Short-sale deregulation and corporate tax aggressiveness

Short-sale deregulation and corporate tax avoidance: evidence from the Chinese market

Yue Cao[a], Yizhe Dong[b], Tianxiao Guo[a*], Diandian Ma[c]**

[a] Business School, Hunan University, Lushan south road, Changsha, 410082, China

[b] University of Edinburgh Business School, 29 Buccleuch Place, Edinburgh, EH8 9JS, UK

[c] Department of Accounting and Finance, University of Auckland, Auckland, 1142, New Zealand

Abstract

We study whether short selling affects corporate tax aggressiveness. Exploiting staggered short-sale deregulation in the Chinese stock market as a source of variation in market pressure and monitoring, our difference-in-differences estimates show that the introduction of a short-selling scheme significantly discourages pilot firms from engaging in aggressive tax avoidance, in contrast to the findings by Luo et al. (2020). We also find that the negative effect of short selling on tax aggressiveness is more pronounced for firms that have high advertising costs, high institutional holdings, and CEO duality, and are located in regions with weak tax law enforcement. We further reveal that short selling has an indirect effect on tax aggressiveness through the additional external pressure exerted by auditors, media, and financial analysts, and lastly, challenge the main analysis by Luo et al. (2020). Our evidence highlights the monitoring and disciplinary roles that short sellers play in determining the level of corporate tax aggressiveness.

Keywords: short selling; tax aggressiveness; tax planning; monitoring; Chinese market

JEL classification: G12, G14, G15, G18

* Corresponding author. E-mail: gtx1993@hnu.edu.cn

**We thank Chris Adcock (the Editor), three anonymous referees and participants at the 2019 Impact of Macroeconomic Conditions and Policies Conference (Guangzhou, China) for their valuable comments. Cao acknowledges financial support from the National Social Science Fund of China (NSSFC) (Grant Number: 20BGL071) and Dong acknowledges financial support from the National Natural Science Foundation of China (NSFC) (Grant Number: 71873103 and 72071142).

Short-sale deregulation and corporate tax aggressiveness: evidence from the Chinese market

Abstract

We study whether short selling affects corporate tax aggressiveness. Exploiting staggered short-sale deregulation in the Chinese stock market as a source of variation in market pressure and monitoring, our difference-in-differences estimates show that the introduction of a short-selling scheme significantly discourages pilot firms from engaging in aggressive tax avoidance, in contrast to the findings by Luo et al. (2020). We also find that the negative effect of short selling on tax aggressiveness is more pronounced for firms that have high advertising costs, high institutional holdings, and CEO duality, and are located in regions with weak tax law enforcement. We further reveal that short selling has an indirect effect on tax aggressiveness through the additional external pressure exerted by auditors, media, and financial analysts, and lastly, challenge the main analysis by Luo et al. (2020). Our evidence highlights the monitoring and disciplinary roles that short sellers play in determining the level of corporate tax aggressiveness.

Keywords: short selling; tax aggressiveness; tax planning; monitoring; Chinese market

JEL classification: G12, G14, G15, G18

1. Introduction

A firm's tax planning, as opposed to simple recognizing of tax expenses, is a complicated and costly process that considers organizational goals and characteristics, and balances relevant parties' benefits and costs. Firms sometimes have to restructure a business to meet tax-planning requirements, despite the sizable cost of doing so (Scholes, Wilson and Wolfson 1992). The extant literature documents firm characteristics, including profitability, leverage, and capital intensity (Chen et al. 2010; Cheng et al. 2012; Gupta and Newberry 1997; Klassen and Laplante 2012; Rego 2003; Wilson 2009), corporate governance, such as ownership, managerial incentives, and executive compensation (Desai and Dharmapala 2006; Desai et al. 2007; Dyreng et al. 2010; Rego and Wilson 2012; Slemrod 2004), and external forces, for example, customers, the government, labor unions, and audit firms (Chyz et al. 2013; Hoopes et al. 2012; Huang et al. 2016; McGuire, Omer, and Wang 2012; Watts and Zimmerman 1986), as characteristics that interact with and shape firms' tax planning. A large number of studies on corporate tax decisions reflect the tradeoff between organizational goals and influence from various parties. Recent studies have recognized short sellers as one of the influential external parties that affect firms' tax planning. Three research articles have attempted to explore the relation between short selling and corporate tax avoidance based on different settings. However, their results and conclusions are disparate. Guo, Chi and Cook (2018) conducted the first empirical study to examine the effect of short selling on corporate tax avoidance. Kim, Lu, and Peng (2020) examined a similar relationship using the pilot program on short sales in Regulation SHO. They both found a positive association between short-selling pressure and the level of tax avoidance among U.S. firms. On the contrary, Luo, Ni and Tian (2020) (henceforth LNT), using Chinese data, found that short selling encourages corporate tax avoidance, in support of the financial constraint view instead of the monitoring view. In order to better understand the roles short selling plays in emerging markets, in this research, we also use Chinese data and conduct a thorough analysis to investigate the relationship between short-selling and tax aggressiveness. However, we find that short selling deters tax aggressiveness in Chinese listed companies, consistent with the studies of Guo et al. (2018) and Kim et al. (2020) based on U.S. settings. Meanwhile, we also demonstrate that the LNT empirical work can be challenged in terms of both data selection and the methodology employed, which may invalidate their results.

The three aforementioned studies consider two opposing views, that short selling can either reduce or induce tax avoidance and aggressiveness. Firms with a high level of short

interest may have incentives to reduce aggressive tax avoidance because of short sellers' important role in the information efficiency of financial markets, due to their information advantages and incentives to reveal negative news about firms. Short sellers are more informative and sophisticated investors, and are capable of maximizing investment returns by using fundamental ratios (Dechow et al. 2001), successfully anticipating earnings surprises (Christophe, Ferri, and Angel 2004), and accessing private information that enables them to front-run insider sales (Khan and Lu 2013). In addition, because short sellers profit massively when stock prices decline, they have a strong incentive to detect firms' financial misconduct and other suspicious activities, and to quickly publicize negative news to depress share prices (e.g., Boehmer et al. 2008; Diamond and Verrecchia 1987). Therefore, short selling can mitigate tax aggressiveness through the monitoring and disciplinary roles that it plays.

Several anecdotes also show that short sellers can drive down share prices by detecting, publicizing, and accusing firms of questionable tax-related issues. For example, J Capital Research, a short seller, claimed that China Green Agriculture, a China-based company that trades on the New York Stock Exchange, had overstated its revenue and was involved in aggressive tax avoidance. China Green Agriculture shares plunged by more than 10% on 5 January 2020 and the SEC launched an inquiry following this claim. Another anecdote tells that Boohoo's shares slid 10.9% on 26 May 2020 after ShadowFall Fund took a short position and published a 53-page report accusing the fashion giant of having overstated its free cash flow without including tax payments, a strong indicator of unethical practice. Similarly, in the Shenzhen Stock Exchange in China, short sellers reacted ahead of the disclosure by China's Ministry of Finance on 29 October 2018 that 2345 Network (stock code: 002195.SZ) had engaged in tax evasion and aggressive tax avoidance. Figure 1 shows that the levels of 2345 Network's short interest started to increase significantly from a month before the news release and fell drastically afterwards, while its industrial counterparts did not experience such fluctuation around the news. The revelation of such questionable tax-related issues not only triggers a cascade of selling and a sharp drop in the share price, imposing substantial pressure on the management, but also exposes a firm to the risk of litigation, penalties, and unfavorable publicity, and damages its relationship with the taxing authorities, one of the most powerful stakeholders in a firm. Therefore, given short sellers' incentives and capabilities to uncover and publicize firms' unethical and illegal behaviors, their presence should discipline firms and discourage them from engaging in aggressive tax avoidance activities.

[Insert Figures 1(a) and (b) about here]

The impact of short selling on corporate tax aggressiveness can also be positive in that short selling may motivate firms to engage in more aggressive tax planning. A firm that attracts high short-selling interest sends negative signals to the market, and its equity suffers from a decline in value, a situation which leaves the firm reluctant to raise funds through share issuance (Grullon et al. 2015). In addition, because high short interest signals a firm's negative outlook, banks are likely to impose higher interest rates and stricter conditions on loans, increasing the firm's capital costs (Campello and Gao 2017). Faced with these suboptimal external financing conditions, firms may choose to raise or save cash internally and are likely to practice aggressive tax avoidance. We can therefore see that, in the presence of short sellers, firms may be motivated to avoid paying taxes so as to generate cash internally, as a substitute for costly external funds. In summary, the extant studies, particularly LNT, Kim et al. (2020), and Guo et al. (2018), prompt conflicting predictions and present contradictory findings as to whether the short-selling mechanism leads firms to engage in greater or lesser aggressive tax avoidance.

Our study differs from and extends the findings of those studies in several ways. First, and most importantly, we point out flaws in LNT and apply a more appropriate key variable construction, and sample and control variable selection, to investigate the impact of short selling on tax aggressiveness in China[1]. Second, differing from Guo et al. (2018) and Kim et al. (2020), our study is based on the Chinese market, the world's largest emerging market, where a pilot short-selling scheme launched in March 2010 lifted the ban on margin trading and short selling for designated stocks. Chinese public firms are widely characterized by poor corporate governance and a large gap between control rights and cash-flow rights, which enable and stimulate insiders to divert corporate resources (Firth et al. 2007). To conceal their diversionary activities, insiders have an incentive to design sophisticated tax planning on the pretext that all shareholders benefit from lower taxes (Bauer et al. 2020). Moreover, Chinese listed firms are not required to disclose information about income tax uncertainty[2]. As a result, Chinese firms bear high agency costs of tax avoidance activities, which makes our setting an interesting one for studying whether short sellers pay great attention to aggressive tax avoidance activities and could force managers to reduce engagement in aggressive tax avoidance. In addition, unlike in the U.S. market, which has a better governance environment

[1] LNT focuses on examining the relationship between short selling and tax avoidance, which includes both aggressive tax planning and benign tax planning.
[2] As a result, Chinese companies may not record reserves until faced with tax audits and assessment by the Chinese tax authorities.

(e.g. the rule of law), China's investor protection (Morck et al. 2000) and law enforcement (Cai and Liu 2009) remain weak. Thus, the short-selling mechanism has become one major way for Chinese investors to express their bearishness (Li et al. 2018) and its monitoring and disciplinary roles may be more pronounced in the Chinese capital market than in the U.S. market. Third, China has lifted the ban on short-selling in a staggered manner. The pilot scheme allowed only 90 constituent stocks to be placed on the short-selling designated list initially. Since then, the China Securities Regulatory Commission (CSRC) has revised and expanded the list several times. By October 2018, 987 stocks and ETFs (exchange-traded funds) had been included on the designated list and were eligible for short selling. These multiple revisions and expansions of the designated list provide us with a rare opportunity to apply a quasi-natural experiment research design to explore any causal impact of the staggered short-sale deregulation on corporate tax avoidance. This setting is helpful for eliminating the potential concern of omitted variables. We anticipate that our study will enable researchers to understand that an adequate measurement of variables and a careful selection of data and control variables are important for generating credible results, and provide more reliable and useful implications for countries and jurisdictions wanting to launch short-selling schemes gradually and selectively.

In our study, we focus on the impact of Chinese short-sale deregulation on tax aggressiveness (or tax sheltering), which generally refers to the more egregious end of the tax-planning continuum (Hanlon and Heitzman 2010). We construct our primary measure of tax aggressiveness following Balakrishnan, Blouin and Guay (2019), and alternative measures taking into account China's unique tax settings, select sample data from A-share firms listed on the Shanghai Stock Exchange (SSE) and Shenzhen Stock Exchange (SZSE) and control variables following prior studies published in highly regarded journals, and apply a difference-in-differences (DID) approach. We compare the change in tax aggressiveness between the pre-pilot and pilot scheme periods, amongst firms on the short-selling designated list (treatment group), with the corresponding change amongst firms not on the list (control group). These procedures allow us to infer whether and to what extent short-selling affects tax aggressiveness. Our evidence suggests that short-selling pressure reduces the level of corporate tax aggressiveness in China, as it does in the U.S.[3]

[3] The CSRC only selects certain 'blue chip' stocks with good earnings performance and minimal volatility to participate in the short-selling scheme. Our logit analysis results show that larger and older firms and firms with lower leverage, larger market capitalization, and higher stock liquidity are more likely to be selected to participate in the pilot program. Because the selected firms generally have better performance and higher creditworthiness, they are less likely to face financing constraints.

Our results hold up to various robustness tests. Appreciating that our causal interpretation will be valid only if the parallel time trend assumption is satisfied, we examine the dynamics of the average treatment effect before and after the short-selling bans were lifted. The results show that the difference in tax aggressiveness between pilot firms and non-pilot firms is not time-varying before the short-selling pilot scheme began. We also extend our DID design to control for pre-existing time trend, year-industry, and year-province fixed effects, as well as unknown changes in firms' characteristics over the sample period. The results of that analysis remain robust. To alleviate concerns about non-random selection of pilot stocks, and to make our treatment and control firms more comparable (Rosenbaum and Rubin 1984), we apply a propensity-score-matching (PSM) method to construct a treatment group whose firm characteristics matched those of the control group before the pilot scheme period. We also perform OLS on an entropy-balance matched sample (McMullin and Schonberger 2015; Wilde 2017). The DID-PSM and the entropy-balance matching results show that, in line with our main results, lifting the short-selling bans decreased tax aggressiveness among the pilot firms, but not among the firms in the matched control group. We also conduct tests on pseudo-pilot firms randomly drawn from the sample based on simulations. The tests show our findings are not driven by random effects. Finally, we re-estimate the DID models using alternative tax aggressiveness measures; the results are again robust.

We then extend our analysis to explore the cross-sectional difference in the effect of the short-selling scheme on tax aggressiveness. A firm's reputation and image among its stakeholders are partially shaped by its advertising and marketing investments. The presence of short sellers increases the likelihood of aggressive tax shelters being detected, which can significantly damage the firm's reputation. Thus, firms that incur higher advertising and marketing costs tend to be those that care about their reputation and are therefore less tax aggressive. Consistent with this argument, we find that the negative effect of short selling on tax aggressiveness is stronger for firms with greater advertising expenditure. Weak law enforcement implies the costs of aggressive tax avoidance are relatively low. External monitoring mechanisms can substitute for weak law enforcement and discourage corporate tax aggressiveness. Commensurate with the fact that tax law enforcement varies across regions in

Moreover, following Almeida, Campello, and Weisbach (2004), we examine whether the financial constraints of the pilot firms significantly increase after the introduction of the short-selling scheme. We capture the effect of financial constraints by the firms' propensity to save cash out of cash flows (i.e. the cash-flow sensitivity of cash). We find that short-selling pressure does not significantly increase firms' financial constraints, suggesting that the financial constraint channel is unlikely to drive the relationship between short selling and tax avoidance. These results are not tabulated but are available from the authors.

China, we find that the effect of short selling on tax aggressiveness is more pronounced for firms located in regions with weaker tax law enforcement.

Recent studies provide evidence that institutional ownership leads to greater tax savings (e.g. Bird and Karolyi 2017; Chen et al. 2019). We, therefore, predict that monitoring and pressure from short sellers will reduce incentives for institutional investors to encourage firms to generate additional cash flow through tax avoidance activities. We find evidence consistent with this prediction and show that the discouraging effect is more pronounced for firms with higher institutional holdings. Furthermore, we find that the negative effect of short selling on tax aggressiveness is more pronounced for firms with poor governance, proxied by CEO duality.

We conduct several additional tests to examine whether short-sale deregulation also has an indirect effect on the level of corporate tax aggressiveness. We predict that the presence of short sellers will increase auditor risk and efforts, as well as media and analyst coverage, and that these increases, in turn, will discourage corporate tax aggressiveness. Thus, we consider abnormal audit fees, negative news, and analyst following as mediating variables. Our results show that the short-sale deregulation has a positive effect on abnormal audit fees, negative news, and analyst following, which means that these mediating factors discourage firms from engaging in aggressive tax avoidance. Our path analysis suggests that short sellers also have an indirect mitigating effect on tax aggressiveness because of the additional external monitoring pressure exerted by auditors, media, and financial analysts.

Our study makes several contributions. First, our study adds to the large body of research examining the determinants of tax avoidance, in particular by demonstrating the impact of external stakeholders on firms' tax planning (see also, in this regard, Hoopes et al. 2012; Huang et al. 2016; Lobo and Zhao 2013; McGuire et al. 2012). Although our study can be regarded as closely related to the LNT study, which examines the impact of the same short-selling pilot program on tax avoidance, we find that their research design and findings are questionable, which we discuss in Section 5. Our study focuses on a related but distinct construct, in tax aggressiveness. In contrast to that of LNT, our evidence suggests that short-selling pressure reduces the level of corporate tax aggressiveness, with that effect exerted mainly through the monitoring and disciplinary channels rather than the financial constraint channel. Second, our study adds to a growing body of literature investigating the impact of short selling on various corporate policies and performance measures, such as earnings announcements and financial reporting quality (Fang, Huang, and Karpoff 2016; Massa et al. 2015), investment (Grullon et

al. 2015), the design of executive incentive contracts (Angelis, Grullon, and Michenaud 2017), mergers and acquisitions (Chang, Lin, and Ma 2019), and bond-rating downgrades (Henry, Kisgen, and Wu 2015). In short, our work complements the literature because it examines whether and how the exogenous impact of short selling leads to changes in firms' aggressive tax planning. Finally, our empirical investigation provides useful insights into the policy implications for tax authorities and other emerging market regulators wanting to introduce or revise short-selling schemes.

The remaining sections of this paper proceed as follows: Section 2 describes our data and the construction of the main variables and outlines our research design. Section 3 presents empirical findings and conducts a battery of robustness tests. Section 4 reports additional test results. Section 5 discusses the flaws of LNT and attempts to replicate their research, and Section 6 provides concluding commentary.

2. Data and empirical methodology

2.1 Sample selection

Our initial sample begins with all A-share firms listed on the Shanghai and Shenzhen stock exchanges that are included in the WIND and China Stock Market and Accounting Research (CSMAR) databases. The sample period is from 2008 to 2017.[4] Given that the short-selling pilot scheme was gradually implemented from March 2010, our sample covers periods both before and after its start. We execute the sample selection procedure shown in Panel A of Table 1. Beginning with a population of 34,991 non-financial listed A-share firm-year observations from 2008 to 2017 in China, we first exclude 2,243 observations with negative pre-tax income. We do this because tax avoidance by firms experiencing losses is likely to differ from tax avoidance by profitable firms (see Bradshaw et al. 2019; Guenther et al. 2019; Rego 2003). We then exclude 11,433 observations with missing current-year tax expenses, prior- and current-year income taxes payable, cash tax paid, and the applicable statutory tax rate. We also delete 6,168 observations with insufficient data for the control variables. Our next step is to match the sample with short-sale information obtained from the China stock exchange websites and iFinD (i.e. the designated short-selling stock list). We further exclude firms that were removed from the short-sale list at any point during our sample period and those that were added to the

[4] Our sample period startsfrom 2008 rather than an earlier year to avoid the test results being confounded by China's adoption of international financial reporting standards (IFRS) in 2007 and the major tax reform the country enforced in 2008.

list in 2017. Our final sample consists of 13,921 firm-year observations. We obtain detailed short-selling and financial accounting information from WIND and CSMAR. Panel B of Table 1 provides the sample distribution across time. The number of observations ranges from 748 in 2008 to 2,263 in 2017 and 23.9% of firm-year observations are classified as shortable firms. We winsorize all continuous variables at 1% and 99%.

[Insert Table 1 about here]

2.2 Tax aggressiveness measures

Following prior literature, we construct several variables to proxy for tax aggressiveness. The effective tax rate (*ETR*) and cash effective tax rate (*CETR*) are the most commonly used measures of tax avoidance in the literature (e.g. Bradshaw, Liao, and Ma 2019; Dyreng, Hanlon and Maydew 2010; Hanlon and Heitzman 2010; Huang et al. 2016). However, these measures fail to capture aggressiveness relative to a benchmark of a normal level of tax planning. Thus, following Balakrishnan et al. (2019), we create peer-adjusted tax aggressiveness measures in two steps. First, we use the *ETR* and *CETR* to measure each firm's tax avoidance, computed as follows:

$$ETR = income\ tax\ expense\ /\ pretax\ book\ income$$

and

$$CETR = cash\ income\ taxes\ paid\ /\ pretax\ book\ income$$

where *cash income taxes paid* is calculated as the tax expense for the current financial year plus beginning-of-year income taxes payable minus end-of-year income taxes payable[5]. *CETR* captures all cash taxes paid in a year. Following prior literature (e.g., Bradshaw et al. 2019; Chen et al. 2019; Hasan et al. 2017; Dyreng et al. 2010), we truncate *ETR* and *CETR* to the range [0, 1]. Then, in the second step, we use the mean of the firm's peers' same-period *ETR* or *CETR* as a proxy for a 'normal' level of tax planning. The peer group consists of firms in the same decile of total assets, the same industry, and the same province. We then measure tax aggressiveness, *TA_ETR* (*TA_CETR*), as the mean of the size-industry-province-matched *ETRs* (*CETRs*) less the firm's *ETR* (*CETR*). A positive value indicates that the firm pays less than its peers, and a higher value suggests greater tax aggressiveness. The above-mentioned peer-adjusted ETR and CETR are the main measures of tax aggressiveness. In the robustness tests,

[5] Unlike with US companies, the cash income taxes paid is a voluntary disclosure item for Chinese listed firms. Therefore, to minimize the loss of observations, we follow Bradshaw et al. (2019) and Li et al. (2017), using the current income tax expenses plus the beginning-of-year income taxes payable minus the end-of-year income taxes payable to calculate cash taxes paid. This is a more appropriate way to proxy for cash income taxes paid in China's voluntary disclosure setting.

we also adopt four alternative measures of tax aggressiveness and discussed them in more details in section 3.3.6.

2.3 Empirical design

When the short-selling ban was lifted in 2010, the CSRC included only 90 securities in an official short-selling designated list. This list was revised a number of times during our sample period.[6] This staggered deregulation of short selling persuades us to use a DID research design to examine the influence of short selling on aggressive tax avoidance activities. Specifically, we use the DID approach to compare the changes in the tax aggressiveness of the pilot firms from *before* to *after* they were admitted onto the short-selling designated list, with the change in the tax aggressiveness of those firms that were banned from short selling.[7] The DID research design also allows us to mitigate concerns regarding omitted variables. Because the designated list was changed multiple times over the sample period, and the incorporation of cross-terms in the model would reduce the degrees of freedom, we follow Beck (2010) and Agrawal (2013) and use the following DID specification with multiple time periods:

$$Tax\ aggressiveness_{i,t}=\alpha+\beta\ Shortable_{i,t}+\gamma_m Control\ variables_{i,t}+Firm_i+Year_t+\varepsilon_{i,t} \quad (1)$$

where the dependent variable, *Tax aggressiveness,* refers to the *TA_ETR* or *TA_CETR* that measures a firm's level of aggressive tax avoidance in a given year. $Shortable_{i,t}$ is an indicator variable that equals 1 if stock *i* is on the short-selling designated list in year *t*. We also control for firm and year fixed effects[8].

Again following prior studies, we control for many of the factors that potentially affect the level of corporate tax aggressiveness. For example, we control for firm size (*Size*), using the natural logarithm of total assets. Prior literature suggests that larger firms are the firms best able to access tax experts and manipulate tax payments (Porcano 1986), although empirical studies show mixed results regarding the effect of firm size on corporate tax avoidance (Higgins et al. 2015; Mills 1998). We include return on assets (*ROA*) to control for firm profitability, which, as extant literature shows, is an important determinant of tax aggressiveness (Gupta and Newberry 1997; McGuire et al. 2012). We also include firm

[6] Appendix 2 of our paper reports the timeline of and revisions to this short-selling pilot scheme during the sample period.
[7] The treatment group includes the firms that were on the short-selling designated list from March 2010 to December 2016. The control group includes firms that have never been on the short-selling list.
[8] Our results remain robust and are even stronger when we control for industry and year fixed effects instead of firm and year fixed effects.

leverage (*Leverage*), measured by total debt scaled by total assets. While firms with a high level of debt may use tax avoidance to save cash to pay off debt (Badertscher et al. 2013), tax relief on loan interests reduces the incentive for firms to engage in tax avoidance (Huang et al. 2016). We also control for other tax-related firm characteristics, such as property, plant, and equipment (*PPE*), intangible assets (*Intan*), the market to book ratio (*MB*), capital expenditure scaled by total assets, an indicator variable for loss carryforward (*NOL*)[9], management ownership (*Mgmt Own*), and CEO duality (*Dual*) (e.g., Armstrong et al. 2015; Chen et al. 2010; Desai and Dharmapala 2009; Graham and Tucker 2006; Huang et al. 2016). Finally, we include market capitalization (*Mkt_Cap*), a specific stock's trading volume scaled by the entire market's trading value (*Value*), the price-earnings ratio (*PE*), and daily share turnover (*Turnover*) in our regression analysis. Appendix 1 provides detailed definitions and calculations for all variables.

3. Empirical results

3.1 Descriptive statistics

Table 2 provides the descriptive statistics for our main regression variables. Panel A reports the statistics for the full sample. In terms of the tax aggressiveness measures, the mean values of *TA_ETR* and *TA_CETR* are nearly equal to 0, which indicates the normal level of tax avoidance (i.e. no tax aggressiveness); the mean values of the modified *ETR* measures are all above 1, which indicates that, on average, firms' effective tax rates are greater than their applicable tax rates. Panel B reports the means and medians separately for the pilot and non-pilot firm subsamples. The means of *TA_ETR*, and *TA_CETR* for the pilot firm subsample are all significantly lower than those for the non-pilot firm subsample. These results show that pilot firms may be more tax aggressive than non-pilot firms on average. The difference in tax aggressiveness between the two subsamples may be due to firm-specific characteristics. Many existing studies have documented that firms with larger capital expenditures, higher leverage, higher profitability, and CEO duality are associated with higher tax avoidance (e.g., Rego 2003; Frank, Lynch and Rego 2009; Hoopes, Mescall and Pittman 2012; Tang et al. 2017; Lanis and

[9] Drake et al. (2020) suggested that recording a valuation allowance associated with prior-period losses will have a significant effect on firms' tax expenses and *ETR*s. Therefore, it is important to control for the tax benefits of carryforwards. According to the tax law of China, firms are allowed to carry losses forward for up to five years, subsequent to the year of the loss. However, carryback of losses is not allowed. This rule can reduce the corporate tax expenses of unprofitable firms. Unlike U.S. firms, Chinese firms do not report the tax benefit of operating losses on the balance sheet. Thus, following Bradshaw, Liao and Ma (2019), we use net operating losses (NOL), which equal the sum of net losses reported in the last five years, or 0 if there was a net profit, to control for the tax benefits of carryforwards.

Richardson 2014). In addition to tax aggressiveness, the pilot firm subsample has a larger *Size*, higher *Leverage*, higher *ROA*, higher capital expenditures, lower *MB*, higher *NOL,* and less CEO duality. Pearson correlations between the control variables are all at low levels, indicating that our regression analysis is not subject to the multicollinearity problem[10].

[Insert Table 2 about here]

3.2 Main results

We use Equation (1) to examine the association between short selling and tax avoidance. Table 3 presents the main regression results. Columns (1) and (2) correspond to the use of *TA_ETR* as the dependent variable and Columns (3) and (4) to the use of *TA_CETR*. As is evident from the table, the introduction of short selling is associated with a statistically and economically significant decrease in corporate tax aggressiveness. For example, the coefficient on *Shortable* (Column 2) is negative and significant at the 1% level after we control for firm characteristics and firm and year fixed effects. We can also see from the table that the firms added to the short-selling designated list tend to engage in aggressive tax avoidance activities to a lesser extent than the firms not allowed to be shorted. These findings are consistent with findings in prior literature (e.g., Guo, Chi, and Cook 2018; Kim, Lu, and Peng 2020), but differ from those in Luo et al. (2020), who used the commonly used *ETR* and *Cash ETR* to measure tax avoidance in China. Tang, Mo, and Chan (2017) argued that a limitation of the commonly used *ETR* measures is that they do not distinguish tax savings from tax preferences and aggressive tax reporting. From a practical perspective, this consideration is important when measuring tax avoidance for Chinese-listed firms. Chinese central and local governments offer tax concession policies to encourage investments that will aid the development of specific industries and regions. Under tax concession policies, Chinese listed firms benefit from favorable tax treatment and are subject to various applicable tax rates (ATRs) which refer to the statutory tax rates applicable to a firm after considering tax holidays, exemption, and tax rebates.[11] Therefore, firms' levels of *ETR* and *CETR* are likely attributable to tax concession policies rather than tax avoidance (Tang et al. 2017). Without stripping off the influence of tax concession policies, the conclusion in Luo et al. (2020), based on the use of inappropriate measurement of tax avoidance in China, that short selling encourages tax avoidance, is not

[10] The untabulated results on the Pearson correlations are available upon request.

[11] The varying ATRs arise from the numerous preferential tax policies for specific firms, regions, industries, etc (Shevlin et al. 2012; Tang et al. 2017). For example, the statutory tax rate applicable to a listed firm is generally 25%, but the tax rate could be reduced to 15% or lower for qualifying enterprises that are engaged in industries encouraged by the Chinese government, such as new/high-tech enterprises and certain integrated-circuit-production enterprises.

reliable. In our robustness tests, we modify the *ETR* and *CETR* by taking into account the effect of tax concession policies, and present our results in Section 3.3.6. Further, we discuss the differences in data selection, variable construction, and research methods between our study and Luo et al. (2020), and present results generated from tests based on the specifications of Luo et al. (2020) in Section 5.

The control variables generally exhibit signs consistent with our predictions, specifically that *ROA*, *Mkt Cap*, and *Traded Value* are positively associated with tax avoidance. Overall, our results show that short sellers provide monitoring and disciplinary effects that discourage managers from engaging in tax avoidance activities.

[Insert Table 3 about here]

To shed further light on the dynamics of the average treatment effect from before to after the introduction of the short-selling pilot scheme, we replace *Shortable* with the indicator variables *Before, Current,* and *After*. The model allows us to control for pre-existing trends, estimate the average dynamic effects of the non-transitory short-selling mechanism, and account for discrete revisions of the designated list during our sample period. We estimate the following model:

$$Tax\ aggressiveness_{i,t} = \alpha + \beta_1 Before^1_{i,t} + \beta_2 Before^2_{i,t} + \beta_3 Current_{i,t} + \beta_4 After^1_{i,t} + \beta_5 After^2_{i,t} + \beta_6 After^3_{i,t} + \gamma\ Controls + \varepsilon_{i,t} \quad (2)$$

where $Before^t$ is an indicator for whether the observation for firm i in year t occurs (2 years, 1 year) before firm i is allowed to short sell its stock. Similarly, $After^t$ is an indicator for whether the observation for firm i in year t occurs (1 year, 2 years, 3 years) after firm i is allowed to short sell its stock. *Current* is an indicator for whether the observation for firm i in year t occurs in the year in which the firm was added onto the designated short-selling list.

The coefficients on these indicator variables provide estimates of the average changes in the level of tax aggressiveness in the years immediately preceding and following the short-sale deregulation, and are illustrated in Table 4 and Figure 2. Each point on the graph reflects the average difference in the level of tax aggressiveness (measured by *TA_ETR* and *TA_CETR*) between pilot and non-pilot firms. The dashed lines that go through each hollow point represent the 95% confidence intervals.

[Insert Table 4 about here]

[Insert Figure 2 about here]

The most important characteristic displayed by both graphs in Figure 2 is that, throughout the sample period, the coefficients associated with *Before*^{2} and *Before*^{1} are not much different from zero, suggesting that the difference in tax aggressiveness between pilot firms and non-pilot firms is not significantly time-varying before the short-sale deregulation. The coefficients associated with *Current* and *After*^{1} to *After*^{3} are negative and significantly larger than zero, implying a strong suppressing effect of short selling on tax aggressiveness after the deregulation. The results address our concern over pre-existing tax aggressiveness trends and provide evidence that further solidifies the causal interpretation of the estimated treatment effect.

3.3 Robustness tests

3.3.1 Controlling for various firm characteristics and the influence of year and industry effects

One assumption associated with applying Equation (1), the standard DID design, to our sample data is that the treatment and control samples do not differ significantly in terms of firm characteristics over time. If the treatment and control samples became dissimilar in those characteristics over time or in one or some particular year(s), the results generated by the standard DID design could be problematic. Given this, adopting the method of Bertrand and Mullainathan (1999), we create a set of interactions between the control variables and the year dummy variables, and include them in Equation (1) to allow firm characteristics to change over time. The coefficients associated with the firm characteristics and the tax-related variables differ from one year to another and thus can control for any unknown changes in firm characteristics across the sample period. Columns (1) and (3) of Panel A, Table 5, present the estimated treatment effect results. The coefficients on *Shortable* remain significantly negatively associated with both *TA_ETR* and *TA_CETR*.

Another potential problem with DID is that there may be a confounding of the dynamic effects of short selling with pre-existing differences in time trends across the treatment and control groups (Moser and Voena 2012). We extend our baseline regressions by including a linear time trend for all of the treatment group observations for the pre-deregulation period, i.e. $Pilot \times t$, where *Pilot* equals 1 if firm i belongs to the treatment group (i.e. the firm is on the short-selling designated list), otherwise 0; and t is the year indicator. The results of this analysis are reported in Columns (2) and (4) of Panel A of Table 5 and confirm that the treatment effect on tax aggressiveness remains robust, even controlling for pre-existing time trends.

Finally, we also include interactions between the year dummy variables and industry dummy variables, and between the year dummy variables and province dummy variables, respectively, in Equation (1). These inclusions allow our industry dummies and region dummies to vary over the sample period and therefore allow us to control for aggressive tax avoidance behaviors driven by time-varying factors at the industry and region levels. The results are presented in Panel B of Table 5. Comparing them to the results in Table 3, we can see that our estimate of the treatment effect of the short-selling deregulation remains virtually unchanged.

[Insert Table 5 about here]

3.3.2 Propensity score and entropy-balanced matching

Unlike the situation in the U.S., where, under the Regulation SHO, stocks are randomly selected for short selling, China's shortable stocks must meet criteria set by the CSRC (Chang et al. 2014; Sharif et al. 2014). Therefore, to address potential selection bias arising from the CSRC's criteria, we use the PSM method to match a control sample with the treatment sample based on observable firm characteristics. We then apply the DID method to compare changes in tax aggressiveness among the treatment firms to changes in tax aggressiveness among the control firms during our sample period.

According to the 'Measures for Administration of the Margin Trading and Short Selling Business of Securities Companies' issued by the CSRC, firms that are eligible for short selling must meet criteria regarding earnings performance, liquidity, volatility, trading volume, and so on. We therefore match the treatment sample and the control sample based on the following variables reflected in those guidelines: *Year, Size, Leverage, ROA, MB, CAPEX, PP&E, Intan, NOL, Mkt Cap, Traded Value, PE,* and *Turnover*. See Appendix 1 for definitions of all variables.

To generate the propensity score, we run a logit model that includes the 12 selected criteria variables that potentially affect firms' eligibility for short selling in each sample year. We then use the predicted probabilities to calculate the propensity score and use one-to-one nearest-neighbor matching to match our treatment and control samples. This process results in 10,998 firm-year observations. We conduct tests for differences in means and medians using matched-pair t-tests and Wilcoxon rank tests, respectively. Panel A of Table 6 reports the results of the tests. We find that the mean and median values of most variables used in the logit model (9 of the 12 variables) are not significantly different between the treatment and the

control samples, suggesting that our treatment and control samples are reasonably similar in firm characteristics. We then apply our matched observations to Equation (1), our baseline DID model, to compare short selling's impact on tax aggressiveness between the two samples. Panel B of Table 6 reports the negative and highly significant coefficients for *Shortable*, consistent with the results reported in Table 3.

[Insert Table 6 about here]

Although PSM is a popular approach for addressing endogeneity concerns, this matching technique is not without its weaknesses. PSM can be sensitive to a number of design choices, such as matching criteria, calliper distance, and whether to match with or without replacement, and it also eliminates observations that may lack counterfactuals. In our PSM sample, we find no significant difference in majority of the firm characteristics between the pilot firms and PSM control firms. However, we notice that there are significant differences in *Size, Mkt Cap,* and *Traded Value* between these two groups. Given these arguments, following Wilde (2017), we use entropy balancing to assemble a new control sample that exhibits covariate balance with the shortable pilot firms. Entropy-balance matching is a technique that weights control sample units to achieve covariate balance between the pilot and non-pilot firm groups, without requiring design choices that could affect the composition of the matched sample (Wilde 2017). It allows observation weights to vary smoothly and, in this way, we can retain the complete sample for regression analysis. We then perform OLS on the entropy-balance matched sample and report the regression results in Panel C of Table 6. We find the coefficients on *shortable* negative and significant, confirming that the short-sale deregulation has reduced the pilot firms' levels of aggressive tax planning.

3.3.4 Placebo tests

We perform two types of placebo tests to examine whether our results are driven by random correlations between explanatory variables, rather than the short-selling mechanism. We begin by randomly assigning firms to the placebo treatment to examine whether the treatment effect is exclusive at the firm level. We then randomize the shortable timing of the treatment firms to examine whether the treatment effect is exclusive at the time level. We re-run the basic model 1,000 times and plot the distribution of the treatment effect estimators and related *t*-statistics in Figure 3. The results show that most of the placebo treatment effects are not statistically or

economically significant, which implies that random correlations between the explanatory variables do not explain the short-selling effects.

[Insert Figure 3 about here]

3.3.5 Controlling for intensity of treatment

Although our previous test results provide corroborative evidence that short selling has a significant constraining effect on corporate tax aggressiveness, we do not test whether different levels of short interest impose different constraining effects on individual firms. To identify the effect of firm-level short interest, we estimate the following regression based on a sample consisting only of shortable observations:

$$Tax\ aggressiveness_{i,t+1} = \alpha + \beta Short\ Interest_{it} + \gamma_m Control\ variables_{i,t} + Firm_i + Year_t + \varepsilon_{i,t} \quad (3)$$

where $Short\ Interest_{i,t}$ measures a firm's short interest in year t, calculated as short sales divided by the market value of the firm's tradable shares. We test the level of short interest on one-year-lead tax aggressiveness to shed light on the issue of causality.[12] Table 7 reports the results. The coefficients on $Short\ Interest_{i,t}$ are all negative and statistically significant, indicating that a higher level of short interest imposes a stronger constraining effect on tax aggressiveness. Note also that the results in Columns (3) and (4) show that this constraining effect remains after controlling for the influence of margin purchase.

[Insert Table 7 about here]

3.3.6 Alternative tax aggressiveness measures

Besides the tax aggressiveness measures discussed in section 2.2, we use four alternative measures of corporate tax aggressiveness to examine the robustness of our results, namely, *BTD*, book income less taxable income scaled by total assets; *DDBTD*, a component of *BTD* where accrued profit effects are excluded; *METR,* the ratio of the effective tax rate to the applicable tax rate; and *MCETR*, the ratio of the cash effective tax rate to the applicable tax rate.

[12] Our untabulated results on tax avoidance in year t are consistent with the results on tax avoidance in t+1.

Firstly, following Kim et al. (2011) and Huang, Ying and Shen (2018), we use book-tax differences (*BTD*) to measure aggressive tax avoidance, calculating it as follows:

$$\frac{\textit{pretax book income-}\ \frac{\textit{income tax expense} - \textit{deferred tax}}{\textit{ATR}}}{\textit{total assets}}$$

where $\frac{\textit{income tax expense} - \textit{deferred tax}}{\textit{ATR}}$ is the taxable income, and *ATR* is the statutory tax rate applicable to a listed firm after considering tax rebates and tax preferences, and it accounts for tax concessions (Tang et al. 2017). A high *BTD* indicates a large difference between pretax book income and taxable income, and therefore a high level of tax avoidance.

Secondly, because a high BTD may be attributable to the upward management of earnings, we compute, in accordance with Desai and Dharmapala (2006), the abnormal book-tax differences by regressing the total book-tax differences on total accruals (*TACC*). Here, total accruals are a proxy for earnings management, and we use them in the regression below to capture the influence of earnings management on *BTD*. We calculate *TACC* as income before extraordinary items less net operating cash flows adjusted for discontinued operations and extraordinary items, scaled by lagged total assets.

$$BTD_{i.t}=\alpha TACC_{i,t}+u_i+\varepsilon_{i,t}$$

The residual u_i is the average value of the residuals for firm *i* over the sample period of 2008 to 2018, and $\varepsilon_{i,t}$ is the deviation in year *t* from firm *i*'s average residual u_i. The sum of u_i and $\varepsilon_{i,t}$, denoted by *DDBTD*, is the component of *BTD* that is attributable to a firm's tax avoidance. A higher value of *DDBTD* indicates greater tax aggressiveness.

Furthermore, we follow Chen and Lin (2017) and use *ETR* and *CETR* as our third and fourth alternative proxies for aggressive tax avoidance. However, using the *ETR* and *CETR* measures of corporate tax avoidance in the Chinese setting presents a concern, since the two measures capture both tax avoidance and tax concession policy effects. The tax concession policies offered by the Chinese central and local governments are designed to encourage investments that will aid the development of specific industries and regions. By benefitting from tax concession policies, Chinese listed firms can enjoy favorable tax treatment and are subject to various applicable tax rates (*ATRs*). Therefore, firms' levels of *ETR* and *CETR* are likely attributable to tax concession policies rather than tax avoidance (Tang et al. 2017). In order to strip off the influence of tax concession policies and more accurately measure corporate tax aggressiveness in the Chinese setting, we modify the *ETR* and *CETR* measures,

dividing them by *ATR*. Specifically, we calculate *METR* and *MCETR* as $\frac{ETR}{ATR}$ and $\frac{CETR}{ATR}$, respectively. A low (high) *METR* (or *MCETR*) indicates a high (low) level of tax aggressiveness. The results of the DID models that use the alternative tax aggressiveness measures as dependent variables are reported in Table 8. We find that the results are robust to using these four alternative tax aggressiveness measures.

[Insert Table 8 about here]

4. Additional tests

4.1 Cross-sectional analysis

In this section, we conduct additional analyses to investigate the cross-sectional variation in the relation between the introduction of the short-selling scheme and tax aggressiveness. Table 9 presents the results of these analyses. Advertising and marketing activities help shape a firm's image and reputation among customers and the public. A firm's reputation is one of the most important intangible assets contributing to its performance. Austin and Wilson (2017) find that firms with valuable brands and good reputations tend to engage in less tax avoidance. The benefits of advertising diminish if the firm is viewed as, to use a colloquialism, a greedy tax dodger, and this negative impact is of greater concern for a firm whose advertising and marketing expenses are high (Mansi, Qi, and Shi 2020). The presence of short sellers increases the likelihood of revelation of a firm's aggressive tax avoidance, and of the punishment for perceived corporate greed. Thus, firms that spend higher sums on advertising tend to be those that care more about their reputation and public image and are therefore less likely to risk their reputation and public image by engaging in aggressive tax avoidance activities under the surveillance of short sellers. Consequently, we predict that the negative effect of short selling on tax aggressiveness will be more pronounced for firms with greater advertising expenditure.

To test this prediction, we modify the baseline model by adding the following to it: a dummy variable, *High Adv*, representing firms that spend considerable sums on advertising, and, as a second additional explanatory variable, the interaction of *High Adv* with *Shortable*. *High Adv* equals 1 if a firm's advertisement expense relative to its operating income is greater than the median ratio for other firms in the same year and industry, and 0 otherwise. In Columns (1) and (2) of Table 9, where the measures of aggressive tax avoidance are *TA_ETR* and *TA_CETR*, we find significantly negative coefficients on the interaction terms. The results

suggest that the mitigating effect of short selling on tax aggressiveness is stronger for firms that spend more on advertising.

We next examine the effect of tax law enforcement on the relationship between short-sale deregulation and tax aggressiveness. Weak tax law enforcement implies that illegal tax avoidance is less likely to be detected and that the costs of aggressive tax avoidance are relatively low[13]. However, external monitoring pressures exerted by short sellers can substitute for weak law enforcement. If the substitution effect is strong, increased short-selling pressure will decrease tax aggressiveness. Because tax law enforcement varies across regions in China, we conjecture that the effect of short selling on tax aggressiveness will be more pronounced for firms located in weaker tax law enforcement regions. We thus incorporate the indicator variable, *Low TE*, indicating firms located in weaker tax law enforcement provinces, and its interaction with *Shortable,* in the baseline model. Columns (3) and (4) of Table 9 presents the results of this test. The coefficients for *Shortable* × *Low TE* are significantly negative for both tax aggressiveness measures, *TA_ETR* and *TA_CETR*, suggesting that the effect of short-sale deregulation on tax avoidance is more pronounced for firms located in weaker tax law enforcement provinces. The results imply that external short-selling monitoring pressures can substitute for weak law enforcement.

Furthermore, we examine whether institutional ownership affects the association between short-selling pressure and tax aggressiveness. Large institutional investors have more incentive and capability to influence managerial actions to increase shareholders' share of earnings (Monks and Minow, 1995; Shleifer and Vishny, 1997). Bird and Karolyi (2017) and Khan et al. (2017) document that managers are more likely to engage in aggressive tax planning to improve after-tax performance in firms with high levels of institutional ownership. Therefore, we predict that under short sellers' monitoring and intervention, institutional investors are less likely to push firms' managers to increase after-tax income through aggressive tax planning, preventing firms' reputation and market loss. Similarly, we augment Equation (1) by adding *High IO* and its interaction with *Shortable*, and report the results of estimating this augmented equation in columns (5) and (6) of Table 9. The coefficients on the interaction terms are negative and statistically significant, suggesting that the discouraging effect of short-selling pressure on tax aggressiveness is more pronounced for firms with institutional holdings above the sample median. The results indicate that monitoring and pressure from short sellers reduce

[13] Some examples of weak enforcement include a lack of manpower, insufficient training and skills, an ineffective management system in the tax-collecting agency, etc. See Cai and Liu (2009) for more details.

incentives for institutional investors to encourage firms to generate additional cash flow through aggressive tax avoidance activities.

Lastly, we attempt to examine whether the monitoring and disciplinary pressure of short selling is more pronounced in firms with poor governance. From an agency perspective, CEO duality reduces a board's effectiveness in monitoring and disciplining management (Mallette and Fowler 1992; Finkelstein and D'Aveni 1994), and is often considered a sign of poor corporate governance. We, therefore, modify the baseline model by adding the interaction between *Shortable* and *Dual,* to test whether the short-selling mechanism more effectively reduces aggressive tax avoidance for firms with poor corporate governance, where *Dual* equals 1 if the CEO also occupies the position of chair of the board, and 0 otherwise. The results are reported in Columns (7) and (8) of Table 9. The coefficients on the interaction terms are negative and statistically significant, suggesting that the discouraging effect of short-selling pressure on tax aggressiveness is more pronounced for firms with CEO duality. This result supports our conjecture that the monitoring and discipline exerted by short sellers can substitute for weak corporate governance.

[Insert Table 9 about here]

4.2 Mediation analysis

Our previous analysis shows that short selling has mitigating effects on tax aggressiveness. We are now interested in investigating whether the effects are direct or transmitted through other (mediating) mechanisms. As discussed previously, short sellers are informed and sophisticated traders who have the incentive and capability to discover and publicize any misconduct and negative information about the firms they short. Therefore, short-selling pressure could increase audit risk and effort, help the media publicize negative news about the firm, and lead to a high following of financial analysts. The increased external pressure exerted by auditors, media, and financial analysts will discourage the firm from undertaking opportunistic behavior, such as aggressive tax avoidance. Previous tax studies have documented that auditor efforts (Kanagaretnam et al. 2016), media coverage (Chen, Schuchard and Stomberg 2019), and analyst coverage (Allen et al. 2016; Chen and Lin 2017) have significant impacts on corporate tax aggressiveness. Thus, we further explore the indirect effect of the presence of short sellers on aggressive tax avoidance by examining three mediating mechanisms: audit effort, media coverage, and analyst following. We use the following system of equations to perform our mediation analysis:

$$Mediator_{i,t}=\alpha+\delta Shortable_{i,t}+\lambda Control\ variables_{i,t}+Firm_i+Year_t+v_{i,t} \quad (4)$$

$$Tax\ aggressiveness_{i,t}=\alpha+\varphi Mediator_{i,t}+\beta Shortable_{i,t}+\gamma Control\ variables_{i,t}+Firm_i+Year_t+\varepsilon_{i,t} \quad (5)$$

where *Mediator* is one of three mediators (i.e. *Abnormal Audit Fee*, *Negative News*, or *Analyst Following*). Consistent with prior research (Donahoe and Knechel 2014; Lobo and Zhao 2013; Srinidhi and Gul 2007), we use the estimated *Abnormal Audit Fee* as our proxy for audit effort. *Negative News* is calculated as the ratio of the number of negative news items to the total number of news items in a given year (Garcia-Sanchez et al. 2014; Reverte 2009). *Analyst Following* is defined as the natural logarithm of the number of financial analysts following the firm in a given year (Allen et al. 2016). If the estimated δ and φ are statistically significant, while β is statistically insignificant, then it will indicate that the mediating variables have a full mediating effect, meaning that the effect of *Shortable* on *Tax aggressiveness* is indirect and completely transmitted through the mediators. However, if δ, φ, and β are all significant, then this will suggest that the total effect of *Shortable* on *Tax aggressiveness* is comprised of a direct effect of *Shortable* on *TA_ETR (TA_CETR*), and an indirect effect of *Shortable* on *TA_ETR (TA_CETR)* conveyed by the mediating variables. The detailed definitions and calculations of the related variables are presented in Appendix 1.

Table 10 reports the results of the regression analysis, testing the mediating roles of *Audit Effort*, *Negative News*, and *Analyst Following* on the relationship between short-sale deregulation and tax aggressiveness. Columns (1) to (3) of the table correspond to the impacts of *Audit Effort*. In Column (1), the coefficient on *Abnormal Audit Fee* is positive and statistically significant, indicating that short-selling pressure increases auditors' efforts. The coefficients on *Abnormal Audit Fee* and *Shortable* in Column 2 (Column 3) are both negative and significant, suggesting that, in addition to a direct influence of *Shortable* on *Tax aggressiveness*, there is also an indirect effect of *Shortable* on *TA_ETR* (*TA_CETR*) conveyed by the mediating variable, *Abnormal Audit Fee.* Similarly, the results in Columns (4) to (9) show that short selling has a significant effect on the mediators (*Negative News* and *Analysts Following*), and that both mediators and short-selling pressures have a significant effect on the level of tax aggressiveness. These results suggest that the presence of short selling encourages the media to publicize negative news and thefinancial analyst following, which in turn discourage firms from engaging in aggressive tax avoidance. In summary, our results reveal that, in addition to the direct effect of short selling on aggressive tax planning, an indirect (mediating) effect through *Audit Effort*, *Negative News*, and *Analyst Following* also exists[14].

[14] We further conduct seemingly unrelated regressions (SUR) to test the equality of coefficients on *Shortable* in Equation (5)

[Insert Table 10 about here]

4.3 Loss of short-selling eligibility and tax aggressiveness

Our previous evidence suggests that, after firms are added to the short-selling designated list, their level of tax aggressiveness decreases. A natural question is whether firms exhibit a significant increase in tax aggressiveness after being removed from the short-selling designated list. The findings for these 'delisted' firms will provide corroborating evidence of the monitoring and disciplinary roles short selling plays in reducing corporate tax aggressiveness. Thus, we construct two new samples which include firms removed from the short-selling list during our sample period, to examine whether tax aggressiveness increases after a firm is taken off the list. The first sample includes all shortable firm-year observations and firm-year observations that were added to the short-selling list but removed later. In this case, we assign firms that were removed from the short-selling eligibility list to a treatment group and firms that were added to the short-selling list and never removed to a control group. We create an indicator variable, *Removed*, equal to 1 in the years when and after a firm was removed from the short-selling eligibility list and never re-joined the list, and 0 otherwise. The regression results based on the first sample are reported in Columns (1) and (2) of Table 11. We find that the coefficients on *Removed* are positive and significant, suggesting that the 'delisted' firms exhibit a significant increase in tax aggressiveness after their removal, compared with shortable firms. In addition, we construct another sample containing only firms taken off the short-selling list during the period after the short-selling deregulation was implemented[15]. As shown in Columns (3) and (4) of Table 11, the coefficients on *Removed* are positive and significant. This suggests that, after these firms are removed from the short-selling list, their tax aggressiveness increases significantly. The findings from these two samples provide corroborating evidence that short selling discourages corporate tax aggressiveness.

[Insert Table 11 about here]

and Equation (1). Our untabulated results show that the coefficients on *Shortable* in Equation (1) are significantly larger than those in Equation (5), suggesting the mediating effect is not negligible.

[15] In this subsample, we require firms to have two or more observations before and after the 'delisted' treatment and to never re-join the short-selling designated list over the sample period. We also drop the firm-year observations of firms before they were first added to the list. The final subsample consists of 225 firm-year observations for 42 firms.

5. Replicating the LNT paper

In our empirical analysis, we find that the short-sale deregulation in China discourages the pilot firms from engaging in aggressive tax avoidance, supporting the monitoring view of short selling. However, LNT claimed that the deregulation of short sales significantly increases firms' tax avoidance. Therefore, in this section, we attempt to replicate LNT's main analysis, following the steps and information disclosed in their paper, and demonstrate that their findings can be challenged on methodological grounds.

5.1 The measurement of tax avoidance

LNT claimed that, following Edwards et al. (2016), one of their primary measures of tax avoidance, the cash effective tax rate (*CASH_ETR*), is calculated as (total income tax expense – deferred tax expense) / pre-tax profit. However, Edwards et al. (2016) defined *CashETR* as the ratio of *cash taxes paid* to *pre-tax income* adjusted for special items, and not *income tax expenses* as LNT did. The difference is that income tax expenses only represent tax expenses and cannot fully reflect actual cash tax paid due to the accrual accounting method. Therefore, LNT's measure is not an appropriate measure for *CashETR*. In our replicated analysis, we construct a more appropriate measure than LNT's for cash income taxes by taking into account that cash income taxes paid is a voluntary disclosure item for Chinese listed companies.

Moreover, Tang et al. (2017) pointed out that one limitation of the commonly used ETR tax avoidance measures is that they do not distinguish tax savings from tax preferences and aggressive tax reporting. However, it is important to take this limitation into account when conducting studies in the Chinese setting, because Chinese listed firms are subject to "the varying applicable tax rates (*ATR*s) that arise from the numerous tax preferential policies for specific firms, regions, industries, etc" (Shevlin et al. 2012; Tang et al. 2017). For example, the statutory tax rate applicable to a listed firm is generally 25%, but the tax rate could be reduced to 15% or lower for qualifying enterprises that are engaged in industries encouraged by the Chinese government, such as new/high-tech enterprises and certain integrated-circuits-production enterprises. Therefore, *ETR* measures in the Chinese setting should be adjusted by the *ATR*s to reflect the extent to which a firm's actual tax burden diverges from its *ATR*. However, LNT ignored this important issue when constructing their primary measures of tax avoidance. Finally, truncating the ETR-based tax avoidance measures into the range [0,1] is a common practice in prior tax avoidance literature (e.g., Bradshaw et al. 2019; Chen et al. 2019; Hasan et al. 2017; Dyreng et al. 2010). However, whether LNT's measures were truncated to

this range was unclear, as the minimum and maximum values of the key variables were not reported in their study. Given the above discussion, we believe that LNT's tax avoidance measures are not appropriate and do not suit the Chinese setting.

5.2 Control variables

The LNT models may be subject to the omitted variables problem. Their regression models did not include some important firm characteristics, such as capital expenditure (*CAPEX*), capital intensity (*PP&E*), intangible assets (*Intan*), and net loss carryforward (*NOL*), which are common firm attributes that impact corporate tax avoidance (see Bradshaw et al. 2019; Chen et al. 2019; Li et al. 2017; Hasan et al. 2017). The omitted variable issue will potentially cause bias in the estimation of the parameters of the regressions. Moreover, given the non-random selection of pilot firms in China, it is necessary to control for variables that are related to the criteria set forth by the CSRC for selecting shortable firms. According to the Quarterly Report of the Regular Adjustment to Shortable Stocks, the SSE and SZSE rank all non-shortable stocks based on their *PE* and a weighted index calculated as 2×(average market capitalization / the average market capitalization of all A-shares) + (average trading value / the average trading value of all A-shares), and then add the stocks with the highest values to the short-selling eligibility list. Hence, to account for the selection criteria, market capitalization (*Mkt Cap*), trading value (*Trade Value*), and price-earnings ratio (*PE*) should be included in the regressions.

5.3 Sample issues

In terms of the sample selection, one of the main differences between LNT and our study is that LNT excluded Chinese ST and *ST firms but we do not. In China, publicly listed firms that are experiencing financial distress or other abnormalities are required by the CSRC to use the prefix 'ST' in front of their trading stock code. The special treatment system is just used to provide an early warning signal of high delisting risk to both the firms and investors, and does not result in any different tax policies or treatment. Hence, recent Chinese tax studies include ST and *ST firms (e.g. Bradshaw et al. 2019; Tang et al. 2017; Bauer et al. 2020). Given this, by excluding this important group of observations, LNT's models may generate misleading parameter estimations.

Moreover, we notice that LNT's 1:4 PSM matched sample only consists of 3,858 firm-year observations (i.e. the treatment group may only have about 800 observations). Meanwhile, they indicate that there were 899 firms on the short-sale list before 4 December 2014. This

information suggests that either each firm, on average, has less than one firm-year observation over the sample period (2007-2015), or many of the pilot firms are not included in their treatment sample. The results derived from such a sample are highly likely to be biased. Therefore, the conclusion they drew (i.e. that short-sale deregulation induces firms to engage in more aggressive tax avoidance activities) is problematic. Finally, we also notice that there are some errors in the sample used by LNT. In their Table 2, the means of *ROA* and *Asset_turn* for the treatment and control groups are 0.76/0.63 and 102.83/97.54 respectively, which are far beyond the normal range. Meanwhile, the means of *ROA* and *Asset_turn* in their full sample are only 0.037 and 10.06, respectively. These mistakes indicate that their PSM procedure is flawed, and the results are not reliable.

5.4 Replicating the main results of LNT

In previous subsections, we have demonstrated that there are several issues that cast serious doubt on LNT's conclusions. Thus, we further replicate their analysis based on the replicated sample and their tax avoidance measures described in their paper. We carefully follow the LNT sample selection procedures and present a description of the sample selection steps in Panel A of Table 12. Our replicated sample contains 11,834 firm-year observations, as opposed to the 7,657 in LNT. Based on the difference in sample size, we believe that LNT did not disclose all sample selection steps in their paper and/or made mistakes in their sample selection process.

[Insert Table 12 about here]

Then, we replicate LNT's main results by re-running their baseline specifications (i.e. Table 3 in LNT) based on our replicated sample. The results are reported in Panel B of Table 12. Columns (1)-(4) report the results of LNT (i.e., Columns (2), (4), (6) and (8) of Table 3 in their paper). Our replicated results are presented in Columns (5)-(8) of Panel B. In contrast to LNT, all the coefficients on *Short* in our replicated regressions become positive and are not statistically significant, suggesting that the main findings of LNT are invalid. Given the empirical problems we noted above, we were skeptical and attempted to correct the errors in the LNT paper by choosing a more appropriate sample selection process and variables to re-evaluate the results. The related results are presented in Columns (1) and (2) of Table 8, where *shortable* is positively and significantly related to the modified ETR tax avoidance measures, suggesting that the short-sale deregulation significantly discourages corporate tax avoidance.

6. Conclusions

The CSRC has been gradually removing the short-selling ban on stocks since 2010. This gradual deregulation process provides a great opportunity to investigate the effect of short selling on firms' decisions. Our study examines how short selling affects aggressive tax planning and provides novel evidence that the introduction of short selling reduces firms' tax aggressiveness. We apply a DID approach to A-share firms listed on the Shanghai Stock Exchange and Shenzhen Stock Exchange and compare the change in tax aggressiveness amongst firms on the short-selling designated list (treatment sample) with that of firms not on the list (control sample) between 2008 and 2017. The results of this comparison indicate that, relative to non-shortable firms, shortable firms significantly reduced their aggressive tax avoidance, suggesting that short selling restrains corporate tax aggressiveness.

We also find that the effect of short selling on tax aggressiveness is more pronounced for firms with higher advertising intensity, and that are located in regions with weak tax law enforcement, and with higher institutional holdings. In addition, our mediation tests reveal that short selling not only directly, but also indirectly, through pressure from auditors, media, and financial analysts, reduces tax aggressiveness. Overall, our findings highlight the monitoring and disciplinary roles that short sellers play in determining the level of corporate tax aggressiveness, and suggest that the reduction in tax aggressiveness is likely driven by the external monitoring and disciplinary mechanism.

Finally, we provide a critique of LNT which claims that short selling encourages corporate tax avoidance. We demonstrate that LNT's empirical results and inference are derived from inappropriate handling of data and non-negligible measurement problems with the key variables that lead to a misunderstanding of the role of short selling. We are confident that our findings are sound.

Given that our study focuses on a regulatory setting where the selection of shortable firms is based on criteria set by regulators, and the deregulation of short-selling has followed a gradual process, documenting evidence on whether and how short selling restrains corporate tax behaviors has important policy implications. Our findings provide generalized but valuable guidance for countries and jurisdictions whose regulators and governments are considering launching short-selling schemes through a *gradual* and *selective* process.

Reference

Agrawal, A. K. 2013. "The impact of investor protection law on corporate policy and performance: Evidence from the blue sky laws." *Journal of Financial Economics*, 107 (2): 417–435.

Allen, A., Francis, B. B., Wu, Q., and Zhao, Y. 2016. "Analyst coverage and corporate tax aggressiveness." *Journal of Banking Finance*, 72: 84–98.

Almeida, H., Campello, M., and Weisbach, M. S. 2004. "The cash flow sensitivity of cash." *The Journal of Finance*, 59 (4): 1777–1804.

Angelis, D. D., Grullon, G., and Michenaud, S. 2017. "The effects of short–selling threats on incentive contracts: evidence from an experiment." *Review of Financial Studies*, 30 (5):1627–1659.

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

Aspara, J. and Tikkanen, H. 2011. "Corporate marketing in the stock market: The impact of company identification on individuals' investment behavior." *European Journal of Marketing*, 45 (9): 1446–1469.

Austin, C. R. and Wilson, R. J. 2017. "An examination of reputational costs and tax avoidance: evidence from firms with valuable consumer brands." *The Journal of the American Taxation Association,* 39 (1): 67-93.

Badertscher, B. A., Katz, S. P., and Rego, S. O. 2013. "The separation of ownership and control and corporate tax avoidance." *Journal of Accounting and Economics*, 56 (2): 228–250.

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

Balmer, J. M. T., Fukukawa, K. and Gray, E. R. 2007. "The nature and management of ethical corporate identity: a commentary on corporate identity, corporate social responsibility and ethics." *Journal of Business Ethics*, 76 (1): 7–15.

Bauer, A. M., Fang, J., Pittman, J., Zhang, Y., and Zhao, Y. 2019. "How aggressive tax planning facilitates the diversion of corporate resources: Evidence from path analysis." *Contemporary Accounting Research*: 1–32.

Beck, T., Levine, R., and Levkov, A. 2010. "Big bad banks? The winners and losers from bank deregulation in the United States." *The Journal of Finance*, 65 (5): 1637–1667.

Bertrand, M. and Mullainathan, S. 1999. "Is there discretion in wage setting? A test using takeover legislation." *The RAND Journal of Economics*, 30 (3): 535-554.

Bird, A. and Karolyi, S. A. 2015. "Governance and taxes: evidence from regression discontinuity." *The accounting review*, 92 (1): 29-50.

Black, E., Carnes, T. and Richardson, V. 2000. "The market valuation of corporate reputation." *Corporate Reputation Review*, 3 (1): 31–42.

Blouin, J. and L. Krull. 2016. "Does organizational form affect firms' foreign operations? The role of check–the–box on multinational tax planning." Working paper, University of Pennsylvania.

Boehmer, E. and Wu, J. 2013. "Short selling and the price discovery process." *Review of Financial Studies*, 26 (2): 287–322.

Boehmer, E., Jones, C. M., and Zhang, X. 2008. "Which shorts are informed?" *The Journal of Finance*, 63 (2): 491–527.

Boehmer, E., Jones, C. M., and Zhang, X. 2013. "Shackling short sellers: the 2008 shorting ban." *Review of Financial Studies*, 26 (6): 1363–1400.

Bohl, M. T., Reher, G., and Wilfling, B. 2016. "Short selling constraints and stock returns volatility: Empirical evidence from the German stock market." *Economic Modelling*, 58: 159–166.

Bradshaw, M., Liao, G., and Ma, Mark. 2019. "Agency costs and tax planning when the government is a major shareholder." *Journal of Accounting and Economics*, 67 (2–3): 255–277.

Bris, A., Goetzmann, W. N., and Zhu, N. 2007. "Efficiency and the bear: short sales and markets around the world." *The Journal of Finance*, 62 (3): 1029–1079.

Brown, T. J., Dacin, P. A., Pratt, M. G. and Whetten, D. A. 2006. "Identity, intended image, construed image, and reputation: an interdisciplinary framework and suggested terminology." *Journal of the Academy of Marketing Science*, 34 (2): 99–106.

Cai, H., and Liu, Q. 2009. "Competition and corporate tax avoidance: evidence from Chinese industrial firms." *The Economic Journal*, 119 (537): 764–795.

Campello, M. and Gao, J. 2017. "Customer concentration and loan contract terms." *Journal of Financial Economics*, 123 (1):108–136.

Chan, K. H., Mo, P. L. L. and Zhou, A. Y. 2013, "Government ownership, corporate governance and tax aggressiveness: evidence from China." *Accounting and Finance*, 53: 1029-1051.

Chang, E. C., Cheng, J. W., and Yu, Y. 2007. "Short–sales constraints and price discovery: evidence from the Hong Kong market." *The Journal of Finance*, 62 (5): 2097–2121.

Chang, E. C., Lin, T. C., and Ma, X. 2019. "Does short–selling threat discipline managers in mergers and acquisitions decisions?" *Journal of Accounting and Economics*, 68 (1): 101–223.

Chang, E. C., Luo, Y., and Ren, J. 2014. "Short–selling, margin–trading, and price efficiency: Evidence from the Chinese market." *Journal of Banking and Finance*, 48: 411–424.

Chen, J., Hong H., and Stein J. C. 2002. "Breadth of ownership and stock returns." *Journal of Financial Economics*, 66:171–205.

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

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

Chen, S., Schuchard, K., and Stomberg, B. 2019. "Media coverage of corporate taxes." *The Accounting Review*, 94 (5): 83-116.

Chen, T. and Chen, L. 2017. "Does information asymmetry affect corporate tax aggressiveness?" *Journal of Financial and Quantitative Analysis*, 52 (5): 2053-2081.

Cheng, A., Huang, H. H. , Li, Y., and Stanfield, J. 2012. "The effect of hedge fund activism on corporate tax avoidance." *The Accounting Review*, 87: 1493–1526.

Chi, S. S., Pincus, M., and Teoh, S. H. 2014. "Mispricing of book–tax differences and the trading behavior of short sellers and insiders." *The Accounting Review*, 89 (2): 511–543.

Christophe, S. E., Ferri, M. G., and Angel, J. J. 2004. "Short–selling prior to earnings announcements." *The Journal of Finance*, 59 (4): 1845–1876.

Chyz, J., Leung, S. C., Li, O., Rui, O. M. 2013. "Labor unions and tax aggressiveness." *Journal of Financial Economics*, 108: 675–698.

Cohen, L., Diether, K. and Malloy, C. 2007. "Supply and demand shifts in the shorting market." *The Journal of Finance*, 62: 2061–96.

Dechow, P. M., Hutton, A. P., Meulbroek, L., and Sloan, R. G. 2001. "Short–sellers, fundamental analysis, and stock returns." *Journal of Financial Economics*, 61 (1): 77–106.

Deng, X. and Gao, L. 2018. "The monitoring of short selling: evidence from China." *Research in International Business and Finance*, (43): 68–78.

Desai, H., Krishnamurthy, S., and Venkataraman, K. 2006. "On distinguishing between valuation and arbitrage motivated short selling." Working Paper.

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

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

Desai, M. A., Dyck, A. and Zingales, L. 2007. "Theft and taxes." *The Journal of Financial Economics*, 84 (3): 591–623.

Diamond, D. W. and Verrecchia, R. E. 1987. "Constraints on short–selling and asset price adjustment to private information." *Journal of Financial Economics*,18 (2): 277– 311.

Diether, K., Lee, K., and Werner, I. 2010. "Who blows the whistle on corporate fraud?" *The Journal of Finance*, 65 (6): 2213–2253.

Donahoe, M. P. and Knechel, W. R. 2014. "Does corporate tax aggressiveness influence audit pricing?" *Contemporary Accounting Research*, 31 (1): 284-308.

Drake, K. D., Hamilton, R., and Lusch, S. J. 2020. "Are declining effective tax rates indicative of tax avoidance? Insight from effective tax rate reconciliations." *Journal of Accounting and Economics*, 70 (1), 101317.

Dyck, I. J. A, Volchkova, N., Zingales, L. 2008. "The corporate governance role of the media: evidence from Russia." *The Journal of Finance*, 63: 1093–1135.

Dyreng, S. D., Hanlon, M., and Maydew, E. 2010. "The Effects of Executives on Corporate Tax Avoidance." *The Accounting Review*, 85 (4): 1163–1189.

Engelberg, J. E., Reed, A. V., and Ringgenberg, M. C. 2012. "How are shorts informed? Short sellers, news, and information processing." *Journal of Financial Economics*, 105 (2): 260–278.

Fang, V. W., Huang, A. H., and Karpoff, J. M. 2015. "Short selling and earnings management: A controlled experiment." *The Journal of Finance*, 71 (3): 1251–1294.

Firth, M., Fung, P., and Rui. O. M. 2007. "Ownership, two-tier board structure, and the informativeness of earnings — Evidence from China." *Journal of Accounting and Public Policy*, 26 (4): 463–96.

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

Frieder, L. and Subrahmanyam, A. 2005. "Brand perceptions and the market for common stock." *Journal of Financial and Quantitative Analysis*, 40 (1): 57–85.

Gallemore, J., Maydew, E., and Thornock, J. 2014. "The reputational costs of Tax avoidance." *Contemporary Accounting Research*, 31 (4): 1103–1133.

Garcia‐Sanchez, I. M., Cuadrado‐Ballesteros, B., and Sepulveda, C. 2014. "Does media pressure moderate CSR disclosures by external directors?" *Management Decision*, 52 (6): 1014‐1045.

Graham, J. R., and Tucker, A. L. 2006. "Tax shelters and corporate debt policy." *Journal of Financial Economics*, 81 (3): 563‐594.

Graham, J. R., Hanlon, M., Shevlin, T., and Shroff, N. 2014. "Incentives for tax planning and avoidance: Evidence from the field." *The Accounting Review*, 89 (3): 991–1023.

Grullon, G. S., Michenaud and J. Weston. 2015. "The real effects of short selling constraints." *Review of Financial Studies*, 28(6):1737–1767.

Grullon, G., Michenaud, S. and Weston, J. 2015. "The real effects of short-selling constraints." *Review of Financial Studies*, 28: 1737-1767.

Guenther, D. A., Wilson, R. J., and Wu, K. 2019. "Tax uncertainty and incremental tax avoidance." *The Accounting Review*, 94 (2): 229–247.

Guo, S., Chi, S., and Cook, K. 2018. "Short selling and corporate tax avoidance." *Advances in Taxation*: 1–28.

Gupta, S. and Newberry, K. 1997. "Determinants of the variability in corporate effective tax rates: evidence from longitudinal study." *Journal of Accounting and Public Policy*, 16: 1–34.

Hanlon, M. 2005. "The persistence and pricing of earnings, accruals, and cash flows when firms have large book–tax differences." *The Accounting Review*, 80: 137–166.

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

Hanlon, M. and Slemrod, J. 2009. "What does tax aggressiveness signal? Evidence from stock price reactions to news about tax shelter involvement." *Journal of Public Economics*, 93 (1): 126–141.

Hasan, I., Hoi, C. K., Wu, Q., and Zhang, H. 2017. "Does social capital matter in corporate decisions? Evidence from corporate tax avoidance." *Journal of Accounting Research*, 55 (3): 629–668.

Henry, T. R., Kisgen, D. J., and Wu, J. 2015. "Equity short selling and bond rating downgrades." *Journal of Financial Intermediation*, 24 (1): 89–111.

Higgins, D., Omer, T., Phillips, J. 2015. "The influence of a firm's business strategy on its tax aggressiveness." *Contemporary Accounting Research*, 32: 674–702.

Ho, K. 1996. “Short–sales restrictions and volatility: The case of the Stock Exchange of Singapore.” *Pacific–Basin Finance Journal*, 4: 377–91.

Hong, H. and Stein, J. C. 2003. “Differences of opinion, short–sales constraints, and market crashes.” *Review of Financial Studies*,16 (2): 487–525.

Hoopes, J. L., Mescall, D. and Pittman, J. 2012. “Do IRS audits deter corporate tax avoidance?” *The Accounting Review*, 87: 1603–1639 .

Huang, Lobo, Wang and Xie. 2016. “Customer concentration and corporate tax avoidance.” *Journal of Banking and Finance*, 72: 184–200.

Huang, W., Ying, T., and Shen, Y. 2018. “Executive cash compensation and tax aggressiveness of Chinese firms.” *Review of Quantitative Finance and Accounting*, 51 (4): 1-30.

Jones, C. M. and Lamont, O. A. 2002. “Short–sale constraints and stock returns.” *Journal of Financial Economics*, 66: 207–39.

Kanagaretnam, K., Lee, J., Lim, C. Y., and Lobo, G. J. 2016. “Relation between auditor quality and tax aggressiveness: Implications of cross-country institutional differences.” *AUDITING: A Journal of Practice and Theory*, 35(4):105-135.

Karpoff, J. M. and X. Lou, 2010, “Short sellers and financial misconduct,” *The Journal of Finance*, 65 (5):1879–1913.

Khan, M., and Lu, H. 2013. “Do short sellers front–run insider sales?” *The Accounting Review*, 88 (5): 1743–1768.

Kim, E. H., Lu, Y., and Zhang, P. 2020. “Short–selling pressure and aggressive corporate tax avoidance.” Working Paper.

Kim, J. B., Li, Y., and Zhang, L. 2011. “Corporate tax avoidance and stock price crash risk: firm–level analysis.” *Journal of Financial Economics*, 100: 639–662 .

Klassen, K. J. and Laplante, S. K. 2012. “Are US multinational corporations becoming more aggressive income shifters?” *Journal of Accounting Research*, 50 (5): 1245–1285.

Lanis, R. and Richardson, G. 2015. “Is corporate social responsibility performance associated with tax avoidance?”. *Journal of Business Ethics*, 127: 439–457.

Lev, B. and Nissim, D. 2004. “Taxable income, future earnings, and equity values.” *The Accounting Review*, 79: 1039–1074.

Lewellen, K. and L. Robinson. 2014. “Internal ownership structures of multinational firms.” Working paper, Dartmouth College.

Li, Z. S., Lin, B., Zhang, T., and Chen, C. 2018. "Does short selling improve stock price efficiency and liquidity? Evidence from a natural experiment in China." *The European Journal of Finance*, 24 (15): 1350–1368.

Lobo, G. J. and Zhao, Y. 2013. "Relation between audit effort and financial report misstatements: Evidence from quarterly and annual restatements." *The Accounting Review*, 88 (4):1385–1412.

Lu, L. Y., Yu, Y., and Zhang, L. 2018. "Short selling pressure and corporate social responsibility performance: Evidence from a natural experiment." Working Paper.

Luo, J., Ni, X., and Tian, G. G. 2020. "Short selling and corporate tax avoidance: Insights from a financial constraint view." *Pacific–Basin Finance Journal*, 61, 101323.

Mansi, S., Qi, J., and Shi, H. 2020. "Advertising and tax avoidance." *Review of Quantitative Finance and Accounting*, 54: 479–516.

Massa, M., Qian, W., Xu, W., and Zhang, H. 2015. "Competition of the informed: Does the presence of short sellers affect insider selling?" *Journal of Financial Economics*, 118 (2): 268-288.

Massa, M., Zhang, B., Zhang, H. 2015. "The invisible hand of short selling: Does short selling discipline earnings Management?" *Review of Financial Studies*, 28 (6):1701–1736.

McGuire, S., Omer, T., and Wang, D. 2012, "Tax avoidance: Does tax–specific industry expertise make a difference?" *The Accounting Review*, 87: 975–1003.

McMullin, J. L., and Schonberger, B. 2020. "Entropy-balanced discretionary accruals." *Review of Accounting Studies*, 25: 84–119.

Miller, E. M. 1977. "Risk, uncertainty, and divergence of opinion." *The Journal of Finance*, 32 (4): 1151-1168.

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

Monks, R., and N. Minnow. 1995. "Corporate Governance. " Cambridge, MA: Blackwell.

Morck, R., Yeung, B. and Yu, W. 2000. "The information content of stock markets: why do emerging markets have synchronous stock price movements?" *Journal of Financial Economics*, 58: 215–260.

Moser, P. and Voena, A. 2012. "Compulsory licensing: Evidence from the trading with the Enemy Act." *The American Economic Review*, 102 (1): 396-427.

Porcano, T. M. 1986. "Corporate tax rates: progressive, proportional, or regressive." *Journal of American Taxation Association*, 7 (2):17–31.

Rego, S. O. 2003. “Tax avoidance activities of U.S. multinational corporations.” *Contemporary Accounting Research*, 20: 805–833.

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

Reverte, C. 2009. “Determinants of corporate social responsibility disclosure ratings by Spanish listed firms.” *Journal of Business Ethics*, 88 (2): 351–366.

Rosenbaum, P. R. and Rubin, D. B. 1984. “Reducing bias in observational studies using subclassification of the propensity score.” *Journal of the American Statistical Association*, 79: 516–524.

Rusinova, V. and Wernicke, G. 2019. “Short selling and performance on corporate social responsibility: Evidence from a natural experiment.” In G. Atinc (Ed.), *Proceedings of the Seventy–ninth Annual Meeting of the Academy of Management*: 25–30.

Saffi, P. and Sigurdsson, K. 2011. “Price efficiency and short selling.” *Review of Financial Studies*, 24: 821–852.

Scholes, M., Wilson, P., and Wolfson, M. 1992. “Firms’ responses to anticipated reductions in tax rates: The Tax Reform Act of 1986.” *Journal of Accounting Research*, 30 (Suppl.): 161–191.

Sharif, S., Anderson H., and Marshall, B. 2014. “Against the tide: the commencement of short selling and margin trading in mainland China.” *Accounting and Finance*, 54:1391–1355.

Shleifer, Andrei, and Robert W Vishny. 1997. “A Survey of Corporate Governance.” *Journal of Finance*, 52 (2): 737-783.

Shulman, D. 2009. Speech to the National Association of Corporate Directors Governance Conference, October 19.

Slemrod, J. 2004. “The economics of corporate tax selfishness.” *National Tax Journal*, 57 (4): 877 – 899.

Srinidhi, B. N., and Gul, F. A. 2007. “The differential effects of auditors' nonaudit and audit fees on accrual quality.” *Contemporary Accounting Research*, 24 (2): 595–629.

Tang, T., Mo, P. L. L., and Chan, K. H. 2017. “Tax collector or tax avoider? An investigation of intergovernmental agency conflicts.” *The Accounting Review*, 92 (2): 247–270.

Wang, N., Wang, L., Zhang, L., and Yu, Y. 2020. “Tax shelters, reputational costs and CEO turnover: Evidence from tax-violating enterprises in China.” *Emerging Markets Finance and Trade*: 1-20.

Watts, R., and J. Zimmerman. 1986. “Positive accounting theory.” Englewood Cliffs, NJ: Prentice Hall.

Wilde, J. H. 2017. “The deterrent effect of employee whistle blowing on firms’ financial misreporting and tax aggressiveness.” *The accounting review*, 92(5): 247-280.

Wilson, R. 2009. “An examination of corporate tax shelter participants.” *The Accounting Review*, 84: 969–999.

Appendix

Appendix 1 Variable definitions

Variable	Definition
Measures of tax aggressiveness	
TA_ETR	The mean of the firm's peers' *ETR* minus the firm's *ETR*, where *ETR* is total income tax expense divided by pretax income. The peer firms are those in the same decile of total assets, the same industry, and the same province. Source: CSMAR database.
CETR	Current income tax expenses plus the beginning-of-year income taxes payable minus the end-of-year income taxes payable.
TA_ CETR	The mean of the firm's peers' C*ETR* minus the firm's C*ETR*, divided by pretax income. The peer firms are those in the same decile of total assets, the same industry, and the same province. Source: CSMAR database.
BTD	Book-tax difference, equals book income less taxable income scaled by total assets. Taxable income is calculated as current income tax expense (= total income tax expense – deferred tax expense) divided by *ATR*.
DDBTD	The residual of the following firm fixed effects regression: $BTD = \alpha_1 TACC_{i,t} + \mu_i + \varepsilon_{i,t}$ (Desai and Dharmapala 2006). *TACC* is total accruals measured using Hribar and Collins' (2002) cash flow method, divided by total assets.
METR	Modified *ETR*, the ratio of *ETR* (total income tax expense divided by pretax income) to *ATR* (applicable tax rate). Source: *ATR* is collected from WIND database; others are collected from CSMAR database.
MCETR	Modified *CETR*, the ratio of *CETR* (cash income tax paid divided by pretax income) to *ATR* (applicable tax rate). Source: *ATR* is collected from WIND database; others are collected from CSMAR database.
Independent variables of interest	
Pilot	An indicator variable equal to 1 if a firm was added to the shortable stocks list and never excluded from that list during 2010–2017. Source: iFinD database.
Shortable	An indicator variable equal to 1 in the years when a firm's stocks were allowed to be short sold, otherwise 0. Source: iFinD database.
Short Interest	The ratio of the annual shorted shares to the outstanding shares. Source: iFinD database.
Margin Purchase	The ratio of the annual margin purchased volume to the annual turnover. Source: iFinD database.
Removed	An indicator variable equal to 1 in the years when a firm was removed from the short-selling eligibility list, and otherwise 0. Source: iFinD database.
Control variables in baseline regression	
Size	Firm size, measured by the natural logarithm of the book value of total assets at the end of the year. Source: CSMAR database.
Leverage	Financial leverage, measured by total debt divided by total assets at the end of the year. Source: CSMAR database.
ROA	Return on assets: operating income divided by total assets at the end of the year. Source: CSMAR database.
MB	Market to book ratio: the sum of the market value of equity at the end of the year divided by the book value of equity at the end of the year. Source: CSMAR database.
CAPEX	Capital expenditure divided by total assets at the end of the year. Source: CSMAR database.
PP&E	Net property, plant, and equipment divided by total assets at the end of the year. Source: CSMAR database.
Intan	Total intangible assets divided by total assets at the end of the year. Source: CSMAR database.
NOL	The accumulated pretax losses divided by total assets in the prior five years; set to 0 if the accumulated earnings in the prior five years are positive. Source: CSMAR database.
Mgmt Own	An indicator variable equal to 1 if the management has equity ownership, otherwise 0. Source: CSMAR database.
Dual	An indicator variable equal to 1 if the CEO is also the chairman of the board of directors, otherwise 0. Source: CSMAR database.
Mkt Cap	The ratio of a firm's market capitalization to the total A-share market capitalization of the firm's exchange. Source: CSMAR database.
Trade Value	The ratio of a firm's average trading value to the average trading value of all A-shares. Source: CSMAR database.
PE	Price-earnings ratio, calculated as the closing price on the last day of the year divided by EPS. Source:

CSMAR database.

Turnover The ratio of average daily trading volume to the average number of shares outstanding. Source: CSMAR database.

Other variables

High Adv An indicator variable equal to 1 if a firm's *Adv* (ratio of advertisement expense to its operating income) is above the median *Adv* over other firms in the same year and same industry. Source: CSMAR database.

Low TE An indicator variable equal to 1 if a firm is located in a province with *TE* below the median *TE* over other provinces in the same year, where *TE* is calculated as the actual tax ratio (regional tax revenue scaled by regional GDP) divided by the fitted tax ratio estimated from the following regression:

$$\frac{Tax\ Revenue_{i,t}}{GDP_{i,t}} = \alpha + \beta_1 \frac{Primary\ Industry_{i,t}}{GDP_{i,t}} + \beta_2 \frac{Second\ Industry_{i,t}}{GDP_{i,t}} + \beta_3 \frac{Imports\ and\ Exports_{i,t}}{GDP_{i,t}} + \varepsilon_{i,t}.$$

Tax Revenue is the tax revenue of a firm's province. *Primary Industry*, *Second Industry*, and *Imports and Exports* are the local GDP from primary industry, second industry, and imports and exports. Source: National Bureau of Statistics of China.

High IO An indicator variable equal to 1 if a firm's *IO* (the ratio of institutional ownership) is above the median *IO* over other firms in the same year and same industry. Source: CNRDS database.

Abnormal Audit Fee The residuals from the following model:

$$Audit\ Fee_{it} = \alpha + \beta_1 Size_{i,t} + \beta_2 ROA_{i,t} + \beta_3 Leverage_{i,t} + \beta_4 Big4_{i,t} + \beta_5 Audit\ Type_{i,t} + \beta_6 Loss_{i,t} + \beta_7 MB_{i,t} + \beta_8 Tenure_{i,t} + \beta_9 REC_{i,t} + \beta_{10} INV_{i,t} + \beta_{11} Merger_{i,t} + \beta_{12} Restated_{i,t} + \varepsilon_{i,t}.$$

Audit Fee is a firm's total audit fee. *Big*4 is an indicator variable equal to 1 if a firm's auditing firm is one of the big four accounting firms, otherwise 0. *Audit Type* is an indicator variable equal to 1 if the firm receives an unqualified opinion, otherwise 0. *Loss* equals 1 if the firm has a negative net profit, otherwise 0. *Tenure* equals 1 if the current auditor has provided an auditing service for the firm for no more than three years, otherwise 0. *REC* and *INV* are accounts receivable and inventory values, respectively, scaled by total assets. Merge equals 1 if a firm has an acquisition, otherwise 0. Restated equals 1 if a firm's annual report of the prior year is restated, otherwise 0. Source: CSMAR database.

Negative News Ratio of number of negative news items to total number of news items. Source: CNRDS database.

Analyst Following The natural logarithm of the number of analyst teams following the firm. Source: CSMAR database.

Appendix 2 Short-sale deregulation timeline during the sample period

Event dates	No. of stocks added	No. of stocks removed	No. of stocks on the list	Percentage of total A-share firms
31/03/2010	90		90	4.36
01/07/2010	5	5	90	4.36
29/07/2010	1	1	90	4.36
05/12/2011	189	1	278	11.87
31/01/2013	276	54	500	20.09
06/03/2013-03/05/2013		6	494	19.85
16/09/2013	206		700	28.12
28/03/2014-05/05/2014		5	695	26.60
22/09/2014	218	13	900	34.44
04/12/2014-25/10/2016		27	873	28.60
12/12/2016	77		950	31.13
17/01/2017		1	949	27.23
20/03/2017	10	9	950	27.26
21/03/2017-03/05/2017		17	933	26.77
10/07/2017	20	3	950	27.26
30/10/2017	8	8	942	27.03
27/12/2017		1	941	27.00

Tables and figures

Table 1 Sample descriptive statistics

Panel A: Sample selection	
	Firm years
Observations of Chinese firms (non-financial) from 2008 to 2017	34,991
Observations with negative pretax income	(2,243)
Observations with missing tax expense	(11,049)
Observations with missing cash tax data or missing *ATR*	(384)
Observations with missing data for control variables	(6,168)
Observations of firms removed from the shortable stocks list during the sample period	(1,049)
Observations of firms listed as shortable stocks in 2017	(177)
Final sample size	13,921

Panel B: Year distribution of sample			
Year	No. of obs.	No. of shortable obs.	No. of non-shortable obs.
2008	748	0	748
2009	891	0	891
2010	948	37	911
2011	1,010	135	875
2012	1,107	141	966
2013	1,595	466	1,129
2014	1,664	613	1,051
2015	1,738	590	1,148
2016	1,957	668	1,289
2017	2,263	683	1,580
Total	13,921	3,333	10,588

Notes: Panel A presents the sample selection procedure. Beginning with a population of 34,991 non-financial listed A-share firm-year observations from 2008 to 2017 in China. After excluding 2,243 observations with negative pre-tax income, 11,433 observations with missing current-year tax expenses, prior- and current-year income taxes payable, cash tax paid, and the applicable statutory tax rate, 6,168 observations with insufficient data for the control variables, 1,049 observations that are removed from the short-sale list at any point during our sample period and 177 that were added to the list in 2017, our final sample consists of 13,921 firm-year observations. Short-selling and financial accounting information is obtained from WIND and CSMAR. Panel B provides the sample distribution across time. The number of observations ranges from 748 in 2008 to 2,263 in 2017 and 23.9% of firm-year observations are classified as shortable firms.

Table 2 Summary statistics and correlation matrix

Panel A: Descriptive statistics

	N	5th	25th	50th	75th	95th	Mean	S.D.
TA_ETR	13,921	-0.332	-0.041	0.014	0.085	0.217	-0.000	0.173
TA_CETR	13,921	-0.374	-0.041	0.020	0.092	0.226	-0.000	0.184
METR	13,921	0.000	0.547	0.895	1.267	3.266	1.098	1.040
MCETR	13,921	0.029	0.649	0.979	1.396	3.868	1.226	1.109
BTD	13,921	-0.048	-0.013	0.001	0.016	0.061	0.003	0.037
DDBTD	13,921	-0.052	-0.017	-0.002	0.013	0.057	-0.001	0.036
Size	13,921	20.327	21.285	22.026	22.886	24.556	22.163	1.292
Leverage	13,921	0.130	0.301	0.459	0.617	0.795	0.460	0.204
ROA	13,921	0.009	0.022	0.045	0.079	0.157	0.058	0.478
MB	13,921	0.658	1.359	2.180	3.496	7.686	2.969	2.877
CAPEX	13,921	0	0.012	0.032	0.066	0.142	0.045	0.048
PP&E	13,921	0.012	0.099	0.200	0.335	0.578	0.234	0.173
Intan	13,921	0	0.015	0.033	0.060	0.153	0.048	0.058
NOL	13,921	-0.179	0	0	0	0	-0.034	0.124
Mgmt Own	13,921	0	1	1	1	1	0.757	0.429
Dual	13,921	0	0	0	0	1	0.212	0.409
Mkt Cap	13,921	0	0	0	0.001	0.003	0.001	0.001
Traded Value	13,921	0	0	0.001	0.001	0.003	0.001	0.001
PE	13,921	0.006	0.015	0.031	0.070	0.347	0.082	0.162
Turnover%	13,921	0.524	1.207	2.078	3.608	8.271	2.908	2.726

Panel B: Comparison of pilot firms and non-pilot firms

	Pilot firm-years		Non-pilot firm-years		Difference	
	Mean	Median	Mean	Median	Mean	Median
TA_ETR	0.006	0.015	-0.004	0.014	0.010***	0.001
TA_CETR	0.008	0.020	-0.005	0.019	0.013***	0.001
METR	1.035	0.874	1.140	0.913	0.105***	-0.039***
MCETR	1.174	0.958	1.260	0.992	0.086***	-0.034***
BTD	0.003	0.002	0.002	0.001	-0.002**	0.001**
DDBTD	0.000	-0.002	-0.002	-0.003	-0.002***	0.001***
Size	22.924	22.787	21.655	21.652	1.269***	1.135**
Leverage	0.487	0.492	0.443	0.434	0.044***	0.058***
ROA	0.066	0.052	0.053	0.041	0.013***	0.011***
MB	2.818	2.230	3.070	2.145	-0.253***	0.085***
CAPEX	0.048	0.036	0.043	0.030	0.005***	0.006***
PP&E	0.232	0.193	0.236	0.205	-0.005	-0.012***
Intan	0.051	0.032	0.047	0.034	0.004***	-0.002***
NOL	-0.016	0.000	-0.046	0.000	0.030***	0
Mgmt Own	0.764	1.000	0.752	1.000	0.012	0
Dual	0.162	0.000	0.246	0.000	-0.083***	0***
Mkt Cap	0.001	0.001	0.000	0.000	0.001***	0.001**
Traded Value	0.001	0.001	0.001	0.000	0.001***	0.001**
PE	0.063	0.025	0.095	0.036	-0.032***	-0.011***
Turnover%	2.146	1.660	3.418	2.414	-1.273***	-0.754***

Notes: This table presents the descriptive statistics for our main regression variables. Panel A reports the statistics for the full sample. The mean values of *TA_ETR* and *TA_CETR* are nearly equal to 0, which indicates the normal level of tax avoidance. The mean values of *METR* and *MCETR* are all above 1, which indicates that, on average, firm's effective tax rates are greater than their applicable tax rates. Panel B reports the means and medians separately for the pilot and non-pilot firm subsamples. The means of *TA_ETR*, *TA_CETR*, *METR*, and *MCETR* for the pilot firm subsample are all significantly lower than those for the non-pilot firm subsample, and the means (medians) of *BTD* and *DDBTD* for

the pilot subsample are significantly higher than those for the non-pilot subsample. All continuous variables are winsorised at 1% and 99%. All variables are defined in Appendix 1.

Table 3 the relation between short-sale deregulation and corporate tax aggressiveness

	TA_ETR		*TA_CETR*	
	(1)	(2)	(3)	(4)
Shortable	-0.018***	-0.017***	-0.019***	-0.018***
	(-2.78)	(-2.80)	(-2.74)	(-2.71)
Size		-0.015**		-0.015**
		(-2.51)		(-2.41)
Leverage		0.031		-0.014
		(1.31)		(-0.57)
ROA		0.137***		0.193***
		(3.00)		(3.99)
MB		0.000		0.000
		(0.01)		(0.10)
CAPEX		0.007		0.027
		(0.15)		(0.55)
PPE		-0.056**		-0.053*
		(-2.13)		(-1.93)
Intan		-0.012		-0.013
		(-0.19)		(-0.20)
NOL		0.004		-0.027
		(0.12)		(-0.83)
Mgmt Own		0.001		-0.001
		(0.18)		(-0.07)
Dual		0.007		0.007
		(0.90)		(0.93)
Mkt Cap		12.019***		19.321***
		(3.16)		(4.05)
Traded Value		7.684**		6.052*
		(2.29)		(1.70)
PE		-0.340***		-0.374***
		(-13.67)		(-14.60)
Turnover%		-0.001		0.000
		(-0.84)		(0.19)
Intercept	-0.023***	0.289**	-0.041***	0.273**
	(-2.95)	(2.24)	(-4.69)	(2.03)
Year fixed effects	YES	YES	YES	YES
Firm fixed effects	YES	YES	YES	YES
Standard errors clustering	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Observations	13,921	13,921	13,921	13,921
R^2	0.009	0.085	0.015	0.101

Notes: This table presents the results for the tests examining the impact of short-sale deregulation on corporate tax aggressiveness, generated by the following regression: $TA\_ETR_{i,t}$ ($TA\_CETR_{i,t}$)=α + $\beta Shortable_{i,t}$ + $\gamma_m Control\ variables_{i,t}$ + $Firm_i$ + $Year_t$ + $\varepsilon_{i,t}$. *TA_ETR* (*TA_CETR*) is the peer-adjusted effective tax rate (cash effective tax rate). *Shortable* is an indicator variable equal to 1 in the years when a firm's stocks were allowed to be short sold, otherwise 0. Firm and year fixed effects are included in all specifications. Calculation of the *t*-statistics, reported in parentheses, was based on standard errors obtained by clustering at the firm level and year level. The superscript asterisks ∗∗∗,

∗∗, and ∗ denote statistical significance at the 1%, 5%, and 10% levels, respectively, using two-tailed tests. All variables are defined in Appendix 1.

Table 4 Dynamic effects of short-sale deregulation on corporate tax aggressiveness

	TA_ETR	*TA_CETR*
	(1)	(2)
Before2	-0.010	-0.013*
	(-1.28)	(-1.66)
Before1	0.000	0.003
	(0.01)	(0.40)
Current	-0.014*	-0.008
	(-1.72)	(-1.02)
After1	-0.025***	-0.023***
	(-3.11)	(-2.73)
After2	-0.016*	-0.018*
	(-1.84)	(-1.82)
After3	-0.023**	-0.024**
	(-2.46)	(-2.40)
Intercept	0.066	0.018
	(0.69)	(0.18)
Control variables	YES	YES
Year fixed effects	YES	YES
Firm fixed effects	YES	YES
Standard errors clustering	Firm, Year	Firm, Year
Observations	13,092	13,092
R^2	0.075	0.084

Notes: This table reports the results for the dynamic analyses of the association between the short-sale deregulation and corporate tax aggressiveness. We estimated the following regression: $TA\_ETR_{i,t}$ ($TA\_CETR_{i,t}$) = $\alpha + \beta_1 Before^1_{i,t} + \beta_2 Before^2_{i,t} + \beta_3 Current_{i,t} + \beta_4 After^1_{i,t} + \beta_5 After^2_{i,t} + \beta_6 After^3_{i,t} + \varepsilon_{i,t}$. *TA_ETR* (*TA_CETR*) is the peer-adjusted effective tax rate (cash effective tax rate). $Before^j$ equals 1 for pilot firms in the *j*th year before short-sale deregulation, *Current* equals 1 for pilot firms in the year of short-sale deregulation, and $After^j$ equals 1 for pilot firms in the *j*th year after short-sale deregulation. Firm and year fixed effects are included in all specifications. Calculation of the *t*-statistics, reported in parentheses, was based on standard errors obtained by clustering at the firm level and year level. The superscript asterisks ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1%, 5%, and 10% levels, respectively, using two-tailed tests. All variables are defined in Appendix 1.

Table 5 Controlling for unparallel trends driven by unobservables

Panel A: Controlling for pre-existing different time trends and the effect of unknown shocks				
	TA_ETR		*TA_CETR*	
	(1)	(2)	(3)	(4)
Shortable	-0.019***	-0.023***	-0.020***	-0.015*
	(-2.87)	(-2.98)	(-2.80)	(-1.83)
Pilot × *t*		0.001		-0.001
		(0.88)		(-0.42)
Intercept	1.683*	1.574**	1.136	1.112
	(1.89)	(1.98)	(1.14)	(1.31)
Control variables	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES
Firm fixed effects	YES	YES	YES	YES
Year × control variables	YES	NO	YES	NO
Standard errors clustering	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Observations	13,921	13,921	13,921	13,921
R^2	0.097	0.085	0.113	0.101
Panel B: Controlling for time-variant unobservable at industry and province levels				
	TA_ETR		*TA_CETR*	
	(1)	(2)	(3)	(4)
Shortable	-0.017***	-0.020***	-0.017**	-0.018***
	(-2.65)	(-3.12)	(-2.52)	(-2.66)
Intercept	0.260*	0.307**	0.214	0.284**
	(1.94)	(2.32)	(1.53)	(2.04)
Control variables	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES
Firm fixed effects	YES	YES	YES	YES
Year × industry	YES	NO	YES	NO
Year × province	NO	YES	NO	YES
Standard errors clustering	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Observations	13,921	13,921	13,921	13,921
R^2	0.102	0.112	0.116	0.128

Notes: This table reports the results for baseline regressions after controlling for unparallel trends across
pilot firms and non-pilot firms. Column (1) and Column (3) of Panel A present the result for the modified baseline models including interactive terms between year dummies and industry dummies. Column (2) and Column (4) present the results for the modified baseline models including interactive terms between year dummies and province dummies. Column (1) and Column (3) of Panel B present the results for the modified baseline models that included interactive terms between year dummies and control variables. Column (2) and Column (4) of Panel B present the results for the modified baseline models including a linear time trend for all pilot firms for the period preceding the short-sale deregulation. *TA_ETR* (*TA_CETR*) is the peer-adjusted effective tax rate (cash effective tax rate). *Shortable* is an indicator variable equal to 1 in the years when a firm's stocks were allowed to be shorted, otherwise 0. Firm and year fixed effects are included in all specifications. Calculation of the *t*-statistics, reported in parentheses, was based on standard errors obtained by clustering at the firm level and year level. The superscript asterisks ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1%, 5%, and 10% levels, respectively, using two-tailed tests. All variables are defined in Appendix 1

Table 6 The effect of short-sale deregulation on tax aggressiveness using the matched sample

Matching variables
Exact: *Year*
Nearest neighbor: *Size*, *Leverage*, *ROA*, *MB*, *PP&E*, *Intan*, *NOL*, *Mkt Cap*, *Traded Volume*, *PE*, and *Turnover*

Panel A: Post-match descriptive statistics

	Pilot firm-years (*N*=5,499)		Non-pilot firm-years (*N*=5,499)		Difference	
	Mean	Median	Mean	Median	Mean	Median
Size	22.909	22.780	22.783	22.685	0.126*	0.095**
Leverage	0.486	0.491	0.486	0.491	0	0
ROA	0.058	0.045	0.055	0.044	0.003	0.001
MB	2.829	2.247	2.897	2.242	-0.068	0.005
CAPEX	0.048	0.036	0.047	0.038	0.001	-0.002
PP&E	0.233	0.194	0.236	0.195	-0.003	-0.001
Intan	0.049	0.032	0.048	0.034	0.000	-0.002
NOL	-0.016	0.000	-0.020	0.000	0.004*	0
Mkt Cap	0.001	0.001	0.000	0.000	0.001**	0.001**
Traded Value	0.001	0.001	0.000	0.000	0.001**	0.001**
PE	0.064	0.025	0.074	0.028	-0.010	-0.003
Turnover%	2.148	1.675	2.168	1.726	-0.020	-0.051

Panel B：Propensity matched sample tests

	TA_ETR	*TA_CETR*
Shortable	-0.017***	-0.020***
	(-2.65)	(-2.90)
Control variables	YES	YES
Year fixed effects	YES	YES
Firm fixed effects	YES	YES
Observations	10,998	10,998

Panel C：Entropy balance weighted regressions

	TA_ETR	*TA_CETR*
Shortable	-0.012**	-0.012**
	(-2.02)	(-2.03)
Control variables	YES	YES
Year fixed effects	YES	YES
Firm fixed effects	YES	YES
Observations	13,921	13,921

Notes: This table reports the results after matching. Panel A reports the post-PSM descriptive statistics for all matching covariates. Panel B reports the results for the baseline models based on the matched sample. Panel C reports the results from the entropy balance weighted baseline regressions. *TA_ETR* (*TA_CETR*) is the peer-adjusted effective tax rate (cash effective tax rate). The superscript asterisks ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively, using two-tailed tests. All variables are defined in Appendix 1.

Table 7 The relation between the intensity of short interest and tax aggressiveness

	TA_ETR_{t+1}	TA_CETR_{t+1}	TA_ETR_{t+1}	TA_CETR_{t+1}
	(1)	(2)	(3)	(4)
Pilot × *Short Interest*	-1.683***	-2.059***	-1.592***	-1.904***
	(-3.96)	(-4.02)	(-3.66)	(-3.60)
Pilot × *Margin Purchase*			-0.051	-0.087
			(-0.96)	(-1.48)
Control variables	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES
Standard Errors Clustering	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Observations	4,609	4,609	4,609	4,609
R^2	0.038	0.051	0.038	0.051

Notes: This table reports the results for tests examining the relation between the intensity of short interest and tax aggressiveness. We estimated the following regressions:
$TA\_ETR_{i,t}(TA\_CETR_{i,t})=\alpha + \beta Short\ Interest_{i,t} + \gamma_m Control\ variables_{i,t} + Firm_i + Year_t + \varepsilon_{i,t}$. and
$TA\_ETR_{i,t}(TA\_CETR_{i,t})=\alpha + \beta_1 Short\ Interest_{i,t} + \beta_2 Margin\ Purchase + \gamma_m Control\ variables_{i,t} + Firm_i + Year_t + \varepsilon_{i,t}$.
TA_ETR (*TA_CETR*) is the peer-adjusted effective tax rate (cash effective tax rate). *Short Interest* is the ratio of the annual shorted shares to the outstanding shares. *Margin Purchase* is the ratio of the annual margin purchased volume to the annual turnover. Firm and year fixed effects are included in all specifications. Calculation of the *t*-statistics, reported in parentheses, was based on standard errors obtained by clustering at the firm level and year level. The superscript asterisks ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1%, 5%, and 10% levels, respectively, using two-tailed tests. All variables are defined in Appendix 1.

Table 8 Using alternate measures of tax aggressiveness

	BTD	*DDBTD*	*METR*	*MCETR*
	(1)	(2)	(3)	(4)
Shortable	-0.007***	-0.007***	0.099***	0.106***
	(-4.59)	(-4.60)	(2.69)	(2.73)
Intercept	-0.013	-0.010	1.958**	1.599*
	(-0.43)	(-0.34)	(2.48)	(1.90)
Control variables	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES
Firm fixed effects	YES	YES	YES	YES
Observations	13,921	13,921	13,921	13,921
R^2	0.075	0.075	0.103	0.122

Notes: This table reports the results for modified baseline regressions using alternate measures of tax aggressiveness. *BTD* equals book income less taxable income scaled by total assets. *DDBTD* was calculated by deducting the effect of *Accrued Profit* from *BTD*. *METR* (*MCETR*) is the ratio of effective tax rate (cash effective tax rate) to applicable tax rate. *Shortable* is an indicator variable equal to 1 in the years when a firm's stocks are allowed for being shorted, otherwise 0. Firm and year fixed effects are included in all specifications. Calculation of the *t*-statistics, reported in parentheses, was based on standard errors obtained by clustering at the firm level and year level. The superscript asterisks ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1%, 5%, and 10% levels, respectively, using two-tailed tests. All variables are defined in Appendix 1.

Table 9 The interactive effects of short-sale deregulation with advertisement expense, regional tax enforcement, institutional ownership, and duality

	TA_ETR	*TA_CETR*	*TA_ETR*	*TA_CETR*	*TA_ETR*	*TA_CETR*	*TA_ETR*	*TA_CETR*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Shortable	-0.010	-0.006	-0.012	-0.011	-0.008	-0.008	-0.013**	-0.012*
	(-1.27)	(-0.75)	(-1.60)	(-1.43)	(-0.87)	(-0.79)	(-1.97)	(-1.68)
High Adv	-0.006	-0.003						
	(-1.05)	(-0.46)						
Shortable × *High Adv*	-0.014**	-0.023**						
	(-2.49)	(-2.30)						
Low TE			0.013**	0.009				
			(2.09)	(1.47)				
Shortable × *Low TE*			-0.012*	-0.013**				
			(-1.90)	(-2.44)				
High IO					0.009	0.012**		
					(1.61)	(2.07)		
Shortable × *High IO*					-0.012*	-0.013**		
					(-1.80)	(-2.34)		
Dual							0.012	0.015*
							(1.46)	(1.73)
Shortable × *Dual*							-0.026**	-0.037***
							(-2.33)	(-2.91)
Intercept	0.278***	0.263**	0.286**	0.272**	0.295**	0.280**	0.285**	0.267**
	(2.66)	(2.41)	(2.22)	(2.02)	(2.29)	(2.08)	(2.21)	(1.98)
Control variables	YES	YES	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Firm fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Standard errors clustering	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Observations	13,921	13,921	13,921	13,921	13,921	13,921	13,921	13,921
R^2	0.085	0.101	0.085	0.101	0.085	0.101	0.085	0.102

Notes: This table reports the results for tests examining the interactive effects of short-sale deregulation with other attributes of firms. *High Adv* is an indicator variable equal to 1 if a firm's *Adv* (ratio of advertisement expense to its operating income) is above the median *Adv* over other firms in the same year and same industry. *Low TE* is an indicator variable equal to 1 if a firm is located in the province with *TE* below the median *TE* over other provinces in the same year, where *TE* is calculated as the actual provincial tax ratio divided by the fitted tax ratio estimated from the following model: $\frac{Tax\ Revenue_{i,t}}{GDP_{i,t}} = \alpha + \beta_1 \frac{Primary\ Industry_{i,t}}{GDP_{i,t}} + \beta_2 \frac{Second\ Industry_{i,t}}{GDP_{i,t}} + \beta_3 \frac{Imports\ and\ Exports_{i,t}}{GDP_{i,t}} + \varepsilon_{i,t}$. *Tax Revenue* is the tax revenue of a firm's province, *Primary Industry*, *Second Industry*, and *Import and Export* are the local GDP from primary industry, second industry, and imports and exports. We ran this regression by year and calculated the fitted tax ratio, $\left(\frac{\widehat{Tax\ Revenue_{i,t}}}{GDP_{i,t}}\right)$. *High IO* is an indicator variable equal to 1 if a firm's *IO* (the ratio of institutional ownership) is above the median *IO* over other firms in the same year and same industry. *Shortable* is an indicator variable equal to 1 in the years when a firm's stocks were allowed to be shorted, otherwise 0. *Dual* is an indicator variable equal to 1 if the CEO is also the chairman of the board of directors, otherwise 0. Firm and year fixed effects are included in all specifications. Calculation of the *t*-statistics, reported in parentheses, was based on standard errors obtained by clustering at the firm level and year level. The superscript asterisks ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1%, 5%, and 10% levels, respectively, using two-tailed tests. All variables are defined in Appendix 1.

Table 10 Mediating roles of auditors, media, and analysts

	Incremental auditing pressure			Incremental media pressure			Incremental analyst attention		
	Abnormal Audit Fee	*TA_ETR*	*TA_CETR*	*Negative News*	*TA_ETR*	*TA_CETR*	*Analyst Following*	*TA_ETR*	*TA_CETR*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Abnormal Audit Fee		-0.011***	-0.017***						
		(-3.32)	(-4.97)						
Negative News					-0.086***	-0.103***			
					(-5.42)	(-6.16)			
Analyst Following								-0.017***	-0.020***
								(-6.86)	(-7.47)
Shortable	0.029**	-0.015**	-0.017**	0.010**	-0.016***	-0.017***	0.108***	-0.016***	-0.017**
	(2.18)	(-2.29)	(-2.43)	(2.25)	(-2.69)	(-2.58)	(3.49)	(-2.66)	(-2.56)
Difference in *Shortable*: Eq.(1) – Eq.(5)		-0.002***	-0.001		-0.001**	-0.001*		-0.001*	-0.001*
p-value (SUR test)		0.003	0.104		0.026	0.064		0.050	0.056
Intercept	-0.401***	-0.013	-0.032***	0.991***	0.027**	0.021	-10.298***	0.368***	0.365***
	(-14.85)	(-1.52)	(-3.36)	(11.99)	(2.17)	(1.53)	(-18.60)	(2.76)	(2.61)
Control variables	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Standard errors clustering	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Observations	13,659	13,659	13,659	13,912	13,912	13,912	13,921	13,921	13,921
R^2	0.026	0.010	0.017	0.125	0.012	0.019	0.224	0.085	0.102

Notes: This table reports the results for the analyses of whether there is an indirect effect of short-sale deregulation on corporate tax aggressiveness. We tested three indirect mechanisms: auditing pressure, media pressure, and analyst attention, by using *Abnormal Audit Fee*, *Negative News*, and *Analyst Following* as proxies, respectively. *Abnormal Audit Fee* was calculated as the residuals from the following model: $Audit\ Fee_{it} = \alpha + \beta_1 Size_{i,t} + \beta_2 ROA_{i,t} + \beta_3 Leverage_{i,t} + \beta_4 Big4_{i,t} + \beta_5 Audit\ Type_{i,t} + \beta_6 Loss_{i,t} + \beta_7 MB_{i,t} + \beta_8 Tenure_{i,t} + \beta_9 REC_{i,t} + \beta_{10} INV_{i,t} + \beta_{11} Merger_{i,t} + \beta_{12} Restated_{i,t} + \varepsilon_{i,t}$. *Negative News* was calculated as the ratio of the number of negative news to the total number of news. *Analysts following* was calculated as the natural logarithm of the number of analyst teams following the firm. Column (1), Column (4), and Column (7) report the results for the tests examining whether short-sale deregulation has an impact on the three indirect mechanisms. Column (1), Column (4), and Column (7) report the results for the analyses of whether there was a residual effect of short-sale deregulation on corporate tax aggressiveness after incorporating the three indirect mechanisms. *Shortable* is an indicator variable equal to 1 in the years when a firm's stocks are allowed for being shorted, otherwise 0. Firm and year fixed effects are included in all specifications. Calculation of the *t*-statistics, reported in parentheses, was based on standard errors obtained by clustering at the firm level and year level. The superscript asterisks ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1%, 5%, and 10% levels, respectively, using two-tailed tests. All variables are defined in Appendix 1.

Table 11 The effect of loss of short-selling eligibility on tax aggressiveness

	pilot firms + delisted firms		'delisted' firms	
	TA_ETR	*TA_CETR*	*TA_ETR*	*TA_CETR*
	(1)	(2)	(3)	(4)
Removed	0.060*	0.089***	0.072*	0.073*
	(1.88)	(2.60)	(1.74)	(1.72)
	(0.04)	(1.07)	(-0.08)	(0.99)
Control variables	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES
Standard Errors Clustering	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Observations	3,558	3,558	225	225
R^2	0.131	0.159	0.227	0.235

Note: This table reports the results for tests examining the effect of loss of short-selling eligibility on corporate tax aggressiveness. We create two new samples. The first sample includes all shortable firm-year observations and firm-year observations that were added to the short-selling list but removed from it later. The second sample only contains firms that were taken off the short-selling list during the period after the short-selling deregulation was implemented. We estimate the following regression based on two new samples: $TA\_ETR_{i,t}$ ($TA\_CETR_{i,t}$)= $\alpha + \beta Removed_{i,t} + \gamma_m Control\ variables_{i,t} + Firm_i + Year_t + \varepsilon_{i,t}$. *TA_ETR* (*TA_CETR*) is the peer-adjusted effective tax rate (cash effective tax rate). *Removed* is an indicator variable equal to 1 in the years when a firm was removed from the short-selling eligibility list, and otherwise 0. Firm and year fixed effects are included in all specifications. The t-statistics, reported in parentheses, are calculated based on standard errors obtained by clustering at the firm level and year level. The superscripts asterisks ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1%, 5%, and 10% levels, respectively, using two-tailed tests. All variables are defined in Appendix 1.

Table 12 Replication of Table 3, LNT

Panel A – Replicated sample

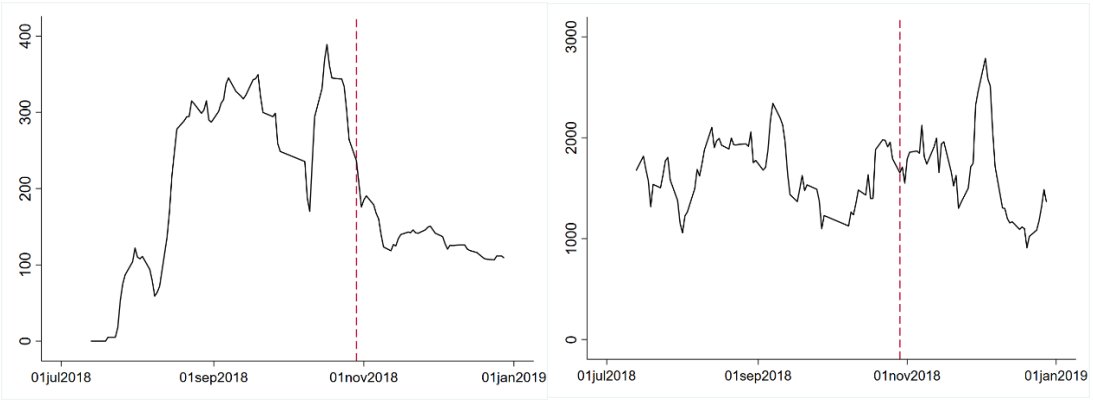
	Firm-year obs.
Observations of Chinese firms from 2007 to 2015	19,850
Observations of firms from the financial industry	(561)
Observations of ST, *ST, suspension, and delisted firms	(4,269)
Observations with missing values for dependent variables and control variables	(2,306)
Observations of firms that are eventually dropped from the short-sale list during the sample period	(880)
Final sample size	11,834

Panel B - Replication

	LNT				Our replication			
	Full sample		Matched sample		Full sample		Matched sample	
	CASH_ETR	ETR	CASH_ETR	ETR	CASH_ETR	ETR	CASH_ETR	ETR
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Short	-0.021***	-0.092***	-0.026***	-0.013***	0.002	0.009*	0.004	0.004
	(-3.020)	(-3.089)	(-3.741)	(-3.36)	(0.39)	(1.71)	(0.61)	(0.73)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
Firm Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
N	7,657	7,657	3,858	3,858	11,834	11,834	8,916	8,916

Notes: Reanalysis of baseline results from Table 3 in LNT. Panel A reports the sample selection based on the procedure described in LNT. Panel B presents the replication results of LNT. Dependent variables, *Cash_ETR* and *ETR*, are constructed based on the definitions of LNT. Independent variable *short* is the same as variable *shortable* in our paper. The t-statistics, reported in parentheses, are calculated based on standard errors obtained by clustering at the firm level. The superscripts asterisks ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1%, 5%, and 10% levels, respectively.

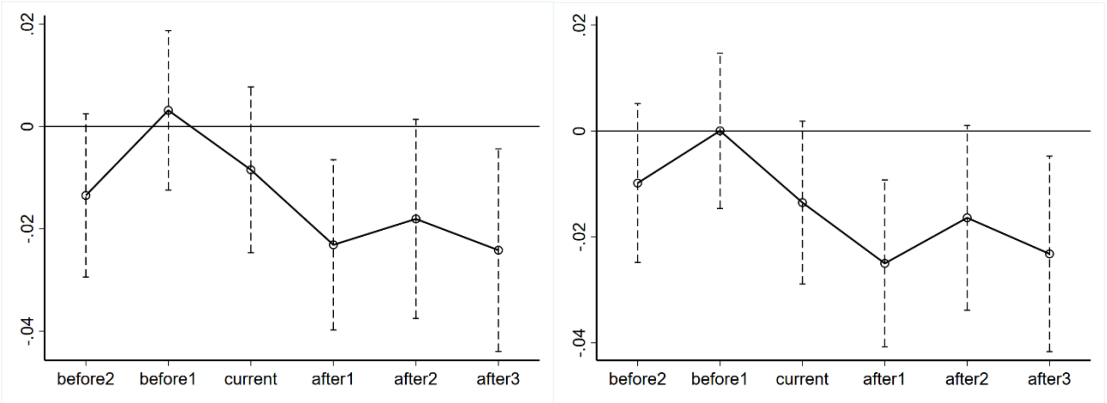
Figures:



Panel A: Short interest of 2345 Network around October 29, 2018

Panel B: Short interest of other pilot firms in the same industry (Software and IT service , two-digit code: I65) as 2345 Network around October 29, 2018

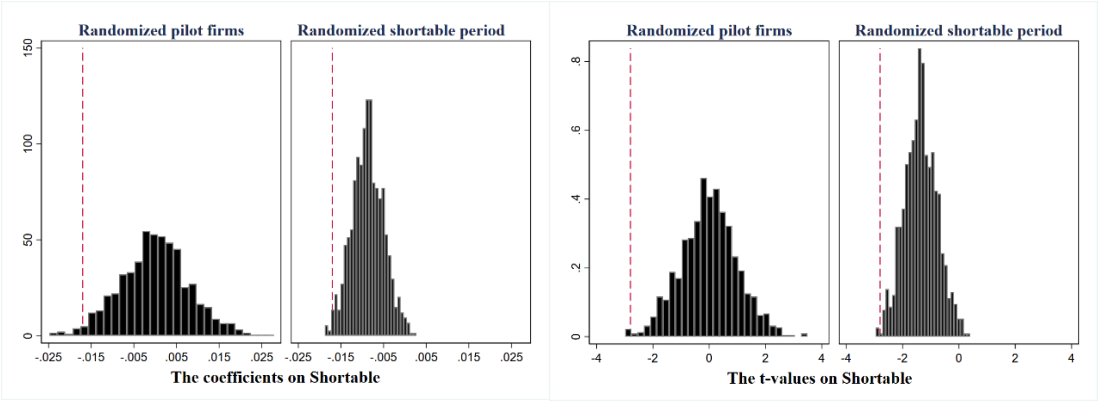
Figures 1. We separately plot the trend of five days rolling-cumulative sum of short interest of 2345 Network (Stock code: 002195.SZ) and other pilot firms in the same industry (Software and IT service , two-digit code: I65) as 2345 Network but not accused of tax evasion in second half of 2018. The red vertical dash line is the October 29, 2018.



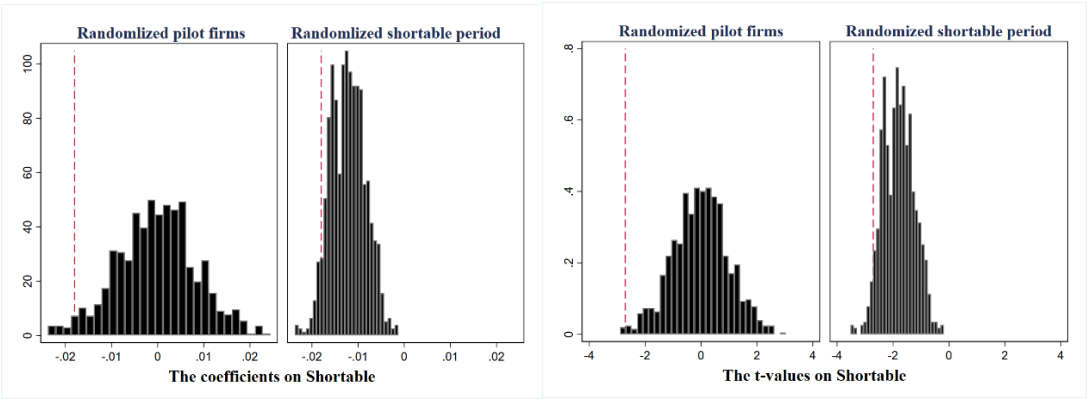
Panel A: The dynamic impact of short-sale deregulation on *TA_ETR*

Panel B: The dynamic impact of short-sale deregulation on *TA_CETR*

Figure 2. these two graphs exhibit the dynamic association between *Shortable* and *TA_ETR* (*TA_CETR*). In both of the graphs, the vertical axis represents coefficient values associated with β_1 to β_6 from the following regression: $TA\_ETR_{i,t}$ ($TA\_CETR_{i,t}$) = $\alpha + \beta_1 Before^1_{i,t} + \beta_2 Before^2_{i,t} + \beta_3 Current_{i,t} + \beta_4 After^1_{i,t} + \beta_5 After^2_{i,t} + \beta_6 After^3_{i,t} + \gamma$ Controls+ $\varepsilon_{i,t}$. The horizontal axis displays a timeline, from 2 years prior to firm *i*'s stock was allowed for short selling, moving forward to 3 years after firm *i*'s stock was allowed for short selling. Each hollow point on the graphs represents a coefficient value. The dash lines that go through each hollow point represent the 95% confidence intervals.



Panel A: The placebo treatment effects on *TA_ETR*



Panel B: The placebo treatment effects on *TA_CETR*

Figure 3. These four figures illustrate the difference between the randomized placebo treatment effects and the real effects of short-sale deregulation on corporate tax aggressiveness. The black bars show the distribution of the coefficients and t-values on Shortable from 1,000 placebo tests. The red vertical dash lines represent the magnitude and significance of real effects.