit that firms in high-tech industries are usually sued more than firms in other industries (Hassan et al. 2021). According to this, we expect high-tech firms to be more sensitive to the adoption of the 1999 ruling. Following Hsu et al. (2014) and Hassan et al. (2021), we first calculate the time-series median annual R&D expenditure growth in the state of the firm's headquarters. We then identify a firm as high (low)-tech intensive firm within a state if its annual R&D expenditure growth is higher (below) than the median annual R&D expenditure growth of that state (*High-tech Intensity* and *Low-tech Intensity*).[13] In Panel E of Table 7, we re-estimate our baseline model by partitioning our sample into *High-tech Intensity* and *Low-tech Intensity* subgroups. We find that the coefficient estimates of *NinthCircuit* × *Post* are negative and significant for *High-tech Intensity* subgroup only.

[12] We require at least 24 nonmissing monthly return observations for a particular stock and that there should be five stocks in the state for entering the regression analysis (Custódio et al. 2019). We collect monthly T-bill from the CRSP.

[13] Rajan and Zingales (1998) suggest that R&D expenditure is an appropriate measure of high-tech intensity as the financial reporting standard (i.e., Financial Accounting Standards Board Statement No. 2) requires US public firms to disclose sufficient information of firm-level R&D expenditure.

Table 8 Controlling for confounding legal changes

	Smoothing1	Smoothing2	Smoothing1	Smoothing2	Smoothing1	Smoothing2	Smoothing1	Smoothing2	Smoothing1	Smoothing2	Smoothing1	Smoothing2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Ninth Circuit × Post	−0.080***	−0.051**	−0.080***	−0.051**	−0.080***	−0.052**	−0.080***	−0.051**	−0.079***	−0.051**	−0.080***	−0.051**
	(0.029)	(0.022)	(0.029)	(0.022)	(0.029)	(0.022)	(0.029)	(0.022)	(0.029)	(0.022)	(0.029)	(0.022)
DD	−0.177***	−0.046	−0.176***	−0.046	−0.178***	−0.047	−0.179***	−0.043	−0.177***	−0.043	−0.177***	−0.044
	(0.043)	(0.031)	(0.044)	(0.031)	(0.044)	(0.031)	(0.043)	(0.031)	(0.044)	(0.031)	(0.044)	(0.031)
PP			0.011	−0.003	0.014	−0.001	0.014	−0.001	0.014	−0.001	0.015	−0.001
			(0.038)	(0.023)	(0.038)	(0.023)	(0.038)	(0.023)	(0.038)	(0.023)	(0.040)	(0.024)
BC					−0.066	−0.059	−0.054	−0.098*	−0.054	−0.098*	−0.054	−0.097*
					(0.067)	(0.044)	(0.076)	(0.052)	(0.076)	(0.052)	(0.076)	(0.052)
UD							−0.014	0.046	−0.013	0.046	−0.014	0.046
							(0.048)	(0.035)	(0.048)	(0.035)	(0.048)	(0.035)
IDD									0.012	−0.001	0.011	−0.001
									(0.024)	(0.017)	(0.024)	(0.017)
RIDD											−0.006	−0.003
											(0.036)	(0.023)
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.061	0.063	0.061	0.063	0.061	0.063	0.061	0.064	0.061	0.064	0.061	0.064
Observations	15,953	15,953	15,953	15,953	15,953	15,953	15,953	15,953	15,953	15,953	15,953	15,953

Table 8 Controlling for confounding legal changes

This table examines the impact of shareholder litigation on income smoothing by controlling for confounding legal changes. The main dependent variables are *Smoothing1* and *Smoothing2*, respectively. *Smoothing1* is the standard deviation of operating earnings divided by the standard deviation of cash flows from operations, where both the earnings and cash flows are scaled by lagged total assets. *Smoothing2* is the Spearman correlation between the change in cash flow from operations scaled by lagged total assets and the change in total accruals scaled by lagged assets. Our main variable of interest is the interaction term *NinthCircuit* × *Post*, in which *NinthCircuit* equals one if a firm's headquarter is located in one of the Ninth Circuit states, and zero otherwise, while *Post* equals one in the 2000–2003 period, and zero in the 1995–1998 period. In columns (1)–(6), we repeat the baseline regression estimation by sequentially adding indicator variables of three additional state-level antitakeover laws, namely directors' duties laws (DD), poison pill laws (PP), and business combination laws (BC), respectively. In columns (7)–(8), we further control for the universal demand laws (UD), which refers to legal changes that affect shareholders' ability to file derivative lawsuits. In columns (9)–(12), we follow Flammer and Kacperczyk (2019) and control for the enactment of the inevitable disclosure doctrine (IDD) and the rejection of the inevitable disclosure doctrine (RIDD). Detailed definitions of all control variables, included in the regression analysis, are provided in Appendix 1. Statistical significance is based on the heteroscedasticity-robust firm-clustered standard errors reported in parentheses

***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. All accounting variables are winsorized at the 1st and 99th percentiles

Table 9 Additional robustness checks

	INC_NI (1)	Accr_MJ (2)	Smoothing1 (3)	Smoothing2 (4)	Smoothing1 (5)	Smoothing2 (6)	Smoothing1 (7)	Smoothing2 (8)	Smoothing1 (9)	Smoothing2 (10)	Smoothing1 (11)	Smoothing2 (12)	Smoothing1 (13)	Smoothing2 (14)
Ninth Circuit × Post	−0.025**	−0.015**	−0.052**	−0.041**	−0.080***	−0.051**	−0.075**	−0.052**	−0.106***	−0.052**	−0.077***	−0.050**	−0.088***	−0.051**
	(0.012)	(0.007)	(0.027)	(0.021)	(0.021)	(0.022)	(0.031)	(0.022)	(0.034)	(0.026)	(0.029)	(0.022)	(0.028)	(0.021)
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	No	No	No	No	No	No	No	No	No	No	No
State Variables	No	No	No	No	No	No	No	No	No	No	No	No	Yes	Yes
Adjusted R^2	0.047	0.080	0.022	0.028	0.061	0.063	0.058	0.056	0.074	0.073	0.060	0.063	0.063	0.065
Observations	18,076	16,326	21,511	21,511	15,953	15,953	14,068	14,068	12,424	12,424	15,606	15,606	15,912	15,912

In this table, we provide additional robustness checks of our main findings. In columns (1)–(2), we use the indicator of increasing earnings patterns for at least five years (*INC_NI*) (Barth et al. 1999) and the discretionary accruals based on Dechow et al. (1995) (*Accr_MJ)* as two alternative measures of income smoothing. To ensure that our measures of income smoothing are calculated using data after the ruling, in columns (3)–(4) we repeat the baseline regression with an extended sample between 1995 and 2007. In columns (5)–(6), we re-estimate our baseline results by clustering standard errors at the state of location level. In columns (7)–(8), we repeat our baseline regression by excluding utility (SIC 4000–4999) and financial (SIC 6000–6999) industries since they are regulated and may have different reporting environments. In columns (9)–(10), we exclude high technology industries, which are identified using the Fama–French five-industry classification from the data library. In columns (11)–(12), we exclude firms incorporated in Nevada, as the personal legal liability of corporate managers and directors can be limited in Nevada. In columns (13)–(14), we control for local economic conditions by adding several state-level measures to the baseline model, such as GDP growth rate, personal income growth rate, population growth rate, unemployment growth rate, total capital expenditure growth rate, total R&D growth rate, and asset-weighted market-to-book ratio. The main dependent variables across columns (3)–(14) are *Smoothing1* and *Smoothing2*, respectively. *Smoothing1* is the standard deviation of operating earnings divided by the standard deviation of cash flows from operations, where both the earnings and cash flows are scaled by lagged total assets. *Smoothing2* is the Spearman correlation between the change in cash flow from operations scaled by lagged total assets and the change in total accruals scaled by lagged assets. The main variable of interest is the interaction term *NinthCircuit* × *Post*, in which *NinthCircuit* equals one if a firm's headquarter is located in one of the Ninth Circuit states, and zero otherwise, while *Post* equals one in the 2000–2007 period, and zero in the 1995–1998 period. Detailed definitions of all control variables included in the regression analysis are provided in Appendix 1. Statistical significance for columns (1)–(4) and columns (7)–(14) is based on the heteroscedasticity-robust firm-clustered standard errors reported in parentheses

***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. All accounting variables are winsorized at the 1st and 99th percentiles

4.2.2 Additional robustness checks

A natural question to ask is whether our baseline results might be driven by other confounding legal changes. As noted in Karpoff and Wittry (2018), our placing legal changes under the spotlight might be linked to state-level antitakeover laws, for example. To mitigate such a concern, in columns (1)–(6) of Table 8, we repeat the regression estimation as shown in Eq. (1) by sequentially adding indicator variables of three additional state-level antitakeover laws, namely directors' duties laws (DD), poison pill laws (PP), and business combination laws (BC), to the model. In columns (7)–(8), we further control for the universal demand laws (UD), which refer to legal changes that affect shareholders' ability to file derivative lawsuits. Appel (2019) points out the significant difference between class action lawsuits and derivative lawsuits, while there are not absolute substitutes for one another. Compared to class action lawsuits that simply permit managers to be sued by a subset of shareholders, derivative lawsuits allow shareholders to sue managers and/or directors on behalf of the corporation for a breach of their fiduciary duty (Ni and Yin 2018). Thus, a decreasing threat of class action lawsuits and of derivative lawsuits may not yield similar effects. Moreover, in columns (9)–(12), we follow Flammer and Kacperczyk (2019) and control for the enactment of the inevitable disclosure doctrine (IDD) and the rejection of the inevitable disclosure doctrine (RIDD), which may affect employee turnover. Both laws may impact firms' disclosure decisions and thereby influence income smoothing. Our results show that the estimated coefficients of $NinthCircuit \times Post$ remain negative and significant throughout all columns in Table 8.

We perform further robustness checks and present the results in Table 9. First, in columns (1)–(2), we use the indicator of increasing earnings patterns for at least five years (*INC_NI*) (Barth et al. 1999) and the discretionary accruals based on the Dechow et al. (1995) (*Accr_MJ*) as two alternative dependent variables.[14] Second, one can argue that our measures of income smoothing based on a rolling five-year window may make it is less likely that the observed changes in income smoothing can be attributed to the ruling. To alleviate this concern and ensure that our measures of income smoothing are calculated using data after the ruling, we repeat the baseline regression with an extended sample between 1995 and 2007 and report the results in columns (3)–(4). Third, in columns (5)–(6), we re-estimate our baseline results by clustering standard errors at the state of location level. Fourth, in columns (7)–(8), we repeat our baseline regression by excluding utility (SIC 4000–4999) and financial (SIC 6000–6999) industries since they are regulated and may have different reporting environments (Tucker and Zarowin 2006; Mahajan and Tartaroglu 2008). Fifth, given that the enactment year of the ruling was 1999 and one of the Ninth Circuit states is California, it is possible that the main results are driven by the technology bubble, which co-occurred in the period 1999–2000 (Chu 2017). We therefore exclude high technology industries, which are identified using the Fama–French

[14] Rationales of using these two alternative dependent variables are, first, if managers are more likely to smooth income to show stable income over time, the negative ruling effect should hold for the likelihood of firms showing increasing income patterns (*INC_NI*) and, second, if managers smooth income through discretionary accruals, the negative association between the 1999 ruling and the discretionary accruals based on Dechow et al. (1995) (*Accr_MJ*) should also hold. We thank two anonymous referees for pointing out these. Moreover, as presented in column (2) of Table 9, we replace the industry-year fixed effect by the year fixed effect because *Accr_MJ* is calculated at the industry level.

five-industry classification from the data library (Chang et al. 2019) in columns (9)–(10).[15] Sixth, in columns (11)–(12), we exclude firms incorporated in Nevada because the personal legal liability of corporate managers and directors can be limited in Nevada (Donelson and Yust 2014). Finally, in columns (13)–(14), we control for local economic conditions by adding several state-level measures, such as GDP growth rate, personal income growth rate, population growth rate, unemployment growth rate, total capital expenditure growth rate, total R&D growth rate, and asset-weighted market-to-book ratio (Chen and Vashishtha 2017), to Eq. (1). We find that our results are robust across all these empirical specifications.

4.2.3 Reusing natural experiments

In a recent study, Heath et al. (2023) point out the multiple hypothesis testing problem of repeated using a natural experiment. They show that business combination laws and Regulation SHO pilot have been exploited by more than 500 different dependent variables and such repeated use of a natural experiment may increase the likelihood of false discoveries. Compared with these two laws and the universal demand laws, which have been reused in more than 30 studies, the 1999 ruling has been much less exploited.[16]

Nevertheless, in unreported results, we examine the association between litigation risk and income smoothing using a more recent sample period between 2004 and 2019 (these unreported results can be found from the online appendix). We manually search for the information on filings of securities class action lawsuits from the Stanford Law School *Securities Class Action Clearinghouse* (Kim and Skinner 2012).[17] After matching the litigation data with the public companies from the Compustat/CRSP merged and Execucomp databases, we identify 153 public firms as being involved in security class action lawsuits and 284 lawsuit cases over the period of 2004 to 2019. We follow previous studies (Gande and Lewis 2009; Kim and Skinner 2012; Arena 2018; Arena and Julio 2023) and estimate a probit regression with a dependent variable equal to one if a class period of a lawsuit filing occurred for a firm during a given year, and zero otherwise.[18] Our alternative measure of litigation risk (i.e., litigation likelihood) is therefore the predicted probabilities through estimating the probit regression. We then repeat the baseline regression analysis using the litigation likelihood and find a significant

[15] The Fama–French five-industry classification refers to consumer goods (Cnsmr), manufacturing (Manuf), high technology (HiTec), health care (Hlth), and other (Other). The data can be obtained from the data library of Kenneth R. French: http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/Data_Library/det_5_ind_port.html).

[16] To our best knowledge, there are about ten published articles that apply the 1999 ruling as a difference-in-differences setting in their baseline regression (Chu 2017; Crane and Koch 2018; Hopkins 2018; Dong and Zhang 2019; Houston et al. 2019; Chung et al. 2020; Hassan et al. 2021; Huang et al. 2020; Arena et al. 2021; Yang et al. 2021). As noted in Gao et al. (2021) and Heath et al. (2023), the possibility of false discoveries can be relatively low when a natural experiment is reused around ten times.

[17] Kim and Skinner (2012) indicate that the Stanford Law School *Securities Class Action Clearinghouse* database is commonly used as a source of lawsuit filings. In their study, they further check the data from the Stanford database with the 10-K disclosures of the involvement in the 10b-5 litigation for S&P 500 companies and assure the completeness of the Stanford database.

[18] We include independent variables (return skewness, return volatility, litigation intensity, CEO share ownership, CEO bonus over to total compensation, regulated industry dummy, high-tech dummy, retail industry dummy, and high-polluting industry dummy) that have been accepted as predictors of the likelihood of class action lawsuits. Moreover, to avoid the identification problem in our baseline regression analysis, the independent variables included in the probit regression are different with any of the independent variables used in the baseline regression model.

and positive association between the likelihood of shareholder litigation and income smoothing. This result lends further support to our main findings that the decline in the threat of class action lawsuits following the 1999 ruling decreases income smoothing.

5 Conclusion

In this paper, we study the relationship between litigation risk and income smoothing by exploiting the ruling of the Ninth Circuit Court of Appeals in 1999 as an exogenous shock to the threat of class action lawsuits. Using a difference-in-differences approach over the sample period 1995–2003, we find that decreasing the threat of litigation reduces the incentives to smooth income. Such findings are robust to different model specifications. We also show that the negative ruling effect on income smoothing is more pronounced for firms where shareholders have a short-term investment horizon, for firms with higher idiosyncratic risk, for firms where managers have limited outside options, for firms in competitive industries, and for firms that are more high-tech intensive. These results are consistent with the view that higher litigation risk may pressure management into taking activities that can reduce the short-term uncertainties and stabilize the tenure.

Our findings raise two questions. First, it is possible that CEO candidates view the time and reputation costs related to shareholder lawsuits as onerous, and thus firms headquartered in states with higher shareholder litigation risk may have difficulty attracting and retaining talented CEOs. Therefore, does reduced shareholder litigation risk influence the CEO labor market? Is there any difference between the quality of CEOs of firms headquartered in the Ninth Circuit states and their counterparts in other states? To investigate this, we can examine whether any change in CEO skill sets is associated with the 1999 ruling. Second, in line with the "pressure hypothesis", fund managers, like CEOs of corporations, may also experience the short-term pressures associated with shareholder litigation risk, which in turn would significantly impact their investment strategies. Thus, it may be useful to explore whether there are any noticeable changes related to the asset allocations and investment horizons of mutual fund managers around the 1999 ruling. These two questions could be the focus of future research.

Appendix 1. Variable definitions

Variable	Description
Smoothing1	The standard deviation of operating earnings divided by the standard deviation of cash flows from operations, where both the earnings and cash flows are scaled by lagged total assets and standard deviations are calculated at the annual level over rolling five-year windows ending in the current fiscal year (Leuz et al. 2003; Dou et al. 2013)
Smoothing2	The Spearman correlation between the change in cash flow from operations scaled by lagged total assets and the change in total accruals scaled by lagged assets (Bhattacharya et al. 2003; Dechow et al. 2010)
Ninth Circuit	Indicator takes the value one when the firm is under the jurisdiction of the Ninth Circuit Court as determined by headquarters location, and zero otherwise
Size	The natural logarithm value of total assets in thousands of dollars
ROA	Earnings before interest, tax, depreciation, and amortization (EBITDA) divided by total assets

Variable	Description
Leverage	Short-term plus long-term debt, divided by common equity
Market-to-book Ratio	Computed as the book value of net assets minus the book value of equity plus the market value of equity, all divided by the book value of assets
Asset Tangibility	Total value of property, plant, and equipment, divided by total assets
Cash Flow	Cash and short-term investments divided by total asset
Stock Return	Annual stock return over the fiscal year
Sales Growth	Current year's sales less prior year's sales less the increase in receivables, scaled by prior year's sales
R&D	Research and development (R&D) expenses divided by total asset
CAPEX	Capital expenditures divided by total asset
Dividends	An indicator variable that equals 1 if a firm pays dividends, and zero otherwise
Institutional Ownership	The number of shares held by institutional investors divided by the number of shares outstanding
Analysts Following	The natural logarithm value of one plus the number of analysts following a firm
Big N Auditor	An indicator variable that equals one when firms are audited by a Big N audit firm, and zero otherwise. Big N firms are defined by Compustat as firms with AU codes between 1 and 8, inclusive
Debt Issue	Computed as Long-term debt issuance minus long-term debt reduction, divided by total assets
Equity Issue	Computed as sale of common stock, divided by shareholder equity
Acquisition	An indicator variable that equals 1 if a firm is involved in mergers and acquisitions in the focal year as reported by the Securities Data Company (SDC), and zero otherwise
INC_NI	An indicator variable that equals 1 if a firm has at least five consecutive prior years of increasing earnings, and zero otherwise
Accr_MJ	Discretionary accruals, defined as residuals (ε_t) from the following model estimated for every industry and year (Jones 1991; Dechow et al. 1995): $\frac{TA_t}{Assets_{t-1}} = \alpha_1 \frac{1}{Asset_{t-1}} + \alpha_2 \frac{\Delta SALES_t - \Delta AR_t}{Asset_{t-1}} + \alpha_3 \frac{PPE_t}{Asset_{t-1}} + \varepsilon_t$ TA_t is is compusted as $TA_t = \Delta CA_t - \Delta CL_t - \Delta Cash_t + \Delta STD_t - DEP_t$; where ΔCA_t is the change in current assets, $\Delta Cash_t$ is the change in cash, ΔCL_t is the change in current liabilities, ΔSTD_t is the change in debt in current liabilities, and DEP_t is the depreciation and amortization expense. $Asset_{t-1}$ is the total assets. $\Delta SALES_t$ is the change in sales, ΔAR_t is the change in accounts receivables; and PPE_t is the gross value of property, plant, and equipment

Appendix 2. Diagnostic tests for the propensity score matching

This table reports the diagnostic test results for the propensity score matching presented in Table 5. We report the univariate comparisons between treated firms (i.e., firms located in states belonging to the Ninth Circuit) and their matched control firms (i.e., firms located in states belonging to other circuits). Definitions of all variables are provided in Appendix 1.

Variables	Treated Firms	Matched Control Firms	Differences	t-statistics
Size	5.150	5.215	−0.065	−0.89
ROA	0.031	0.057	−0.026	−0.97
Leverage	0.293	0.295	−0.003	−0.07

Variables	Treated Firms	Matched Control Firms	Differences	t-statistics
Market-to-book Ratio	2.111	2.030	0.081	0.55
Asset Tangibility	0.250	0.267	−0.017	−0.97
Cash Flow	0.201	0.198	0.003	0.20
Stock Return	−0.204	−0.178	−0.025	−0.52
Sales Growth	0.137	0.122	0.015	0.46
R&D	0.095	0.092	0.003	0.29
CAPEX	0.070	0.072	−0.002	−0.43
Dividends	0.208	0.230	−0.022	−0.67
Institutional Ownership	0.253	0.274	−0.020	−0.92
Analysts Following	0.872	0.990	−0.118	−1.38
Big N Auditor	0.899	0.890	0.009	0.39
Debt Issue	0.018	0.029	−0.010	−1.27
Equity Issue	0.081	0.066	0.015	0.51
Acquisition	0.435	0.426	0.009	0.24

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