JOURNAL OF INTERNATIONAL ACCOUNTING RESEARCH
Vol. XX, No. XX
MONTH YEAR
pp. 1–24

American Accounting Association
DOI: 10.2308/JIAR-2022-050

Say-on-Pay Laws and Financial Reporting Quality Around the World: Evidence from a Natural Experiment

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ABSTRACT: In response to criticisms of executive pay policies, say-on-pay (SoP) laws have been adopted worldwide. However, evidence on the effectiveness of these laws remains inconclusive. This study examines the effect of SoP laws on financial reporting quality (FRQ) by employing a shock-based research design. Using a large sample of firms from 34 economies from 2000 through 2019, we document a significant improvement in FRQ after the adoption of SoP laws, suggesting increased monitoring on financial reporting. In addition, our cross-sectional analyses show that the improvement in FRQ is more pronounced for firms with strong demand for better FRQ or with corporate governance mechanisms that enable effective monitoring. Taken together, our findings support the monitoring effect of SoP laws and suggest that improved FRQ may be an unintended benefit of SoP laws.

Data Availability: All the data are available from the public sources described in the paper.

JEL Classifications: G34; M12; M41.

Keywords: say-on-pay; executive compensation; financial reporting quality; corporate governance.

I. INTRODUCTION

In recent decades, exponentially increasing executive pay has prompted worldwide debates over how to improve corporate governance systems to better align executive pay with shareholder interests. One of the most influential proposals is to empower shareholders regarding executive pay, commonly referred to as say-on-pay (SoP) (Hooghiemstra, Kuang, and Qin 2017). By 2019, this proposal had gained widespread support from lawmakers

We thank Jeong-Bon Kim, Byron Song, Song Tang, and the participants at Fudan University and City University of Hong Kong for their constructive comments. We are grateful for the constructive comments from Linda A. Myers (editor) and two anonymous referees. The authors are grateful for the research assistance from Zhenwen He. All authors contributed equally. Gaoguang Zhou acknowledges financial support by General Research Fund (GRF) from the Research Grants Council of Hong Kong (RGC) [12501918]. Haina Shi acknowledges financial support from the National Natural Science Foundation of China [Grant No. 72372028]. Chang He and Xindong (Kevin) Zhu have no financial conflicts of interest related to this research.

All errors are our own.

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Editor's note: Accepted by Linda A. Myers, under the Senior Editorship of Steve Lin.

Submitted: August 2022
Accepted: August 2024
Early Access: August 2024

and has been translated into SoP laws in 14 economies (Thomas and Van der Elst 2015; Carter, Pawliczek, and Zhong 2021).

Proponents of SoP laws believe they can curb excessive executive pay and enhance pay-for-performance sensitivity (PPS) (Ferri and Maber 2013; Correa and Lel 2016). In contrast, opponents express concerns related to unsophisticated shareholders' abilities to assess executive pay (Fama and Jensen 1983; U.S. House of Representatives 2007; Gordon 2009; Larcker, McCall, Ormazabal, and Tayan 2012; Mangen and Magnan 2012); potential harm to other stakeholders, including minority shareholders (Mangen and Magnan 2012); the enforcement of SoP laws (Brunarski, Campbell, and Harman 2015); and nontrivial compliance costs (Cai and Walkling 2011). Empirical studies examining whether SoP laws have achieved their desired goals yield inconclusive results.[1] Because effective executive compensation contracts rely on accounting quality (Sloan 1993; Core 2020), we contribute to this debate by exploring the relation between SoP laws and financial reporting quality (FRQ).

Although FRQ plays an important role in the executive contracting process (Jensen and Meckling 1976; Watts and Zimmerman 1986), prior studies on FRQ and executive compensation focus predominately on the influence of boards of directors (Armstrong, Guay, and Weber 2010), rather than shareholders. As such, SoP law adoption enables us to extend the literature by shedding light on how shareholder participation in the executive contracting process affects FRQ. In a broader sense, our study is among the few recent attempts to examine the effect of a specific investor protection mechanism on FRQ using a shock-based research design.[2]

It remains unclear whether and how FRQ is affected by SoP laws. The managerial opportunism hypothesis predicts that SoP laws give managers stronger incentives to manipulate earnings to justify their pay, leading to impaired FRQ. However, the monitoring hypothesis suggests that rigorous monitoring by shareholders and boards of directors following SoP laws results in enhanced FRQ. In addition, SoP laws may be irrelevant to FRQ because some economies adopt SoP laws on a nonbinding basis or only weakly enforce SoP laws. Even if executives opt to manage earnings after the enactment of SoP laws, shareholders can adjust accounting-based executive pay, for example, by placing more weight on nonfinancial metrics and adjusting earnings when evaluating performance. Additionally, the efficient contract hypothesis (Watts and Zimmerman 1986) suggests no effect on FRQ because shareholders who benefit from earnings management lack incentives to monitor. Thus, whether and how SoP laws affect FRQ remains a controversial issue.

To empirically examine this issue, we exploit the staggered adoption of SoP laws across economies (Correa and Lel 2016; Carter et al. 2021) and employ a shock-based research design. In our research setting, shareholders' ability to vote on firm compensation policies changes due to an exogenous shock that is orthogonal to firm-level attributes. We employ two widely used measures to capture the extent to which managers opportunistically manipulate earnings: discretionary accruals and income smoothing (Gopalan and Jayaraman 2012; J. Kim, Y. Kim, and Zhou 2017). Based on a sample of firms headquartered across 34 economies (specifically, 14 SoP economies and 20 non-SoP economies) from 2000 through 2019, we find that firms experience significant improvements in FRQ after SoP law adoption, supporting the monitoring hypothesis. This finding is robust to approaches to mitigate potential endogeneity concerns, alternative measures of FRQ, and alternative samples. An additional test shows that SoP laws constrain real earnings management (REM) as well, lending further support to the conclusion that increased monitoring efforts are prompted by SoP laws.

We conduct cross-sectional analyses to corroborate our baseline results and advance our understanding of how SoP laws affect FRQ. Cross-sectional analyses support our expectation that SoP law adoption is more effective for firms that demand high FRQ and for those with effective monitoring mechanisms. Specifically, our results show that the effect of SoP laws on FRQ is more pronounced when (1) the firm's information environment is poor and (2) corporate governance is strong.

Our study contributes to the literature in the following ways. First, we add to the debate on whether SoP laws are beneficial by examining FRQ. Prior studies assess SoP laws by examining investor reactions to SoP laws (Cai and Walkling 2011; Ferri and Maber 2013; Iliev and Vitanova 2019), executive pay practices (Ertimur, Ferri, and Oesch 2013; Ferri and Maber 2013; Alissa 2015; Balsam, Boone, Liu, and Yin 2016; Correa and Lel 2016), and executive incentives to engage in insider trading (Bourveau, Brochet, Ferri, and Sun 2024). Our study identifies another benefit of SoP laws. Specifically, as noted in Cornett, Marcus, and Tehranian (2008), FRQ should be considered when assessing the effectiveness of incentive-based compensation. Our finding of enhanced FRQ indicates that the observed increase in PPS, as documented in Correa and Lel (2016), is unlikely to be driven by earnings manipulation, suggesting the effectiveness of SoP laws. Our findings also contribute to the literature by identifying enhanced FRQ as an unintended consequence of SoP laws. Taken together, our findings should be informative to capital market regulators and lawmakers considering the adoption of SoP laws.

[1] See Obermann and Velte (2018) for a comprehensive review of this literature.
[2] Khurana and Wang (2019) is another notable study that focuses on takeover laws.

Second, this study contributes to the investor protection and earnings quality literature by examining the effect of SoP legislation on FRQ. Prior international studies show that economy-level investor protection is associated with firm-level FRQ (Leuz, Nanda, and Wysocki 2003; DeFond, Hung, and Trezevant 2007; Cahan, Liu, and Sun 2008; Haw, Hu, Lee, and Wu 2012). However, most prior studies are subject to endogeneity concerns.[3] Our study draws inferences by treating the staggered adoption of SoP laws worldwide as a series of exogenous events and by conducting a shock-based analysis. Because SoP laws are among the most important and controversial types of investor protection legislation to have emerged in recent decades, insight into the relation between SoP law adoption and FRQ contributes to the investor protection and earnings quality literatures.

Finally, and more broadly, this study contributes to the literature on executive incentive compensation and financial reporting incentives. The implications of executive incentive schemes for FRQ are a central theme of corporate governance research. Studies usually examine this issue in a context in which boards of directors, rather than shareholders, negotiate with executives (Armstrong et al. 2010). The adoption of SoP laws creates a new setting in which shareholders can participate in the contracting process by directly expressing their approval or dissent. Our study thus complements and extends the literature by showing that participation by shareholders in the contracting process following SoP law adoption affects management's financial reporting behavior.

This paper proceeds as follows. We outline the institutional background, review the literature, and develop our hypothesis in Section II. In Section III, we present our empirical model. We then outline the research design and describe our sample construction in Section IV. We report our empirical results in Section V. Finally, Section VI concludes.

II. INSTITUTIONAL BACKGROUND, LITERATURE REVIEW, AND HYPOTHESIS DEVELOPMENT

Institutional Background

In recent decades, high levels and seemingly unconstrained growth of executive pay have prompted a public debate over whether executives are overpaid. Many studies contribute to this debate by offering evidence that a large proportion of executive pay cannot be justified by performance (e.g., Jensen and Murphy 1990; Bebchuk, Fried, and Walker 2002; Murphy 2013; Correa and Lel 2016). This points to a serious corporate governance issue pertaining to insufficient alignment among shareholder interests and those of directors and managers. To address this concern, policymakers around the world have legislated SoP laws to empower shareholders with a voice in executive pay issues. The United Kingdom (U.K.) led the wave of SoP legislation in 2002 by mandating that quoted firms provide director pay reporting and by empowering shareholders to cast nonbinding votes on director pay at the annual general meeting. Many other economies subsequently legislated SoP laws. As of 2019, 14 developed economies had passed SoP laws, authorizing shareholders to vote on executive pay issues on a binding, advisory, or comply-or-explain basis. These SoP laws represent one of the most influential waves of corporate governance legislation in the past decade (Thomas and Van der Elst 2015; Hooghiemstra et al. 2017). Despite variation in SoP laws across economies, their main purposes are to increase PPS, curb excessive executive pay, and promote executive compensation transparency.

Although proponents of SoP laws argue that these laws may increase firm value by making executives more accountable for firm performance (Ferri and Maber 2013; Correa and Lel 2016), opponents of SoP laws worry that they may not achieve their desired goals for several reasons. First, SoP legislation may result in the homogenization of executive pay policies and lead to suboptimal executive compensation policies (U.S. House of Representatives 2007; Mangen and Magnan 2012). Second, shareholders may not be as sophisticated and informed as the board of directors in assessing executive pay (Fama and Jensen 1983; U.S. House of Representatives 2007; Gordon 2009; Larcker et al. 2012; Mangen and Magnan 2012). Empowering shareholders to vote on pay policies may also harm other stakeholders, including minority shareholders (Mangen and Magnan 2012). Third, enforcement of SoP laws may not be strong enough to achieve the desired goals.[4] Finally, nontrivial compliance costs could offset potential benefits of these laws (Cai and Walkling 2011).

Motivated by these competing views on SoP laws, many researchers empirically examine their consequences. Because the U.S. and U.K. were pioneers in passing SoP laws, many researchers examine whether SoP laws achieve their intended goals in these two economies. Exceptions are Correa and Lel (2016) and Bourveau et al. (2024), which investigate the effects of SoP laws worldwide. These prior studies take different perspectives and provide mixed results.

One strand of literature examines the determinants of SoP dissent and shows that firms with excessive executive pay are more likely to receive SoP disapprovals (Conyon and Sadler 2010; Ertimur, Ferri, and Muslu 2011; Ertimur et al. 2013;

[3] A notable exception is Khurana and Wang (2019), which finds that adoption of merger and acquisition laws leads to more conservative financial reporting.

[4] For example, directors in economies adopting these laws on a nonbinding basis, such as in the U.S., may have little obligation to oversee executive pay policies (Brunarski et al. 2015).

Alissa 2015; Balsam et al. 2016; Kimbro and Xu 2016). Another strand examines the consequences of SoP on executive pay and other policy changes, yielding mixed results. These studies show that firms that receive SoP disapprovals reduce executive pay levels (Ertimur, Ferri, and Stubben 2010; Ertimur et al. 2011; Ertimur et al. 2013; Balsam et al. 2016) and pay growth (Kimbro and Xu 2016), especially when firm performance is poor (Alissa 2015), and remove controversial executive compensation provisions (Ferri and Maber 2013), suggesting that SoP disapprovals can effectively mitigate excessive executive pay issues. In an international setting, Correa and Lel (2016) provide corroborative evidence that SoP laws are effective, as evidenced by decreased executive pay growth and increased PPS. However, executives increase insider trading to offset compensation risk (Bourveau et al. 2024). In contrast, other studies find little or no change in executive pay after shareholders express dissatisfaction with executive pay practices (Conyon and Sadler 2010; Ferri and Maber 2013; Armstrong, Gow, and Larcker 2013; Brunarski et al. 2015). For example, Brunarski et al. (2015) show that overcompensated U.S. managers respond to low SoP approval by appeasing dissatisfied shareholders (e.g., increasing investment and dividends and decreasing leverage) without experiencing a substantial decrease in their compensation.

Furthermore, many studies use stock market reactions to the passage of SoP laws and firms' adoption of SoP policies to assess the effectiveness of SoP laws. Cai and Walkling (2011) and Ferri and Maber (2013) report positive stock market returns after the announcement of SoP regulations in the U.S. and U.K. Cuñat, Giné, and Guadalupe (2016) show that the market reacts positively to proposals to adopt SoP policies for Standard & Poor's 1500 firms. Iliev and Vitanova (2019) show that investors react negatively to exemptions from SoP rules in the U.S. In contrast, Larcker, Ormazabal, and Taylor (2011) find negative stock market reactions to the passage of SoP regulation in the U.S.

Hypothesis Development

Executive incentive schemes are designed to align executive and shareholder interests (Jensen and Meckling 1976). An incentive scheme typically includes many forms of compensation, such as bonuses, stocks, and options, all of which are directly or indirectly linked to accounting performance (Dechow, Hutton, and Sloan 1996; Ittner, Larcker, and Rajan 1997; Cornett et al. 2008). More specifically, various performance metrics are based on accounting numbers (Sloan 1993; Core 2020). Although accounting numbers are important for compensation contracting, it remains unclear whether and how SoP law affects FRQ. We make three predictions.

Our first prediction is based on the *managerial opportunism hypothesis*, which suggests that executive compensation is influenced by accounting numbers and that executives have strong incentives to manipulate earnings to extract more wealth at the expense of shareholders (Healy 1985; DeAngelo 1988; McNichols and Wilson 1988; J. Gaver, K. Gaver, and Austin 1995; Bergstresser and Philippon 2006; Burns and Kedia 2006). These incentives could increase after SoP law adoption because firms respond to shareholder disapproval by reducing the level and growth of executive pay (Ertimur et al. 2010; Ertimur et al. 2013; Alissa 2015; Kimbro and Xu 2016). To justify excessive pay after the adoption of SoP laws, managers may have stronger incentives to manage earnings.[5] The managerial opportunism hypothesis thus predicts impaired FRQ after the adoption of SoP laws.[6]

Second, under the *monitoring hypothesis*, after SoP law adoption, shareholders and directors will more closely monitor the quality of financial reporting. Because of the role of FRQ in assessing the reasonableness of executive compensation (Sloan 1993; Cornett et al. 2008; Core 2020) and in shaping executive compensation contracting (Baber, Kang, and Kumar 1998; Cheng and Farber 2008; Dechow, Ge, and Schrand 2010), shareholders and directors closely monitor the financial reporting process (Chung, Firth, and Kim 2002; Klein 2002; Masulis and Mobbs 2023). SoP laws give shareholders an additional channel to opine on executive pay (Mangen and Magnan 2012). Consequently, they may have stronger incentives to monitor FRQ after SoP law adoption because FRQ is important in their voting decisions.[7] With

[5] Managers are likely to engage in income-increasing and income-decreasing earnings management to maximize their wealth. For example, Gaver et al. (1995) find that managers select income-increasing discretionary accruals when earnings before discretionary accruals fall below the lower bound of bonus plans, but Holthausen, Larcker, and Sloan (1995) find that managers manipulate earnings downward when they are at the upper bound of bonus plans. In addition, because executive compensation can be sticky (Anderson, Banker, and Janakiraman 2003; Chen, Lu, and Sougiannis 2012) and all accruals must eventually reverse (Allen, Larson, and Sloan 2013), managers have incentives to engage in income-decreasing activities to secure high compensation in the future without fully sacrificing their current period compensation. Thus, we do not distinguish between the two types of earnings management. We thank one anonymous reviewer for pointing out this issue.

[6] It is possible that, if FRQ decreases, firms will put more weight on nonfinancial measures, which are not affected by accounting earnings, and less weight on financial measures (e.g., accounting earnings and stock returns). However, evidence documented in prior studies suggests the opposite: the sensitivity of pay to financial measures increases following SoP law adoption (Ferri and Maber 2013; Correa and Lel 2016), suggesting that firms do not substitute nonfinancial measures for financial measures in determining executive compensation after adopting SoP laws.

[7] Investors may use accounting information in their investment decisions and thus monitor FRQ even before the adoption of SoP laws. However, Paul (1992) demonstrates that valuing firms and evaluating managers are distinct activities. In addition, Kothari, Ramanna, and Skinner (2010) argue that the primary objective of financial reporting is to provide information useful for performance evaluation and stewardship. To the extent that the valuation and stewardship roles of accounting earnings do not always overlap (Paul 1992), shareholders are likely to have stronger incentives to monitor firms' financial reporting when they are empowered to vote on executive pay. We thank an anonymous reviewer for this helpful discussion.

more influence on executive pay in the post-SoP period, shareholders can vote against firms' compensation policies when FRQ is low (Kimbro and Xu 2016). To the extent that shareholder SoP disapprovals reflect directors' inability to oversee executive pay, resulting in negative publicity for directors (Mangen and Magnan 2012), directors who have career concerns have strong incentives to avoid shareholder disapproval by closely monitoring FRQ (Alissa 2015; Kimbro and Xu 2016). Similarly, reputation and career concerns are likely to deter executives from committing to earnings management (Francis, Huang, Rajgopal, and Zang 2008), especially when monitoring efforts from shareholders, the board, the media, and regulators increase after the adoption of SoP laws. These arguments together suggest a positive relation between FRQ and SoP laws.

Given the above arguments, it is unclear *ex ante* which of the above mechanisms dominates. Additionally, it is possible that SoP laws do not affect FRQ. First, some economies adopt nonbinding SoP laws, which may have little effect on the executive compensation process (Conyon and Sadler 2010; Brunarski et al. 2015). Consequently, we expect no significant change in executives' incentives to manipulate earnings or in directors' and investors' incentives to monitor FRQ after SoP law adoption. That is, the assumptions under the opportunism and monitoring hypothesis might not hold. Second, contracting theory suggests that the association between executive compensation and earnings depends on the ability of earnings to reflect managerial effort (Bushman, Engel, and Smith 2006). Hence, after the adoption of SoP laws, firms can increase the weight on performance measures other than accounting earnings (e.g., nonfinancial measures) if FRQ deteriorates. This undermines the benefits executives can obtain from opportunistic behavior.[8] This suggests that the opportunism arguments might not hold. Third, the efficient contracting hypothesis (Watts and Zimmerman 1986) suggests that managers' attempts to increase their own compensation by manipulating accounting numbers may align shareholder interests because the stock price and the likelihood of circumventing debt covenants increase with better accounting performance. Additionally, earnings management may not result in excessive pay because rational shareholders can adjust executive pay downward (Watts and Zimmerman 1986) or use adjusted earnings in performance evaluation (Curtis, Li, and Patrick 2021). Hence, shareholders have no incentives to monitor executives' earnings management behavior. Taken together, these arguments suggest that SoP law adoption could have an insignificant impact on FRQ.

Given these competing views, the effect of SoP law adoption on FRQ is an empirical question. We therefore propose our hypothesis in the null form:

H1: There is no relation between SoP law adoption and FRQ.

III. MODEL SPECIFICATION

To empirically test our hypothesis, we follow DeFond, Hung, S. Li, and Y. Li (2015); Li and Yang (2016); and Chen, Goyal, and Zolotoy (2022) and estimate the following regression model:

$$FRQ_{it} = a_0 + a_1 POSTSOP_{it} + a_2 Controls_{it} + Economy, Industry, \text{ and } Year\ Fixed\ effects + error\ term_{it}, \quad (1)$$

where i and t denote firms and fiscal years, respectively; FRQ is measured by two widely used proxies that capture the extent to which managers opportunistically manipulate earnings using accruals: discretionary accruals and income smoothing (Leuz et al. 2003; Gopalan and Jayaraman 2012; J. Kim, Y. Kim, and Zhou 2017; Lel 2019).[9]

Following DeFond and Subramanyam (1998); Kothari, Leone, and Wasley (2005); and Kim, Park, and Wier (2012), we compute firm i's discretionary accruals (DA) based on the residuals from a regression model estimated annually by industry as follows: $TA_{it} / A_{it-1} = \beta_0(1 / A_{it-1}) + \beta(_1\Delta REV_{it} - \Delta REC_{it}) / A_{it-1} + \beta_2 PPE_{it} / A_{it-1} + \beta_3 EBXI_{it} / A_{it-1} + \varepsilon_{it}$. See Appendix A for detailed variable definitions. The residual term ε is the discretionary accruals (DA) for firm i in fiscal year t. We use the absolute value of discretionary accruals (ABS_DA) as our first proxy for FRQ, with a higher value of ABS_DA indicating a higher level of earnings manipulation and lower FRQ.[10]

Our second measure of FRQ is earnings smoothing behavior, $SMOOTH$. We use a firm-level income smoothness measure ($SMOOTH$) following Cahan et al. (2008) and Baik, Choi, and Farber (2020). The measure is calculated as the

[8] We thank one anonymous reviewer for this helpful argument.

[9] As discussed in footnote 5, managers are likely to engage in income-increasing and income-decreasing earnings management to maximize their wealth (Gaver et al. 1995; Holthausen et al. 1995). Thus, in testing our hypothesis, we operationalize FRQ using the absolute values of abnormal accruals and income smoothing.

[10] Prior research also uses the absolute value of discretionary accruals to examine the association between FRQ and compensation (e.g., Choi, Gipper, and Malik 2023).

TABLE 1

Sample Selection Procedures

	Number of Firm-Year Obs.	Number of Unique Firms
Firms covered in Compustat Global, 2000–2019	840,784	73,518
Less:		
Observations in economies whose SoP law adoption status is unknown	(113,666)	(9,293)
Observations in the financial sector	(159,256)	(15,213)
Observations with negative book value or total assets	(42,782)	(1,805)
Observations with fiscal year-end price less than U.S. $1	(155,191)	(4,623)
Observations with missing data	(167,651)	(17,513)
Test sample	**202,238**	**25,071**
Contains:		
Observations with $SOP = 1$ (SoP economies)	90,061	11,665
Observations with $SOP = 0$ (non-SoP economies)	112,177	13,406

Spearman correlation between the change in total accruals (item IB − item OANCF) and the change in operating cash flows (item OANCF), both scaled by lagged total assets, A_{it-1}, over fiscal years $t-4$ through t. We multiply *SMOOTH* by −1 for easier interpretation. Similar to *ABS_DA*, a higher value of *SMOOTH* indicates a higher level of earnings manipulation through accruals and thus lower FRQ.

Our variable of interest, *POSTSOP*, is an indicator variable that equals 1 if firm *i* is headquartered in an economy after it adopts SoP laws and 0 otherwise. Note that our sample economies adopt SoP laws in a staggered manner. The variable thus captures changes in FRQ after an economy adopts SoP laws.[11] A significant and positive (negative) coefficient on *POSTSOP* would suggest that FRQ decreases (increases) after the adoption of SoP laws. Following Haw, Hu, Hwang, and Wu (2004) and Kim et al. (2017), we include a number of firm-level controls that are deemed to affect FRQ: firm size (*SIZE*), cash flows from operations (*CFO*), leverage (*LEV*), financial performance (*LAG_ROA*), financial distress probability (*LAG_LOSS*), and growth potential (*MB*, *S_GROWTH*, and *P_GROWTH*). We also control for audit quality (*BIG4*) because Big 4 auditors provide high-quality financial reporting assurance services and constrain earnings management (Becker, DeFond, Jiambalvo, and Subramanyam 1998). Our empirical model also includes economy, industry, and year fixed effects to control for economy-, industry-, and year-specific effects on FRQ not captured by the control variables. Robust standard errors are clustered at the firm level.[12] To reduce the impact of extreme values, we Winsorize all continuous variables at the bottom and top 1 percent levels.

IV. SAMPLE AND DATA

To examine whether SoP law adoption affects FRQ on a worldwide basis, we construct a sample of firms headquartered across 34 economies, comprising SoP adopters and nonadopters, from 2000 through 2019. We initially obtain 840,784 firm-year observations from Compustat Global. We then exclude observations (1) in economies whose SoP law adoption status is unknown (n = 113,666), (2) from the financial sector (n = 159,256), (3) with negative book value or total assets (n = 42,782), (4) with stock prices lower than U.S. $1 at fiscal year-end (n = 155,191), and (5) without sufficient data to calculate our variables (n = 167,651). As shown in Table 1, our final sample consists of 202,238 firm-year observations, corresponding to 25,071 unique firms.[13] Among these, 90,061 firm-year observations (11,665 firms) are from economies that adopted SoP laws during the sample period (i.e., SoP economies) and 112,177 firm-year observations (13,406 firms) are from economies that did not adopt SoP laws during the sample period (i.e., non-SoP economies). Because of the staggered adoption of SoP laws, our control sample includes firm-year observations from SoP law-adopting economies in the prelaw adoption period and observations from non-SoP economies.

[11] We do not include the indicator variable for the SoP economies (*SOP*), because it is subsumed by the economy fixed effects.
[12] Following Bourveau et al. (2024), in our main analyses, we cluster standard errors by firm. In an additional test (untabulated), we re-estimate Equation (1) with standard errors clustered by economy and year, or by economy, and our inferences hold.
[13] The number of firm-year observations used in our analyses can vary from one test to another because of missing data.

TABLE 2

Sample Distribution

Panel A: *SOP* = 1

Economy	# of Firm-Year Obs.	# of Firms	SoP Law Year	*ABS_DA*	*SMOOTH*
Australia	3,093	576	2005	0.081	0.524
Belgium	203	33	2012	0.068	0.614
Denmark	401	69	2007	0.058	0.565
France	5,785	660	2016	0.053	0.654
Israel	1,639	228	2012	0.060	0.559
Italy	1,016	169	2011	0.042	0.750
Netherlands	371	57	2004	0.069	0.466
Norway	999	169	2008	0.066	0.431
South Africa	608	73	2011	0.054	0.631
Spain	441	67	2014	0.037	0.718
Sweden	2,744	451	2006	0.073	0.576
Switzerland	1,769	153	2013	0.044	0.575
United Kingdom	9,933	1,497	2003	0.065	0.561
United States	61,059	7,463	2011	0.069	0.491
Total	**90,061**	**11,665**		**0.067**	**0.520**

Panel B: *SOP* = 0

Economy	# of Firm-Year Obs.	# of Firms	*ABS_DA*	*SMOOTH*
Bermuda	124	16	0.030	0.367
Canada	6,966	1,152	0.068	0.463
Chile	337	35	0.041	0.713
China	26,657	3,566	0.057	0.758
Finland	571	61	0.051	0.566
Germany	5,914	659	0.061	0.594
Hong Kong	936	185	0.058	0.566
India	7,543	1,522	0.073	0.755
Ireland	100	18	0.075	0.474
Japan	49,407	3,881	0.037	0.714
Jordan	119	30	0.049	0.751
Malaysia	1,284	227	0.049	0.676
New Zealand	162	32	0.042	0.484
Oman	32	9	0.041	0.863
Pakistan	781	106	0.076	0.841
Philippines	175	21	0.041	0.774
Poland	2,233	383	0.073	0.704
Singapore	642	102	0.055	0.658
Taiwan	7,266	1,264	0.061	0.710
Thailand	928	137	0.049	0.676
Total	**112,177**	**13,406**	**0.050**	**0.703**

This table presents sample distribution by economy. The last two columns report the mean values of the two dependent variables (*ABS_DA* and *SMOOTH*) for each economy in the test sample.

The SoP law adoption status of each economy is from Correa and Lel (2016) and Carter et al. (2021). As illustrated in Table 2, the U.K. was the first economy to mandate that public firms adopt SoP legislation (in 2003). The last economy that adopted SoP laws in Carter et al. (2021) was France, which adopted SoP laws in 2016. To observe changes in FRQ, we ensure that both pre and postadoption periods are included for each firm in the SoP law-adopting economies

TABLE 3

Descriptive Statistics

	SOP = 1 (n = 90,061)			*SOP* = 0 (n = 112,177)			Test for Difference	
Variables	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**	**Mean**	**t-stat.**
Dependent Variables								
ABS_DA	0.067	0.046	0.066	0.050	0.033	0.054	0.017***	60.852
SMOOTH	0.520	0.700	0.489	0.703	0.900	0.391	−0.183***	−90.338
Control Variables								
SIZE	5.877	5.766	2.129	5.770	5.649	1.680	0.107***	12.656
CFO	0.061	0.083	0.174	0.070	0.066	0.109	−0.009***	−14.665
LEV	0.551	0.526	0.306	0.521	0.499	0.272	0.030***	23.896
LAG_ROA	0.012	0.055	0.245	0.070	0.059	0.125	−0.058***	−69.171
LAG_LOSS	0.304	0.000	0.460	0.154	0.000	0.361	0.150***	82.533
MB	3.862	2.062	4.784	2.813	1.573	3.648	1.049***	55.920
S_GROWTH	0.172	0.075	0.517	0.134	0.066	0.403	0.038***	18.344
P_GROWTH	0.168	0.076	0.418	0.145	0.067	0.383	0.023***	13.250
BIG4	0.722	1.000	0.448	0.354	0.000	0.478	0.368***	180.000

*** Represents two-tailed significance at the 1 percent level.

This table presents descriptive statistics for the dependent and control variables for the subsample of firms from SoP economies and the subsample of firms from non-SoP economies. The difference in mean values of the dependent and control variables between the two subsamples and corresponding t-statistics are shown in the last two columns.

by requiring data from at least three years before and after SoP law adoption. Therefore, our sample period starts in 2000 (i.e., three years before the U.K. adopted SoP laws) and ends in 2019 (i.e., three years after France adopted SoP laws). Of the 34 economies in our sample, only 14 adopted SoP laws within the sample period.

Table 3 reports descriptive statistics for the SoP economy subsample (*SOP* = 1) and for the non-SoP economy subsample (*SOP* = 0). The last two columns present differences in the mean values of the dependent and control variables across the two subsamples. The mean (median) value of *ABS_DA* is 0.067 (0.046) and 0.050 (0.033) for the SoP economy and non-SoP economy subsample, respectively. The mean (median) values of *SMOOTH* for the SoP economy and non-SoP economy subsample are 0.520 (0.700) and 0.703 (0.900), respectively.[14] With respect to the control variables, Table 3 shows that firm-level characteristics differ significantly between the two subsamples. Compared with firms from the non-SoP economy subsample, firms from the SoP economy subsample tend to be larger, are more highly leveraged, are more likely to be audited by Big 4 auditors, and have smaller cash flows, poorer firm performance, higher loss-making probability, and greater growth potential.

V. REGRESSION RESULTS

Main Results

Table 4 presents the results from estimating Equation (1), which tests for a relation between SoP law adoption and FRQ. Columns (1) and (2) report the results with FRQ proxied by *ABS_DA* and *SMOOTH*, respectively. The coefficient on *POSTSOP* is significant and negative at the 1 percent level in both columns (−0.003 in column (1) and −0.031 in column (2)), rejecting our null hypothesis and supporting the monitoring hypothesis. That is, the adoption of SoP laws improves firm-level FRQ.[15] The coefficients are economically significant as well. For example, in column (1), the coefficient on *POSTSOP* is −0.003, which suggests that, on average, the absolute value of discretionary accruals decreases by 0.3 percent of lagged total assets after the enactment of SoP laws, compared with the control sample (i.e., firm-year observations from SoP economies in the preadoption period and observations from non-SoP economies).

[14] The mean value of *ABS_DA* (*SMOOTH*) for the non-SoP economy subsample is significantly lower (higher) than that for the SoP economy subsample, suggesting that, on average, firms from SoP economies exhibit lower (higher) FRQ than firms from non-SoP economies. Therefore, we find mixed results in our univariate tests using the two measures of FRQ.

[15] The SoP law adoption effect on FRQ is unlikely to be driven by a time-trend effect because our research design includes firms from non-SoP economies as the control group.

TABLE 4

Main Results: SoP Law Adoption and Financial Reporting Quality

Panel A: Baseline Regressions

Dep. Var. =	***ABS_DA***		***SMOOTH***	
Column	**(1)**		**(2)**	
	Coeff.	**t-stat.**	**Coeff.**	**t-stat.**
POSTSOP	**−0.003*****	**−4.339**	**−0.031*****	**−4.656**
SIZE	−0.006***	−47.367	−0.001	−0.797
CFO	−0.004	−1.298	0.013	0.973
LEV	0.021***	25.755	0.099***	14.951
LAG_ROA	−0.006***	−3.373	0.220***	18.382
LAG_LOSS	0.005***	9.648	−0.156***	−36.831
MB	0.001***	23.319	−0.007***	−14.341
S_GROWTH	0.013***	24.520	−0.016***	−4.999
P_GROWTH	0.007***	11.668	−0.016***	−4.455
BIG4	0.001**	2.407	−0.026***	−5.880
Economy FE	Included		Included	
Industry FE	Included		Included	
Year FE	Included		Included	
n	202,238		189,369	
R^2	0.188		0.145	

Panel B: Dynamic Effects

Dep. Var. =	***ABS_DA***		***SMOOTH***	
Column	**(1)**		**(2)**	
	Coeff.	**t-stat.**	**Coeff.**	**t-stat.**
SOP[−2]	**−0.001**	**−0.949**	**−0.007**	**−0.925**
SOP[−1]	**−0.001**	**−0.614**	**0.002**	**0.272**
SOP[0]	**−0.004*****	**−4.124**	**−0.020****	**−2.147**
SOP[1]	**−0.004*****	**−3.860**	**−0.028*****	**−2.987**
SOP[2+]	**−0.003*****	**−3.730**	**−0.035*****	**−4.259**
Control variables	Included		Included	
Economy FE	Included		Included	
Industry FE	Included		Included	
Year FE	Included		Included	
n	202,238		189,369	
R^2	0.188		0.145	

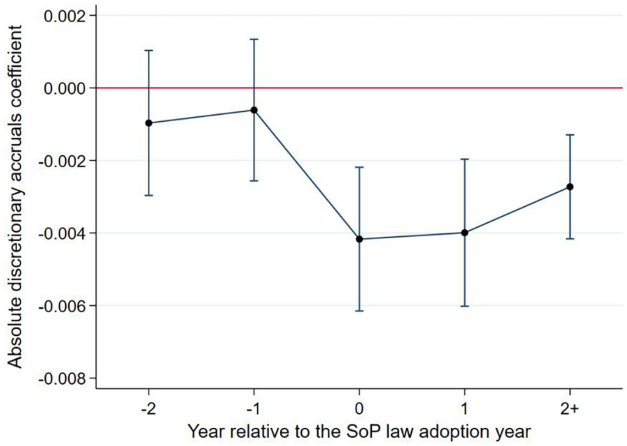
, * Represent two-tailed significance at the 5 percent and 1 percent levels, respectively.
This table presents the impact of SoP law adoption on FRQ. Panel A (B) shows estimates from the baseline regressions (dynamic effects). Results for control variables (Panel B), the intercept, economy, industry, and year fixed effects are not reported for brevity. t-statistics are based on robust standard errors clustered at the firm level. n denotes the number of firm-year observations.
See Appendix A for variable definitions.

We compare this effect with that of firm size (*SIZE*) on absolute abnormal accruals. A one-standard-deviation increase in *SIZE* leads to a decrease in absolute abnormal accruals equal to 1.14 percent (-0.006×1.9) of lagged total assets. Thus, the coefficient of *POSTSOP* in column (1) is economically meaningful.[16]

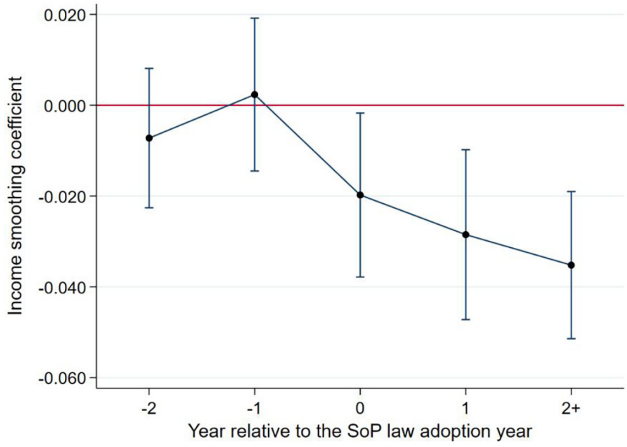
[16] Regarding the control variables, smaller firms (*SIZE*), firms with higher leverage (*LEV*), poorer financial performance (*LAG_ROA*), higher loss-making probability (*LAG_LOSS*), and greater growth potential (*MB*, *S_GROWTH*, and *P_GROWTH*), as well as firms audited by Big 4 auditors (*BIG4*) exhibit more accruals earnings management. Meanwhile, more highly leveraged firms (*LEV*), firms reporting better financial performance (*LAG_ROA*), firms with lower loss-making probability (*LAG_LOSS*), firms with less growth potential (*MB*, *S_GROWTH*, and *P_GROWTH*), and firms audited by non-Big 4 auditors (*BIG4*) are associated with more income-smoothing behavior.

FIGURE 1
Dynamic Effects of SoP Law Adoption on Financial Reporting Quality

Panel A: Dynamic Effects of SoP Law Adoption on *ABS_DA*



Panel B: Dynamic Effects of SoP Law Adoption on *SMOOTH*



Panel A (B) plots the coefficients on the five time indicator variables (*SOP[−2]*, *SOP[−1]*, *SOP[0]*, *SOP[1]*, and *SOP[2+]*) and their 95 percent two-sided confidence intervals when the dependent variable is *ABS_DA* (*SMOOTH*).
See Appendix A for variable definitions.
(The full-color version is available online.)

To mitigate endogeneity concerns and to examine the timing of the effect of SoP law adoption on FRQ, we replace *POSTSOP* in Equation (1) with five time indicators representing the years surrounding SoP law adoption: *SOP[−2]*, *SOP[−1]*, *SOP[0]*, *SOP[1]*, and *SOP[2+]*. For example, *SOP[−1]* equals 1 for the fiscal year immediately preceding SoP law adoption in the economy in which the firm is headquartered and 0 otherwise. Table 4, Panel B reports the results. The coefficients on *SOP[−2]* and *SOP[−1]* are insignificant, whereas the coefficients of *SOP[0]*, *SOP[1]*, and *SOP[2+]* are significant and negative.[17]

Figure 1 plots the coefficients on the five time indicators and their 95 percent two-sided confidence intervals. As shown in Figure 1, Panel A (Panel B), the coefficient estimates are close to 0 in the pre-SoP law period and decrease significantly in the post-SoP law period when the dependent variable is *ABS_DA* (*SMOOTH*). Overall, the results in Table 4, Panel B and Figure 1 show there is no discernible difference in FRQ between firms from SoP economies and those from non-SoP economies before SoP law adoption and, importantly, that FRQ increases significantly after SoP law adoption.

[17] The results for the controls in Table 4, Panel B, and Tables 5, 6, and 8 are untabulated for brevity. They are available from the authors upon request.

Cross-Sectional Analyses

To corroborate our baseline results and deepen our understanding of how SoP laws affect FRQ, we conduct two sets of cross-sectional analyses. FRQ is affected by several factors related to demand for FRQ and supply of proper monitoring mechanisms (Wang 2006). Because SoP laws affect the demand for FRQ and their effectiveness hinges on the supply of relevant monitoring mechanisms, we examine these two moderating factors separately. More specifically, we examine the moderating role of a firm's information environment from the demand side and that of corporate governance mechanisms from the supply side.

The Demand for FRQ: The Role of Information Asymmetry

We investigate whether board monitoring efforts, captured by FRQ in our setting, vary with firm information asymmetry. Compared with transparent firms, firms with higher information asymmetry are more likely to receive shareholder SoP disapprovals (Kimbro and Xu 2016). In response, executives are more likely to refrain from manipulating earnings to mitigate career concerns. These arguments suggest that the relation between SoP law adoption and FRQ should be more pronounced in more opaque firms.

We use two measures of information asymmetry. The first is an economy-level measure, based on whether a particular non-U.S. economy adopts International Financial Reporting Standards (IFRS). Prior studies document that IFRS adoption improves the information environment (Byard, Li, and Yu 2011; Horton, G. Serafeim, and I. Serafeim 2013) and accounting quality (Barth, Landsman, and Lang 2008; Kim and Shi 2012). We form an indicator variable, *IFRS*, that equals 1 if a firm is from an economy that has adopted IFRS in fiscal year t and 0 otherwise. Given the stringent disclosure mandates and high litigation exposure in the U.S. (La Porta, Lopez-de-Silanes, and Shleifer 2006), FRQ is potentially higher in the U.S. than in other economies (Leuz et al. 2003; Francis and Wang 2008). We partition our sample into two subsamples: a subsample with low information asymmetry (composed of U.S. firms and firms from non-U.S. economies with *IFRS* equal to 1) and a subsample with high information asymmetry (composed of firms from non-U.S. economies with *IFRS* equal to 0). We then re-estimate Equation (1) separately for the two subsamples. The results are reported in Table 5, Panel A, with columns (1)–(2) (columns (3)–(4)) reporting the results when the dependent variable is *ABS_DA* (*SMOOTH*). We find that the coefficient on *POSTSOP* remains significant and negative for the subsample with high information asymmetry, regardless of which dependent variable is used. In contrast, the coefficient on *POSTSOP* is insignificant for the subsample with low information asymmetry. In addition, the difference in the coefficients on *POSTSOP* across the two subsamples is significant at the 5 percent and 10 percent levels when the dependent variable is *ABS_DA* and *SMOOTH*, respectively.

Our second measure of information asymmetry (*OPACITY*) is at the firm level. Following Maffett (2012), we compute *OPACITY* as the average of the following four components in the pre-SoP law adoption period: 1 minus the percentile rank of *ANALYST*, 1 minus the percentile rank of *ACCURACY*, the percentile rank of *DISP*, and 1 minus the percentile rank of *BIG4*, where *ANALYST* is the number of unique analysts forecasting a firm's annual earnings; *ACCURACY* is the absolute value of the analyst forecast error multiplied by −1, scaled by the stock price at the end of fiscal year $t-1$, and forecast error is actual earnings less the consensus analyst forecast of earnings in fiscal year t; *DISP* is the standard deviation of analyst forecasts of a firm's annual earnings in fiscal year t, scaled by the stock price at the end of fiscal year $t-1$; and *BIG4* is an indicator variable that equals 1 if a firm is audited by a Big 4 audit firm in fiscal year t and 0 otherwise. Higher values of *OPACITY* indicate higher information asymmetry. We partition firms that are to be subject to SoP laws into two subgroups based on the median value of firm-level financial opacity (*OPACITY*) in the pre-SoP law period.

Table 5, Panel B reports the results with *ABS_DA* (*SMOOTH*) as the dependent variable in columns (1) and (2) (columns (3) and (4)).[18] In columns (1) and (3), the coefficient on *POSTSOP* is negative and significant at the 1 percent level for the subsample of firms with higher firm-level financial opacity. In contrast, the coefficient on *POSTSOP* is negative or insignificant for the subsample of firms with lower financial opacity when the dependent variable is *ABS_DA* and *SMOOTH*, respectively. Importantly, the difference in the coefficients on *POSTSOP* between the two subsamples is significant at the 5 percent level for the *ABS_DA* model and for the *SMOOTH* model.

In summary, the results shown in Table 5 support our expectation that boards of directors allocate more monitoring effort to more opaque firms. As a result, the improvement in FRQ induced by SoP law adoption is more pronounced for more opaque firms.

[18] The sum of the number of observations in the two subsamples exceeds that of our baseline regressions (Table 4, Panel A) because firms from the non-SoP economies serve as control groups in both subsamples.

TABLE 5

Cross-Sectional Test: Role of Information Asymmetry

Panel A: Sample Partition Based on Economy-Level Information Asymmetry

Dep. Var. =	***ABS_DA***		***SMOOTH***	
Column	**(1)**	**(2)**	**(3)**	**(4)**
	High Asymmetry	***Low Asymmetry***	***High Asymmetry***	***Low Asymmetry***
POSTSOP	**−0.004****	**−0.001**	**−0.034***	**−0.011**
	(−2.320)	**(−0.739)**	**(−1.755)**	**(−1.415)**
Test for difference	**(1) − (2)**	**−0.003****	**(3) − (4)**	**−0.023***
		(p = 0.046)		**(p = 0.083)**
Control variables	Included	Included	Included	Included
Economy FE	Included	Included	Included	Included
Industry FE	Included	Included	Included	Included
Year FE	Included	Included	Included	Included
n	77,245	124,993	68,832	120,537
R^2	0.211	0.165	0.107	0.148

Panel B: Sample Partition Based on Firm-Level Financial Opacity (*OPACITY*) in the Pre-SoP Period

Dep. Var. =	***ABS_DA***		***SMOOTH***	
Column	**(1)**	**(2)**	**(3)**	**(4)**
	High OPACITY	***Low OPACITY***	***High OPACITY***	***Low OPACITY***
POSTSOP	**−0.005*****	**−0.003*****	**−0.036*****	**−0.014**
	(−5.888)	**(−3.774)**	**(−3.685)**	**(−1.295)**
Test for difference	**(1) − (2)**	**−0.002****	**(3) − (4)**	**−0.022****
		(p = 0.015)		**(p = 0.033)**
Control variables	Included	Included	Included	Included
Economy FE	Included	Included	Included	Included
Industry FE	Included	Included	Included	Included
Year FE	Included	Included	Included	Included
n	143,723	136,854	133,836	127,572
R^2	0.184	0.173	0.136	0.131

*, **, *** Represent two-tailed significance at the 10 percent, 5 percent, and 1 percent levels, respectively.
This table presents estimates of changes in FRQ around the passage of SoP laws, conditional on the economy-level (Panel A) and firm-level (Panel B) measure of information asymmetry. In Panel A, we divide the sample into two subsamples: the subsample with low information asymmetry (comprising U.S. firms and firms from economies that require public firms to adhere to IFRS in a given year) and the subsample with high information asymmetry (comprising firms from non-U.S. economies that do not adopt IFRS). In Panel B, we divide firms that are to be subject to SoP laws into two subgroups based on the median value of firm-level financial opacity (*OPACITY*) in the pre-SoP law period. Results for control variables, intercept, economy, industry, and year fixed effects are not reported for brevity. t-statistics, shown in parentheses, are based on robust standard errors clustered at the firm level. n denotes the number of firm-year observations.
See Appendix A for variable definitions.

The Supply of Monitoring: The Role of Corporate Governance Mechanisms

Because corporate governance mechanisms enable effective monitoring of FRQ, our second set of cross-sectional tests examines whether and how other corporate governance mechanisms moderate the relation between SoP laws and FRQ. Firms are typically regulated by various governance mechanisms at the economy and firm levels. At the economy level, the degree of investor protection varies internationally. DeFond and Hung (2004) contend that investor protection regimes can make CEOs accountable for their performance. Therefore, in economies with stronger investor protection regimes, shareholders are more likely to be empowered to discipline underperforming CEOs. Potential collusion between the board and management is likely to be curbed if investors are well protected (La Porta, Lopez-de-Silanes, Shleifer, and Vishny 1997). In our SoP setting, we expect that SoP laws and shareholder voting decisions are likely to be more

stringently enforced if investors are better protected. Regarding firm-level governance mechanisms, prior studies show that effective governance mechanisms, such as institutional investors, independent board directors, financial analysts, and credit rating agencies, can deter managers from engaging in earnings management (Klein 2002; Yu 2008; Gul and Goodwin 2010; Zhong, Chourou, and Ni 2017). If poor FRQ is detected or suspected by these governance bodies, shareholders are more likely to vote against executive compensation policies (Kimbro and Xu 2016), pressuring directors to more closely scrutinize the financial reporting process (Alissa 2015). Therefore, the board of directors is likely to exercise more monitoring effort when firm governance mechanisms are stronger.

Empirically, we partition the sample based on the effectiveness of firm corporate governance mechanisms. We measure corporate governance mechanisms at the economy and firm levels. At the economy level, we use the degree of investor protection to partition the sample. Following La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1998); La Porta et al. (2006); Spamann (2010); and Fung, Zhou, and Zhu (2016), we construct an aggregate measure of investor protection (*PROTECT*) by summing the instances in which an economy's (1) legal origin is English common law, (2) antidirector rights index is higher than the sample median, (3) disclosure requirement index is above the sample median, (4) liability standard index is above the sample median, and (5) public enforcement of securities laws index is greater than the sample median. Thus, *PROTECT* ranges from 0 through 5, with higher values indicating higher investor protection. Next, we divide the sample into two subsamples based on the median value of *PROTECT* and re-estimate Equation (1) for each subsample. The results are reported in Table 6, Panel A.

Consistent with our expectation, we find a significantly negative coefficient on *POSTSOP* for firms domiciled in economies with a strong investor protection regime (i.e., the high *PROTECT* subsample), as indicated in columns (1) and (3). For the low *PROTECT* subsample, the coefficient on *POSTSOP* is insignificant in columns (2) and (4). In addition, the difference in the coefficient on *POSTSOP* between the two subsamples is significant at the 1 percent and 5 percent levels for the *ABS_DA* and *SMOOTH* models, respectively. This finding supports the view that the effect of SoP law adoption on FRQ is more pronounced for firms in economies with stronger investor protection regimes.

At the firm level, we focus on institutional ownership as a corporate governance mechanism. Institutional investors are usually viewed as active monitors and are likely to be influential in the SoP voting process (Del Guercio, Seery, and Woidtke 2008). We divide firms that are to be subject to SoP laws into two subgroups based on the median value of firm-level institutional ownership (*INOWN*) in the pre-SoP period and then re-estimate Equation (1) for each subsample.

As shown in Table 6, Panel B, for both measures of FRQ, the coefficient on *POSTSOP* is significant and negative in the high institutional ownership subsample (columns (1) and (3)). For firms with low levels of institutional ownership, the coefficient on *POSTSOP* is significant and negative in column (2) and insignificant in column (4). More importantly, the differences in the coefficients on *POSTSOP* for the *ABS_DA* and *SMOOTH* models are significant at the 1 percent level, consistent with our expectation that the improvement in FRQ after SoP law adoption is more pronounced for firms with higher levels of institutional ownership. This result also supports the argument that the effectiveness of SoP laws is influenced by the sophistication of shareholders (Fama and Jensen 1983; U.S. House of Representatives 2007; Gordon 2009; Larcker et al. 2012; Mangen and Magnan 2012). In short, the results shown in Table 6 suggest that the improvement in FRQ resulting from SoP laws is more salient for firms with strong governance mechanisms that enable effective monitoring.

Additional Test: Impact of SoP Law Adoption on REM

Roychowdhury (2006) and others also suggest that managers can manipulate accounting numbers using REM. Therefore, we test whether SoP laws affect the use of REM. Although the monitoring hypothesis suggests that intensive monitoring accompanied by the passage of SoP laws should deter REM, it is possible that managers respond to SoP laws and switch to REM as a less detectable way of manipulating earnings (Cohen, Dey, and Lys 2008; Zang 2012; Chan, K. Chen, T. Chen, and Yu 2015) or because it does not violate generally accepted accounting principles.

Following Chan et al. (2015) and A. Choi, J. Choi, and Sohn (2018), we use the following model to empirically test the impact of SoP law adoption on REM:

$$REALEM_{it} = a_0 + a_1 POSTSOP_{it} + a_2 Controls_{it} + Economy, Industry, \text{and } Year\ Fixed\ Effects + error\ term_{it}, \quad (2)$$

where *i* and *t* denote firms and years, respectively, and *REALEM* represents proxies for REM. We use four commonly used measures (Roychowdhury 2006; Chan et al. 2015): abnormal cash flow from operations (*ABCASH*), abnormal product cost (*ABPROD*), abnormal discretionary expenses (*ABEXP*), and total amount of REM (*TREM*).

TABLE 6

Cross-Sectional Test: Role of Corporate Governance Mechanisms

Panel A: Sample Partition Based on Economy-Level Investor Protection (*PROTECT*)

Dep. Var. =	***ABS_DA***		***SMOOTH***	
Column	**(1)**	**(2)**	**(3)**	**(4)**
	High PROTECT	***Low PROTECT***	***High PROTECT***	***Low PROTECT***
POSTSOP	**−0.003*****	**0.001**	**−0.030*****	**−0.005**
	(−2.645)	**(0.663)**	**(−2.784)**	**(−0.473)**
Test for difference	**(1) − (2)**	**−0.004*****	**(3) − (4)**	**−0.025****
		(p = 0.003)		**(p = 0.020)**
Control variables	Included	Included	Included	Included
Economy FE	Included	Included	Included	Included
Industry FE	Included	Included	Included	Included
Year FE	Included	Included	Included	Included
n	82,626	90,566	78,603	82,974
R^2	0.183	0.185	0.138	0.101

Panel B: Sample Partition Based on Firm-Level Institutional Ownership (*INOWN*) in the Pre-SoP Period

Dep. Var. =	***ABS_DA***		***SMOOTH***	
Column	**(1)**	**(2)**	**(3)**	**(4)**
	High INOWN	***Low INOWN***	***High INOWN***	***Low INOWN***
POSTSOP	**−0.006*****	**−0.003*****	**−0.048*****	**−0.005**
	(−7.014)	**(−3.822)**	**(−4.879)**	**(−0.606)**
Test for difference	**(1) − (2)**	**−0.003*****	**(3) − (4)**	**−0.043*****
		(p < 0.001)		**(p < 0.001)**
Control variables	Included	Included	Included	Included
Economy FE	Included	Included	Included	Included
Industry FE	Included	Included	Included	Included
Year FE	Included	Included	Included	Included
n	146,061	149,515	136,622	139,218
R^2	0.174	0.190	0.128	0.141

, * Represent two-tailed significance at the 5 percent and 1 percent levels, respectively.
This table presents estimates of changes in FRQ around the passage of SoP laws, conditional on the economy-level (Panel A) and firm-level (Panel B) measure of corporate governance mechanisms. In Panel A, we divide the sample into two subsamples based on the median value of economy-level investor protection index (*PROTECT*). In Panel B, we divide firms that are to be subject to SoP laws into two subgroups based on the median value of firm-level institutional ownership (*INOWN*) in the pre-SoP law period. Results for control variables, intercept, economy, industry, and year fixed effects are not reported for brevity. t-statistics, shown in parentheses, are based on robust standard errors clustered at the firm level. n denotes the number of firm-year observations.
See Appendix A for variable definitions.

See Appendix A for variable definitions. We multiply *ABCASH* and *ABEXP* by −1 for easier interpretation. *TREM* is the sum of *ABCASH*, *ABPROD*, and *ABEXP*. Higher values of *ABCASH*, *ABPROD*, *ABEXP*, and *TREM* indicate greater REM. If REM decreases (increases) after SoP law adoption, the coefficient on *POSTSOP* will be significant and negative (positive). We follow Choi et al. (2018) in including control variables that are found to be correlated with REM: firm size (*SIZE*), leverage (*LEV*), financial performance (*LAG_ROA*), loss probability (*LAG_LOSS*), growth potential (*MB*, *S_GROWTH*), Big 4 auditors (*BIG4*), and accrual earnings management proxies (*ABS_DA*). We also include economy, industry, and year fixed effects. Robust standard errors are clustered at the firm level. All continuous variables are Winsorized at the bottom and top 1 percent.

Table 7 presents the empirical results. The coefficients on *POSTSOP* are significant and negative when the dependent variable is *ABCASH* or *ABPROD*, suggesting that firms engage in less REM (e.g., price discounting and overproduction) after SoP law adoption. In column (3), when the dependent variable is *ABEXP*, the coefficient on *POSTSOP* is

TABLE 7

Impact of SoP Law Adoption on Real Earnings Management

Dep. Var. = Column	*ABCASH* (1)	*ABPROD* (2)	*ABEXP* (3)	*TREM* (4)
POSTSOP	**−0.015*****	**−0.010*****	**0.002**	**−0.023*****
	(−9.404)	**(−3.516)**	**(0.956)**	**(−4.759)**
SIZE	−0.006***	0.006***	0.019***	0.020***
	(−14.895)	(8.112)	(27.651)	(14.698)
LEV	0.048***	0.084***	−0.004	0.129***
	(23.880)	(23.964)	(−1.354)	(19.828)
LAG_ROA	−0.264***	−0.236***	0.097***	−0.332***
	(−61.604)	(−34.606)	(16.761)	(−30.359)
LAG_LOSS	−0.007***	−0.023***	−0.009***	−0.030***
	(−6.517)	(−12.668)	(−5.658)	(−9.148)
MB	−0.001***	−0.004***	−0.004***	−0.009***
	(−4.108)	(−14.073)	(−17.874)	(−19.337)
S_GROWTH	−0.039***	−0.027***	−0.048***	−0.107***
	(−29.569)	(−15.580)	(−30.371)	(−34.314)
BIG4	−0.011***	−0.018***	−0.017***	−0.047***
	(−9.277)	(−7.418)	(−7.529)	(−10.706)
ABS_DA	−0.074***	−0.132***	−0.260***	−0.513***
	(−6.300)	(−8.828)	(−20.869)	(−18.258)
Economy FE	Included	Included	Included	Included
Industry FE	Included	Included	Included	Included
Year FE	Included	Included	Included	Included
n	202,236	199,484	192,343	190,915
R^2	0.199	0.063	0.137	0.088

*** Represents two-tailed significance at the 1 percent levels.
This table presents estimates of the effect of SoP adoption on real earnings management. We follow prior studies (e.g., Roychowdhury 2006) and use four measures of real earnings management: *ABCASH*, *ABPROD*, *ABEXP*, and *TREM*. Results for intercept, economy, industry, and year fixed effects are not reported for brevity. t-statistics, shown in parentheses, are based on robust standard errors clustered at the firm level. n denotes the number of firm-year observations.
See Appendix A for variable definitions.

insignificant, indicating that managers do not change abnormal expenses after SoP law adoption. When the dependent variable is the total amount of REM, in column (4), we find a negative and significant coefficient on *POSTSOP*, indicating that firms decrease their total amount of REM after SoP law adoption. The results in Table 7 lend support to our baseline result that the improvement in monitoring induced by SoP law adoption deters managers from engaging in opportunistic earnings management.

Robustness Tests

We conduct a series of tests to check the robustness of our main results. First, we re-estimate Equation (1) using a matched sample of firms from SoP economies and firms from non-SoP economies with similar firm characteristics. Second, we instrument SoP law adoption using economies' political environments to reduce concerns arising because the adoption of SoP laws is endogenously determined. Third, we assess whether our results are robust to using three alternative proxies for FRQ and several alternative samples.

Matched Sample Analysis

In our main empirical specification (i.e., Equation (1)), the indicator variable for years after SoP law adoption (*POSTSOP*) cannot be defined for firms from non-SoP economies. Thus, we are essentially comparing the post-period for firms from SoP economies with the pre-period for these firms while controlling for the time-invariant difference

TABLE 8

Robustness Tests

Panel A: Matched Sample Analysis

Dep. Var. =	***ABS_DA***	***SMOOTH***
Column	**(1)**	**(2)**
POST	0.001	0.014
	(0.554)	(1.461)
POSTSOP	**−0.004*****	**−0.031*****
	(−3.497)	**(−2.736)**
Control variables	Included	Included
Economy FE	Included	Included
Industry FE	Included	Included
Year FE	Included	Included
n	49,661	48,197
R^2	0.174	0.127

(continued on next page)

between firms from SoP economies and firms from non-SoP economies. As an alternative, we adopt a difference-in-differences (DiD) research design using a matched sample of firms with similar characteristics from SoP economies and non-SoP economies. Specifically, for a time window of [−3, +3] years around the SoP law adoption, we match each firm from a SoP economy (treatment firm) with a firm from a non-SoP economy (control firm) from the same industry in the same year (i.e., the year prior to the adoption of SoP laws), with the same quintile ranking of FRQ, and the closest firm size (based on total assets). To calculate the quintile ranking, we sort firms within each industry-year by each FRQ measure into quintiles (i.e., the reverse of *ABS_DA* and of *SMOOTH*). Then, we take the average of the two quintile rankings as the measure of overall FRQ, rounded to the nearest integer to facilitate matching.

The above procedure yields 49,661 firm-years from 4,300 matched pairs of treatment and control firms for years [−3, +3] around the SoP law adoption year. In the matched sample, each control firm is assigned a pseudo-adoption year (using the actual SoP law adoption year of the corresponding treatment firm). We then add *POST* to Equation (1) and estimate the new model using this matched sample, where *POST* is an indicator variable that takes a value of 1 for years following the adoption of SoP laws in the economy (for control firms, after the pseudo-adoption year). Note that the *POSTSOP* variable is essentially the interaction term between *TREAT* (the indicator for treated units) and *POST* (the indicator for years in the post-period) in a typical DiD design. We do not include the indicator variable for the treatment group (i.e., the SoP economies), because it would be subsumed by the economy fixed effects.

Table 8, Panel A presents the results. The coefficients on *POST* are insignificant in columns (1) and (2), suggesting that there are no time trends common to the treatment and control firms. More importantly, the coefficients on our variable of interest, *POSTSOP*, remain negative and significant at the 1 percent level (−0.004, t-statistic of −3.497; −0.031, t-statistic of −2.736), suggesting that our main results are unlikely to be driven by differences in firm characteristics between firms from SoP economies and those from non-SoP economies.

Instrumental Variable Test

To help mitigate the concern that the passage of SoP laws is endogenously determined (i.e., that some omitted factors are related to both FRQ and the passage of SoP laws), we follow Correa and Lel (2016) and use proxies for the political environment as instrumental variables. We use the following political environment variables, obtained from the World Bank Database of Political Institutions 2012 (Beck, Clarke, Groff, Keefer, and Walsh 2001)[19]: *LEFTLEAN* is an indicator variable that equals 1 if the economy's party orientation regarding economic policy is left leaning in the economy-year and 0 otherwise, *RIGHTLEAN* is an indicator variable that equals 1 if the economy's party orientation regarding economic policy is right leaning in the economy-year and 0 otherwise, *OPPOVOTE* represents the largest opposition party's voting share in the economy-year, and *MAJ* represents the margin of majority in the economy-year.

[19] The sample ends in 2012 because data on these political environment variables are available only until 2012.

TABLE 8 (continued)

Panel B: Instrumental Variable Test

Dep. Var. =	**Probability of Adopting SoP Laws**	***ABS_DA***	***SMOOTH***
	1st Stage	**2nd Stage**	
Column	**(1)**	**(2)**	**(3)**
P_ADOPTION		**−0.013*****	**−0.089****
		(−2.766)	**(−2.154)**
LEFTLEAN	**0.161*****		
	(24.214)		
RIGHTLEAN	**0.065*****		
	(9.144)		
OPPOVOTE	**−0.000**		
	(−1.290)		
MAJ	**−0.954*****		
	(−33.602)		
Control variables	Included	Included	Included
Economy FE	Included	Included	Included
Industry FE	Included	Included	Included
Year FE	Included	Included	Included
n	92,284	92,284	82,467
R^2	0.754	0.188	0.147
First-stage F-stat.	414.83 ($p < 0.01$)		

Panel C: Alternative Measures of Financial Reporting Quality

Dep. Var. =	***ABS_DA2***	***SMOOTH2***	***SPROFIT***
Column	**(1)**	**(2)**	**(3)**
POSTSOP	**−0.004*****	**−0.056*****	**−0.163*****
	(−5.699)	**(−6.085)**	**[−3.274]**
Control variables	Included	Included	Included
Economy FE	Included	Included	Included
Industry FE	Included	Included	Included
Year FE	Included	Included	Included
n	199,498	159,484	202,238
R^2	0.240	0.036	0.145

(continued on next page)

Our first-stage regressions are presented in column (1) of Table 8, Panel B, where we regress the passage of SoP laws on the four instrumental variables. The results show that economies with a left or right party orientation and economies with a lower margin of majority are more likely to adopt SoP laws. The first-stage partial F-statistic is 414.83, which is above the critical values for testing weak instruments (Stock, Wright, and Yogo 2002; Stock and Yogo 2005). Columns (2) and (3) report the results from the second-stage analyses that regresses *ABS_DA* and *SMOOTH* on the predicted value of SoP law adoption, denoted *P_ADOPTION*, from the first-stage regression. The results show that the adoption of SoP laws is associated with significantly higher FRQ in the *ABS_DA* and *SMOOTH* models, suggesting that our main results are not driven by the endogenous nature of SoP law adoption.

Alternative Measures of FRQ

In this section, we employ three alternative measures of FRQ (*ABS_DA2*, *SMOOTH2*, and *SPROFIT*) to ensure that our main results are not unduly influenced by potential measurement error. *ABS_DA2* represents the absolute value of abnormal accruals (*DA2*), where abnormal accruals are calculated following Francis and Wang (2008). A higher value of *ABS_DA2* indicates lower FRQ. *SMOOTH2* represents the Spearman correlation between the change in

TABLE 8 (continued)

Panel D: Alternative Samples

Dep. Var. =	*ABS_DA*	*SMOOTH*	*ABS_DA*	*SMOOTH*	*ABS_DA*	*SMOOTH*	*ABS_DA*	*SMOOTH*
	U.S. Only		*Non-U.S.*		*Non-Japan*		*Non-China*	
Column	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
POSTSOP	**−0.005****	**−0.052****	**−0.002****	**−0.028*****	**−0.002****	**−0.029*****	**−0.002*****	**−0.032*****
	(−2.016)	**(−2.103)**	**(−2.171)**	**(−2.665)**	**(−2.250)**	**(−4.141)**	**(−3.739)**	**(−4.702)**
Control variables	Included	Included	Included	Included	Included	Included	Included	Included
Economy FE	Included	Included	Included	Included	Included	Included	Included	Included
Industry FE	Included	Included	Included	Included	Included	Included	Included	Included
Year FE	Included	Included	Included	Included	Included	Included	Included	Included
n	61,059	58,627	141,179	130,742	152,831	145,189	175,581	163,827
R^2	0.197	0.127	0.172	0.114	0.159	0.147	0.202	0.140

, * Represent two-tailed significance at the 5 percent and 1 percent levels, respectively.
This table presents results of the robustness tests. In Panel A, we use a traditional DiD design using a matched sample. We follow Correa and Lel (2016) and use four proxies for political environment (*LEFTLEAN*, *RIGHTLEAN*, *OPPOVOTE*, and *MAJ*) as instrument variables (Panel B) for the passage of SoP laws. Column (1) of Panel B presents the results from a first-stage regression of the passage of SoP laws on the instrumental variables using the linear probability model. The last row of column (1) reports the first-stage partial F-statistic and the p-value. Columns (2) and (3) present the results from second-stage regressions of *ABS_DA* and *SMOOTH* on the predicted value of SoP law adoption, denoted *P_ADOPTION*, from the first-stage regression in column (1). We also re-estimate Equation (1) using three alternative measures of FRQ (Panel C) and several alternative samples (Panel D). We use the logistic regression method to estimate the *SPROFIT* model. Results for control variables, intercept, economy, industry, and year fixed effects are not reported for brevity. t-statistics [Z-statistics], shown in parentheses [brackets], are based on robust standard errors clustered at the firm level. n denotes the number of firm-year observations.
See Appendix A for variable definitions.

discretionary accruals (*DA2*) and the change in premanaged income, both scaled by lagged total assets, over fiscal years $t-4$ through t, where premanaged income is net income less discretionary accruals (*DA2*). We multiply *SMOOTH2* by −1 for easier interpretation. A higher value of *SMOOTH2* indicates lower FRQ. *SPROFIT* is an indicator variable that equals 1 if the value of earnings before extraordinary items scaled by the lagged market value of equity is between 0.00 and 0.01 (inclusive) and 0 otherwise.

We then re-estimate Equation (1) using the three variables as dependent variables. Because *SPROFIT* is an indicator variable, we use logistic regression to estimate the *SPROFIT* model. Table 8, Panel C shows that the coefficients on *POSTSOP* all remain negative and significant at the 1 percent level, suggesting that our main findings are robust to alternative methods of measuring FRQ.

Alternative Samples

Finally, we examine whether our results hold in alternative samples. Because the U.S. comprises the largest number of observations in the sample and makes up a majority of the SoP economy sample, we first estimate Equation (1) for U.S. firms and for non-U.S. firms separately. We report the results in columns (1)–(4) of Table 8, Panel D.

Results show that the coefficients on *POSTSOP* are significant and negative across the two proxies for FRQ for both U.S. and non-U.S. firms, indicating that our findings are not isolated to U.S. firms. To further examine whether our findings are driven by a single economy, we also remove each economy, one at a time, and find similar results. For brevity, we report only results for regressions where we remove (one at a time) the other two economies with the largest number of observations (i.e., Japan and China). As shown in columns (5)–(8) of Panel D, our inferences remain unchanged.

VI. CONCLUSION

Many economies have enacted SoP laws to curb excessive executive pay and hold executives accountable for firm performance. However, empirical studies examining whether SoP laws have achieved the desired goals yield inconclusive results. Motivated by prior findings that SoP laws increase PPS (Correa and Lel 2016) and that accounting quality influences the effectiveness of executive pay (Sloan 1993; Core 2020), we inform the debate by examining the impact of SoP

law adoption on firm FRQ. Using a large sample of firms, composed of SoP adopters and non-SoP adopters, from 34 economies from 2000 through 2019, we document a significant improvement in FRQ (using both accruals and REM) after the adoption of SoP laws. In addition, our cross-sectional analyses show that the improvement in FRQ from SoP laws is more pronounced for firms with (1) high demand for FRQ or (2) stronger corporate governance mechanisms that enable more effective monitoring. Taken together, our findings consistently support the monitoring hypothesis and suggest that improved FRQ is an unintended benefit of SoP laws.

This paper contributes to the debate surrounding SoP laws by documenting a benefit in the form of increased FRQ. Additionally, the results provide further support for the findings of Correa and Lel (2016) by showing that increased PPS following SoP law adoption is less likely to be driven by earnings manipulation, suggesting that SoP laws promote efficient contracting. Our study also contributes to the investor protection and earnings quality literatures because it documents an association between corporate governance legislation and FRQ. By showing that participation by shareholders in the contracting process following SoP law adoption affects financial reporting behavior, our study adds to the literature on executive contracting. Finally, our study provides findings with practical implications that should be informative to lawmakers considering SoP law adoption around the world.

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APPENDIX A

Variable Definitions

Variable	Definition
Dependent Variables and Variables of Interest	
ABS_DA	Absolute value of discretionary accruals (*DA*), where *DA* is computed as the residual from the modified Jones model (e.g., Kothari et al. 2005): $TA_{it} / A_{it-1} = \beta_0(1 / A_{it-1}) + \beta_1(\Delta REV_{it} - \Delta REC_{it}) / A_{it-1} + \beta_2 PPE_{it} / A_{it-1} + \beta_3 EBXI_{it} / A_{it-1} + \varepsilon_{it}$, where *TA* is total accruals for firm *i* in fiscal year *t* and is computed as earnings before extraordinary items less operating cash flows; A_{it-1} is lagged total assets; ΔREV is change in net revenues; ΔREC is change in net receivables; *PPE* is gross property, plant and equipment; and *EBXI* is earnings before extraordinary items. The model is estimated for each economy-industry-year group with at least ten observations to obtain the parameter estimates β_0, β_1, β_2, and β_3, where industries are defined according to their two-digit SIC codes. A higher value of *ABS_DA* indicates lower FRQ.
SMOOTH	Income-smoothing measure, calculated as the Spearman correlation between the change in total accruals (item IB − item OANCF) and the change in operating cash flows (item OANCF), both scaled by lagged total assets, A_{it-1}, over fiscal years $t-4$ through t. We multiply *SMOOTH* by −1 for easier interpretation. A higher value of *SMOOTH* indicates lower FRQ.
SOP	Economy-level indicator variable that takes a value of 1 if an economy adopted say-on-pay (SoP) laws during the sample period (SoP economy) and 0 otherwise (non-SoP economy).
POSTSOP	Indicator variable that equals 1 if firm *i* is headquartered in an economy after it adopts SoP laws and 0 otherwise.
Control Variables	
SIZE	Natural logarithm of total assets in U.S. dollars in fiscal year $t-1$.
CFO	Cash flows from operations in fiscal year *t*, scaled by lagged total assets.
LEV	Total liabilities in fiscal year *t*, scaled by lagged total assets.
LAG_ROA	Pretax income in fiscal year $t-1$, scaled by total assets in fiscal year $t-2$.
LAG_LOSS	Indicator variable that equals 1 if a firm's net income in fiscal year $t-1$ is below 0 and 0 otherwise.
MB	Market value of equity scaled by book value of equity at the end of fiscal year *t*.
S_GROWTH	Growth in sales from fiscal year $t-1$ to fiscal year *t*.
P_GROWTH	Growth in gross property, plant, and equipment (PPE) from fiscal year $t-1$ to fiscal year *t*.
BIG4	Indicator variable that equals 1 if a firm is audited by a Big 4 audit firm and 0 otherwise.
Variables in the Test of Dynamic Effects	
SOP[−2]	Indicator variable that equals 1 for the fiscal year two years prior to SoP law adoption in the economy in which the firm is headquartered and 0 otherwise.
SOP[−1]	Indicator variable that equals 1 for the fiscal year immediately preceding SoP law adoption in the economy in which the firm is headquartered and 0 otherwise.
SOP[0]	Indicator variable that equals 1 for the fiscal year in which SoP laws are adopted in the economy in which the firm is headquartered and 0 otherwise.
SOP[1]	Indicator variable that equals 1 for the fiscal year immediately following SoP law adoption in the economy in which the firm is headquartered and 0 otherwise.
SOP[2+]	Indicator variable that equals 1 for the fiscal year two or more years after SoP law adoption in the economy in which the firm is headquartered and 0 otherwise.
Variables in the Cross-Sectional Tests	
IFRS	Economy-level indicator variable that equals 1 if the economy requires public firms to adhere to International Financial Reporting Standards (IFRS) in an economy-year and 0 otherwise.
OPACITY	Firm-level measure of financial reporting opacity in the pre-SoP law adoption period, computed as the average of the following four components (Maffett 2012): 1 minus the percentile rank of *ANALYST*, 1 minus the percentile rank of *ACCURACY*, the percentile rank of *DISP*, and 1 minus the percentile rank of *BIG4*, where *ANALYST* is the number of unique analysts forecasting the firm's annual earnings; *ACCURACY* is the absolute value of the forecast error multiplied by −1, scaled by the stock price at the end of fiscal year $t-1$, and forecast error is actual earnings less consensus analyst forecasts of earnings in fiscal year *t*; *DISP* is the standard deviation of analysts' forecasts of a firm's annual earnings in fiscal year *t*, scaled by the stock price at the end of fiscal year $t-1$; and *BIG4* is an indicator variable that equals 1 if a firm is audited by a Big 4 audit firm in fiscal year *t* and 0 otherwise.

(continued on next page)

APPENDIX A (continued)

Variable	Definition
PROTECT	Economy-level investor protection index ranging from 0 to 5, constructed by summing the instances in which an economy's (1) legal origin is based on English common law (La Porta et al. 1998), (2) antidirector rights index is higher than the sample median (Spamann 2010), (3) disclosure requirement index is above the sample median (La Porta et al. 2006), (4) liability standard index is higher than the sample median (La Porta et al. 2006), and (5) public enforcement of securities laws index is above the sample median (La Porta et al. 2006). A higher value of *PROTECT* indicates higher investor protection.
INOWN	Percentage of a firm's shares owned by institutional investors in fiscal year *t* in the pre-SoP laws period.
Variables in the Real Earnings Management Test	
ABCASH	Abnormal cash flow from operations, calculated as deviation from the predicted values of the corresponding regression defined by Roychowdhury (2006), where the regression is estimated for each economy-industry-year group with at least ten observations. We multiply *ABCASH* by −1 for easier interpretation. A higher value of *ABCASH* indicates greater real transactions management.
ABPROD	Abnormal product cost, calculated as deviation from the predicted values of the corresponding regression defined by Roychowdhury (2006), where the regression is estimated for each economy-industry-year group with at least ten observations. A higher value of *ABPROD* indicates greater real transactions management.
ABEXP	Abnormal discretionary expenses, measured as deviation from the predicted values of the corresponding regression defined by Roychowdhury (2006), where the regression is estimated for each economy-industry-year group with at least ten observations. We multiply *ABEXP* by −1 for easier interpretation. A higher value of *ABEXP* indicates greater real transactions management.
TREM	Proxy for total amount of REM, calculated as the sum of *ABCASH*, *ABPROD*, and *ABEXP*. A higher value of *TREM* indicates more REM.
Variables in the Sensitivity Tests	
POST	Indicator variable that takes a value of 1 for years following the adoption of SoP laws in the economy. In the matched sample analysis, each matched control firm is assigned an artificial SoP law adoption year (based on the adoption year of the corresponding treatment firm).
LEFTLEAN	Economy-level indicator variable that equals 1 if the economy's party orientation regarding economic policy is left leaning in the economy-year and 0 otherwise.
RIGHTLEAN	Economy-level indicator variable that equals 1 if the economy's party orientation regarding economic policy is right leaning in the economy-year and 0 otherwise.
OPPOVOTE	Largest opposition party's voting share in the economy-year.
MAJ	Margin of majority in the economy-year.
P_ADOPTION	Predicted value of SoP law adoption, estimated from the first-stage regression in column (1) of Table 8, Panel B.
ABS_DA2	Absolute value of abnormal accruals (*DA2*), where abnormal accruals are calculated following Francis and Wang (2008). A higher value of *ABS_DA2* indicates lower FRQ.
SMOOTH2	Spearman correlation between the change in discretionary accruals (*DA2*) and the change in premanaged income, both scaled by lagged total assets, over fiscal years *t*−4 through *t*, where premanaged income is net income less discretionary accruals (*DA2*). We multiply *SMOOTH2* by −1 for easier interpretation. A higher value of *SMOOTH2* indicates lower FRQ.
SPROFIT	Indicator variable that equals 1 if the value of earnings before extraordinary items scaled by the lagged market value of equity is between 0.00 and 0.01 (inclusive) and 0 otherwise.