Taxes and director independence: evidence from board reforms worldwide

Qingyuan Li[1] · Edward L. Maydew[2] · Richard H. Willis[3] · Li Xu[4]

Abstract

We examine whether changes to corporate governance resulting from board reforms affect corporate tax behavior. While the connection between corporate governance and tax behavior has been the subject of intense interest in the literature, a lack of exogenous variation in governance has hampered inferences. Our inquiry exploits a set of major board reforms that capture shocks to board reforms for firms in 31 countries. The results indicate that corporate tax avoidance decreases significantly following major board reforms. We find that the influence of board reforms on corporate tax behavior is stronger in firms with relatively higher agency conflicts and more opaque information environments.

Keywords Corporate taxation · Board reform · Director independence · Corporate governance

JEL classification H26 · K42

✉ Richard H. Willis
richard.willis@owen.vanderbilt.edu

Qingyuan Li
qyli@whu.edu.cn

Edward L. Maydew
edward_maydew@unc.edu

Li Xu
li.xu3@wsu.edu

1 School of Economics and Management, Wuhan University, Luojia Hill, Wuchang District 430072, China

2 Kenan-Flagler Business School, University of North Carolina at Chapel Hill, McColl Building—CB 3490, Chapel Hill, NC 27599, USA

3 Owen Graduate School of Management, Vanderbilt University, 401 21st Avenue South, Nashville, TN 37203, USA

4 College of Business, Washington State University, WA 98686 Vancouver, USA

1 Introduction

The connection between corporate governance and tax behavior has been the subject of intense interest in the literature (e.g., Desai and Dharmapala 2006; Desai et al. 2007; Chen et al. 2010; Rego and Wilson 2012; Armstrong et al. 2015; Seidman and Stomberg 2017; Gleason et al. 2020). Despite some empirical advances, research in the area has produced mixed results. The major limiting factor in the literature is that firms' corporate governance practices are endogenously determined. This limitation is well recognized and acknowledged (Hanlon and Heitzman 2010; Armstrong et al. 2015) but is difficult to overcome without exogenous variation in governance. Our inquiry exploits a set of major country-level board reforms from Fauver et al. (2017) that captures shocks to board independence for firms in 31 countries. Our setting's primary appeal versus prior research studying corporate taxation and governance is that major board reforms provide variation in corporate governance that is plausibly exogenous to individual firms. In addition, our cross-country setting provides greater variation in governance than is typically found in single-country settings.

Board reforms, affecting both developed and emerging economies, typically focus on increasing board independence (Faleye et al. 2011), such as the board's role and composition, including appointing independent directors on the board and audit committee and separating the chairman and CEO positions. The major board reforms we study represent shocks to individual firms' governance practices, mitigating endogeneity concerns and providing us with a powerful setting to study the relationship between governance and taxes. We study the direct and indirect effects of major board reforms on firms' tax avoidance. Indirect effects occur through changes in firms' transparency, which research suggests is associated with firms' tax behavior (Balakrishnan et al. 2019).

Regarding direct effects, on the one hand, corporate governance improvements from board reforms may lead to lower tax avoidance. If tax avoidance is an outcome of unresolved agency conflicts, corporate governance changes that better align managerial incentives may lead to lower tax avoidance (Armstrong et al. 2015). Following different argumentation, Desai and Dharmapala (2006) propose that, because board reforms should enable better monitoring, managers may reduce their tax avoidance. On the other hand, board reforms may lead to greater tax avoidance. This rationale is as follows. Without effective monitoring, managers may not avoid taxes as much as shareholders would prefer. For example, the quiet life hypothesis suggests that entrenched managers are more likely to avoid costly effort (Bertrand and Mullainathan 2003; Atanassov 2013). Thus managers may avoid the effort and risk-taking involved in tax planning, as with other risky investment opportunities, to enjoy a quiet life. If board reforms impose more discipline on managers, managers may exert more effort and take more risks, including greater tax planning.

Regarding indirect effects, we posit that board reforms may reduce tax behavior indirectly by promoting greater corporate transparency. Armstrong et al. (2014) find that corporate transparency increases in the proportion of outside board members. Balakrishnan et al. (2019) document a negative relation between transparency and aggressive tax avoidance (see also Kerr 2019). Kim et al. (2011) find a positive

association between corporate tax avoidance and stock price crash risk, where crash risk suggests high opacity through the hoarding and accumulation of bad news. Therefore desirable governance attributes may be negatively associated with tax avoidance. In summary, the literature suggests that board reforms can create different *direct* and *indirect* effects on firms' tax behavior and offers different arguments regarding the relationship between these effects and tax behavior.

To study these relations, we investigate changes in firms' cash effective tax rates following major board reforms. Because board reforms at the country level are staggered over time, we can use a difference-in-differences research design. In a sample of 75,205 firm-year observations from 31 countries experiencing board reforms during 1995 to 2010, we find robust evidence that cash effective tax rates increase significantly following the passage of major board reform laws, consistent with less tax avoidance after reform. This result holds after controlling for other factors that affect cash effective tax rates, such as growth, leverage, size, firm investment, country-specific variables, and various fixed effects. This result also holds if, rather than a difference-in-differences estimation, we employ a changes specification, regressing the change in tax avoidance on the first difference of all continuous independent variables—again consistent with our conclusion that tax avoidance declines as boards become more independent. It also holds using propensity-score matching to generate a matched sample of treatment and control firms in the periods surrounding board reforms. We conduct a battery of tests to rule out potential variation in firms' taxes due to country-level tax law changes.

We hypothesize that the relation between board reforms and cash effective tax rates is attributable to both a direct channel (through enhanced board independence after reforms) and an indirect one because of improved corporate transparency. We conduct a path analysis, which confirms both channels. Similarly, we posit that the post-reform reduction in tax avoidance will be greater, as evidenced by larger cash effective tax rates, for firms with more opaque information environments or cases in which the private benefits of control are greater. Consistent with both direct and indirect channels operating on tax behavior, we find a greater increase in cash effective tax rates for firms with greater agency conflicts and more opaque accounting ex ante. These results are generally consistent with our predictions.

Following Armstrong et al. (2015), we estimate a quantile regression to examine whether the relation between board independence and tax avoidance differs across the tax avoidance distribution. Consistent with Armstrong et al. (2015), we find evidence that the relationship varies across the distribution of the magnitude of tax avoidance. However, direct comparison with Armstrong et al. (2015) is difficult due to differences in the nature of the variation in board independence (a shock caused by board reforms versus a choice by private parties) and the settings (cross-country versus single country).

Our study contributes to the literature on the relation between corporate governance and taxes in the following ways. First, research finds mixed results regarding the effect of corporate governance on tax behavior. However, studies of the relationship between governance and tax behavior have been hampered by endogeneity and typically been limited to U.S. firms (see Hanlon and Heitzman 2010 and Armstrong et al. 2015 for discussions). We bring to bear shocks to board independence caused

by major country-level board reforms, providing variation in corporate governance that is plausibly exogenous at the firm level. Second, our cross-country setting provides greater variation in the variables of interest than is available in a single-country study. It allows us to address questions that the literature has been unable to address, such as specific factors that explain the effectiveness of board reforms in curtailing tax avoidance.

Third, we provide evidence of both direct and indirect channels through which board reforms affect tax avoidance. In this way, we synthesize research that finds a negative relationship between corporate transparency and tax avoidance (Balakrishnan et al. 2019) and research that finds a positive relation between board independence and corporate transparency (Armstrong et al. 2014). Fourth, we extend the literature on board reforms to consider their potential effect on firms' tax behavior. Fauver et al. (2017) show that board reforms lead to increases in firm value but do not consider the possible effects of board reforms on tax avoidance. Last, we believe that our study is the first to provide broad-based empirical evidence that government-imposed board reforms affect firms' tax behavior. This finding is noteworthy for regulators and policymakers, given the current focus on curtailing corporate tax avoidance. Our results suggest that country-level efforts to improve corporate governance have an unintended byproduct of reducing tax avoidance.

This paper proceeds as follows. Section 2 summarizes the relevant literature. Section 3 describes the data, primary empirical measures, and provides descriptive statistics. Section 4 discusses the empirical strategy to test the impact of board reforms on firms' taxes. Section 5 presents the main empirical results. Section 6 presents tests of the underlying mechanisms, and Section 7 provides robustness tests. Section 8 concludes.

2 Prior research

The potential connection between corporate governance and tax behavior has been the subject of intense interest among researchers (e.g., Desai and Dharmapala 2006; Desai et al. 2007; Chen et al. 2010; Rego and Wilson 2012; Armstrong et al. 2015; Seidman and Stomberg 2017; Gleason et al. 2020; Beasley et al. 2021) and practitioners (KPMG 2007; Maclean and Dixon 2015). Studies have found mixed results regarding the link between corporate governance and tax behavior, possibly because of the endogeneity concerns previously described. For example, Desai and Dharmapala (2006) posit that tax avoidance requires complex financial structures and reduced transparency, facilitating managerial diversion in poorly governed firms. Rego and Wilson (2012) report that managers avoid more tax when their equity incentives encourage more risk-taking but find no evidence of other governance mechanisms affecting tax avoidance. Robinson et al. (2012) find a positive relationship between audit committee financial expertise and tax planning but find a negative association in cases of aggressive tax planning. Armstrong et al. (2015) find that the association between corporate governance and tax avoidance is strongest in the tax avoidance distribution tails but is insignificant in the middle of the distribution. They consider tax avoidance as one of many potential risky investment opportunities

and not necessarily indicative of managerial rent extraction. Overall, the studies paint a mixed picture and are generally limited to examining endogenously determined variation in governance practices across firms. We extend the research by using shocks to governance provided by major board reforms to mitigate the endogeneity concerns and provide direct evidence on the effect of board independence on tax avoidance.

Some recent research uses reconstitutions of the Russell Index as an exogenous shock to quasi-indexer institutional ownership and examines its effect on tax behavior, hypothesizing that quasi-indexers may influence tax behavior through their role as external monitors. Khan et al. (2017) and Chen et al. (2019) both document a positive association between increases in quasi-indexer institutional ownership and tax avoidance. Chen et al. (2019) conclude that the tax savings are attributable to quasi-indexers focusing on overall improved performance rather than a specific concentration on tax planning. Khan et al. (2017), on the other hand, find evidence that tax avoidance is associated with executive equity compensation. In contrast, we focus on shocks from board reforms, which affect important aspects of internal corporate governance, such as board independence in a cross-country setting, which provides broader variation in corporate governance practices.

Fauver et al. (2017) use a difference-in-differences design with firm and year fixed effects as well as other controls and find that board reforms increase firm value (measured by Tobin's q). Further tests indicate that board reforms involving board and audit committee independence increase firm value but those involving the separation of the CEO and chairmanship position do not. Fauver et al. (2017) do not consider or examine the effects of board reforms on firms' tax planning, which is our focus.

3 Sample and descriptive statistics

3.1 Sample

We focus on major board reforms, which significantly impact board independence, rather than voluntary governance changes. Almost all major board reforms mandate that firms increase board independence by putting more independent members on the board, the audit committee, or separating the CEO and chairman positions. We obtain data on major board reforms from 1995 through 2010 from Fauver et al. (2017), who identify 41 countries that enacted major corporate governance reforms from 1998 through 2007. We begin with these countries.

Table 1 presents the sample selection process. Since the first (most recent) major board reform occurred in 1998 (2007), our sample begins three years before the first reform, 1995, and ends in 2010, three years after the last reform. We obtain firm-level data from the Thomson Financial WorldScope database. To mitigate the potential influence of extreme observations, we winsorize all continuous variables at the

Table 1 Sample selection

	Number of firm-year observations	Number of countries
Firm-years in Worldscope for countries with board reforms between 1995 and 2010	552,862	41
Less:		
Firm-years with missing annual cash effective tax rates	(394,354)	(0)
firm-years in the financial industry	(20,575)	(0)
firm-years with a missing statutory tax rate	(2798)	(0)
firm-years with missing control variables	(21,021)	(3)
firm-years with no observations in pre- or post-reform periods	(38,840)	(2)
firm-years of countries with less than 50 firm-years	(69)	(5)
Total	75,205	31

first and 99th percentiles each year (Li et al. 2020).[1] We exclude firm-years with missing tax measures and control variables. In addition, we exclude firms in financial industries (with standard industrial classification codes 6000–6999). To implement our difference-in-differences design, we require that all firms in the sample have observations in both the pre-reform and post-reform periods. Countries enter the sample either in 1995 or the first year when the necessary data are available. We then eliminate firms in countries with fewer than 50 observations, yielding a full sample of 75,205 firm-year observations from 31 countries. We estimate that these countries account for close to 90% of total global market capitalization.[2]

3.2 Descriptive statistics

Tables 2 and 3 present descriptive statistics for the countries and firms in our sample, respectively. Variables are defined in Appendix Table 11. For each country, Table 2 reports the board reform year, the year in which the country enters the sample, the number of firm-year observations, the mean statutory tax rate during the sample period, and the mean cash effective tax rate (*Cash ETR*) of firm-years in the sample.[3] The mean statutory tax rate by country (region), *Statutory tax rate*, ranges

[1] Following Leone et al. (2019) and Li et al. (2020), we implement several methods to address influential observations, including truncation based on influence diagnostics and robust regression. The conclusions are unchanged.

[2] The percentage of global market capitalization for each year is calculated by dividing the calendar year-end total market capitalization of listed companies of these 31 countries (in USD) by the total market capitalization of listed companies of all countries listed in the World Bank's World Development Indictors (in USD). The mean percentage of global market capitalization from 1995 to 2010 is reported.

[3] *Cash ETR* is annual cash effective tax rates. We calculate *Cash ETR* as Cash taxes paid (WC04150) divided by pre-tax income before discontinued operations and extraordinary items (WC01401 – WC04054 – WC04225). We constrain these values to fall between 0 and 1. We exclude discontinued operations and extraordinary items in the *Cash ETR* calculation because these items are usually income decreasing and introduce significant volatility into the *Cash ETR* measure. However, recalculating *Cash ETR* to include these items in the denominator calculation does not change our conclusions. Specifically, the coefficient estimate on *Post* in Table 4 is 0.032 with t-statistic 2.04 (two-tailed $p < 0.05$).

Table 2 Descriptive statistics by country (N = 75,205 firm-year observations)

| Country | Reform year | Starting year | N | Statutory tax rate | CASH ETR | Country-level characteristics (Median of all sample years within a country) | | | | | | Key components in the reform and reform type | | | | Reform Type |
						Book tax conformity	Return on assets volatility	Law and order strength	Financial development	GDP per capita	Board independence	Audit comm. Independence	CEO/Chairman	Other	
Australia	2004	1995	2585	31.2%	30.54%	0.223	0.188	1.761	1.120	0.833	1	1	1	1	Comply-or-explain
Brazil	2002	1997	66	33.6%	14.77%	0.151	0.662	0.673	0.746	−0.965	0	0	0	1	Rule-based
Canada	2004	1995	2292	36.0%	24.96%	0.099	0.121	1.783	1.800	0.904	1	1	1	0	Rule-based
Chile	2001	2000	152	16.1%	15.96%	0.833	0.565	1.604	0.808	−0.553	0	1	0	1	Rule-based
China	2001	1997	2113	31.7%	44.68%	0.896	0.835	1.495	1.339	−2.000	1	1	0	1	Rule-based
Czech	2001	1997	75	32.5%	21.71%	0.854	0.498	1.626	0.493	−0.535	0	0	0	1	Rule-based
Denmark	2001	1995	372	30.7%	29.97%	0.547	0.496	1.792	1.295	1.090	1	0	0	1	Comply-or-explain
Finland	2004	1995	233	27.5%	30.10%	0.557	0.600	1.792	0.727	0.927	1	1	1	1	Comply-or-explain
France	2003	1995	167	36.1%	27.30%	0.458	0.650	1.608	1.134	0.798	0	1	0	1	Rule-based
Germany	2002	1995	947	41.2%	32.85%	0.143	0.461	1.646	1.381	0.857	1	1	0	1	Comply-or-explain
Greece	2002	2000	191	31.3%	37.18%	0.755	0.668	1.335	1.092	0.276	1	1	0	0	Rule-based
HK(China)	2005	1997	4222	16.7%	16.03%	0.791	0.333	1.575	1.437	1.094	1	1	1	0	Comply-or-explain
India	2002	1997	2510	35.5%	26.61%	0.434	0.270	1.386	0.575	−2.906	1	1	0	1	Rule-based
Indonesia	2007	1997	742	30.1%	37.79%	0.382	0.458	0.951	0.461	−2.379	1	1	0	0	Rule-based
Italy	2006	1995	176	32.6%	36.99%	0.591	0.831	1.431	1.184	0.671	1	1	0	1	Rule-based
Japan	2002	1995	10,557	40.9%	47.87%	0.617	0.904	1.628	3.070	1.353	0	1	0	0	Rule-based
Malaysia	2001	1997	2381	27.6%	27.79%	0.742	0.619	1.307	1.341	−0.836	1	1	0	0	Comply-or-explain

Table 2 (continued)

| Country | Reform year | Starting year | N | Statutory tax rate | CASH ETR | Country-level characteristics (Median of all sample years within a country) | | | | | Key components in the reform and reform type | | | | Reform Type |
						Book tax conformity	Return on assets volatility	Law and order strength	Financial development	GDP per capita	Board independence	Audit comm. Independence	CEO/Chairman	Other	
Netherlands	2004	1995	353	33.0%	27.66%	0.204	0.513	1.792	1.585	0.883	1	1	1	0	Comply-or-explain
Norway	2005	1995	302	28.0%	19.99%	0.216	0.371	1.792	0.790	1.353	1	1	1	1	Comply-or-explain
Pakistan	2002	1997	157	35.2%	26.71%	0.419	0.227	1.109	0.442	−2.875	0	1	0	0	Comply-or-explain
Peru	2005	1998	56	29.7%	39.87%	0.538	0.080	1.125	0.194	−1.439	0	1	0	0	Comply-or-explain
Philippines	2002	1997	241	32.7%	30.70%	0.559	0.534	0.912	0.535	−2.143	1	1	0	1	Comply-or-explain
Poland	2002	1997	59	23.4%	32.32%	0.701	0.618	1.477	0.476	−0.619	1	0	0	1	Comply-or-explain
Portugal	2001	1996	58	35.4%	22.19%	0.650	0.868	1.633	1.222	0.101	1	1	0	0	Rule-based
Singapore	2003	1997	2295	21.7%	23.87%	0.849	0.611	1.671	0.766	0.985	1	1	0	1	Comply-or-explain
Sweden	2006	1995	677	27.7%	29.72%	0.249	0.387	1.792	1.140	1.098	1	1	1	1	Comply-or-explain
Switzerland	2002	1995	661	23.7%	23.44%	0.465	0.599	1.672	1.716	1.289	0	0	0	1	Comply-or-explain
Thailand	2002	1997	1535	30.0%	23.02%	0.672	0.684	1.176	1.334	−1.492	1	1	0	1	Comply-or-explain
Turkey	2002	2000	144	27.2%	27.24%	0.507	0.272	1.434	0.513	−0.768	1	0	1	1	Comply-or-explain
UK	1998	1995	8875	30.6%	28.71%	0.473	0.385	1.771	1.414	0.914	1	1	1	1	Comply-or-explain

Table 2 (continued)

| Country | Reform year | Starting year | N | Statutory tax rate | CASH ETR | Country-level characteristics (Median of all sample years within a country) | | | | | Key components in the reform and reform type | | | | |
						Book tax conformity	Return on assets volatility	Law and order strength	Financial development	GDP per capita	Board independence	Audit comm. Independence	CEO/Chairman	Other	Reform Type
US	2003	1995	30,011	39.3%	28.63%	0.108	0.158	1.693	2.110	1.261	1	1	0	1	Rule-based
Mean				31.2%	30.80%	0.488	0.492	1.527	1.154	−0.005	1	1	0	1	–

Mean values for variables used in the cross-sectional tests. Variables measured at the country-level are defined in Appendix Table 11. We obtain information on major corporate governance reforms from 1998 through 2007 from Fauver et al. (2017). We restrict the sample to all countries with available stock price and financial data in the Thomson Financial DataStream and WorldScope databases between 1995 and 2010. We exclude firm-year observations associated with the financial services industry. The primary sources for governance reforms are reports from the World Bank, European Corporate Governance Institute, local stock exchange regulators, and prior studies (Kim and Lu 2013). We obtain firm-level descriptive data from the Thomson Financial WorldScope database, which contains firm-level financial data for public firms beginning in 1995. The availability of these data varies by country. There are 31 countries with available data. We winsorize all continuous variables at the first and 99th percentiles in each year to mitigate the potential influence of extreme observations

from 16.1% for Chile to 41.2% in Germany. The mean *Cash ETR* ranges from 14.8% in Brazil to 47.9% in Japan.

Table 2 also contains the median values of the key country-level variables. *Country-level book-tax conformity* captures the degree of book-tax conformity (Atwood et al. 2012). Higher values of *Country-level book-tax conformity* indicate greater agreement between reported earnings and taxable income. Countries with high book-tax conformity represent environments in which tax avoidance is costlier because larger reported differences between book and taxable income may flag possible tax manipulation (e.g., Mills 1998; Desai and Dharmapala 2009). It is also costlier to avoid taxes in such countries because firms will also report lower financial accounting income. *Country-level return on assets volatility* shows considerable variation across countries. *Country-level law and order strength* measures the quality of a country's legal system. It is a survey-based summary assessment of countries' legal system strength and impartiality (La Porta et al. 1997). *Country-level financial development* is measured as domestic credit provided by the banking sector divided by GDP (annual data from the World Bank's World Development Indicators).

Table 2 also presents three important aspects of countries' board reforms obtained from Fauver et al. (2017), specifically whether the board reforms include board independence, audit committee and auditor independence, and whether the chairman and CEO positions are separated. In our sample, 29 out of 31 countries have at least one of the three components in their reforms. We also report how often board reforms include governance reforms not involving the board. In our sample, 23 countries have reforms that include board independence-related components, 26 countries have auditor-related board reforms, nine countries have reforms related to the separation of the chairman and CEO positions, and 21 countries have reforms related to nonboard governance reforms. Fauver et al. (2017) further classify board reforms as either comply-or-explain reforms or rule-based reforms. With comply-or-explain reforms, firms may adopt the recommendations or explain why they did not. In contrast, rule-based reforms require firms to adopt specific governance features. In our sample, 13 countries have rule-based board reforms.

Table 3 provides summary statistics, with univariate statistics in Panel A and bivariate correlations in Panel B. We present the key variables, *Cash ETR*, *Post*, and the control variables. *Post* is an indicator variable that equals 1 starting in the year in which a major board reform takes effect for a given country and 0 otherwise. As reflected in Panel A, the sample firms have a mean (median) *Cash ETR* of 30.8% (26.5%). The median values of return on assets (*Pretax return on assets*) and sales growth (*Sales growth*) are 8.3% and 8.4%, respectively. The median values for *Leverage*, firm leverage, and *R&D*, the expenditure for research and development deflated by assets, are 9.9% and 0.0%, respectively. The correlations in Panel B are generally of the expected sign. *Cash ETR* has a positive and significant Pearson correlation with *Post*.

Table 3 Summary statistics and correlations

Panel A – Summary statistics

	N	10%	25%	Mean	Median	75%	90%	STD
Key variables								
Cash ETR	75,205	0.025	0.121	0.308	0.265	0.402	0.660	0.255
Post	75,205	0.000	0.000	0.590	1.000	1.000	1.000	0.492
Control variables (measured at firm-year level)								
Pretax return on assets	75,205	0.017	0.041	0.111	0.083	0.148	0.235	0.101
Leverage	75,205	0.000	0.005	0.142	0.099	0.233	0.355	0.150
Log(Total assets)	75,205	10.868	12.212	14.289	13.931	16.053	18.414	2.826
Capital intensity	75,205	0.048	0.130	0.316	0.272	0.457	0.672	0.230
Intangible assets	75,205	0.000	0.000	0.094	0.017	0.124	0.317	0.151
Sales growth	75,205	−0.113	−0.002	0.121	0.084	0.206	0.415	0.323
R&D	75,205	0.000	0.000	0.017	0.000	0.013	0.061	0.039
Foreign operations	75,205	0.000	0.000	0.521	1.000	1.000	1.000	0.500
Discretionary accruals	75,205	−0.126	−0.054	0.002	0.000	0.051	0.124	0.122
Control variables (measured at country-year level)								
Country-level book tax conformity	434	0.091	0.253	0.488	0.485	0.717	0.879	0.281
Statutory tax rate	434	0.212	0.280	0.312	0.300	0.350	0.394	0.073

Table 3 (continued)

Control variables (measured at country-year level)

Variable								
Country-level book tax conformity	434	0.091	0.253	0.488	0.485	0.717	0.879	0.281
Statutory tax rate	434	0.212	0.280	0.312	0.300	0.350	0.394	0.073
Country-level return on assets volatility	434	0.121	0.253	0.492	0.505	0.717	0.879	0.273
Country-level law and order strength	434	1.099	1.386	1.527	1.609	1.792	1.792	0.308
Country-level financial development	434	0.472	0.644	1.154	1.086	1.446	1.975	0.622
GDP per capita	434	−2.286	−0.915	−0.005	0.749	1.027	1.282	1.362

Panel B – Pearson (above) and spearman (below) correlations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
(1) Cash ETR	1	0.003	−0.215	−0.052	0.201	−0.075	−0.025	−0.190	−0.038	−0.015	−0.034	0.071	0.175	0.211	0.009	0.194	0.016
(2) Post	0.017	1	−0.085	−0.085	0.155	−0.088	0.084	−0.129	−0.054	0.062	−0.014	0.264	−0.139	0.248	−0.332	0.158	−0.017
(3) Pretax return on assets	−0.160	−0.085	1	−0.192	−0.243	−0.117	0.002	0.314	0.181	−0.010	0.057	−0.181	0.012	−0.236	0.080	−0.122	0.002
(4) Leverage	−0.043	−0.085	−0.219	1	0.115	0.323	0.222	0.056	−0.170	−0.026	0.012	−0.213	0.138	−0.170	0.087	0.009	0.052

Table 3 (continued)

(5) Log(Total assets)	0.167	0.161	−0.251	0.194	1	0.095	−0.099	−0.181	−0.111	0.070	−0.029	0.326	0.140	0.542	−0.303	0.336	−0.142
(6) Capital intensity	−0.051	−0.099	−0.072	0.332	0.122	1	−0.315	0.055	−0.243	−0.174	−0.078	0.039	−0.026	0.013	−0.031	−0.113	−0.130
(7) Intangible assets	0.045	0.151	0.022	0.194	0.017	−0.286	1	0.073	0.037	0.125	−0.042	−0.280	0.092	−0.254	0.133	0.053	0.183
(8) Sales growth	−0.159	−0.094	0.360	0.029	−0.140	0.005	0.057	1	0.094	−0.032	−0.030	−0.107	−0.016	−0.154	0.071	−0.136	−0.022
(9) R&D	0.059	0.008	0.062	−0.140	0.085	−0.212	0.135	−0.013	1	0.202	0.048	−0.193	0.179	−0.097	0.130	0.137	0.163
(10) Foreign operations	−0.014	0.062	0.007	0.013	0.076	−0.145	0.197	−0.025	0.292	1	−0.013	0.029	−0.128	−0.005	0.140	0.013	0.216
(11) Discretionary accruals	−0.033	−0.003	−0.012	0.017	−0.017	−0.072	−0.042	−0.078	0.001	−0.018	1	−0.033	0.016	−0.031	0.016	0.013	0.028
(12) Country-level book tax conformity	0.018	0.314	−0.204	−0.186	0.311	0.052	−0.235	−0.102	−0.103	0.033	−0.027	1	−0.553	0.742	−0.372	−0.167	−0.324
(13) Statutory tax rate	0.237	−0.202	−0.045	0.085	0.221	−0.023	0.066	−0.027	0.289	−0.112	−0.001	−0.410	1	−0.072	0.202	0.600	0.225
(14) Country-level return on assets volatility	0.141	0.275	−0.235	−0.145	0.430	0.039	−0.168	−0.148	0.039	0.022	−0.021	0.759	−0.072	1	−0.286	0.245	−0.157
(15) Country-level law and order strength	0.040	−0.494	0.135	0.121	−0.354	0.011	0.073	0.063	0.052	0.093	0.015	−0.450	0.194	−0.355	1	0.189	0.638

Table 3 (continued)

(16)Country-level financial development	0.181	0.195	−0.124	0.023	0.252	−0.121	0.202	−0.135	0.272	0.009	0.016	−0.225	0.683	0.053	−0.053	1	0.593
(17)GDP per capita	0.122	0.244	−0.087	0.018	0.201	−0.165	0.221	−0.113	0.272	0.073	0.021	−0.206	0.572	0.031	−0.025	0.892	1

Panel A provides descriptive statistics for key variables in the study. The 10th, 25th, 75th, and 90th distribution percentiles are provided along with the mean, median, and standard deviation (STD). Variables are defined in Appendix Table 11. Panel B contains Pearson (Spearman) bivariate correlations above (below) the diagonal. Correlations in bold in Panel B are statistically significant at two-tailed $p \leq 0.05$

Table 4 Board reforms and cash ETR

Dependent Variable = *Cash ETR*						
	(1)		(2)		(3)	
Intercept	0.291	***	−0.249	*	−0.604	***
	(45.017)		(−1.813)		(−4.523)	
Post	0.028	**	0.033	**	0.030	**
	(2.570)		(2.090)		(2.050)	
After insider trading reforms			0.026		0.019	
			(0.954)		(1.130)	
After takeover law reforms			0.068	**	0.071	***
			(2.514)		(2.875)	
After IFRS implementation			−0.036	**	−0.037	**
			(−2.350)		(−2.579)	
Pretax return on assets			−0.40	***	−0.666	***
			(−4.850)		(−5.859)	
Leverage			−0.104	***	−0.074	***
			(−9.515)		(−4.960)	
Log(Total assets)			0.004	***	0.035	***
			(4.358)		(8.510)	
Capital intensity			−0.086	***	−0.089	***
			(−9.325)		(−7.180)	
Intangible assets			0.001		−0.053	***
			(0.051)		(−4.773)	
Sales growth			−0.069	***	−0.051	***
			(−7.134)		(−4.237)	
R&D			−0.195	**	0.315	***
			(−2.230)		(4.874)	
Foreign operations			0.002		0.001	
			(0.259)		(0.141)	
Discretionary accruals			−0.059	***	−0.031	***
			(−7.923)		(−6.064)	
Country-level book tax conformity			−0.005		−0.002	
			(−0.161)		(−0.054)	
Statutory tax rate			0.230		0.284	
			(1.071)		(1.388)	
Country-level return on assets volatility			0.014		0.014	
			(0.358)		(0.363)	
Country-level law and order strength			0.053		0.059	
			(1.400)		(1.518)	
Country-level financial development			0.063	*	0.052	
			(1.977)		(1.556)	
GDP per capita			0.346	***	0.256	***
			(7.584)		(8.193)	
Fixed effects	C, I, Y		C, I, Y		F, Y	

Table 4 (continued)

Dependent Variable = *Cash ETR*			
	(1)	(2)	(3)
Number of Obs.	75,205	75,205	75,205
R-squared (%)	12.5	17.6	30.5

This table reports the ordinary least squares regression results of firms' cash effective tax rates (*Cash ETR*) on an indicator variable reflecting the enactment of major board reforms (*Post*) and control variables. T-statistics are reported in parentheses and calculated using standard errors clustered at the country level. Variables are defined in the appendix. *, **, and *** represent two-tailed significance levels at 10%, 5%, and 1%, respectively. Fixed effects are included as identified: C=country fixed effects; I=industry fixed effects; F=firm fixed effects; Y=year fixed effects. Coefficient estimates (t-statistics) are reported in the top row (parentheses)

4 Empirical strategy

Our main empirical strategy uses a difference-in-differences design to examine the effect of board reforms on tax avoidance. This approach is consistent with the work of Bertrand and Mullainathan (2003) and assumes that firms are unaffected (i.e., control) firms before their countries enact board reforms.[4] Once their countries enact board reforms, these firms enter the treatment group.

We estimate the following pooled panel regression model using ordinary least squares regression. We cluster standard errors by country because board reforms are a country-level outcome.[5]

---

[4] Studies have cautioned the use of a single-coefficient two-way fixed effects difference-in-differences (TWFEDID) specification to summarize time-varying effects when there is treatment effects heterogeneity and variation in treatment timing (Chasemartin and D'Haultfoeuille 2020). Two-way fixed effect difference-in-differences models with differential treatment timing can result in nonconvex weights (Chasemartin and D'Haultfoeuille 2020), with certain treatment effects receiving more weight than others (Goodman-Bacon 2018). In some cases, heterogeneity can cause estimates of the average treatment effects to be negative, even though the individual treatment effects are positive. Chasemartin and D'Haultfoeuille (2020) recommend examining the number of treatments with negative weights and the ratio of negative to positive treatments. A large number of negative weights and high negative-to-positive ratio may indicate that the estimator in the TWFEDID model is a biased estimator of the overall treatment effect. Another diagnostic is to regress the weights on a variable that is associated with the size of the treatment effect. A significant correlation indicates that the estimator in the TWFEDID model is a biased estimator of the overall treatment effect. We calculate the number of treatments that receive a negative weight using Chaisemartin and D'Haultfoeuille's (2020) "*twowayfeweights*" Stata command. The negative weights present in our regressions are minimal (98 out of 5318) and sum to approximately zero weight (−0.0031). The regression coefficient of the weights of the fixed effects is not significant (t-statistic = 1.60). We conclude that the corresponding weights are not correlated with the treatment effects, and our main results are not biased.

[5] Bertrand et al. (2004) demonstrate that clustered robust standard errors exhibit downward-bias that asymptotically resolves as the number of clusters exceeds 50. As a check, we implement two-way clustering of standard errors by firm and country-year. This approach mitigates correlated errors stemming from a reform year in given country. It yields more than 200 clusters, reducing the likelihood that our conclusions are affected by downward bias associated with robust standard errors. The conclusions are unchanged.

$$Cash\ ETR_{i,j,t} = \beta_0 + \beta_1 Post_{j,t} + \beta_k X_{i,j,t} + \varepsilon_{i,j,t}. \tag{1}$$

Subscripts i, j, and t correspond to firm, country, and year, respectively. $Post_{j,t}$ is an indicator variable that equals 1 starting in the year in which a major board reform takes effect in the country and 0 otherwise. $X_{i,j,t}$ represents the control variables, some measured at the firm-year level and some at the country-year level, depending upon data availability. β_k is the vector of coefficient estimates. In later tests, we modify Eq. (1) to examine the relation between board reforms and taxes using: a changes specification, a propensity-score-matched (PSM) sample, and alternative tax avoidance measures.

We include a full set of group effects (i.e., country and industry fixed effects) and time effects (i.e., year fixed effects) because our research design involves multiple treatment groups and periods (Wooldridge 2007; Fauver et al. 2017). The country, industry, and year fixed effects help identify the within-country, within-industry, and within-year change in firm cash effective tax rates between the treatment and control firms. This approach uses firms from countries without reforms at a given time as the benchmark and follows the literature (e.g., Bertrand and Mullainathan 2003; Fauver et al. 2017). In later tests, we replace country and industry fixed effects with firm fixed effects to isolate within-firm variation. Our conclusions are similar, regardless of the combinations of fixed effects.

We control for the possible confounding effects of reforms concurrent with those related to corporate boards. Following Fauver et al. (2017), we consider concurrent reforms related to insider trading, anti-takeover provisions, and IFRS adoption. Thus we also include in Eq. (1) *After Insider trading laws*, an indicator variable that is equal to 1 beginning in the year in which insider-trading laws are enforced in the country and 0 otherwise, *After takeover law reforms*, an indicator variable that is equal to 1 beginning in the year in which mergers and acquisitions (M&A) law enactment becomes effective in the country and 0 otherwise, and *After IFRS implementation*, an indicator variable that is equal to 1 beginning in the year in which IFRS is adopted in the country and 0 otherwise. The regression models also control for other time-varying, firm-level, and country-level variables from prior research examining cash effective tax rates.

Motivating our firm-level control variables, profitable firms (*Pretax return on assets*) and firms with greater leverage or complex financing arrangements (*Leverage*) may have different incentives and opportunities to avoid taxes (Amiram et al. 2019).[6] The natural log of total assets (*Log(Total assets)*) controls for the influence of firm size. Some research suggests that larger firms are more sophisticated at tax planning but also may face higher political costs (Zimmerman 1983). Thus we make no sign prediction for *Log(Total assets)*. We also include *Capital intensity* and

---

[6] When tax avoidance is measured using *Cash ETR* (as in our paper), most of the studies find a negative association between tax avoidance and ROA (see Donohoe 2015 [Table 4, p. 14]; Cen et al. 2017 [Table 4, p. 385]; Chen et al. 2010 [Table 4, p. 52]; Rego and Wilson 2012 [Table 4, p. 759]; McGuire, Wang and Wilson 2014 [Table 3, p. 1502]. However, when tax avoidance is measured using other measures, such as GAAP ETR or BTD, most studies find a positive association between tax avoidance and ROA.

Intangible assets to control for potential tax effects attributable to investment activity (Koester et al. 2017). We include sales growth (*Sales growth*) because research (e.g., Atwood et al. 2012) finds that revenue growth is positively associated with tax avoidance. We include research and development, *R&D*, because research suggests that income attributable to intangible assets is relatively easy to shift to low-tax jurisdictions (Dyreng and Lindsey 2009). We include *Foreign operations*, an indicator variable equal to 1 for companies with nonmissing and nonzero foreign income, to capture the presence of operations in foreign jurisdictions. Studies find mixed evidence on the relationship between foreign income and tax avoidance. Rego (2003) finds that firms with extensive foreign operations have greater ETRs, while Wilson (2009) and Cazier et al. (2009) find that firms with greater foreign income are more likely to participate in a tax shelter and report greater uncertain tax benefits, respectively. Therefore we do not predict the direction of the association between *Foreign operations* and *Cash ETR*. We control for financial reporting aggressiveness and include the performance-adjusted discretionary accruals variable, *Discretionary accruals*, using the Kothari et al. (2005) discretionary accrual model (Amiram et al. 2019). Frank et al. (2009) suggest financial reporting aggressiveness and tax aggressiveness are positively related.

We include several country-level control variables. We include the statutory corporate tax rate, *Statutory tax rate*, to control for any changes in country-level tax rates. We include the level of book-tax conformity in the firm's home country, *Country-level book tax conformity*. Atwood et al. (2012) find that greater book-tax conformity is associated with less tax avoidance, consistent with wide variance in book and taxable income being more likely to attract taxing authorities' attention (see Mills 1998 for discussion). We include *Country-level return on assets volatility*, a measure of earnings volatility, *Country-level law and order strength*, a measure of the strength of the country's judicial system, and two metrics to capture economic development, *Country-level financial development*, a measure of financial development that captures an estimate of domestic credit provided by local financial institutions, and *GDP* per capita, a measure of GDP per capita.

5 Empirical results

5.1 Baseline results

We report the baseline difference-in-differences estimation of Eq. (1) in Table 4. A positive coefficient on *Post* would indicate an increase in firms' *Cash ETR*s after major board reforms, suggesting a decrease in tax avoidance. Column (1) of Table 4 presents the regression model, including the independent variable of interest, *Post*, and country, industry, and year fixed effects. Column (2) expands the set of controls to include firm-year control variables. It includes controls for other country-level reforms during the sample period, changes to statutory tax rates and changes in book-tax conformity, and other country-level controls measured at the country-year

level.[7] Column (3) replaces country and industry fixed effects with the more stringent firm fixed effects. We find that the coefficient on *Post* is significant and positive in all estimations (two-tailed p-values <0.05 for all tests), suggesting that firms avoid taxes less (higher *Cash ETR*) on average after board reforms in their home countries. In terms of economic magnitude, the coefficients on *Post* in Table 4 suggest that major board reforms are associated with average increases in *Cash ETR* of 2.8 to 3.0 percentage points, depending on the specification.

5.2 Propensity-score-matching results

To make the treatment group and the control group more comparable, we also use propensity-score matching to generate a matched sample of treatment and control firms in the periods surrounding board reforms. Using a matched sample reduces the possibility that changes in the tax behavior of firms in the countries adopting a board reform, compared to changes in the tax behavior of firms in countries not adopting a board reform, are due to shocks that are unrelated to the board reform, affect tax behavior, and occur around the same time. We expect any unrelated shocks to have similar effects on the treatment and control firms if the control firms are otherwise similar to the treated firms.

Since staggered board reforms involve different event years, we adopt a cohort matching strategy where an observation may serve as both a "control" and a "treatment" for different treatment events (Fauver et al. 2017). In constructing our cohort matched sample, we first identify treatment firms as firms incorporated in countries that adopted board reforms. For each treatment sample, we identify a seven-year window surrounding the year in which the country of incorporation of the treated firm adopts the board reform. For each treatment firm, we identify control firms as those incorporated in countries that do not adopt board reforms *during the same seven-year window* (Fauver et al. 2017). In other words, we compare firms that receive the treatment to those that do not receive it in the matched window.[8] Thus, for each treatment firm and its control firms, the sample period comprises three years before (pre-reform period) and three years after (post-reform period) the treatment firm's adoption year. Furthermore, we require that each treatment firm and its control firm have at least one observation in both the pre-reform and post-reform periods. To further achieve a close match, we use propensity scores with nearest

---

[7] The coefficient on that statutory tax rate is not significant in Table 4. We suspect that firm, country, year fixed effects absorb most of the variance in statutory tax rate. To check whether statutory tax rate impacts *Cash ETR*, we run the following two tests. First, we exclude all fixed effects from the regressions, and the untabulated results show that, after excluding all fixed effects, the statutory tax rate is significantly positively associated with *Cash ETR*. Second, to further explore the proportion of variance in statutory tax rate are absorbed by the fixed effects, we regress statutory tax rate on fixed effects only. As suspected, the untabulated R-squared results show that firm and year fixed effects can explain 94.5% of the variance for the statutory tax rate, and country and year fixed effects can explain 80.7% of the variance for the statutory tax rate.

[8] For example, the treatment group for 2006 consists of firms incorporated in Italy and Sweden, the sample countries that adopted a board reform in 2006. The control group for 2006 consists of firms from countries that do not adopt board reforms during 2003–2009.

neighbor matching with replacement on pre-reform tax avoidance (*Cash ETR*), pre-tax return on assets, leverage, log(Total assets), capital intensity, intangible assets, sales growth, R&D, foreign operations, discretionary accruals, and exact matching on two-digit WorldScope industry codes in the year before the adoption of board reform by the affected firms' incorporating country. We require one treatment firm to three control firms matching and the propensity-score difference between treatment and control groups to be less than 0.1.

With this cohort matched sample, we estimate the following regression model.

$$Cash\ ETR_{i,j,t} = \beta_0 + \beta_1 Post_t + \beta_2 Treat_j + \beta_3 Post_t * Treat_j + \beta_k X_{i,j,t} + \varepsilon_{i,j,t}. \quad (2)$$

For this analysis, $Post_t$ is an indicator variable equal to 1 for *both* treated and control firms beginning in the year in which a board reform is passed and 0 otherwise. $Treat_j$ is an indicator variable equal to 1 for firms incorporated in a country that adopts a board reform (i.e., treated firms) and 0 otherwise. The variable of interest, $Treat_j \times Post_t$, is an indicator variable equal to 1 for firms incorporated in a country that adopts a board reform in the period when the reform is implementable and 0 otherwise. $Treat_j \times Post_t$ captures the change for the treatment firms, relative to the change for the control firms. To the extent that board reforms result in lower tax avoidance, we expect a positive coefficient on β_3. All other control variables are the same as in the baseline difference-in-differences tests.

The matching yields 9,029 firm-year observations for the treatment group and 21,338 observations for the control group. We present the pre- and post-matching univariate differences of firm characteristics between treatment and control firms in the Internet Appendix Table IA.1. Before matching, treatment firms differ from control firms in most covariates, and the differences are highly significant. After matching, the differences between the treatment firms and control firms are greatly reduced. Using a graphical approach, we examine the distribution of the matching variables for our treatment and control firms. Internet Appendix Figure IA.1. shows that our treatment firms appear similar to the control firms after matching. This suggests that the matching significantly improves the comparability of the treatment firms and control firms.

We then estimate Eq. (2) using the matched treatment-control firms. Table 5, Panel A, presents the regression results. Column (1) includes the variable of interest, *Post*Treat* as well as *Post*, *Treat*, and firm and year fixed effects. Column (2) expands the set of controls to include firm-year control variables, variables reflecting other country-level reforms, and variables controlling for variation in other country-level attributes over time. The coefficients on β_3 are significantly positive (at two-tailed p value <0.05) for both models. Consistent with Table 4, this result suggests an increase in *Cash ETR* (reduced tax avoidance) after board reforms. Thus the results from the matched sample are consistent with the results reported in Table 4.

5.3 First difference model results

In addition to the difference-in-differences model with firm fixed effects, a first difference model also addresses unobserved heterogeneity in panel data. To examine

Table 5 Board reforms and cash ETR: Propensity-score matching and first difference model

Panel A – Regression analysis for propensity-score matching

Dependent Variable = Cash ETR

	(1)		(2)	
Intercept	0.292	***	−0.184	
	(55.671)		(−1.440)	
Post	0.004		0.005	
	(0.476)		(0.559)	
Treat	−0.021	*	−0.013	
	(−1.864)		(−1.112)	
Post*Treat	0.024	**	0.023	**
	(2.046)		(2.253)	
After insider trading reforms			0.016	
			(0.655)	
After takeover law reforms			0.026	
			(1.163)	
After IFRS implementation			−0.030	
			(−1.625)	
Pretax return on assets			−0.923	***
			(−8.734)	
Leverage			−0.028	
			(−1.266)	
Log(Total assets)			0.023	***
			(3.050)	
Capital intensity			−0.014	
			(−0.356)	
Intangible assets			−0.004	
			(−0.178)	
Sales growth			−0.037	***
			(−4.553)	
R&D			0.060	
			(0.301)	
Foreign operations			−0.020	***
			(−3.566)	
Discretionary accruals			−0.021	
			(−1.169)	
Country-level book tax conformity			0.051	
			(1.376)	
Statutory tax rate			0.098	
			(0.652)	
Country-level return on assets volatility			−0.026	
			(−1.002)	
Country-level law and order strength			0.093	
			(1.146)	
Country-level financial development			0.028	

Table 5 (continued)

			(0.722)	
GDP per capita			0.060	
			(0.808)	
Fixed effects	F, Y		F, Y	
Number of Obs.	30,367		30,367	
R-squared (%)	27.0		34.3	
Panel B – First difference model				
Dependent Variable = *ΔCash ETR*	(1)		(2)	
Intercept	0.025	***	0.042	**
	(3.059)		(2.206)	
Post1	0.042	*	0.042	*
	(2.003)		(2.010)	
Post2	0.025		0.019	
	(1.371)		(1.088)	
Post3	0.032	**	0.030	**
	(2.542)		(2.233)	
After insider trading reforms			−0.008	
			(−0.600)	
After takeover law reforms			0.013	
			(1.491)	
After IFRS implementation			−0.014	
			(−1.494)	
ΔPretax return on assets			−0.000	
			(−1.435)	
ΔLeverage			0.110	***
			(6.461)	
ΔLog(Total assets)			−0.040	***
			(−3.034)	
ΔCapital intensity			−0.040	
			(−1.212)	
ΔIntangible assets			0.081	*
			(1.677)	
ΔSales growth			0.000	
			(1.272)	
ΔR&D			−0.108	**
			(−2.092)	
Foreign operations			−0.004	**
			(−2.621)	
ΔDiscretionary accruals			−0.031	***
			(−5.431)	
ΔCountry-level book tax conformity			0.013	
			(0.319)	
ΔStatutory tax rate			−0.108	
			(−0.507)	

Table 5 (continued)

ΔCountry-level return on assets volatility		0.057
		(1.606)
ΔCountry-level law and order strength		
		(0.377)
ΔCountry-level financial development		0.056 *
		(1.871)
ΔGDP per capita		−0.074
		(−0.450)
Year Fixed effects	Yes	Yes
Number of Obs.	60,479	60,479
R-squared (%)	0.80	1.20
F joint test: Post1 + Post2 + Post3 = 0	7.24**(p=0.012)	5.76**(p=0.023)

Panel A reports the results for the propensity-score-matching test. For this test, we focus on the seven years surrounding each country adoption event, with three years prior to the adoption year and three years following the adoption year. We require that each firm in the sample has at least one observation in the pre-adoption period and another observation in the post-adoption period. *Treatment* firms indicate firms incorporated in countries that adopted board reforms during the sample period. *Control* firms indicate firms incorporated in countries that did not do so. We use propensity-score matching in which we match treatment firms to control firms with replacement by pre-reform tax avoidance (*Cash ETR*), pre-tax return on assets, leverage, log(Total assets), capital intensity, intangible assets, sales growth, R&D, foreign operations, discretionary accruals, and the two-digit industry classification *in the year prior to* governance reforms. We require one-to-three matching and the propensity-score difference between treatment and control firms to be less than 0.1. Panel B reports the robustness checks using a first-difference model. *Post1* is an indicator variable equal to 1 in the year in which a major board reform becomes effective in the country and 0 otherwise. *Post2* is an indicator variable equal to 1 in the first year after a major board reform in the country becomes effective and 0 otherwise. *Post3* is an indicator variable equal to 1 in the second year and any subsequent years after a major board reform in the country becomes effective and 0 otherwise. T-statistics are reported in parentheses and calculated using standard errors clustered at the country level. Variables are defined in the appendix. *, **, and *** represent two-tailed significance levels at 10%, 5%, and 1%, respectively. Fixed effects are included as identified: F=firm fixed effects; Y=year fixed effects. Coefficient estimates (t-statistics) are reported in the top row (parentheses)

whether our results hold in changes, we re-estimate Eq. (1) by taking the first difference of all continuous variables.

$$\Delta Cash\ ETR_{i,j,t} = \beta_0 + \beta_1 Post1_{j,t} + \beta_2 Post2_{j,t} + \beta_3 Post3_{j,t} + \beta_k \Delta X_{i,j,t} + \varepsilon_{i,j,t}, \quad (3)$$

where *Post1* is an indicator variable equal to 1 in the year in which a major board reform becomes effective in the country and 0 otherwise, *Post2* is an indicator variable equal to 1 in the first year after a major board reform in the country becomes effective and 0 otherwise, and *Post3* is an indicator variable equal to 1 in the second year and any subsequent years after a major board reform in the country becomes effective and 0 otherwise.

In Table 5 Panel B, we present the results with the first difference model specification. Column (1) is the regression model including the variables of interest, *Post1*, *Post2*, and *Post3*, and year fixed effects. Column (2) expands the set of controls to include changes in the control variables from Table 4. Based on Column (2), the

sum of $\beta_1 + \beta_2 + \beta_3$ is significantly positive (two-tailed p value <0.05). Consistent with Table 4, this result suggests that board reforms are associated with an increase in firms' cash effective tax rates (reduced tax avoidance). Overall, the results from the changes model are consistent with the results in Table 4.

6 The channels

Our main results suggest that board reforms are associated with decreases in tax avoidance, as reflected in increased cash effective tax rates. We hypothesize that major board reforms can affect firms' tax behavior directly through increased board independence and indirectly via the impact of board independence on corporate transparency. We investigate these channels using path analysis, and firm-level and country-level cross-sectional tests.

6.1 Path analysis

Figure 1 presents the basic structure of the path analysis, where the path arrows represent the relations. In the first input path diagram, we conjecture that board reforms have a positive direct effect on *Cash ETR* (reduced tax avoidance) from increased board independence. We also show the positive indirect effects of board reforms on *Cash ETR*: board reforms increase corporate transparency (Armstrong et al. 2014), which in turn decreases tax avoidance (increases *Cash ETR)* (Balakrishnan et al. 2019).

The second output path diagram reports the coefficient values from the following system of structural equations.

$$Transparency_{i,j,t} = \alpha_0 + \alpha_1 Post_{j,t} + \alpha_k X_{i,j,t} + \varepsilon_{i,j,t}, \tag{4}$$

$$Cash\ ETR_{i,j,t} = \gamma_0 + \gamma_1 Post_{j,t} + \gamma_2 Transparency_{i,j,t} + \gamma_k X_{i,j,t} + \mu_{i,j,t}. \tag{5}$$

Because corporate transparency is difficult to measure and has many facets, we use a composite index identified by Lang et al. (2012) designed for cross-country research. *Transparency* is an index that equals the mean of the scaled percentile rank of the following five variables: the negative of discretionary accruals, *Big N*, *INTGAAP*, the number of analysts following the firm, and forecast accuracy (Lang et al. 2012). Negative discretionary accruals is the negative of the prior three years' moving sum of the absolute value of discretionary accruals (Hutton et al. 2009). *Big N* is an indicator variable which equals 1 if a firm is audited by a Big N auditing firm and 0 otherwise. *INTGAAP* is an indicator variable that equals 1 if a firm has adopted either IAS/IFRS or U.S. GAAP and 0 otherwise. Analyst following is equal to the average number of analysts making a forecast for year t's earnings during the year t. Forecast accuracy is the negative value of earnings surprise, scaled by the stock price at the end of the prior year, where earnings surprise is the absolute

A The Direct and Indirect Effects of Board Reforms on Cash ETR (Input Diagram)
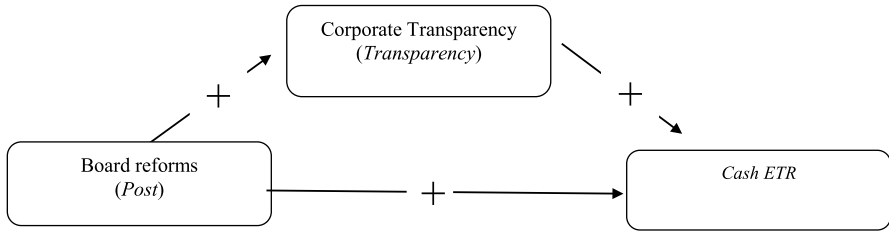
B The Direct and Indirect Effects of Board Reforms on Cash ETR (Output Diagram)
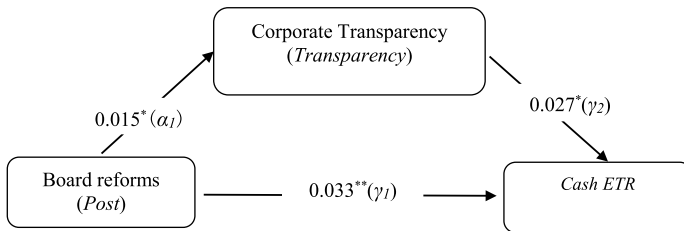

Fig. 1 Path Analysis. This figure describes the direct and indirect paths through which board reforms (as proxied by the board reform indicator– *Post*) affect corporate tax avoidance (increase in annual cash effective tax rate – *Cash ETR* indicates reduction in corporate tax avoidance). Board reforms indirectly affect corporate tax avoidance through the mediated channel of corporate transparency (as proxied by the composite transparency factor; *Transparency*). The path analysis is conducted by estimating a system of the two equations shown below, including control variables. The path coefficient γ_1 is the magnitude of the direct path from board reforms to corporate tax avoidance. The path coefficient α_1 is the magnitude of the path from board reforms to corporate transparency. The path coefficient γ_2 is the magnitude of the path from corporate transparency to tax avoidance. The product of path coefficient $\alpha_1 \times \gamma_2$ measures the magnitude of the indirect path from board reforms to corporate tax avoidance mediated through corporate transparency. ** and * represent 5% and 10% significance levels, respectively, based on two-tailed tests. The path analysis is based on the following system of equations. $Transparency_{i,j,t} = \alpha_0 + \alpha_1 Post_{j,t} + \alpha_k X_{i,j,t} + \varepsilon_{i,j,t}$, $Cash\ ETR_{i,j,t} = \gamma_0 + \gamma_1 Post_{j,t} + \gamma_2 Transparency_{i,j,t} + \gamma_k X_{i,j,t} + \mu_{i,j,t}$.

difference between actual earnings and the mean analyst forecast (Lang et al. 2012).[9] We obtain the standardized regression coefficients of Eq.(4) and Eq.(5)., allowing for straightforward interpretation and comparison since they are expressed in common units (i.e., standard deviations) (Chen et al. 2021).

As indicated in the output diagrams, in terms of the direct effect, the positive path coefficient of 0.033 from *Post* to *Cash ETR* (γ_1 in Eq. (5)) implies that the adoption of board reforms results in a 3.3 percentage point increase in *Cash ETR*. The indirect effect from *Post* to *Cash ETR* via *Transparency* consists of the positive direct effect (path coefficient = 0.015) of board reforms on *Transparency* (information

---

[9] We obtain analyst data from Capital IQ, which provides analyst data for North America since 1999 and for the rest of world since 1996. If analyst data are unavailable, we calculate *Transparency* using the remaining measures.

transparency improves following board reforms) and the positive direct effect (path coefficient = 0.027) from *Transparency* to *Cash ETR* (a more transparent environment results in higher cash effective tax rates). Combining both, we have a positive indirect effect.

6.2 Firm-level cross-sectional tests

We further examine the channels using firm-level cross-sectional tests. We hypothesize that the direct channel will have the most impact on firms with severe agency issues (i.e., high private benefits). The indirect channel will have the most impact for firms with less transparent information environments. Therefore we expect the decrease in tax planning after board reforms to be most pronounced for firms with high private benefits of control and firms with lower transparency.

Consistent with the literature (i.e., Gormley and Matsa 2011; Bourveau et al. 2018; Appel 2019), we create subsamples in our cross-sectional analyses using characteristics measured before board reforms. Specifically, as in our propensity-score matching before, we first identify cohort matched control firms for treatment firms. Each cohort includes treatment firms (from board reform countries during the seven-year window) and control firms (from countries not adopting board reforms during the seven-year window). Each firm in a cohort must have at least one observation in both the pre- and post-reform periods. Within each cohort, we classify firms using the cohort median of the relevant characteristic in the country. We then pool observations above the cohort median of the pre-reform characteristic as one subsample and observations below or equal to the cohort median of the pre-reform characteristic as a subsample for comparison (Bourveau et al. 2018).

Private benefits of control are largest when managers have more control of the firm (Easterbrook and Fischel 1986). We use the percentage of closely held ownership (*Closely held ownership*) by management to capture private benefits of control (Easterbrook and Fischel 1986). As discussed, we hypothesize that the direct channel will have the most impact for firms with severe agency issues (i.e., high private benefits). Thus, all else equal, management's ability to expropriate wealth from minority shareholders will be greater when their control of a firm is higher. We predict the impact of board reforms will be greater in firms with higher closely held ownership. Therefore we expect the decrease in tax avoidance after board reforms will be greater in magnitude for firms with higher closely held ownership. We create an indicator variable, *High closely held ownership,* which is set to 1 if a firm's closely held ownership prior to the major board reforms is above the cohort median and 0 otherwise.

As discussed, we hypothesize that the indirect channel will have the most impact on firms with opaquer information. All else equal, management's ability to expropriate wealth from minority shareholders should be greater when firms are less transparent. Therefore we expect the decrease in tax avoidance after board reforms will be greater in magnitude for higher opacity firms. We create an indicator variable, *High opacity*, set to 1 if a firm's *Transparency* index prior to the major board reforms is *below* the cohort median and 0 otherwise.

In summary, we posit that, in cases of high private benefits of control and opaquer information environments, the post-reform reduction in tax avoidance is greater (higher *Cash ETR)*. We estimate the following model, interacting *Post* with these measures and including the Eq. (1) control variables.

$$Cash\ ETR_{i,j,t} = \beta_0 + \beta_1 Post_{j,t} + \beta_2 High_{j,t} + \beta_3\ Post_{j,t} \times High_{j,t} + \beta_k X_{i,j,t} + \varepsilon_{i,j,t}.$$

(6)

We replace *High* with *High closely held ownership* and *High opacity*, respectively. From Eq. (1), we conclude that *Cash ETR* increases after board reform, consistent with lower tax avoidance. Therefore, in cases of high private benefits of control and an opaquer information environment, we expect that increases in *Cash ETR* will be larger or $\beta_3 > 0$ in Eq. (6).

Table 6 contains the results. Consistent with our predictions, we find that the coefficient estimates associated with the interaction terms are positive and significant: *Post× High closely held ownership* in column (1) (two-tailed P value<0.10) and *Post× High corporate opacity* in column (2) (two-tailed p value <0.01). The evidence indicates that the increase in *Cash ETR* following board reforms is greater when pre-reform private benefits and opacity are high.

6.3 Country-level tests

We also examine country-level differences in investor protection. Strong investor protection mitigates the private benefits of control (Doidge et al. 2009). Therefore we expect that firms in countries with strong investor protection will have smaller increases in cash effective tax rates after board reforms.

To examine the country-level investor protection level, we use an indicator variable (*High law and order*), which takes a value of 1 when the country-level variable *law and order* is above the median and 0 otherwise. We also use an indicator variable (*High anti-self-dealing index*), which reflects the extent to which minority investors are protected from expropriation by corporate insiders. *High anti-self-dealing* takes a value of 1 if the country-level *anti-self-dealing index* developed by Djankov et al. (2008) is greater than the median and 0 otherwise. We then re-estimate Eq. (1) after interacting *Post*, the indicator variable reflecting the post-reform period, with *High law and order* or *High anti-self-dealing index*. We do not include *High law and order* or *High anti-self-dealing* in the regression model on their own because there is no within-country variation in these variables when the model includes firm fixed effects. Table 7 reports the findings. We find that the board reform effect on *Cash ETR* is mitigated in countries with strong investor protections, as indicated by the significantly negative coefficient on *Post* High law and order* and please note that Post*High Anti-self-dealing index in column (2) (two-tailed p value <0.05). These results are consistent with board reforms having less impact on firms' tax planning when they are already subject to strong investor protection.

Table 6 Analyses of the channels through which board reforms affect cash ETR

Dependent Variable = *Cash ETR*	Closely held ownership		Corporate opacity	
	(1)		(2)	
Intercept	−0.620	***	−0.647	***
	(−5.488)		(−4.836)	
Post	0.003		−0.004	
	(0.337)		(−0.377)	
High closely held ownership	−0.010	*		
	(−1.867)			
*High closely held ownership*Post*	0.008	*		
	(1.971)			
High corporate opacity			−0.019	***
			(−4.165)	
*High corporate opacity*Post*			0.021	***
			(5.446)	
Controls	YES		YES	
Fixed effects	C, F, Y		C, F, Y	
Number of Obs.	199,205		171,700	
R-squared (%)	40.2		40.6	

This table reports the regression estimates of the effect of major board reforms on tax avoidance using a cohort-based matching approach within the seven years around major board reforms. The cohort matching approach results in a significantly increased sample size. In column (1), *High closely held ownership* is an indicator variable that is equal to 1 if the firms' percentage of closely held shares before the major board reforms is above cohort median and 0 otherwise, where the firm-level percentage of closely held shares is measured as closely held shares divided by its outstanding shares. In column (2), *High corporate opacity* is an indicator variable that is equal to 1 if corporate transparency (*Transparency*) before the major board reforms is below the cohort median and 0 otherwise, where *Transparency* is an index that equals the mean of the scaled percentile rank of the following five variables: less discretionary smoothing, higher-quality auditors, better accounting standards, more analyst following, and more accurate analyst forecasts following Lang et al. (2012). T-statistics are reported in parentheses and calculated using standard errors clustered at the country level. Variables are defined in the appendix. *, **, and *** represent two-tailed significance levels at 10%, 5%, and 1%, respectively. Fixed effects are included as identified in the body of each panel: C = cohort fixed effects; F = firm fixed effects; Y = year fixed effects. Coefficient estimates (t-statistics) are reported in the top row (parentheses)

7 Robustness and additional tests

7.1 Parallel trends assumption

The key identifying assumption of a difference-in-differences design is the parallel trends assumption, which assumes that both the treatment and control groups would have followed parallel trends in the absence of the exogenous shock (Abadie 2005). We conduct three analyses to examine whether the difference in tax avoidance between the treatment and control firms changes around board reforms or merely is coincident with pretreatment trends. The first test is a graphical analysis, the second test is a placebo test (with two different pseudo reform years), and the final test is a dynamic timing test.

Table 7 Country-level cross-sectional analyses of the effect of major board reforms on cash ETR

Dependent Variable = *Cash ETR*	Law and order		Anti-self-dealing index	
	(1)		(2)	
Intercept	−0.565	***	−0.603	***
	(−4.652)		(−4.791)	
Post	0.061	***	0.066	***
	(2.765)		(3.050)	
*Post*High Law and order*	−0.037	***		
	(−4.294)			
*Post*High Anti-self-dealing index*			−0.039	**
			(−2.571)	
Controls	YES		YES	
Fixed effects	F, Y		F, Y	
Number of Obs.	75,205		75,205	
R-squared (%)	30.6		30.6	

This table reports regression estimates of board reforms' effect on tax avoidance, conditional on the country-level governance. In column (1), *High Law and order* is an indicator variable equal to 1 if the country-level *law and order* is above the median and 0 otherwise. In column (2), *High Anti-self-dealing index* is an indicator variable equal to 1 if the country-level *Anti-self-dealing index* is above the median and 0 otherwise. T-statistics are reported in parentheses and calculated using standard errors clustered at the country level. Variables are defined in the appendix. *, **, and *** represent two-tailed significance levels at 10%, 5%, and 1%, respectively. Fixed effects are included as identified in the body of each panel: F = firm fixed effects; Y = year fixed effects. Coefficient estimates (t-statistics) are reported in the top row (parentheses)

We begin with the graphical analysis. We plot the mean and median *Cash ETRs* of the treatment group and the control group over the pre-reform and post-reform periods. Consistent with prior research (i.e., Gormley and Matsa 2011; Bourveau et al. 2018; Appel 2019), we use the cohort propensity-score-matched sample for this analysis. Figure 1 shows that the treatment and control groups follow similar trends in mean *Cash ETR* (in Panel A) and median *Cash ETR* (in Panel B) during the pre-board reform eras. Figure 2 shows that there is little difference in the trend in *Cash ETR* between control and treated firms until year 0 (the year of the board reforms). This representation suggests that the parallel trends assumption is likely satisfied. The figure also shows that *Cash ETR* increases for treated firms on and after year 0 (the board reform year).

We conduct two placebo tests using pseudo-board-reform years during both the pre- and post-reform periods (Fauver et al. 2017). The first placebo test sets the pseudo-board-reform year as the fifth year before the actual board reform effective year. The second placebo test sets the pseudo-board-reform year as the fifth year after the actual board reform effective year. Table 8, Panel A, Columns 1 and 2, report the results. The results show no evidence that *Cash ETR* changes after pseudo-board-reform years, as demonstrated by the insignificant coefficients on *Post* in both placebo tests.

Last, following Bertrand and Mullainathan (2003) and Fauver et al. (2017), we replace *Post* with indicator variables that capture the timing of the response. The timing indicator variables include (1) *Year 0*, which equals 1 for the year the board reform becomes effective; (2) *Year + 1*, which equals 1 for the first year after the board reform; (3) *Year 2+*, which equals 1 for the second and subsequent years after the board reform; and (4) *Year − 1* and *Year − 2*, which equal 1 for the first (second) year before the board reform and 0 otherwise. Consistent with the parallel trends assumption, the results in Column 3 of Table 8 Panel A show a significant coefficient for *Year + 1* and *Year 2+* and insignificant coefficients for the other years (two-tailed p-values <0.10). In summary, based on these tests, we conclude that the parallel trends assumption of the difference-in-differences research design is valid (Fig. 2).

7.2 Alternative samples

In Table 8, Panel B, we examine the robustness of the results using alternative samples. First, we re-estimate Eq. (1) using a narrower time window, from three years before to three years after each country's board reform year. The seven-year window is chosen given the trade-off between relevance and statistical power. On the one hand, a longer window may include other confounding events unrelated to board reforms that affect the treated and control groups differently. On the other hand, a shorter window contains fewer observations and potentially reduces statistical power. Column (1) presents the results for this subsample period. Consistent with the latter concern, the coefficient estimate associated with *Post* is positive (0.036) but not statistically significant (t-statistic = 1.574). Second, to mitigate the concern that our results are caused mainly by one or two large countries, we exclude the United States and Japan, which account for the most observations (n = 40,568 or 54% of the sample). Column (2) contains those estimation results, for which *Post* remains statistically positive. Third, in column (3), we report the results after excluding U.S. firms only.[10] We study the sample excluding the United States to ensure that our findings are not driven by the contrast between U.S. and U.K. firms (Fauver et al. 2017 conduct a similar robustness check). *Post* remains statistically positive. Fourth, we obtain data on politically connected firms from Faccio (2006) and exclude firms with political connections in Column (4). Again, *Post* remains statistically positive.[11]

---

[10] To assess whether our results may be affected by non-U.S. firms listed on a U.S. exchange that must comply with Sarbanes-Oxley we remove all American Depository Receipt (ADR) firms from the sample and reestimate Eq. (1). We obtain data regarding ADRs listed in the United States from the Center for Research in Security Prices (CRSP) database. Only Level II and Level III ADRs are included in CRSP. Level I ADRs trade over the counter (OTC) on "pink sheets" and are subject to minimal disclosure. The first day an ADR appears in CRSP is used as the listing date. The coefficient estimate on *Post* is statistically negative at two-tailed p < 0.05 (untabulated).

[11] We also adapt the Faccio data to measure political connections at the country level by including all observations. We first rank the firms based on the percentage of top 50 firms connected with a minister or member of Parliament, as identified by Faccio (2006). We define high-connection countries as those ranked at or above the median (i.e., countries for which more than 4% of the firms in the country are politically connected). We then exclude all high-connection countries from the sample; the conclusions are unchanged.

Table 8 Robustness checks

Panel A – Evaluating the parallel trends assumption

Dependent Variable = Cash ETR	Placebo test, pre-reform	Placebo test, post-reform	Reform timing
	(1)	(2)	(3)
Intercept	−0.559 ***	−0.560 ***	−0.579 ***
	(−4.925)	(−5.207)	(−4.366)
Post	0.001	−0.009	
	(0.077)	(−0.634)	
Year − 2			0.004
			(0.573)
Year − 1			−0.011
			(−1.127)
Year 0			0.024
			(1.382)
Year + 1			0.026 **
			(2.090)
Year 2+			0.025 *
			(1.984)
Controls	YES	YES	YES
Fixed effects	F, Y	F, Y	F, Y
Number of Obs.	75,205	75,205	75,205
R-squared (%)	30.5	30.5	30.6

Panel B – Alternative samples

Dependent Variable = Cash ETR	Restricted sample [−3, +3]	Excluding U.S. and Japan	Excluding U.S.	Excluding politically connected firms	Excluding financial crisis period	Excluding country-years with large tax rate changes	Excluding U.S. propensity-score matching
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intercept	−0.776 ***	−0.280 ***	−0.513 ***	−0.600 ***	−0.518 ***	−0.606 ***	0.109
	(−3.304)	(−3.133)	(−4.70)	(−4.550)	(−4.290)	(−4.458)	(1.17)
Post	0.036 *	0.020 *	0.023 *	0.031 *	0.030 *	0.032 **	0.002

Table 8 (continued)

	(1.574)	(1.950)	(1.752)	(1.985)	(1.784)	(2.052)	(0.22)
Treat		***					−0.017
							(−1.43) *
Treat*Post					**		0.023
							(1.857) *
Controls	YES	YES	YES	YES	YES	YES	YES
Fixed effects	F, Y	F, Y	F, Y	F, Y	F, Y	F, Y	F, Y
Number of Obs.	41,501	34,637	45,194	73,816	64,825	74,627	26,197
R-squared (%)	35.9	33.3	36.2	30.5	31.7	30.3	34.4

Panel C – Alternative tax avoidance measures

	(1)	(2)	(3)
Intercept	−0.371	−0.009	−0.779
	(−4.42) ***	(−0.932)	(−5.558) ***
Post	0.006	0.002	0.029
	(1.117)	(2.247) **	(1.905) *
Controls	YES	YES	YES
Fixed effects	F, YF, Y	F, Y	F, Y
Number of Obs.	126,453	103,756	75,205

This table presents a series of robustness tests. Panel A columns 1 and 2 present the results of placebo tests. In column 1, we assign a pseudo-reform year from the pre-reform period. The pseudo reform year is set as the fifth year prior to the actual reform year. The pseudo-reform indicator variable, $Pre\_pseudo\_post_{i,t}$, equals 1 for the pre-pseudo-reform years and 0 otherwise. In column 2, we assign a pseudo-reform year from the post-reform period. The pseudo reform year is set as the fifth year after the actual reform year. The pseudo-reform indicator variable, $Post\_pseudo\_post_{i,t}$, equals 1 for the post-pseudo-reform years and 0 otherwise. Panel A, column 3, reports the timing effects of board reforms by replacing the $Post_{i,t}$ indicator variable in Eq. (2) with four indicator variables. Specifically, we include $Year-2$, $Year-1$, $Year 0$, $Year + 1$, and $Year 2+$. $Year-t$ is an indicator variable that corresponds to t years before board reform for firm i's incorporation country. Panel B reports the robustness using three alternative samples: a restricted [−3, +3] sample that requires a firm to appear in at least one year in the pre-period and one year in the post-period during the [−3, +3] window, excluding the United States, dropping politically connected firms, excluding the financial crisis period from Luc and Valencia (2013), and excluding country-years with large tax rate changes (absolute value changes >5%). Panel C columns 1–3 presents the robustness checks using three alternative measures. *, **, and *** represent two-tailed significance levels at 10%, 5%, and 1%, respectively. Fixed effects are included as identified in the body of each panel: F = firm fixed effects; Y = year fixed effects. T-statistics are reported in parentheses and calculated using standard errors clustered at the country level, and coefficient estimates (t-statistics) are reported in the top row (parentheses)
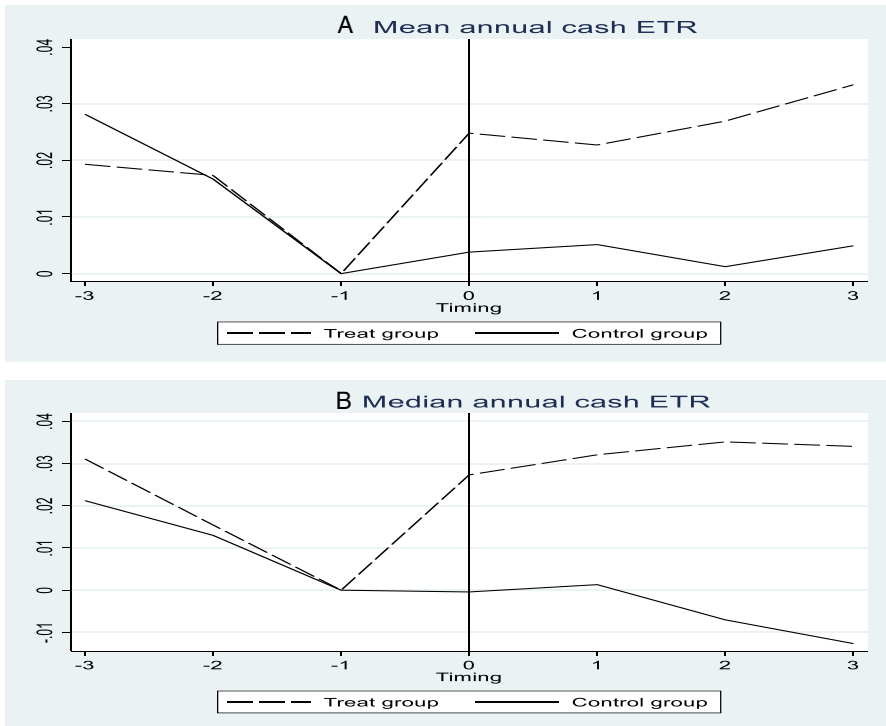
Fig. 2 Parallel Trends Assumption. This figure reports evidence on the parallel trends assumption using the cohort propensity-score-matching sample. Our treatment sample includes firms incorporated in countries that adopted major board reforms, and the control sample includes firms from incorporating countries that did not adopt major board reforms during the sample period. The sample period comprises seven years surrounding each country's board reform, three years before the board reform, and three years following the board reform. We further require that each firm in the sample has observations in the pre- and post-adoption periods. We use propensity-score matching in which we match treatment firms to control firms with replacement by pre-reform tax avoidance (*Cash ETR*), pretax return on assets, leverage, log(Total assets), capital intensity, intangible assets, sales growth, R&D, foreign operations, discretionary accruals, and the two-digit industry classification *in the year prior to* governance reforms. We require one to three matching and that the propensity score difference between treatment and control firms be less than 0.1

Fifth, we examine whether the results are affected by country-level financial crises. There are 17 countries in the sample that experienced a financial crisis at some point during the sample period, according to data from Luc and Valencia (2013). To investigate whether these countries are driving the results, we eliminate them, re-estimate Eq. (1), and report the results in Column (5). Sixth, some countries experience large changes in statutory tax rates during our sample period. Because changes in statutory tax rates that may impact firms' tax planning activities directly affect their taxes, we create an alternative sample that excludes country-years that experience statutory tax rate changes greater than 5% in absolute value (n = 15 country-years; see also the Internet Appendix Table IA.2 and Table IA.3). The results

are reported in Column (6).[12] Last, we exclude the U.S. firms from our propensity-score-matching tests and report the Column (7) results. In summary, the coefficient on *Post* in each of the alternative samples is positive and significant (two-tailed p < 0.10), except for Column (1), where the coefficient is insignificant. Overall, the results appear reasonably robust across alternative samples.

7.3 Additional controls for tax reform changes

We also examine whether changes to countries' tax systems, potentially beyond changes to statutory tax rates, explain the results. In principle, a country could reform its tax system without changing its statutory tax rate by altering other tax system attributes. For example, countries sometimes change their depreciation schedules for tax purposes to allow more rapid cost recovery. Even absent a statutory tax rate change, more rapid depreciation will increase the present value of the tax savings from depreciable property. If changes to tax systems were coincident with board reforms, that feature would complicate the empirical analysis. Accordingly, we examine summary measures of tax system changes and examine whether they explain the results. We use the Devereux and Griffith (1999, 2003) theory-based methodology for taking key attributes of a tax system to estimate the *effective average tax rate* (*Eatr*) and *effective marginal tax rate* (*Emtr*). *Eatr* reflects the tax on inframarginal investments and is based on evaluating the net present value of an investment with and without taxes (Devereux and Griffith 2003, p. 108). *Emtr* is a special case of *Eatr* for marginal investments (Devereux and Griffith 1999, p. 6). We use estimates of the *Eatr* and *Emtr* developed by The Center for European Economic Research (ZEW) (Spengel et al. 2016).

We re-estimate Eq. (1) controlling for *Eatr* and *Emtr*. Because *Eatr* and *Emtr* are highly correlated with statutory tax rates, we orthogonalize them with respect to statutory tax rates to capture tax reform changes other than those occurring through changes in statutory tax rates. After regressing them on statutory tax rates, we include the residual of *Eatr* and *Emtr* as additional control variables in Eq. (1). Columns 1 and 2 of Internet Appendix Table IA.4 reveal that the variable of interest, *Post*, remains positive and statistically significant after controlling for *Eatr_residual* and *Emtr_residual*. Encouraging R&D investment by granting favorable treatment is a common feature of tax systems around the world. In Column (3), we include a control for such incentives, *Implied tax subsidy rates on R&D*. Another common tax change around the world is to enact controlled foreign corporation rules to mitigate incentives for cross-border income shifting. In Column (4), we include an indicator variable, *CFC rules in place*, that takes on a value of 1 when a country adopts controlled foreign company rules and 0 otherwise. We obtain the data on R&D tax subsidies and controlled foreign company rules from the OECD. Columns (3) and (4)

---

[12] The statutory tax rate data show a large tax change for Italy in 1998, apparently attributable to the OECD data excluding regional business taxes in 1998. To be cautious, we treat that year as a large tax change and exclude it.

of Internet Appendix Table IA.4 reveal that the coefficient of interest, *Post*, remains positive and significant.

7.4 Alternative measures of tax avoidance

Our main tests use firms' cash effective tax rates, arguably the most common tax planning measure in the literature. To further test the robustness of the results, we examine three alternative measures. The first is the GAAP effective tax rate (*GAAP ETR*), which uses tax expense for financial reporting purposes in the numerator instead of cash taxes paid. The GAAP effective tax rate is useful for certain research questions. However, it has the disadvantage that it generally captures tax planning that results in permanent differences between tax and book income and not temporary differences (Hanlon and Heitzman 2010). The second, *Cash tax difference*, is defined as the difference between cash taxes paid and the product of the statutory tax rate and the sum of pre-tax income less discontinued operations and extraordinary items, scaled by lagged total assets (Henry and Sansing 2018). The third measure, *Adjusted Cash ETR*, is a modification of the annual cash effective tax rate from Balakrishnan et al. (2019) and is estimated as the difference between the mean of the firm's country-industry-size peers' *Cash ETRs* (i.e., those in the same quintile of total assets in the same country-industry-years) and the firm's cash effective tax rate. Columns 1–3 in Table 8 Panel C report these estimations. The coefficient on *Post* is not statistically significant when using *GAAP ETR* as the measure of tax planning. However, it remains positive and significant (two-tailed p < 0.10) when using *Cash tax difference* and *Adjusted Cash ETR* as tax planning measures.

7.5 Specific board reform characteristics and tax behavior

We examine whether board reforms affect firm-level tax behavior differently depending on their specific characteristics. We begin by examining the major components of reforms. For each board reform, we examine whether it includes the following: 1) board independence, 2) audit committee and auditor independence, and 3) the separation of the chairman and CEO positions (Fauver et al. 2017). We then re-estimate Eq. (1) after restricting the sample to board reforms containing the individual component in question. We set *Post* equal to 1 beginning in the year when the board reform involving the individual component becomes effective. Table 9, Panel A, columns 1 through 3, report the findings. The coefficients on *Post* are positive and significant in each of the estimations (two-tailed p < 0.10). The results show that reforms involving board independence, audit committee independence, and the separation of the chairman and CEO positions are associated with less tax avoidance.

We next examine whether the relationship varies in firms most affected by the board reforms and with the compliance speed of board reforms. We begin by examining changes for firms that we expect will be most affected by the board reforms (Chhaochharia and Grinstein 2007). Fauver et al. (2017) argue that reforms involving board independence are unlikely to affect firms as much if they had majority board independence before the reform. Therefore we expect that board reforms

Table 9 Board reform characteristics and cash ETR

Panel A – Analysis of major components of major board reforms and cash ETR

Dependent Variable = Cash ETR	Board independence		Audit committee independence		Chairman/CEO separation	
	(1)		(2)		(3)	
Intercept	−0.427	***	−0.615	***	−0.504	**
	(−3.765)		(−4.516)		(−2.546)	
Post	0.019	*	0.030	*	0.023	*
	(1.693)		(1.903)		(1.898)	
Controls	YES		YES		YES	
Fixed effects	F, Y		F, Y		F, Y	
Number of Obs.	63,314		73,903		19,683	
R-squared (%)	26.9		30.6		27.3	
Number of countries	23		26		9	

Panel B – Analysis of the impact of major board reforms on firm-level board independence and cash ETR

Dependent Variable = Cash ETR	(1)		(2)	
Intercept	0.187		0.093	
	(0.497)		(0.304)	
Post	−0.026		−0.022	
	(−0.773)		(−0.623)	
Post*Most impacted	0.011	**	0.014	***
	(2.684)		(25.455)	
Post*Comply-or-explain			0.016	
			(0.507)	
Post*Most impacted*Comply-or-explain			−0.046	
			(−1.034)	
Controls	YES		F, Y	
Fixed effects	F, Y		9,807	
Number of Obs.	9,807		21.9	
R-squared (%)	21.9		8	
Number of countries	8			

Table 9 (continued)

Panel C - Analysis of the speed of major board reforms about firm-level board independence and cash ETR

Dependent Variable = Cash ETR	(1)	(2)
Intercept	0.143	0.216
	(0.380)	(0.613)
Post	−0.024	−0.024
	(−0.709)	(−0.759)
Post*High speed compliance	0.017***	0.019***
	(3.658)	(53.328)
Post*Comply-or-explain		0.013
		(0.504)
Post*High speed compliance*Comply-or-explain		−0.126
		(−0.842)
Controls	YES	YES
Fixed effects	F, Y	F, Y
Number of Obs.	9,807	9,807
R-squared (%)	21.9	21.9
Number of countries	9	9

This table examines whether the effects of board reform on tax avoidance vary across board reform characteristics. Panel A reports the estimation of Eq. (1) across major reform components. Panel B reports the results across measures of the impact of the board reforms. Panel C presents the results about firm-level board independence. *Post* is an indicator variable that equals 1 beginning in the year a major board reform becomes effective in the country and 0 otherwise. *Comply-or-explain* is an indicator variable equal to 1 for countries that use a comply-or-explain reform approach. *Most impacted* is an indicator variable equal to 1 for firms with less than 50% independent directors in the year before the reform and 0 otherwise. *High speed of compliance* is an indicator variable equal to 1 if a firm without majority independence in the pre-period adopts majority board independence in Year 0. Only Australia, Canada, Finland, Hong Kong (China), Italy, Netherlands, Norway, Sweden, and the United States have firm-level board independence data in Panels B and C. T-statistics are reported in parentheses and calculated using standard errors clustered at the country level. Variables are defined in the appendix. *, **, and *** represent two-tailed significance levels at 10%, 5%, and 1%, respectively. Fixed effects are included as identified in the body of each panel: F = firm fixed effects; Y = year fixed effects. Coefficient estimates (t-statistics) are reported in the top row (parentheses)

should affect tax avoidance most in firms without major board independence. Accordingly, we estimate Eq. (1), interacting *Post* and *Most impacted*, an indicator variable equal to 1 for firms without more than 50% independent directors in the year before the reform and 0 otherwise (Fauver et al. 2017). We obtain firm-level governance attributes from the RiskMetrics and ASSET4 databases. Due to data availability from these databases, our sample size is reduced to 9807 firm-years. Column 1 of Panel B Table 9 presents the results of the model that includes the *Post* Most impacted* interaction and the controls from the prior analysis.

Different countries use different approaches to board reform. Some countries, such as the United Kingdom, use a "comply-or-explain" approach, whereby firms can choose not to comply but must explain why they did not (Fauver et al. 2017). In contrast, other reforms (e.g., the Sarbanes-Oxley Act in the United States) are rule-based with mandatory compliance. Although Fauver et al. (2017) demonstrate that comply-or-explain reforms exert a larger effect on firm value than do rule-based reforms, the impact of different reform approaches on tax behavior is an empirical question. To answer this question, in column 2 of Panel B Table 9, we include the interaction of *Post* and an indicator variable for whether a firm's country uses a comply-or-explain approach (*Comply-or-explain*), an interaction term between *Post* and an indicator variable for whether the firm had less than 50% independent directors prior to the board reform (*Most impacted*), and the triple interaction term between *Post*, *Most impacted*, and *Comply-or-explain*. Because our model already contains firm fixed effects, we do not include *Most impacted, Comply-or-explain*, and their interaction, which have no within-firm variation. The results indicate that reforms with high impact are associated with a larger decrease in tax planning than those with low impact reforms, as indicated by the significant positive coefficient *Post*Most impacted*. It also shows that the coefficient on the triple interaction term *Post*Most impacted*Comply-or-explain* is negative but insignificant. The coefficient on *Post*Comply-or-explain* is also insignificant (two-tailed p value >0.10).

Finally, if board reforms affect firms' tax behavior, we expect they are more likely to affect firms when they require faster compliance speed, compared to those with slower compliance speed. We classify firms as having a high speed of compliance (*High-speed compliance*) if they do not have majority board independence (more than 50% independent directors) in the pre-period and adopt majority board independence in the year the board reform becomes effective (Fauver et al. 2017). We re-estimate Eq. (1), interacting *Post* with *High-speed compliance*. The results are reported in column 1 of Table 9 Panel C. The results indicate that board reforms with faster compliance speed are associated with a larger decrease in tax planning activities than those with a slower compliance speed, as indicated by the significantly positive coefficient of *Post* High-speed compliance* (two-tailed p value <0.01). Because our model already includes firm fixed effects, we do not include *High-speed compliance*, *Comply-or-explain*, and their interaction, which have no within-firm variation. The results show that *Post*High speed compliance*Comply-or-explain* is insignificant, consistent with the effect of compliance speed on tax avoidance not varying across board reform approaches.
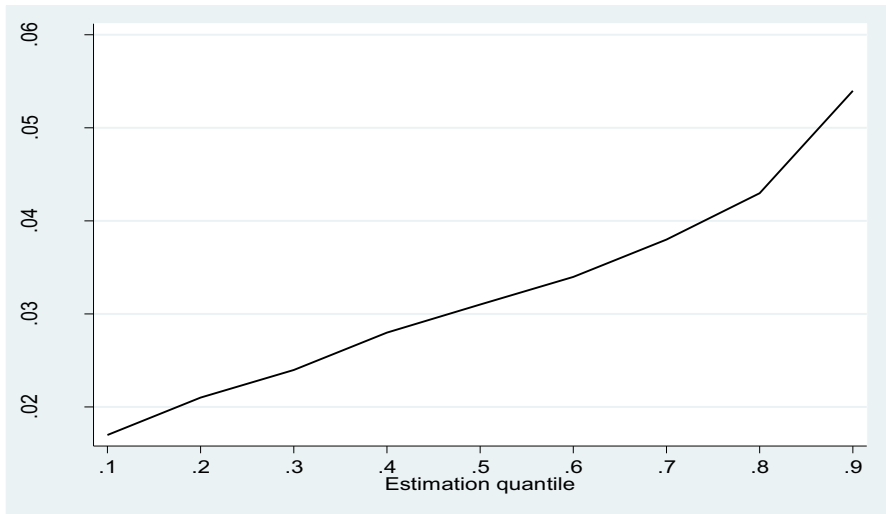
Fig. 3 Coefficient Estimates at Various Quantiles of the Cash ETR Distribution. This figure plots the coefficient estimates at various quantiles of the cash ETR distribution. The coefficient estimates correspond to the quantile regression results reported in Table 10

7.6 Audit committee financial expertise

Robinson et al. (2012) find a positive association between audit committee financial expertise and tax avoidance but also find that the association is negative when the audit committee considers tax planning to be aggressive. To assess whether our results are affected by changes in auditor committee financial expertise, we measure auditor committee financial expertise, *%Financial experts*, as the percentage of financial experts on the audit committee in the previous year (using the ASSETS4 and RiskMetrics databases). We re-estimate Eq. (1) controlling for *%Financial experts;* the conclusions are unchanged (untabulated).

7.7 Extreme tails

Using quantile regression, Armstrong et al. (2015) investigate whether the relation between corporate governance and tax avoidance varies across the distribution of tax avoidance. Taking a classic view of tax avoidance as a risky project in which unresolved agency conflicts may cause managers to deviate from what would maximize firm value, Armstrong et al. (2015) reason that such unresolved agency conflicts are more likely to be present at extreme values of the tax avoidance distribution. Consistent with their reasoning, they find a positive relation between board independence and tax avoidance for low levels of tax avoidance and a negative relation for high levels of tax avoidance, consistent with more independent boards attenuating relatively extreme levels of tax avoidance (Armstrong et al. 2015, p. 2 and Table 2).

While our research question about the relation between board independence and tax behavior relates to the work of Armstrong et al. (2015), our empirical settings

Table 10 Board reforms and cash ETR: Quantile regression analysis

Dependent Variable = *Cash ETR*	Coef.	Z-stat
Quantile		
0.10	0.017	1.195
0.20	0.021	1.340
0.30	0.024	1.616
0.40	0.028	1.588
0.50	0.031	1.648[*]
0.60	0.034	1.796[*]
0.70	0.038	1.729[*]
0.80	0.043	1.795[*]
0.90	0.054	1.841[*]
Tests of coefficient differences between quintiles (*P* value)		
Q(0.80) = Q(0.20)	0.078[*]	
Q(0.90) = Q(0.10)	0.090[*]	
Q(0.90) = Q(0.50)	0.097[*]	
Q(0.10) = Q(0.50)	0.087[*]	

This table reports the quantile regression results of the effect of major board reforms on tax avoidance. T-statistics are reported in parentheses and calculated using standard errors clustered at the country level by bootstrap 1000 times. Two-sided p-values are reported for the tests of coefficient differences between quintiles 0.80 versus 0.20, 0.90 versus 0.10, 0.90 versus 0.50, and 0.10 versus 0.50

differ in two important respects. First, the sample variation in board independence stems from country-level board reforms, which are plausibly exogenous shocks to individual firms. In contrast, the variation in board independence studied by Armstrong et al. (2015) is due to cross-sectional variation in choices made by private parties. Armstrong et al. (2015) acknowledge the limitations of their data and try to rule out alternative explanations, such as reverse causality (e.g., Armstrong et al. 2015, p. 14). Second, Armstrong et al. (2015) examine U.S. firms, and thus their work reflects a single, well-developed country with relatively strong governance practices, whereas the firms in our study are global, reflecting a wide range of governance practices and tax avoidance opportunities.

With these differences in mind, we re-estimate Eq. (1) using quantile regression. We report the regression results in Table 10 and plot the coefficient estimates of *Post* by quantile in Fig. 3. The patterns indicate that, when *Cash ETRs* are very low (10th percentile), representing high tax avoidance, the relationship between *Cash ETR* and board reforms is positive but not significant. As *Cash ETRs* increase (exceeding the 10th percentile), the relationship between *Cash ETR* and board reforms remains positive and increases in magnitude. The positive association between *Cash ETR* and board reforms is most significant at the extreme right tail of the distribution. Tests of equality between coefficients show that coefficient estimates at the extremes of the *Cash ETR* distribution (quantile 0.90 vs. quantile 0.10) are significantly different from each other. Thus consistent with the findings of Armstrong et al. (2015), we

find that the relation between board independence and tax avoidance varies across the level of tax avoidance. However, we are reluctant to draw strong conclusions from these tests in our cross-country setting because the firms in the *Cash ETR* quantiles are not randomly distributed across countries (i.e., high quantiles will tend to have firms from countries with high effective tax rates), so comparisons across quantiles in our setting reflect more than variation in tax avoidance.

8 Conclusion

We examine whether changes in firms' corporate governance stemming from board reforms influence firms' tax behavior. While the connection between corporate governance and tax behavior has been the subject of intense interest in the literature, research has been hampered by a lack of exogenous variation in corporate governance. Our inquiry is made possible by a database of major board reforms from Fauver et al. (2017) that captures shocks to board independence for firms in 31 countries. The results indicate that corporate tax avoidance decreases significantly following major board reforms, as reflected in increased cash effective tax rates.

Using a path analysis, we find evidence that board reforms have both direct and indirect influences on firms' tax behavior, where the indirect effect occurs through changes in corporate transparency. Moreover, we find that the effect of board reforms on corporate tax avoidance is stronger in firms with relatively higher agency costs and opaquer information environments. Furthermore, the impact of board reforms on tax avoidance is mitigated in countries with high country-level investor protection. These results are consistent with both direct (a more independent board reduces agency conflicts) and indirect (a more transparent information environment) channels through which board reforms are associated with reductions in firms' tax avoidance, as reflected in increased cash effective tax rates.

Appendix

Table 11 Variable definitions

Variable	Description	Details and Sources
Tax measures		
Cash ETR	Annual cash effective tax rates	Cash taxes paid (WC04150) divided by pre-tax income less discontinued operations and extraordinary items (WC01401 − WC04054 − WC04225). We constrain these values to fall between 0 and 1. Source: WorldScope
GAAP ETR	Annual GAAP effective tax rates	Tax expense (WC01451) divided by pre-tax income less discontinued operations and extraordinary items (WC01401 − WC04054 − WC04225). We constrain these values to fall between 0 and 1. Source: WorldScope
Cash tax difference		The difference between taxes paid and the product of corporate statutory tax rate in country (Statutory tax rate) and [pretax income less discontinued operations & extraordinary items], scaled by lagged total assets (WC02999). Source: WorldScope
Adjusted Cash ETR	Adjusted one-year cash effective tax rates	The difference between Cash ETR and the average annual cash effective tax rate of the country-industry-size quintile peer during the sample period. Peers are defined as from the same quintile of total assets in the same country-industries. Source: WorldScope
Board reform measures		
Post	Board reforms indicator	An indicator variable that equals 1 beginning in the year in which a major board reform becomes effective in the country and 0 otherwise.
Other reform control variable		
After insider trading reforms	Insider trading enforcement indicator	An indicator variable equal to 1 beginning in the year in which insider-trading laws are enforced in the country and 0 otherwise. Source: Bhattacharya and Daouk (2002).
After takeover law reforms	M&A law enactment indicator	An indicator variable that equals to 1 beginning in the year in which M&A law enactment becomes effective in the country and 0 otherwise. Source: EU: (2004), Lel and Miller (2015), and OECD (2010).

Table 11 (continued)

Variable	Description	Details and Sources
After IFRS implementation	IFRS adoption indicator	An indicator variable that equals to 1 beginning in the year in which IFRS is adopted in the country and 0 otherwise. Source: De George et al. (2016).
Firm-level control variables		
Pretax return on assets	Return on assets	Pre-tax income less extraordinary items divided by lagged total assets. Source: WorldScope
Leverage	Firm leverage	Sum of long-term debt (WC03251) and short-term debt and current portion of long-term debt (WC18232) divided by lagged total assets. Source: WorldScope
Log(Total assets)	Firm size	The natural logarithm of total assets. Source: WorldScope
Foreign operations	Indicator variable for foreign operations	An indicator variable that equals to 1 if foreign income (WC07126) is nonmissing and nonzero and 0 otherwise. Source: WorldScope
Sales growth	Sales growth	Net sales (WC01001) divided by lagged net sales, minus 1. Source: WorldScope
R&D	R&D intensity	R&D expense (WC01201) divided by lagged total assets. Source: WorldScope
Discretionary accruals	Performance-adjusted discretionary accruals	The residual of the Kothari et al. (2005) discretionary accrual model. Source: WorldScope
Capital intensity	Capital intensity	Property, plant and equipment (WC02501) divided by lagged total assets. Source: WorldScope
Intangible assets	Intangible asset intensity	Intangible assets (WC02649) divided by lagged total assets. Source: WorldScope
Firm-level conditioning variables		
Board independence	Board independence	The percentage of independent board members on the auditor committee (Source: ASSETS4 and RiskMetrics databases)
Most impacted	Most impacted firms	An indicator variable that equals to 1 for firms with less than 50% independent board members in the year prior to the board reform and 0 otherwise.
High speed compliance	High speed of compliance	An indicator variable that equals to 1 if a firm with less than 50% independent board members in the year prior to the board reform has at least 50% independent board members in Year 0 (the year in which the board reform becomes effective) and 0 otherwise.

Table 11 (continued)

Variable	Description	Details and Sources
High closely held ownership	High closely held ownership	An indicator variable that equals to 1 if a firm's percentage of closely held shares prior to the board reform is above the cohort median and 0 otherwise. Firm-level percentage of closely held shares is measured as closely held shares (WC05475) divided by shares outstanding (WC05301). Source: WorldScope
	Less corporate transparency	An indicator variable that equals to 1 if a firm's *Transparency* before the major board reforms is below the cohort median and 0 otherwise. *Transparency* is an index that equals the mean of the scaled percentile rank of the following five variables: the negative of discretionary accruals, *Big N*, *INTGAAP*, the number of analysts following the firm, and forecast accuracy. Negative discretionary accruals is the negative of the prior three years' moving sum of the absolute value of discretionary accruals (Hutton et al. 2009); *Big N* is an indicator variable that equals 1 if a firm is audited by a "Big N" auditing firm and 0 otherwise; *INTGAAP* is an indicator variable that equals 1 if a firm has adopted either IAS/IFRS or U.S. GAAP and 0 otherwise; analyst following is equal to the average number of analysts making a forecast for year t's earnings during the year t; forecast accuracy is the negative value of earnings surprise, scaled by the stock price at the end of the prior year, where earnings surprise is the absolute difference between actual earnings and the mean analyst forecast (Lang et al. 2012). If analyst forecast error is unavailable, transparency captures the average percentile rank of the remaining four measures. Source: WorldScope and Capital IQ
Country-level conditioning variables		
Comply-or-explain	Comply-or-explain indicator	An indicator variable that equals 1 for countries adopting a comply-or-explain reform approach and 0 otherwise. Source: Fauver et al. (2017)
Anti-self-dealing index	Anti-self-dealing index	The anti-self-dealing index developed by Djankov et al. (2008), calculated for 72 countries based on legal rules prevailing in 2003. Source: Djankov et al. (2008)

Table 11 (continued)

Variable	Description	Details and Sources
Country-year-level control variables		
Country-level book tax conformity	Required book-tax conformity	A measure of the country level of required book-tax conformity. We use Atwood et al. (2010) by country-year regressions to obtain RMSE and then calculate descending ranks divided by n-1 to obtain country-level book tax conformity. The data is available annually.
Statutory tax rate	Corporate statutory tax rate	Hand collected from OECD and KPMG LLP online. The data is available annually.
Country-level return on assets volatility	Country-level earnings volatility	The scaled descending rank of the standard deviation of return on assets for a specific country-year divided by n-1. Source: WorldScope. The data is available annually.
Country-level law and order strength	Country-level law and order strength index	The natural logarithm of _Law and Order_, an annual assessment of the strength and impartiality of the legal system. Higher values indicate stronger judicial systems. Source: International Country Risk Guide (ICRG), produced by Political Risk Services. The data is available annually.
GDP per capita	GDP per capita (current U.S.$)	GDP per capita is the logarithm of gross domestic product divided by the midyear population*10,000. Larger values correspond to higher economic development. Source: World Development Indicators 2013. The data is available annually.
Country-level financial development	Country-level financial development	Domestic credit provided by banking sector/GDP. Source: World Development Indicators 2013. The data is available annually.

References

Abadie, A. (2005). Semiparametric difference-in-difference estimators. *Review of Economic Studies* 72 (1): 1–19.

Amiram, D., A. Bauer, and M. Frank. (2019). Tax avoidance at public corporations driven by shareholder taxes: Evidence from changes in dividend tax policy. *The Accounting Review* 94: 27–55.

Appel, I. (2019). Governance by litigation. In *Working paper*. College. Available at SSRN https://ssrn.com/abstract=2532278.

Armstrong, C., J. Core, and W. Guay. (2014). Do independent directors cause improvements in firm transparency? *Journal of Financial Economics* 113 (3): 383–403.

Armstrong, C., J. Blouin, A. Jagolinzer, and D. Larcker. (2015). Corporate governance, incentives, and tax avoidance. *Journal of Accounting and Economics* 60 (1): 1–17.

Atanassov, J. (2013). Do hostile takeovers stifle innovation? Evidence from antitakeover legislation and corporate patenting. *The Journal of Finance* 68 (3): 1097–1131.

Atwood, T., M. Drake, and L. Myers. (2010). Book-tax conformity, earnings persistence and the association between earnings and future cash flows. *Journal of Accounting and Economics* 50 (1): 111–125.

Atwood, T., M. Drake, J. Myers, and L. Myers. (2012). Home country tax system characteristics and corporate tax avoidance: International evidence. *The Accounting Review* 87 (6): 1831–1860.

Balakrishnan, K., J. Blouin, and W. Guay. (2019). Tax aggressiveness and corporate transparency. *The Accounting Review* 94 (1): 45–69.

Beasley, M., N. Goldman, C. Lewellen, and M. McAllister. (2021). Board risk oversight and corporate tax-planning practices. *Journal of Management Accounting Research* 33 (1): 7–32.

Bertrand, M., and S. Mullainathan. (2003). Enjoying the quiet life? Corporate governance and managerial preferences. *Journal of Political Economy* 111: 1043–1075.

Bertrand, M., E. Duflo, and S. Mullainathan. (2004). How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics* 119 (1): 249–275.

Bhattacharya, U., and H. Daouk. (2002). The world price of insider trading. *The Journal of Finance* 57: 75–108.

Bourveau, T., Y. Lou, and R. Wang. (2018). Shareholder litigation and corporate disclosure: Evidence from derivative lawsuits. *Journal of Accounting Research* 56 (3): 797–842.

Cazier, R., S. Rego, X. Tian, and R. Wilson. J. (2009). Early evidence on the determinants of unrecognized tax benefits. Working paper, , Ohio State University and University of Oregon. Available at SSRN: https://ssrn.com/abstract=1578485.

Cen, L., E.L. Maydew, L. Zhang, and L. Zuo. (2017). Customer–supplier relationships and corporate tax avoidance. *Journal of Financial Economics* 123 (2): 377–394.

Chen, S., X. Chen, Q. Cheng, and T. Shevlin. (2010). Are family firms more tax aggressive than non-family firms? *Journal of Financial Economics* 95 (1): 41–61.

Chen, S., Y. Huang, N. Li, and T. Shevlin. (2019). How does quasi-indexer ownership affect corporate tax planning? *Journal of Accounting and Economics* 67 (2–3): 278–296.

Chen, F., Q. Li, and L. Xu. (2021). Universal demand laws and the monitoring demand for accounting conservatism. Journal of Business Finance and Accounting forthcoming.

Chhaochharia, V., and Y. Grinstein. (2007). Corporate governance and firm value: The impact of the 2002 governance rules. *The Journal of Finance* 62: 1789–1825.

De Chaisemartin, C., and X. D'Haultfoeuille. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review* 110 (9): 2964–2996.

De George, E., X. Li, and L. Shivakumar. (2016). A review of the IFRS adoption literature. *Review of Accounting Studies* 21: 898–1004.

Desai, M., and D. Dharmapala. (2006). Corporate tax avoidance and high-powered incentives. *Journal of Financial Economics* 79: 145–179.

Desai, M., and D. Dharmapala. (2009). Corporate tax avoidance and firm value. *Review of Economics and Statistics* 91 (3): 537–546.

Desai, M., A. Dyck, and L. Zingales. (2007). Theft and taxes. *Journal of Financial Economics* 84: 591–623.

Devereux, M., and R. Griffith. (1999). The taxation of discrete investment choices. Institute for Fiscal Studies, working paper series no. W98/16.

Devereux, M., and R. Griffith. (2003). Evaluating tax policy for location decisions. *International Tax and Public Finance* 10: 107–126.

Djankov, S., R. La Porta, F. Lopez-de-Silanes, and A. Shleifer. (2008). The law and economics of self-dealing. *Journal of Financial Economics* 88: 430–465.

Doidge, C., G. Karolyi, K. Lins, D. Miller, and R. Stulz. (2009). Private benefits of control, ownership, and the cross-listing decision. *The Journal of Finance* 64 (1): 425–466.

Donohoe, M.P. (2015). The economic effects of financial derivatives on corporate tax avoidance. *Journal of Accounting and Economics* 59 (1): 1–24.

Dyreng, S., and B. Lindsey. (2009). Using financial accounting data to examine the effect of foreign operations located in tax havens and other countries on U.S. multinational firms' tax rates. *Journal of Accounting Research* 47 (5): 1283–1316.

Easterbrook, F., and D. Fischel. (1986). Close corporations and agency costs. *Stanford Law Review* 38 (2): 271–301.

EU. (2004). The takeover bids directive assessment report. European Union.

Faccio, M. (2006). Politically connected firms. *American Economic Review* 96 (1): 369–386.

Faleye, O., R. Hoitash, and U. Hoitash. (2011). The costs of intense board monitoring. *Journal of Financial Economics* 101 (1): 160–181.

Fauver, L., M. Hung, X. Li, and A. Taboada. (2017). Board reforms and firm value: Worldwide evidence. *Journal of Financial Economics* 125 (1): 120–142.

Frank, M., L. Lynch, and S. Rego. (2009). Tax reporting aggressiveness and its relation to aggressive financial reporting. *The Accounting Review* 84 (2): 467–496.

Gleason, C., S. Kieback, M. Thomsen, and C. Watrin. (2020). Monitoring or payroll maximization: What happens when workers enter the boardroom? University of Iowa and University of Munster working paper. Available at SSRN: https://ssrn.com/abstract=3322700.

Gormley, T., and D. Matsa. (2011). Growing out of trouble? Corporate responses to liability risk. *Review of Financial Studies* 24 (8): 2781–2821.

Hanlon, M., and S. Heitzman. (2010). A review of tax research. *Journal of Accounting and Economics* 50 (2–3): 127–178.

Henry, E., and R. Sansing. (2018). Corporate tax avoidance: Data truncation and loss firms. *Review of Accounting Studies* 23 (3): 1042–1070.

Hutton, A., A. Marcus, and H. Tehranian. (2009). Opaque financial reports, R 2, and crash risk. *Journal of Financial Economics* 94 (1): 67–86.

Kerr, J. (2019). Transparency, information shocks, and tax avoidance. *Contemporary Accounting Research* 36 (2): 1146–1183.

Khan, M., N. Srinivasan, and L. Tan. (2017). Institutional ownership and corporate tax avoidance: New evidence. *The Accounting Review* 92 (2): 101–122.

Kim, E., and Y. Lu. (2013). Corporate governance reforms around the world and cross-border acquisitions. *Journal of Corporate Finance* 22: 236–253.

Kim, J., Y. Li, and L. Zhang. (2011). Corporate tax avoidance and stock price crash risk: Firm level analysis. *Journal of Financial Economics* 100 (3): 639–662.

Koester, A., T. Shevlin, and D. Wangerin. (2017). The role of managerial ability in corporate tax avoidance. *Management Science* 63 (10): 3285–3310.

Kothari, S., A. Leone, and C. Wasley. (2005). Performance matched discretionary accrual measures. *Journal of Accounting and Economics* 39 (1): 163–197.

KPMG. (2007). The governance of tax.

La Porta, R., F. Lopez-de-Silanes, A. Shleifer, and R. Vishny. (1997). Legal determinants of external finance. *The Journal of Finance* 52: 1131–1150.

Lang, M., K. Lins, and M. Maffett. (2012). Transparency, liquidity, and valuation: International evidence on when transparency matters most. *Journal of Accounting Research* 50 (3): 729–774.

Lel, U., and D. Miller. (2015). Does takeover activity cause managerial discipline? Evidence from international M&a laws. *The Review of Financial Studies* 28: 1588–1622.

Leone, A., M. Minutti-Meza, and C. Wasley. (2019). Influential observations and inference in accounting research. *The Accounting Review* 94 (6): 337–364.

Li, Q., C. Lin, and L. Xu. (2020). Political investment cycles of state-owned enterprises. *The Review of Financial Studies* 33: 3088–3129.

Luc, L., and F. Valencia. (2013). Systemic banking crises database: An update. IMF working paper WP/12/163. Available at SSRN: https://ssrn.com/abstract =2096234.

Maclean, A., and P. Dixon. (2015). *Best practice in corporate tax governance—From the finance team to the board*, 1–4. Deloitte Touche Tohmatsu Limited.

McGuire, S.T., D. Wang, and R.J. Wilson. 2014. Dual class ownership and tax avoidance. *The Accounting Review* 89 (4): 1487–1516.

Mills, L. (1998). Book-tax differences and internal revenue service adjustments. *Journal of Accounting Research* 36: 343–356.

OECD. (2010). *Country studies: Chile – Accession report on competition law and policy*. OECD.

Rego, S. (2003). Tax-avoidance activities of U.S. multinational corporations. *Contemporary Accounting Research* 20 (4): 805–833.

Rego, S., and R. Wilson. (2012). Equity risk incentives, and corporate tax aggressiveness. *Journal of Accounting Research* 50 (3): 775–810.

Robinson, J., Y. Xue, and M. Zhang. (2012). Tax planning and financial expertise in the audit committee. Working paper, University of : https://ssrn.com/abstract=2146003.

Seidman, J., and B. Stomberg. (2017). Equity compensation and tax avoidance: Disentangling managerial incentives from tax benefits and reexamining the effect of shareholder rights. *Journal of the American Taxation Association* 39 (2): 21–41.

Spengel, C., F. Schmidt, J. Heckemeyer, and K. Nicolay. (2016). Effective tax levels using the Devereux/Griffith methodology. Project for the EU Commission TAXUD/2013/CC/120.

Wilson, R. (2009). An examination of corporate tax-shelter participants. *The Accounting Review* 84 (3): 969–999.

Wooldridge, J. (2007). Difference-in-differences estimation. NBER.

Zimmerman, J. (1983). Taxes and firm size. *Journal of Accounting and Economics* 5 (1): 119–149.