Bank Monitoring and Corporate Tax Planning: Evidence from Loan Covenants

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Abstract

This study examines the effect of bank monitoring on corporate tax planning behavior. To identify the causal effect, we use a regression discontinuity design, taking advantage of the discrete nature of bank control rights surrounding covenant violation thresholds. We find that strengthened bank monitoring triggered by loan covenant violations leads firms to generate more cash tax savings. The positive effect of bank monitoring on cash tax savings is more pronounced among firms without a relationship bank, with severe information asymmetry, facing higher economic policy uncertainty, and having larger institutional ownership before covenant violations. Moreover, we show that there is a decline in tax risk following loan covenant violations. Our findings highlight the important monitoring role of banks in shaping corporate tax planning behavior.

Key words: Bank monitoring, tax planning, loan covenants, cash tax savings, tax risk, regression discontinuity design

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1. Introduction

An emerging stream of the tax literature examines the effect of corporate governance on tax avoidance. Taking a traditional view of governance, this literature mainly focuses on the ability of equity holders to influence managerial tax-planning decisions (e.g., Desai and Dharmapala 2006; Robinson, Xue and Zhang 2012; Armstrong, Blouin, Jagolinzer, and Larcker 2015). The effect of creditors on tax planning, however, is largely unexplored.[1] The lack of research on the role of creditors is somewhat surprising, given the prominent position of banks among firms' stakeholders. In this study, we extend the tax planning literature by examining the effect of bank monitoring on borrowers' tax avoidance via the lens of debt covenant violations.

Incomplete contract theory suggests that optimal contracts allocate control rights in a state-contingent fashion to the party who has stronger incentives to efficiently allocate corporate resources (Aghion and Bolton, 1992; Hart 2001; Roberts and Sufi 2009a). In a debt contract, corporate control rights are shifted to banks upon covenant violations since banks are better incentivized to monitor managers and make value-enhancing corporate decisions (Chava and Roberts 2008; Roberts and Sufi 2009b; Nini, Smith, and Sufi 2012). Therefore, covenant violations provide an ideal setting to examine the effect of bank monitoring on borrowers' tax avoidance.

The tax literature suggests that the agency costs accompanying tax avoidance are the major costs that discourage firms from taking profitable tax planning opportunities. Specifically, complex tax planning transactions often increase information opacity and facilitate managerial rent extraction (e.g., Balakrishnan Billings, Kelly, and Ljungqvist

[1] One recent exception is the study of Gallemore et al. (2016), which examines the role of banks as tax planning intermediaries. Our study focuses on the role of bank monitoring as a governance mechanism that reduces non-tax costs of tax planning.

2014; Desai and Dharmapala, 2006; Hasan, Hoi, Wu, and Zhang 2014; Chung, Goh, Lee, and Shevlin 2015). Due to the contract incompleteness, these agency problems associated with tax avoidance cannot be fully pre-specified in a contract. Unresolved agency problems and the resultant price discounts on loan terms will discourage managers from engaging in tax avoidance (Desai and Dharmapala 2006; Chen, Chen, Cheng, Shevlin 2010; Hasan, Hoi, Wu, and Zhang 2014). Covenant violations transfer the control rights to banks and provide banks an opportunity to intensively monitor the managers. If the intensified bank monitoring following covenant violations helps mitigate managerial agency problems accompanying tax avoidance activities, we would expect a decrease in non-tax costs of tax planning and therefore an increase in the level of tax planning in the post-violation period.[2]

There are also reasons to expect the opposite. Chava and Roberts (2008) articulate that after obtaining more control rights, banks usually discourage firms from taking risky investment opportunities due to the concern over the risk-shifting problem. Since some tax avoidance activities involve substantial cash flow risks, banks may view them as risky investments and force firms to reduce these activities, leading us to observe a lower level of tax avoidance after covenant violations (Rego and Wilson 2012). However, given that a large number of firms have not been able to exhaust all safe and profitable tax planning opportunities (Dyreng, Hanlon, and Maydew, 2008), banks can selectively encourage safe tax strategies and discourage risky ones. In this regard, there would be a non-trivial possibility that intensified bank monitoring increases tax savings and reduces tax-planning risks at the same time.

To empirically examine the effect of bank monitoring on tax avoidance, we employ a quasi-regression discontinuity (RD) design which exploits the discontinuous nature of bank

[2] Note that the efficiency of bank monitoring can be lower before covenant violations since banks have to rely on contract clauses which are likely to be incomplete. Covenant violations transfer control rights to banks and can potentially improve the efficiency of bank monitoring.

control rights surrounding covenant violations. Specifically, once a firm's accounting variable goes from above to below the predetermined covenant threshold (or the other way around, depending on the type of the covenant), certain control rights are transferred to the contracting bank. This arbitrarily creates a discontinuous change in bank control at the covenant threshold. Other variables that can affect tax planning, however, are not expected to change discontinuously with the accounting variable exactly at the threshold. For example, the degree of financial distress or financial constraint should not change discontinuously just by passing through a predetermined value of current ratio. Therefore, any observed discontinuity in tax avoidance at the covenant threshold should be attributed to bank monitoring. In this study, we define tax avoidance or tax planning as all actions taken by firms to reduce their total cash income taxes. Following prior research (e.g., Dyreng, Hanlon, and Maydew 2008; Donohoe 2015; Edwards, Schwab, and Shevlin 2016), we use cash effective tax rates (cash ETRs) to measure the extent of tax avoidance.

Using a sample of 25,124 firm-year observations from 1996 to 2007, we show that strengthened bank monitoring following covenant violations significantly increases firms' cash tax savings. On average, cash ETRs decrease by 2.3 to 2.5 percentage points following a covenant violation. The decline in cash ETRs equates to approximately US$8 to 9 million in annual cash tax savings for the average firm in our sample. This result supports the arguments that strengthened bank monitoring mitigates the agency cost of tax planning and therefore increases firms' capacity in tax avoidance. It also suggests that before covenant violations, the price discount discourages firms from engaging in sufficient tax avoidance activities, which is consistent with the prior literature on "under-sheltering puzzle" (Weisbach 2002; Desai and Dharmapala 2006; Dyreng, Hanlon, and Maydew 2008).

To substantiate our inferences, we conduct several cross-sectional tests and provide corroborating evidence on the effect of bank monitoring on corporate tax avoidance. First, a

relationship bank possesses more information about the borrower and is more active in monitoring the borrowers (e.g., Srinivasan 2014). Therefore, the control right transfer and intensified bank monitoring are expected to play a less significant role in influencing tax avoidance for firms that are already monitored by a relationship bank. Second, information asymmetry aggravates managerial opportunistic behaviors and therefore exaggerates the agency costs associated with tax avoidance (e.g., Dewatripont and Tirole 1994; Garleanu and Zwiebel 2008). The enhanced bank monitoring following covenant violations should play a more important role in reducing agency costs related to tax avoidance and allow firms to engage in more tax avoidance. Third, while the agency cost of tax avoidance would be largely resolved if the bank can pre-specify every single state arising from tax strategies, it would get worse as the incompleteness become severer. The contingent control right allocation would lead to a greater efficiency improvement when the level of incompleteness is higher (e.g., Aghion and Bolton 1992). We use economic policy uncertainty to measure contract incompleteness, and expect the effect of enhanced bank monitoring following covenant violations to be concentrated among firms that face an uncertain operating environment. Lastly, since institutional investors can closely monitor the managers and reduce the agency costs associated with tax avoidance before covenant violations, we expect a reduction in the marginal effect of bank monitoring on corporate tax planning activities. Our empirical findings support all the above four predictions. We find that the effect of bank monitoring on tax avoidance is stronger for firms without a relationship bank, with severe information asymmetry, facing higher operating uncertainty before covenant violations, and having larger institutional ownership, respectively. These cross-sectional results provide further support for the role of bank monitoring in influencing corporate tax avoidance activities.

Lastly, we examine the effect of enhanced bank monitoring following covenant violations on tax risk. We first estimate a series of quantile regressions to assess the relation between covenant violations and cash ETRs across the entire distribution of cash ETRs. We find that the increase in the level of tax avoidance is not driven by observations with an aggressive level of tax avoidance. Second, following prior studies, we use the cash ETR volatility as a proxy for tax risk. We find that ETR volatility decreases after covenant violations, especially for highly leveraged borrowers. We also find weak evidence that enhanced bank monitoring leads to a lower level of unrecognized tax benefits. In sum, these results suggest that enhanced bank monitoring following covenant violations can increase tax avoidance activities but at the same time reduce the overall risk of tax planning.

Our study is closely related to an important stream of research that examines the effect of corporate governance on corporate tax planning. Prior research mainly focuses on equity-centered governance mechanisms, such as board independence and expertise, shareholder rights, and managerial risk-taking incentives (e.g., Desai and Dharmapala 2006; Rego and Wilson 2012; Robinson et al. 2012; Armstrong et al. 2015). Our research advances this literature by examining the effect of bank monitoring on tax planning. Given that covenant violations frequently occur outside of bankruptcy, our findings suggest that banks' monitoring role in shaping corporate tax avoidance activities applies to a broad set of firms. In addition, our RD results allow us to establish a causal relationship between bank monitoring and tax avoidance, which appears to be a tall task in traditional corporate governance research (Armstrong, Guay and Weber 2010).

Our study is also related to a line of literature that examines the consequences of tax avoidance in the debt market. Hasan et al. (2014) show that creditors perceive tax aggressiveness as highly risky activities, and thus charge higher interest rates for firms adopting aggressive tax policies. Their investigation mainly focuses on the price protection

strategy employed by the banks as a response to the incentives of firms in making risky tax avoidance decisions. In contrast, we focus on the effect of bank control rights on tax planning activities. The boundary between external pricing system and the control rights has been widely discussed in the economic literature (see Williamson 2002; Christensen, Nikolaev, and Wittenberg-Moerman 2016). Our findings suggest that a transfer of control rights to banks may allow firms to engage in more tax avoidance activities, which may not be achieved by relying on external pricing mechanism.

Finally, our research adds to the growing literature on the effect of creditor control rights following covenant violations on various firm decisions and policies. Prior research has looked at investment decisions, financing and payout policies, CEO turnover, and accounting and disclosure policies (e.g., Chava and Roberts 2008; Roberts and Sufi 2009b; Nini et al. 2012; Tan 2013; Vashishtha 2014). Our research extends this line of literature by examining the effect of covenant violations on tax planning decisions.

The remainder of the paper is structured as follows: Section 2 reviews the related literature and develops the hypotheses; Section 3 describes the data, the measurement of key variables, and the RD design; Section 4 presents the main empirical results; and Section 5 concludes.

2. Related literature and empirical predictions

2.1. Covenant violations and bank monitoring

The existence of covenants is rationalized by their ability to mitigate incentive conflicts between borrowers and creditors (Jensen and Meckling 1976; Smith and Warner 1979). Firms with higher agency costs of debt tend to include more stringent covenant restrictions in debt contracts (Malitz 1986; Bradley and Roberts 2015; Garleanu and Zwiebel 2008). Theoretical work on financial contracting shows that in the presence of incentive conflicts

and contract incompleteness, optimal debt contracts transfer control rights to creditors following borrower's poor performance (Aghion and Bolton 1992; Dewatripont and Tirole 1994). Specifically, due to contract incompleteness, managers have incentives to behave opportunistically after the contract is written. Granting control rights to banks upon covenant violations can effectively increase the monitoring efficiency and mitigate the managerial opportunistic behavior (e.g., Grossman and Hart 1986; Aghion and Bolton 1992).

Covenant violations give creditors the rights to accelerate any outstanding principal and to terminate any unused revolving credit facility.[3] Creditors use the threat of exercising these rights to demand changes to key corporate policies at borrowing firms as a way to safeguard their claims (Denis and Wang, 2014). In particular, creditors can impose stronger contractual restrictions on firm decision-making via amendments to existing credit agreements. The amended agreements can cover nearly all aspects of financial and investment decisions, including investments in working capital and tangible assets, acquisitions, assets sales, dividend payments, and new capital-raising efforts (Smith and Warner 1979; Chen and Wei 1993; and Nini, Smith, and Sufi 2012). More importantly, creditors can also apply non-contractual control over the governance of the firm, such as increasing informal communications, imposing more reporting requirements, and even replacing top executives (Chava and Roberts 2008).[4] Such interventions work as an important mechanism to protect banks' interests.

A growing body of empirical research provides supporting evidence that the increased control rights creditors receive following covenant violations enable them to exert considerable influence over managerial decision making. For example, Chava and Roberts

[3] However, creditors rarely end up exercising these rights and forcing repayment of debt and liquidation of the firm (Dichev and Skinner 2002). Nini, Smith, and Sufi (2012) find after controlling for firm performance and market valuation, the marginal effect of a violation on firm exit is less than one percentage point.

[4] There is substantial anecdotal evidence that creditors work behind the scenes to offer advice to management and the board, suggesting actions the company can take to maximize the chance of receiving a covenant waiver (Nini, Smith, and Sufi 2012; Falato and Liang 2016).

(2008) document that following covenant violations corporate investment declines sharply, especially for firms with higher leverage. Roberts and Sufi (2009b) show that debt-issuing activity experiences a dramatic and persistent decline following covenant violations. Nini, Smith, and Sufi (2012) find significant reductions in corporate investments, financial leverage, and shareholder payout and a significant increase in CEO turnover, suggesting that creditors apply both contractual and non-contractual control over the governance of firms upon violations. They also find that the violating firm's financial performance improves following covenant violations, suggesting that actions taken by creditors on average increase firm value. Tan (2013) investigates the change of accounting conservatism following covenant violations and finds that banks require firms to report more conservatively using their control rights. In contrast, Vashishtha (2014) finds that delegated monitoring by banks reduces shareholders' demand for voluntary disclosures.

Overall, the prior literature suggests that following covenant violations creditors play a more important role in corporate governance due to their increased bargaining power vis-a-vis borrowers. Covenant violations thus provide a specific mechanism through which bank monitoring can be significantly strengthened.

2.2. Control rights transfer, bank monitoring, and corporate tax planning

In making tax planning decisions, firms trade off the marginal benefits of tax avoidance against the marginal costs. The focal point of bank monitoring on tax planning lies in the change in an important form of non-tax costs of tax planning: managerial rent extraction. Tax planning arrangements often involve complex transactions, such as transfer pricing, allocation of debt and earnings stripping, creation of hybrid entities or instruments, establishment of offshore intellectual property havens, and centralization of operating

activities in tax-friendly jurisdictions, to minimize the overall corporate tax burden (Chung Goh, Lee, and Shevlin 2015). These complex transactions inevitably lead to increased financial opacity, which facilitates managerial rent diversion activities and increases the agency costs of tax planning (Desai and Dharmapala 2006; Frank, Lynch, and Rego 2009; Kim, Li, and Zhang 2011; Balakrishnan et al. 2014). However, given the complex nature of tax planning, banks and borrowers can not contract on all contingencies related to tax planning. Anticipating these agency costs, banks without control rights have to resort to a price protection mechanism, under which the borrowers have to pay a huge cost in the form of loan interest if they are perceived to engage in intensive tax planning activities (Desai and Dharmapala 2006; Hasan et al. 2014). In equilibrium, borrowers may engage in a suboptimal level of tax avoidance, resulting in a phenomenon of "under-sheltering" (Weisbach 2002; Desai and Dharmapala 2006; Dyreng, Hanlon, and Maydew 2008).

Following covenant violations, banks acquire additional control rights and therefore can strengthen their monitoring effect (Chava and Roberts 2008; and Nini, Smith, and Sufi 2012). In particular, with enhanced control rights, banks can opt out of the contract constraints and use private order to intervene the borrowers' operations (e.g., Williamson 2002; Chava and Roberts 2008; and Nini, Smith, and Sufi 2012). Such interventions by banks are particularly meaningful given the expertise possessed by banks in tax planning (Gallemore, Gipper, and Maydew 2016; OECD 2008). Specifically, banks can identify profitable tax planning opportunities and help borrowers to explore these opportunities. From this perspective, a control right transfer to banks strengthens the bank monitoring, mitigates the agency costs, and therefore prompts the tax planning activities. [5]

[5] A relevant question here is that why banks are in a better position, than are shareholders, to monitor managerial rent-seeking behaviors associated with tax avoidance. First, this question pertains to the optimality of contract design, in which the reallocation of control rights to banks is contingent on poor borrower performance, an exact timing when shareholder monitoring is likely to be ineffective and debtholder monitoring

H1: Cash effective tax rates decline following covenant violations.

Some tax avoidance activities are associated with increased uncertainties surrounding current and future tax outcomes (Rego and Wilson 2012; Hanlon, Maydew, and Saavedra 2014), such as uncertainty in the penalty imposed by the IRS. The penalty amount can be substantial; for example, Wilson (2009) finds that the interest charges paid by firms to tax authorities amounted to 40% of the tax savings originally generated by the tax-shelter transactions. Due to the concave payoff function faced by banks, they would have strong incentive to minimize downside risk and thus discourage tax avoidance activities which can increase cash flow uncertainty. Therefore, it is very likely that banks would selectively discourage borrowers from engaging in risky tax avoidance activities but encourage less risky ones, which leads to more cash tax savings and lower tax risk after covenant violations.

3. Empirical design, sample selection, and variables

In this study, we seek to establish the causal effect of bank monitoring on tax avoidance. Debt covenant violations provide an ideal setting for this purpose. Once a borrower violates a financial covenant, the contracting bank obtains superior bargaining power vis-a-vis this borrower to influence corporate decisions and policies. Besides their theoretical appeal,[6] two features of covenant violations also facilitate our empirical exercise. First, covenant violations occur frequently but well outside of financial distress, which

becomes necessary. Second, we can empirically examine the inference that our results only hold when shareholder are less effective, e.g., when shareholders' equity holdings are dispersed and the free-rider problem in monitoring a self-interested manager is a concern. Bank debt, on the other hand, is closely held, which guarantees a strong monitoring incentive once the bank obtains the control rights. The empirical test in this regard is performed in the subsample-test section below.

[6] Control rights are transferred to banks who have stronger incentives to monitor firms and increase firm value following covenant violations.

allows us to study a large sample of firms in a non-bankruptcy-related setting.[7] Second, the discrete nature of the change in bank's bargaining power and monitoring around covenant violation thresholds allows us to use a regression discontinuity (RD) design to establish casual relationships.

3.1. Quasi-RD design using reported violation information

Our main empirical tests use the sample of covenant violations reported by firms in their financial statements (data collected by Nini et al. 2012). Nini et al. (2012) identify the occurrence of covenant violations directly from 10-K and 10-Q Securities and Exchange Commission (SEC) filings based on a text-search algorithm.[8] This "reported violation" sample covers Compustat firms during the period 1996–2007. We obtain accounting information of these firms from Compustat and stock information from CRSP. Financial firms are excluded (SIC codes 6000–6999). The reported violation sample is also widely used in prior studies (e.g., Roberts and Sufi 2009b; Nini, Smith, and Sufi 2012). The merit of using this sample is that we can unambiguously identify each covenant violation.

Since the "reported violation" sample does not report specific covenant types binding each firm, we cannot unambiguously identify observations lying closely enough on either side of the covenant thresholds. We therefore follow prior studies and apply a "quasi-RD" design. Specifically, in order to control for continuous changes of firm performance and to isolate the discontinuity of creditor control rights upon covenant violations, we control for linear and higher-order of an array of financial performance metrics on which the most common financial covenants are written (covenant performance variables hereinafter; Nini,

[7] Nini, Smith, and Sufi (2012) document that in any given year between 10% and 20% of firms report being in violation of a financial covenant in a credit agreement

[8] The data is available on Amir Sufi's website.

Smith, and Sufi 2012).[9] The model is specified as follows:

$$Cash\_ETR_{i,t} = \alpha_0 + \beta_0 Violation_{i,t-1} + \beta_1 X_{i,t} + \beta_1 Z_{i,t-1} + High\text{-}order\ Z_{i,t-1} + \eta_i + \lambda_t + \varepsilon_{i,t}\ , \quad (1)$$

where i indexes firm, t indexes fiscal year; *Cash_ETR* is cash-effective tax rates; X is the set of firm characteristics; Z are performance metrics on which loan covenants are usually written; *High-order Z* are the second powers of all the covenant performance variables; η_i and λ_t are firm and year fixed effects, respectively; and $\varepsilon_{i,t}$ is the error term. Following prior studies, we focus on new violations (Chava and Roberts 2008; Nini, Smith, and Sufi 2012): $Violation_{t-1}$ is defined to be equal to one for firm-years that experienced a violation in t-1 but did not experience a violation in t-2.

The discontinuity design is employed in our main regression to control for all the continuous change of financial variables surrounding covenant violations and isolate the effect of bank control rights on tax avoidance. However, one may still concern that the violating firms and non-violating firms differ in certain unobserved characteristics, such as financial constraint and earnings manipulation. To mitigate this concern, we further perform a propensity-score matching analysis. To construct a propensity-score matched sample, we first estimate the probability of having a new violation in a fiscal year using a probit model. The dependent variable is $Violation_{t-1}$ and the lagged covariates include firm size, market-to-book, ROA, cash holding, leverage, capital expenditure, abnormal accruals, cash flow, interest expenses, current ratio, and net worth. We also control for industry and fiscal year fixed effects in the model. The estimation result generates a fitted probability for each firm-year observation. We then match each violating firm with a non-violating firm that has the closest probability of violation in the same fiscal year. After matching, we have

[9] To yield an unbiased estimate, the functional form of the RD regression needs to be correctly specified. In particular, the outcome variable could have a non-linear relation with the assignment variables. To ensure that non-linearity is not a concern in our RD model, we follow prior studies and control for the higher orders of covenant variables.

in total 13,175 observations left in our sample.

3.2. Sharp-RD design using loan covenant data from Dealscan

To supplement the results using reported violation information, we also employ a sharp RD design that relies on detailed financial covenant information, including covenant types and thresholds, to identify the effect of bank control rights. The merit of this sharp RD design is that we are able to compute the distance to violation threshold, so as to identify firms that barely passed the covenant threshold and those that barely failed it. These two groups of firms should be highly similar to each other such that the violation, or bank's control rights, can be seen as randomly assigned around the threshold.

We obtain information on covenant types and covenant thresholds from Loan Pricing Corporation's (LPC) Dealscan database.[10] We select firms whose loan start dates are between 1996 and 2007, a period that coincides with the quasi-RD design. The basic unit of observation in Dealscan is a loan. Loans are usually grouped into packages, and covenant information is specific to each loan package. To obtain firms' accounting information, we match loan packages and covenant information to Compustat.[11] Financial firms (SIC codes between 6000 and 6999) are excluded.

Covenant violations happen either because the accounting variable falls short of a minimum limit (threshold) or because the variable goes above a maximum limit. To account for both cases, we define a variable that measures the relative distance of the accounting variable to the limit (*Distance*). Specifically, for a covenant that sets a minimum limit, *Distance* is calculated as $(r' - r) / \text{abs}(r')$, where r' is the actual accounting variable, r is the covenant threshold, and abs means absolute value. For a covenant that sets a maximum

[10] Dealscan provides comprehensive information on loans to large U.S. corporations primarily through self-reporting by lenders, SEC filings, and its staff reporters (Strahan 1999).
[11] We use the link table provided by Chava and Roberts (2008) to match Dealscan with Compustat.

threshold, *Distance* is calculated as − (*r'* − *r*) / abs(*r'*). The extra negative sign is to ensure a consistent interpretation with that of the minimum type of covenant: (i) *Distance* being negative indicates a covenant violation; (ii) the smaller the *Distance*, the closer the borrower is to violation (when *Distance* > 0) or the more severely the covenant is violated (when *Distance* < 0).[12]

Next, we construct our sample in a way that minimizes the possibility of managerial manipulation. The detailed sample-selection process is presented in Appendix A, which leads to a sample of three types of covenant: minimum EBITDA, maximum debt to EBITDA, and maximum senior debt to EBITDA. The McCrary (2008) test result for the pooled sample of these types, also reported in Appendix A, suggests the non-existence of precise manipulations at the covenant thresholds.[13] Furthermore, to implement the sharp RD design, we need to estimate the optimal bandwidth for *Distance* surrounding the covenant thresholds. The choice of bandwidths involves a tradeoff between bias and efficiency.[14] We follow the estimation method articulated by Calonico, Cattaneo, and Titiunik (2015), and restrict the absolute value of *Distance* to be less than 0.5 (or bandwidth is 0.5). After imposing this requirement, we have 2,540 firm-year observations in the final sample, only around 10% of our original sample. The small bandwidth ensures that we are looking at firm-year observations that are close enough to the covenant thresholds, such that we can use "local linear regressions" (Lee and Lemieux 2010).

We then estimate the following regression model:

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[12] Some firms have multiple covenants and thus multiple *Distance* values in a fiscal year, in which case, we use the minimum value of *Distance* across covenants, i.e., the covenant that is either closest to violation or most severely violated. This *Distance* more accurately reflects the intensity of bank intervention, since it picks out the bank that have the strongest incentive to take over the control.

[13] McCrary algorithm can empirically test the existence of manipulation for RD designs by looking at the density of observations at the threshold. If there is a discontinuity in the density of observations at the threshold, it suggests that firms can precisely manipulate their accounting variables to avoid covenant violations. We find that the density of observations does not show any evidence of discontinuity at the threshold.

[14] Too wide a bandwidth increases the accuracy of the estimate by including more observations but at the risk of introducing bias (Roberts and Whited 2012).

$$Cash\_ETR_{i,t} = \alpha_0 + \beta_0 Violation_{i,t-1} + \beta_1 Distance_{i,t-1} + \beta_2 X_{i,t-1} + \eta_i + \lambda_t + \varepsilon_{i,t} \ , \quad (2)$$

where *Distance* is defined as earlier; *Violation* indicates a new covenant violation, which is a financial covenant violation in a single fiscal year (as indicated by negative *Distance*) by a firm that has not experienced a violation in the previous fiscal year; *X* is the set of firm characteristics; and η_i and λ_t are firm fixed effects and year fixed effects. To account for potential nonlinearity of *Cash_ETR* vis-à-vis the relative distance, in addition to equation (2), we also estimate polynomial functions by controlling for higher orders of *Distance*.

3.3. Variables and summary statistics

3.3.1. Tax avoidance variables

Following prior research, we use cash effective tax rates (*Cash_ETR*) to measure the overall level of cash tax planning (e.g., Dyreng, Hanlon, and Maydew 2008). Specifically, *Cash_ETR* is defined as cash taxes paid divided by pretax book income less special items.[15] *Cash_ETR* captures all actions taken by firms to reduce their total cash income tax (Law and Mills 2015; Edward, Schwab, and Shevlin 2016).

3.3.2. Covenant performance variables

Following Nini et al. (2012), we control for lagged covenant performance variables to account for the distance to covenant violations in the quasi-RD approach. The covenant performance variables include net worth to assets ratio, current ratio, interest expense to assets ratio, operating cash flow to assets ratio, capital expenditure, market to book, and leverage ratio. Note that market to book and leverage ratio are also important determinants of tax planning. To account for the potential non-linearity in covenant

[15] Our results continue to hold if we calculate effective tax rates without adjusting for special items.

performance variables, we also include the second power of these covenant variables. Appendix B provides detailed definitions of all these variables.

3.3.3. Firm characteristics

To isolate the effect of bank monitoring, we control for various determinants of tax planning or avoidance, including firm size, market-to-book ratio, ROA, foreign assets, intangible assets, property, plant, and equipment, new investments, equity income in earnings, financial leverage, cash holdings, and abnormal accruals. We control for firm size and market-to-book ratio to capture fundamental firm characteristics following most tax avoidance research (e.g., Chen, Chen, Cheng, and Shevlin 2010; Dyreng, Hanlon, and Maydew 2008). We include ROA to capture firm profitability, which can affect the incentives and needs to avoid taxes. We control for foreign assets because Rego (2003) finds that multinational firms with more extensive foreign operations have more opportunities for tax planning. We include intangible assets, property, plant, and equipment, new investments, and equity income in earnings to control for the effect of a firm's investment activities on book-tax differences. We include financial leverage to capture the effect of the tax shield of debt. Higher debt-tax shields can reduce marginal tax rates and the incentives for incremental tax planning (Graham 1996a, 1996b, 2000). We also control for cash holdings to capture the incentives of tax planning given that firms with more cash can have less need to avoid taxes. On the other hand, tax aggressive firms may hold more cash as a precautionary measure for future settlements with the IRS (e.g., Hanlon, Maydew, and Saavedra 2014). Abnormal accruals are included to control the potential effect of earnings management on book-tax differences (e.g., Frank, Lynch, and Rego 2009). Finally, we control for Net Operating Loss, defined as the change in net operating loss carryforwards scaled by lagged assets, to absorb the effect of tax deductions caused by loss carryforwards.

3.3.4. Summary statistics

Panel A of Table 1 presents descriptive statistics for the main regression variables using the sample of reported covenant violations. The mean (median) value of *Cash_ETR* is 0.25 (0.23), largely consistent with prior studies (e.g., Edward et al. 2016, Cen, Maydew, Zhang, and Zuo 2016). The mean of *Violation* is 0.04, suggesting that about 4% of firm-year observations experience a new violation. The proportion of firms that ever violated a covenant during 1996-2007 is 36% (unreported), implying that covenant violations occur frequently (Dichev and Skinner 2002). The average firm in our sample has a book value of total assets of $3,430 million, a market-to-book ratio of 1.93, an ROA of 11%, an intangibility of 15.3%, and a leverage of 22%.

Panel B of Table 1 reports summary statistics of variables using the Dealscan sample. We focus on firm-year observations that fall in the optimal bandwidth. The mean (median) value of *Cash_ETR* is 0.24 (0.21), which is similar to that in the main sample. Since we are focusing on the narrow band around the threshold, the proportion of violating firms are much larger than that in the main sample (21.3%). The average firm size is $2,067 million, smaller than that in the main sample. The average market-to-book, ROA, intangibility, and leverage is 2.38, 7.6%, 27.5%, and 32.1%, respectively, suggesting that firms close to the violation threshold, as compared with those in the main sample, have more growth opportunities, are less profitable, have more intangible assets, and have higher leverage.

4. Empirical results

4.1. Baseline results

Our baseline results are obtained from estimating model (1) using the sample of reported violations. Table 2 presents the regression results. In columns 1 to 3 we control for industry fixed effects and in columns 4 to 6 we control for firm fixed effects. Column 1

shows that a one-year lagged covenant violation leads to a 4.5 percentage point decline in *Cash_ETR*, without controlling for observable firm characteristics. In column 2, after we control for firm characteristics, the coefficient on *Violation* becomes 0.027, which is statistically significant at 1% level. To make sure our results are not affected by possible non-linearity, we additionally control for the quadratic terms of covenant variables (*cash flow, interest expense, current ratio, and net worth ratio*) in column 3, and the results are quantitatively similar (0.025, t = 3.277). The coefficients, after controlling for firm fixed effects, are 0.039, 0.024, and 0.023, respectively, all statistically significant at 1% level.

The effect of covenant violations on tax avoidance is also economically significant. The coefficient on *Violation* is 0.023 in Column 6, which amounts to about 9.4% of the sample average of *Cash_ETR,* or approximately $8.2 million in annual cash tax savings for an average firm in our sample. Regarding control variables, we find that large firms and firms with a lower ROA, lower property, plants and equipment, higher new investment, lower leverage, higher capital expenditures, higher net operating loss, and higher absolute abnormal accruals tend to have higher cash effective tax rates. These findings are consistent with prior studies.

Overall, our baseline regressions show that bank monitoring encourages firms to save more cash income taxes. More importantly, to the extent that the discontinuity design isolates the discontinuity of banks' monitoring upon covenant violations, our baseline findings point to a causal inference on the positive effect of bank monitoring on tax avoidance.

4.2. Analysis based on propensity-score matched sample

One concern to our baseline findings is that the results are driven by unobservable differences between violating and non-violating firms before the covenant violation, such as

the degree of financial distress or the propensity of earnings manipulation to avoid the violation. Prior studies find that financially constrained firms are more likely to engage in more tax avoidance (Law and Mills 2015; Edwards et al. 2016). It is also well documented in accounting literature that firms usually manipulate earnings to avoid covenant violations (Watts and Zimmerman 1986; DeFond and Jiambalvo 1994; Dichev and Skinner 2002).[16]

To mitigate this omitted-variable concern, we perform additional analyses based on propensity-score matched sample.[17] A merit of using propensity-score matched sample lies in that the treatment firms (violating firms) and matched control firms are similar in matched dimensions such that they are likely to have a parallel trend (of both observable and unobservable traits) before the violation event. The matching procedures, which result in a sample of 13,175 observations of treated and control firms, are detailed in an earlier section.

Using this propensity score matched sample, we estimate our regression model (1) and report the regression results in Panel A of Table 3. Columns 1 to 3 control for match-pair fixed effects and 4 to 6 control for firm fixed effect. The pair fixed effects enable us to examine the effect of bank monitoring on cash-effective tax rates within each matched pair. Coefficients on control variables are omitted for brevity. Column 3 shows that after a new violation, the cash-effective tax rates decline by 2.6 percentage points, a result highly

[16] There are also reasons to expect that firms may not avoid covenant violations through earnings manipulations. First, covenant thresholds are chosen in a bargaining process between the borrowers and the lenders and covenant violations are not determined from SEC filings, but from private compliance reports to the lenders. Therefore, borrowers may not have incentives to manipulate their reports in this private communication process. Second, the features of bank loan contracting, such as repeated nature of corporate lending, the importance of lending relationships, and the expertise and monitoring role of relationship lenders, may further mitigate the incentives and ability of borrowers to manipulate their reports. Finally, to the extent that manipulation is more likely to occur when a firm has more financial flexibility (i.e., the financial condition is not too bad), we should observe more large-amount violations because firms can avoid small-amount violations through manipulation. However, Roberts and Chava (2008) show that when firms violate, they are more likely to violate covenants by a small amount than a large amount.

[17] We have controlled for absolute abnormal accruals in all our regressions to mitigate the influence of earnings manipulation.

similar with those in Table 2. In columns 4 to 6, the coefficient estimates are almost identical to those in columns 4 to 6 of Table 2.

We further perform a difference-in-differences analysis based on the propensity-score matched pairs. More specifically, for each violation event, we retain the seven-year window [-3, +3] around the violation year for both the violating firm and the matched control firm. We define years after each violation, [0, +3], as a post-violation period. We then replace new violation dummy with *Treat*Post* and retain all the control variables in equation (1) and estimate the regression model. The regression results are presented in Panel B, Table 3. We find that the difference-in-differences coefficients range from 0.025 to 0.032 controlling for pair fixed effects and from 0.024 to 0.031 controlling for firm fixed effects, which are similar to those obtained in previous regression analyses.

A critical assumption of difference-in-differences is that the treatment and control firms have a parallel trend of the outcome variable before the event.[18] To check this, we plot the time series of cash-effective tax rates for violating firms and matched control firms separately in Figure 1. Year 0 means experiencing a new violation in the prior year. Before a new violation, the *Cash_ETRs* of violating and non-violating firms are close to each other, implying that matched control firms represent a good counterfactual group for the treated firms. Right after a new violation, the *Cash_ETR* of treated firms declines and that of control firms increases, creating a divergence between the two groups. In addition, the divergence remains significant even several years after the new violation (goes as far as year 3). The median values, in the bottom panel, show similar patterns.

[18] Given the parallel trend of the two groups, a difference-in-differences estimate can difference out the inherent differences between the two groups, such as the degree of financial distress and the ability of manipulating earnings to pass the violation threshold.

4.3. Ruling out an alternative explanation

An alternative explanation to our empirical results is that the increase in tax avoidance following covenant violations is driven by tax loss carryforwards. To the extent that covenant violations are trigger by poor performance, the violating firms are more likely to have losses in the violating year. Because tax losses can be carried forward, in the next year or two after the firm violates the covenant, taxes are in part offset by the tax losses. This would generate a pattern of low effective tax rates for a couple of years after violations. However, it is unlikely that main findings are driven by loss carryforwards for at least two reasons. First, prior studies find that violating firms are outside of bankruptcy (Nini, Smith, and Sufi 2012), it is less likely that most violating firms are loss firms. Second, we have controlled for the change in net operating loss carryforwards in all our regressions.

To further rule out this possibility, we restrict our covenant-violation sample to firm-years that never experience a negative pre-tax income during the three years prior to a covenant violation. This step excludes only 247 firm-year observations in our sample. We further estimate our main regressions in the non-loss sample, and report the regression results in Table 4. We find that the results are very similar to our main findings in Table 2, suggesting that the increase in tax avoidance following covenant violations is not driven by the loss carryforwards.

4.4. Sharp-RD using Dealscan sample

The Dealscan sample provides detailed information on the type(s) of covenant a loan package contains and the precise threshold for each covenant. Utilizing this information, we compute the relative distance of each borrower to the covenant threshold(s), choose the types of covenant that are less subject to managerial manipulation, and conduct a sharp RD design. In particular, we estimate equation (2), in which *Violation* is inferred by a negative

Distance, indicating a new violation in the prior year. The optimal choice of *Distance* follows Calonico, Cattaneo, and Titiunik (2015) and is set to be 0.5 on either side of the threshold.

Table 5 presents the results. In the first column we only control for firm and year fixed effects in addition to *Violation* dummy. The result shows that after a covenant violation, the cash-effective tax rates decline by 2.5 percentage point. When controlling for all firm characteristics in column 2, the reduction becomes 2.3 percentage point. Adding the second power of *Distance* in column 3 generates a similar estimate (-0.023).[19] Note that these estimates are quantitatively similar as those when we use the reported violation sample.

To further mitigate the concern of potential model misspecification, we also conduct nonparametric analyses by graphically showing the tax avoidance of violating firms and non-violating firms following the violation year ($Cash\_ETR_t$). Violating (non-violating) firms are defined based on $Distance_{t-1}$. We divide each side of the covenant threshold into 10 bins based on $Distance_{t-1}$ and calculate the average $Cash\_ETR_t$ of firms within a bin.[20] Figure 2 presents the results. We find a significant discontinuity at the covenant threshold: The *Cash_ETR* of violating firms is significantly lower than that of non-violating firms around the covenant threshold. This finding further points to a significant and positive effect of bank monitoring on tax avoidance.

As a robustness check, we repeat the sharp RD design using two alternative bandwidths surrounding the covenant thresholds. First, we apply the method articulated by Imbens and Kalyanaraman (2009), which yields an optimal bandwidth of 0.8. Second, to mitigate the concern that the bandwidth is still too large and our results could be biased, we arbitrarily set the bandwidth to be as small as 0.3. In columns 5 to 8 of Table 5, we

[19] There is no need to control for covenant variables since we can observe the exact assignment variables based on covenant types.

[20] To account for the effect of individual firms' inherent level of tax avoidance and the yearly fluctuation of macro economy, we use the residual terms from the firm-level regressions of *Cash_ETR* on firm and year fixed effects.

perform the RD design using these two bandwidths and find very similar results with our main findings. Overall, results from the sharp RD design buttress the causal effect of bank monitoring on corporate tax avoidance.

4.5. Cross-sectional variations in the effect of bank monitoring

To further understand the underlying reasons why bank control rights and enhanced monitoring lead firms to save more cash tax, we explore cross-sectional variations in the relation between covenant violations and tax avoidance.

4.5.1. Relationship banking and the effect of covenant violations

Through repeated lending, a relationship bank can acquire private information about the borrower and monitor the borrower through frequent renegotiations (Srinivasan 2014). The tight monitoring by a relationship bank can reduce the agency costs of tax avoidance and allow firms to engage in a higher level of tax avoidance. Therefore, we expect the effect of enhanced bank monitoring following covenant violations to be weaker in the presence of relationship banking.

To test this conjecture, we follow Bharath, Dahiya, Saunders, and Srinivasan (2007) and define a loan as relationship loan if a bank has lent to the same firm at least once during the past three years. The bank in the relationship loan is defined as a relationship bank. We then split our sample into two subsamples: firms with a relationship bank and firms without a relationship bank in year *t*-1.

We estimate our main regression using the two subsamples and report the results in columns 1 and 2 of Table 6. For firms with a relationship bank, we find that the effect of covenant violations on *Cash_ETR* is only -0.016 and insignificant. In contrast, for firms without a relationship bank, the effect is -0.045 and statistically significant. Note that this

magnitude is almost twice as much as that obtained in the whole sample. The difference of coefficients between these two subsamples is statistically significant (p = 0.05). These findings support the importance of ex ante renegotiation and monitoring in the relation between ex post bank monitoring and corporate tax planning.

4.5.2. Information asymmetry and the effect of covenant violations

Ex ante information asymmetry would exacerbate the problem of managerial rent seeking under the cover of complex tax planning activities. To the extent that such problems associated with information asymmetry are not resolved ex ante by loan contract terms (such as interest rates and collateral requirement), they increase the necessity and strength of creditor control rights upon covenant violations (Garleanu and Zwiebel 2008). In this case, banks' intensified monitoring following covenant violations can play a more pronounced role in reducing agency costs associated with tax avoidance. We thus expect the increase in tax avoidance after covenant violations to be more pronounced for borrowers with a higher level of information asymmetry.

We proxy for information asymmetry using the number of analysts following a firm (analyst coverage). Analyst coverage is widely documented by prior studies to mitigate the degree of information asymmetry (Kelly and Ljungqvist 2012; among others). Using the IBES database, we first compute the number of analysts following each firm in year t-1, and then split our sample into two subsamples based on the sample median of analyst coverage. We then estimate the baseline regression separately in these two subsamples. Columns 3 and 4 of Table 6 report the results. We find a large decrease in *Cash_ETR* for firms with a lower level of analyst coverage following a covenant violation (-0.036). For firms with high ex ante analyst coverage, the effect of covenant violation is small and

statistically insignificant (-0.007). The *p*-value of testing the difference of coefficients between the two subsamples is 2%.

4.5.3. Economic policy uncertainty and the effect of covenant violations

The fundamental reason for which an optimal contract will allocate control rights to the lender is the incomplete nature of debt contracts. Ex ante, lenders and borrowers cannot specify all future contingencies in a contract. Without frequent renegotiations (with the bank) about the sharing of benefits and costs of tax planning, shareholders may have to give up valuable opportunities of tax avoidance. The problem of contract incompleteness and consequent suboptimal level of tax planning would vanish only if the states of the world are perfectly predictable. While such a scenario never exists in the real world, we can proxy for scenarios with extremely low uncertainties using empirical measures.

Based on economic news in 10 leading U.S. newspapers, Baker, Bloom and Davis (2016) develop an index of economic policy uncertainty (EPU). The index is higher in a given time period when the newspapers contain more terms regarding economic policy uncertainty in the same period. Using this index, we are able to test if our results are weaker when the states of the world are less volatile (more predictable). We classify observations as facing extremely low uncertainties when the EPU index in the prior fiscal year is in the bottom tercile among all years. Since Baker et al. (2016) construct the EPU index at a monthly frequency, we compute the 12-month moving average value for the month prior to the fiscal year beginning.

We then estimate model (1) for low-uncertainty group and high-uncertainty group, respectively. The result is presented in columns 5 and 6 of Table 6. When economic policy uncertainties are low, the coefficient on *Violation* is only -0.007 and insignificant. However,

when uncertainties are high, the estimate becomes -0.032 and statistically significant. The differences between the two coefficients are also statistically significant ($p = 0.08$).

4.5.4. Institutional ownership and the effect of covenant violations

The free-rider problem due to the dispersed equity ownership structure places shareholder in a worse position to mitigate managers' rent-seeking incentives. Such a free-rider problem can be resolved by concentrated ownership of institutional investors since the monitoring can be largely delegated to institutional investors who have stronger incentives to monitor the self-interested managers. In this respect, a presence of large institutional investors would reduce the importance of bank monitoring and therefore decrease the effect of covenant violations on tax planning activities.

Following prior studies, we compute the percent of shares held by the institutional investor that holds the largest ownership of a firm in year *t*-1 (*Largest inst. own.*), and divide our sample into two groups by the median level of *Largest inst. Own.*[21] We then estimate the baseline regression separately in these two subsamples. The results are reported in columns 7 and 8 of Table 6. Consistent with our expectation, we find that covenant violations in firms with a higher level of *Largest inst. own.* exhibit a small and insignificant effect on *Cash_ETR* (-0.011). However, we find a much larger decrease in *Cash_ETR* for firms with a lower level of *Largest inst. own.* (-0.034). The *p*-value of testing the difference of coefficients between the two subsamples is 12%.

[21] Using alternative measures of institutional ownership, such as blockholder dummy (1 if a company has an institutional investor that owns more than 5% of the total shares and 0 otherwise) and top 5 institutional ownership (percent of total shares owned by the largest 5 institutional investors in a firm), does not alter our results.

4.6. Covenant violations and tax risk

Due to the fact that the payoffs to creditors are concave whereas those to shareholders are convex, distressed firms may have incentives to invest in risky tax planning activities when the company is under shareholders' control, since such behavior can transfer wealth from creditors to shareholders (Jesen and Meckling 1976; Eisdorfer 2008). These risk-shifting actions may not be contractible ex ante, but once banks get the control rights, they are able to closely monitor the firm and force it to reduce these activities (Rego and Wilson 2012).

To test if banks discourage risky tax avoidance activities, we first estimate a series of quantile regressions across the entire tax avoidance distribution. Quantile regression allows us to determine whether the relationship between covenant violations and cash-effective tax rates varies across the distribution of cash-effective tax rates. As documented by Armstrong et al. (2015), only the most aggressive tax avoidance activities are perceived to be value-destructive and would be discouraged by an efficient corporate governance mechanism. In this respect, if the reduction in effective tax rates is driven by observations with a moderate or a lower level of tax avoidance activities, we can reach the inference that banks selectively encourage firms to explore safe and profitable tax planning opportunities.

We estimate model (1) at every tenth percentile of cash-effective tax rates, and report the coefficients on $Violation_{t-1}$ in Figure 3. We can see that the coefficient conditional on the bottom tenth percentile is the smallest, and keeps increasing with the percentile conditioned on until the 60th percentile, and then starts to decrease all the way to the 90th percentile. This evidence indicates a weaker relation between covenant violations and an increase in tax planning for lowest levels of cash-effective tax rates, and a stronger relation between violations and tax planning for a moderate and higher level of cash-effective tax rates.

To provide more evidence on the effect of bank monitoring on tax risk, we directly examine the change in tax risk after covenant violations. Following Gallemore and Labro (2015), we construct a measure of tax risk as the 5-year volatility of cash ETRs. In particular, for each firm in fiscal year *t*, we compute the standard deviation of cash ETRs over the 5-year window from *t* to *t* + 4. As argued by Gallemore and Labro (2015), firms with higher tax risk are likely to experience more volatility in tax outcomes. Next, to control for firm characteristics that determine tax risk, we construct moving average measures of all the determinants of tax planning activities, including firm size, market/book, ROA, foreign assets, intangible assets, PPE, cash holding, new investment, equity income in earnings, leverage, CAPEX, net operating loss, and abnormal accruals. Since we are interested in the effect of covenant violations on tax risk, we also control for *Violation*(*t*-1), covenant variables in *t*-1 and high-orders of covenant variables.

Panel A of Table 7 reports the results. All specifications control for firm and year fixed effects. In column 1, we do not control for any covariates, and the coefficient on *Violation* is negative and significant, implying that bank monitoring reduces the volatility of tax planning. In column 2, we control for 5-year moving average firm characteristics and covenant controls, and in column 3 we additionally include the quadratic terms of covenant performance controls. Again, both regressions show a negative and significant coefficient on the *Violation* dummy. Jointly with the evidence regarding cash ETRs, these findings suggest that while banks allow firms to engage in a higher level of tax avoidance following covenant violations, they also effectively reduce the risk of tax planning activities.

We also use unrecognized tax benefits (UTB) to gauge the risky tax avoidance activities. Since the disclosure of uncertain tax positions is required by FIN 48, we can only observe UTB after 2006. Therefore, our regressions using UTB are based on a very short panel, only including observations in 2006 and 2007. Panel B of Table 7 reports the results. We control

for industry fixed effects in columns 1 and 2, and both industry and year fixed effects in columns 3 and 4. Columns 2 and 4 additionally control for quadratic terms of covenant variables. All regressions show a negative and insignificant coefficient on the *Violation* dummy. This result suggests that bank control rights reduce the uncertain tax positions, but the effect is small.

5. Conclusion

In this study, we examine the effect of bank monitoring on corporate tax avoidance using an RD research design. The RD design helps us isolate the effect of bank monitoring from any other variables that could affect a firm's tax-planning behavior.

Our findings reveal that enhanced bank monitoring triggered by covenant violations lead firms to save more cash taxes through tax planning. We further hone in on the underlying mechanisms driving this result and find that the increase in tax planning concentrates among firms without a relationship bank, with severe information asymmetry, facing higher economic policy uncertainty, and having larger institutional ownership. We also find that bank monitoring leads to a lower level of tax risk.

Overall, our findings imply that financing friction (contract incompleteness) can distort the tax planning activities. The resolution of this friction, i.e., control rights reallocation and enhanced bank monitoring, can mitigate the agency costs and lead to a higher level of tax avoidance. To the extent that the transfer of control right and consequent tax planning activities lead to an improvement in the joint utility of the borrower and the lender (Agion and Bolton 1992; Dewatripont and Tirole 1994), our findings add to the under-sheltering puzzle in the tax literature (Weisbach 2002). We also contribute to the literature on the effect of corporate governance on tax avoidance by documenting the role of bank governance in facilitating corporate tax avoidance.

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Table 1. Summary statistics

This table presents summary statistics of the main variables for violating firms and non-violating firms separately. Panel A uses the sample of reported covenant violations. The sample includes 25,124 firm-year observations during the period 1996–2007. Violating firms refers to firms that violated a financial covenant at least once during the sample period. Non-violating firms refers to firms that never violated any financial covenant. Panel B uses the sample constructed from Dealscan. The sample includes 2540 firm-year observations during the period 1996–2007. Violating observations refers to firm-years that experienced a violation in the prior year, as inferred from the distance between covenant thresholds and corresponding accounting variables, and non-violating observations refers to the rest of observations. Appendix A provides detailed definitions for all variables.

Panel A. Sample of reported violations

	N	Mean	Median	Std. Dev.
Cash ETR	25124	0.245	0.231	0.199
Violation	25124	0.041	0.000	0.198
Size (million $)	25124	3429.622	407.951	12268.885
Market/book	25124	1.932	1.503	1.311
ROA	25124	0.114	0.092	0.086
Foreign assets	25124	0.207	0.031	0.285
Intangible assets	25124	0.153	0.067	0.197
PPE	25124	0.351	0.273	0.270
Cash holding	25124	0.160	0.070	0.208
New investment	25124	0.085	0.045	0.127
Equity income in earnings	25124	0.001	0.000	0.003
Leverage	25124	0.218	0.208	0.173
CAPEX	25124	0.060	0.044	0.054
Net operating loss	25124	0.004	0.000	0.055
Abnormal accruals	25124	0.058	0.039	0.063
Cash flow	25124	0.097	0.096	0.085
Interest exp.	25124	0.016	0.013	0.014
Current ratio	25124	2.446	1.898	1.954
Net worth	25124	0.516	0.503	0.199

Panel B. Dealscan sample

	N	Mean	Median	Std. Dev.
Cash ETR	2540	0.236	0.208	0.209
Violation	2540	0.213	0.000	0.410
Size (million $)	2540	2067.022	721.361	4092.928
Market/book	2540	2.375	1.915	1.783
ROA	2540	0.076	0.067	0.053
Foreign assets	2540	0.153	0.000	0.217
Intangible assets	2540	0.275	0.219	0.248
PPE	2540	0.360	0.269	0.285
Cash holding	2540	0.055	0.026	0.073
New investment	2540	0.073	0.025	0.146
Equity income in earnings	2540	0.001	0.000	0.003
Leverage	2540	0.321	0.318	0.152
CAPEX	2540	0.060	0.040	0.061
Net operating loss	2540	0.004	0.000	0.039
Abnormal accruals	2540	0.043	0.031	0.041

Table 2. Covenant violations and cash effective tax rates

Regressions estimating the effect of covenant violations on cash-effective tax rates. The sample period is 1996–2007. The dependent variable is *Cash_ETR* in year *t*. A new covenant violation (*Violation*) is defined as a financial covenant violation in a fiscal year by a firm that has not experienced a violation in the previous fiscal year. In parentheses below coefficient estimates, we report the robust standard errors adjusted for industry-level clustering (columns 1 to 3) and firm-level clustering (columns 4 to 6). ***, **, and * indicate statistical significance at levels 1%, 5%, and 10%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Violation(t-1)	-0.045***	-0.027***	-0.025***	-0.038***	-0.024***	-0.023***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Size(t-1)		0.004**	0.004**		0.041***	0.037***
		(0.002)	(0.002)		(0.004)	(0.005)
Market/book(t-1)		-0.013***	-0.008*		-0.014***	0.000
		(0.002)	(0.004)		(0.002)	(0.007)
ROA(t)		-0.004	-0.018		-0.353***	-0.371***
		(0.025)	(0.025)		(0.033)	(0.034)
Foreign assets(t)		0.010	0.011		-0.024*	-0.024*
		(0.017)	(0.016)		(0.013)	(0.013)
Intangible assets(t)		-0.018*	-0.004		-0.013	-0.006
		(0.010)	(0.011)		(0.018)	(0.018)
PPE(t)		-0.089***	-0.076***		-0.054**	-0.056**
		(0.020)	(0.019)		(0.022)	(0.022)
Cash holding(t)		-0.131***	-0.133***		0.012	0.008
		(0.010)	(0.011)		(0.015)	(0.016)
New investment(t)		-0.033	-0.050**		0.051***	0.041**
		(0.020)	(0.020)		(0.018)	(0.018)
Equity income in earnings(t)		0.140	0.355		-0.785	-0.737
		(1.032)	(1.043)		(0.818)	(0.823)
Leverage(t-1)		-0.036	-0.135**		-0.139***	-0.255***
		(0.033)	(0.052)		(0.038)	(0.068)
CAPEX(t)		0.014	0.025		0.191***	0.198***
		(0.063)	(0.060)		(0.055)	(0.055)
Net operating loss(t)		0.070***	0.066***		0.063**	0.061**
		(0.023)	(0.024)		(0.028)	(0.028)
Abnormal accruals(t)		-0.032	-0.030		0.085***	0.084**
		(0.030)	(0.030)		(0.033)	(0.033)
Cash flow(t-1)		0.214***	0.210***		0.170***	0.136***
		(0.024)	(0.027)		(0.027)	(0.030)
Interest exp.(t-1)		-1.768***	-2.270***		-0.711***	-1.153**
		(0.273)	(0.537)		(0.259)	(0.571)
Current ratio(t-1)		0.002	0.020***		-0.004**	0.004
		(0.002)	(0.003)		(0.002)	(0.004)
Net worth(t-1)		-0.006	0.307***		-0.078**	0.212***
		(0.027)	(0.051)		(0.035)	(0.075)
High-order controls	No	No	Yes	No	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	No	No	No
Firm FE	No	No	No	Yes	Yes	Yes
Adj. R squared	0.032	0.091	0.096	0.291	0.332	0.333
Observations	25124	25124	25124	25124	25124	25124

Table 3. Analyses using a propensity-score matched sample

The sample period is 1996–2007. In parentheses below coefficient estimates, we report the robust standard errors adjusted for matched pair-level clustering (columns 1 to 3) and firm-level clustering (columns 4 to 6). ***, **, and * indicate statistical significance at levels 1%, 5%, and 10%, respectively. Panel A presents results from regressions estimating the baseline model using a propensity-score matched sample. The matching process is described in the main text. The dependent variable is *Cash_ETR* in year *t*. A new covenant violation (*Violation*) is defined as a financial covenant violation in a fiscal year by a firm that has not experienced a violation in the previous fiscal year. Panel B reports results from difference-in-differences approach using the propensity-score matched sample. The dependent variable is *Cash_ETR* in year *t*. For each new covenant violation, we retain a seven-year window [-3, +3] around the year of violation (year 0). Post equals one for the violation year and the three years after each violation [0, +3], and equals zero otherwise. Treat equals one for firms that experienced a violation in the whole sample period, and equals zero for matched control firms.

Panel A. Baseline model

	(1)	(2)	(3)	(4)	(5)	(6)
Violation(t-1)	-0.040***	-0.027***	-0.026***	-0.039***	-0.024***	-0.023***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
All controls	No	Yes	Yes	No	Yes	Yes
High-order controls	No	No	Yes	No	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pair FE	Yes	Yes	Yes	No	No	No
Firm FE	No	No	No	Yes	Yes	Yes
Adj. R squared	0.173	0.217	0.222	0.248	0.292	0.295
Observations	13175	13175	13175	13175	13175	13175

Panel B. Difference-in-differences

	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Post	-0.032***	-0.027**	-0.025**	-0.031***	-0.025**	-0.024**
	(0.009)	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)
Treat	0.002	-0.006	-0.009			
	(0.010)	(0.010)	(0.010)			
All controls	No	Yes	Yes	No	Yes	Yes
High-order controls	No	No	Yes	No	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pair FE	Yes	Yes	Yes	No	No	No
Firm FE	No	No	No	Yes	Yes	Yes
Adj. R squared	0.088	0.134	0.139	0.25	0.297	0.299
Observations	6484	6484	6484	6484	6484	6484

Table 4. Firm-years that never experienced a negative pre-tax income during the past three years

Regressions estimating the effect of covenant violations on cash-effective tax rates. We only include firms that never experienced a negative pre-tax income during the past three years. The sample period is 1996–2007. The dependent variable is *Cash_ETR* in year *t*. A new covenant violation (*Violation*) is defined as a financial covenant violation in a fiscal year by a firm that has not experienced a violation in the previous fiscal year. In parentheses below coefficient estimates, we report the robust standard errors adjusted for industry-level clustering (columns 1 to 3) and firm-level clustering (columns 4 to 6). ***, **, and * indicate statistical significance at levels 1%, 5%, and 10%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Violation(t-1)	-0.044***	-0.027***	-0.025***	-0.038***	-0.024***	-0.023***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Size(t-1)		0.003**	0.003**		0.041***	0.037***
		(0.002)	(0.002)		(0.004)	(0.005)
Market/book(t-1)		-0.013***	-0.008*		-0.014***	-0.001
		(0.002)	(0.004)		(0.002)	(0.007)
ROA(t)		-0.004	-0.018		-0.352***	-0.370***
		(0.025)	(0.025)		(0.034)	(0.035)
Foreign assets(t)		0.010	0.011		-0.023*	-0.023*
		(0.017)	(0.016)		(0.014)	(0.014)
Intangible assets(t)		-0.018	-0.004		-0.010	-0.003
		(0.011)	(0.011)		(0.018)	(0.018)
PPE(t)		-0.091***	-0.079***		-0.060***	-0.061***
		(0.020)	(0.018)		(0.022)	(0.023)
Cash holding(t)		-0.133***	-0.134***		0.013	0.010
		(0.010)	(0.010)		(0.016)	(0.016)
New investment(t)		-0.034	-0.050**		0.049***	0.040**
		(0.021)	(0.020)		(0.018)	(0.018)
Equity income in earnings(t)		0.172	0.372		-0.746	-0.698
		(1.043)	(1.053)		(0.829)	(0.833)
Leverage(t-1)		-0.038	-0.135**		-0.147***	-0.255***
		(0.033)	(0.053)		(0.039)	(0.068)
CAPEX(t)		0.015	0.026		0.197***	0.204***
		(0.065)	(0.061)		(0.055)	(0.055)
Net operating loss(t)		0.071***	0.067***		0.065**	0.064**
		(0.023)	(0.024)		(0.028)	(0.028)
Abnormal accruals(t)		-0.037	-0.035		0.081**	0.080**
		(0.030)	(0.030)		(0.033)	(0.033)
Cash flow(t-1)		0.214***	0.211***		0.167***	0.133***
		(0.024)	(0.027)		(0.027)	(0.030)
Interest exp.(t-1)		-1.764***	-2.251***		-0.699***	-1.225**
		(0.273)	(0.552)		(0.264)	(0.585)
Current ratio(t-1)		0.002	0.020***		-0.004**	0.003
		(0.002)	(0.003)		(0.002)	(0.004)
Net worth(t-1)		-0.007	0.307***		-0.082**	0.209***
		(0.027)	(0.052)		(0.035)	(0.076)
High-order controls	No	No	Yes	No	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	No	No	No
Firm FE	No	No	No	Yes	Yes	Yes
Adj. R squared	0.032	0.091	0.096	0.293	0.334	0.335
Observations	24877	24877	24877	24877	24877	24877

Table 5. Sharp regression discontinuity design

Regression discontinuity designs estimating the effect of covenant violations on cash-effective tax rates. The dependent variable is *Cash_ETR* in year *t*. The sample period is 1996–2007. In columns 1 to 4, *Distance* is less than the optimal bandwidth proposed by Calonico, Cattaneo, and Titiunik (2015). In columns 5 and 6, the optimal bandwidth is estimated based on Imbens and Kalyanaraman (2009). Colums 7 and 8 use a bandwidth of 0.3. A new covenant violation (*Violation*) is defined as a financial covenant violation in a fiscal year by a firm that has not experienced a violation in the previous fiscal year. In parentheses below coefficient estimates, we report the robust standard errors adjusted for firm-level clustering. ***, **, and * indicate statistical significance at levels 1%, 5%, and 10%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Violation(t-1)	-0.025**	-0.023**	-0.023**	-0.039**	-0.019*	-0.018*	-0.039***	-0.039***
	(0.010)	(0.011)	(0.011)	(0.017)	(0.010)	(0.010)	(0.015)	(0.015)
Distance(t-1)		0.064***	0.064***	0.071***	0.051***	0.059***	0.035	0.034
		(0.022)	(0.022)	(0.023)	(0.015)	(0.015)	(0.047)	(0.047)
Violation*Distance				-0.092				
				(0.078)				
Size(t-1)		0.020*	0.020*	0.020*	0.014	0.014	0.028*	0.029*
		(0.012)	(0.012)	(0.012)	(0.010)	(0.010)	(0.017)	(0.017)
Market/book(t-1)		0.012**	0.012**	0.012**	0.010***	0.010***	0.011	0.011
		(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.007)	(0.007)
ROA(t)		-0.882***	-0.883***	-0.881***	-0.702***	-0.697***	-0.851***	-0.853***
		(0.120)	(0.120)	(0.120)	(0.095)	(0.095)	(0.170)	(0.170)
Foreign assets(t)		0.052	0.052	0.051	0.032	0.034	-0.047	-0.051
		(0.055)	(0.055)	(0.055)	(0.047)	(0.047)	(0.075)	(0.076)
Intangible assets(t)		0.031	0.031	0.033	0.012	0.007	0.034	0.034
		(0.046)	(0.046)	(0.046)	(0.036)	(0.036)	(0.066)	(0.066)
PPE(t)		-0.124**	-0.124**	-0.124**	-0.067	-0.072	-0.100	-0.102
		(0.062)	(0.062)	(0.062)	(0.048)	(0.048)	(0.091)	(0.091)
Cash holding(t)		0.036	0.036	0.036	-0.030	-0.027	-0.177	-0.175
		(0.112)	(0.112)	(0.112)	(0.089)	(0.089)	(0.166)	(0.166)
New investment(t)		0.056	0.056	0.053	0.051	0.052	0.042	0.045
		(0.046)	(0.046)	(0.046)	(0.037)	(0.037)	(0.068)	(0.069)
Equity income in earnings(t)		1.078	1.094	1.080	0.045	-0.037	1.423	1.374
		(1.935)	(1.937)	(1.936)	(1.695)	(1.694)	(2.706)	(2.708)
Leverage(t-1)		-0.017	-0.016	-0.021	-0.002	-0.005	0.003	0.004
		(0.063)	(0.063)	(0.063)	(0.051)	(0.051)	(0.086)	(0.086)
CAPEX(t)		0.062	0.064	0.066	0.056	0.050	0.121	0.124
		(0.145)	(0.145)	(0.145)	(0.121)	(0.121)	(0.198)	(0.198)
Net operating loss(t)		-0.106	-0.104	-0.106	-0.066	-0.061	-0.283	-0.283

		(0.111)	(0.111)	(0.111)	(0.093)	(0.093)	(0.174)	(0.174)
Abnormal accruals(t)		0.247**	0.249**	0.258**	0.114	0.124	0.135	0.135
		(0.120)	(0.120)	(0.120)	(0.100)	(0.100)	(0.172)	(0.172)
High-order Distance	No	No	Yes	No	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R squared	0.358	0.403	0.402	0.403	0.379	0.381	0.378	0.378
Observations	2540	2540	2540	2540	3347	3347	1696	1696

Table 6. Subsample analyses

Regressions estimating the effect of covenant violations on cash-effective tax rates using subsamples. For each violation event, we find the violating firm a matched control firm that has the closest propensity of experiencing a violation in the same fiscal year. The dependent variable is *Cash_ETR* in year *t*. The sample period is 1996–2007. *Relationship bank* refers to a bank that has lent to the same borrower at least once during the past 3 years before year *t*. *Analyst coverage* is high when the average number of analysts covering the firm in *t*-1 is higher than the sample median, and is low otherwise. *EPU* is classified as *Low* when the 12-month moving average index for the month prior to the fiscal year beginning of *t* is in the bottom tercile of the sample distribution. the *Largest inst. own.* of a firm in *t*-1 is higher than the sample median in that year, and is low otherwise. All variable definitions are in Appendix. In parentheses below coefficient estimates, we report the robust standard errors adjusted for firm-level clustering. ***, **, and * indicate statistical significance at levels 1%, 5%, and 10%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Relationship bank		Analyst coverage		EPU		Largest inst. own.	
	Yes	No	High	Low	Low	High	High	Low
Violation(t)	-0.016	-0.045**	-0.007	-0.036***	-0.007	-0.032**	-0.011	-0.034**
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
All controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R squared	0.331	0.355	0.372	0.308	0.343	0.353	0.355	0.373
Observations	14955	10169	12481	12643	8303	16574	9537	9536

p-value for the coefficient difference between (1) and (2): 0.05
p-value for the coefficient difference between (3) and (4): 0.02
p-value for the coefficient difference between (5) and (6): 0.08
p-value for the coefficient difference between (7) and (8): 0.12

Table 7. Covenant violations and tax risk

Regressions estimating the effect of covenant violations on tax risk. In panel A, the dependent variable is ETR volatility during *t* to *t+5*. The sample period is 1996–2007. Covenant variables are market/book, leverage, operating cash flow, interest expenses, current ratio, and net worth measured at *t-1*. High-order controls are the quadratic terms of these covenant variables. All other variables are measured as the moving average value over the five-year period ending in year *t+5*. High (Low) leverage denotes the group of firms that have a leverage higher than the sample median in year *t-1*. In panel B the dependent vairable is uncertain tax benefits scaled by total assets (UTB) in year *t*. The sample period is 2006–2007. A new covenant violation (*Violation*) is defined as a financial covenant violation in a fiscal year by a firm that has not experienced a violation in the previous fiscal year. In parentheses below coefficient estimates, we report the robust standard errors adjusted for industry-level clustering. ***, **, and * indicate statistical significance at levels 1%, 5%, and 10%, respectively.

Panel A. ETR volatility

	(1)	(2)	(3)	(4)	(5)
		Whole sample		High leverage	Low leverage
Violation(t-1)	-0.007**	-0.006**	-0.006**	-0.009**	-0.007
	(0.003)	(0.003)	(0.003)	(0.00)	(0.00)
Size_5		0.020***	0.020***	0.017***	0.026***
		(0.002)	(0.002)	(0.00)	(0.00)
Market/book_5		-0.012***	-0.012***	-0.012***	-0.012***
		(0.002)	(0.002)	(0.00)	(0.00)
ROA_5		-0.167***	-0.168***	-0.263***	-0.154***
		(0.014)	(0.014)	(0.03)	(0.02)
Foreign assets_5		-0.014	-0.014	-0.019	-0.020
		(0.009)	(0.009)	(0.02)	(0.01)
Intangible assets_5		-0.016	-0.017	0.047***	-0.049***
		(0.011)	(0.011)	(0.02)	(0.01)
PPE_5		-0.012	-0.013	-0.034*	0.026
		(0.013)	(0.014)	(0.02)	(0.02)
Cash holding_5		0.029***	0.030***	0.075***	0.011
		(0.010)	(0.010)	(0.02)	(0.01)
New investment_5		0.000	0.004	0.001	0.010
		(0.014)	(0.014)	(0.02)	(0.02)
Equity income in earnings_5		-0.153	-0.153	-0.134	-0.061
		(0.111)	(0.111)	(0.16)	(0.17)
Leverage_5		0.045***	0.046***	0.010	0.065**
		(0.016)	(0.016)	(0.02)	(0.03)
CAPEX_5		0.072*	0.073*	0.139**	0.025
		(0.038)	(0.038)	(0.05)	(0.06)
Net operating loss_5		-0.056***	-0.056***	-0.098***	-0.051**
		(0.015)	(0.015)	(0.03)	(0.02)
Abnormal accruals_5		0.055**	0.053**	0.132***	0.013
		(0.023)	(0.023)	(0.04)	(0.03)
Covenant variables(t-1)	Yes	Yes	Yes	Yes	Yes
High-order controls(t-1)	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj. R squared	0.510	0.521	0.521	0.553	0.559
Observations	23546	23546	23546	11774	11772

Panel B. UTB

	(1)	(2)	(3)	(4)
Violation(t-1)	-0.000	-0.000	-0.001	-0.000
	(-0.074)	(-0.028)	(-0.088)	(-0.046)
Size(t-1)	0.001**	0.001**	0.001**	0.001**
	(2.122)	(2.421)	(2.052)	(2.372)
Market/book(t-1)	0.001	0.001	0.001	0.001
	(0.712)	(0.513)	(0.735)	(0.552)
ROA(t)	-0.018*	-0.018*	-0.017*	-0.018*
	(-1.813)	(-1.935)	(-1.800)	(-1.922)
Foreign assets(t)	0.013	0.014	0.013	0.014
	(1.154)	(1.214)	(1.141)	(1.197)
Intangible assets(t)	-0.015***	-0.012*	-0.015***	-0.012*
	(-2.715)	(-1.944)	(-2.749)	(-2.002)
PPE(t)	-0.019***	-0.020***	-0.019***	-0.020***
	(-4.366)	(-3.205)	(-4.423)	(-3.252)
Cash holding(t)	-0.027*	-0.026	-0.027*	-0.027
	(-1.759)	(-1.588)	(-1.758)	(-1.591)
New investment(t)	-0.002	-0.008	-0.001	-0.008
	(-0.087)	(-0.376)	(-0.067)	(-0.359)
Equity income in earnings(t)	-0.035	-0.049	-0.036	-0.051
	(-0.401)	(-0.456)	(-0.413)	(-0.468)
Leverage(t-1)	-0.029	-0.106	-0.029	-0.105
	(-1.325)	(-1.375)	(-1.306)	(-1.363)
CAPEX(t)	-0.020	-0.014	-0.020	-0.015
	(-0.984)	(-0.812)	(-0.994)	(-0.827)
Net operating loss(t)	0.004	0.003	0.004	0.003
	(0.583)	(0.498)	(0.584)	(0.495)
Abnormal accruals(t)	0.101	0.105	0.101	0.105
	(1.117)	(1.140)	(1.120)	(1.144)
Cash flow(t-1)	-0.014	-0.000	-0.014	0.000
	(-1.255)	(-0.012)	(-1.232)	(0.005)
Interest exp.(t-1)	0.023	0.592	0.019	0.586
	(0.103)	(1.410)	(0.086)	(1.395)
Current ratio(t-1)	-0.000	-0.000	-0.000	-0.000
	(-0.074)	(-0.107)	(-0.087)	(-0.124)
Net worth(t-1)	-0.002	-0.005	-0.003	-0.005
	(-0.246)	(-0.088)	(-0.258)	(-0.097)
High-order controls	No	Yes	No	Yes
Year FE	No	No	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R squared	0.009	0.015	0.008	0.014
Observations	1302	1302	1302	1302

Figure 1. Difference-in-differences using propensity-score matched sample
The figure plots mean and median values of cash-effective tax rates against years to violation. Year 0 means experiencing a new violation in the prior year.

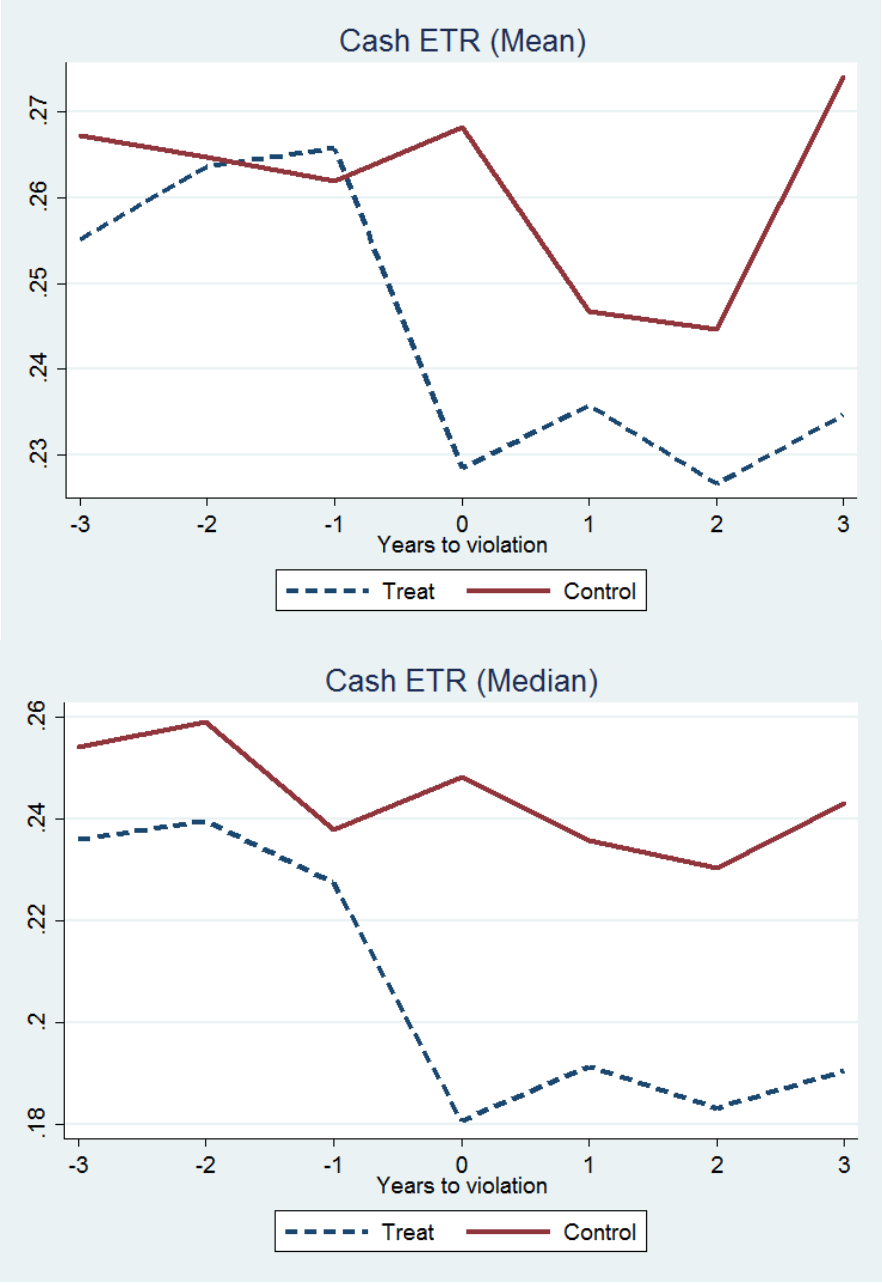


Figure 2. Sharp regression discontinuity design

Nonparametric analysis of the effect of covenant violations on cash-effective tax rates. The figure plots the mean values of cash-effective tax rates of violating firms and non-violating firms (in year t) that are close to the violating threshold (in year $t-1$). The horizontal axis shows the distance between the accounting variable and covenant threshold in year $t-1$ ($Distance_{t-1}$), which is measured as $(r' - r)/abs(r')$ for a minimum type of covenant and is $-(r' - r)/abs(r')$ for a maximum type of covenant, where r' is the actual accounting variable and r is the covenant threshold. The absolute value of $Distance_{t-1}$ is restricted to a value less than 0.5. We divide each side of the covenant threshold into 10 bins based on $Distance_{t-1}$, and each dot represents the average residual $Cash\_ETR_t$ of firms within a bin. Residual *Cash_ETR* is obtained from a regression of *Cash_ETR* on firm and fiscal year dummies.

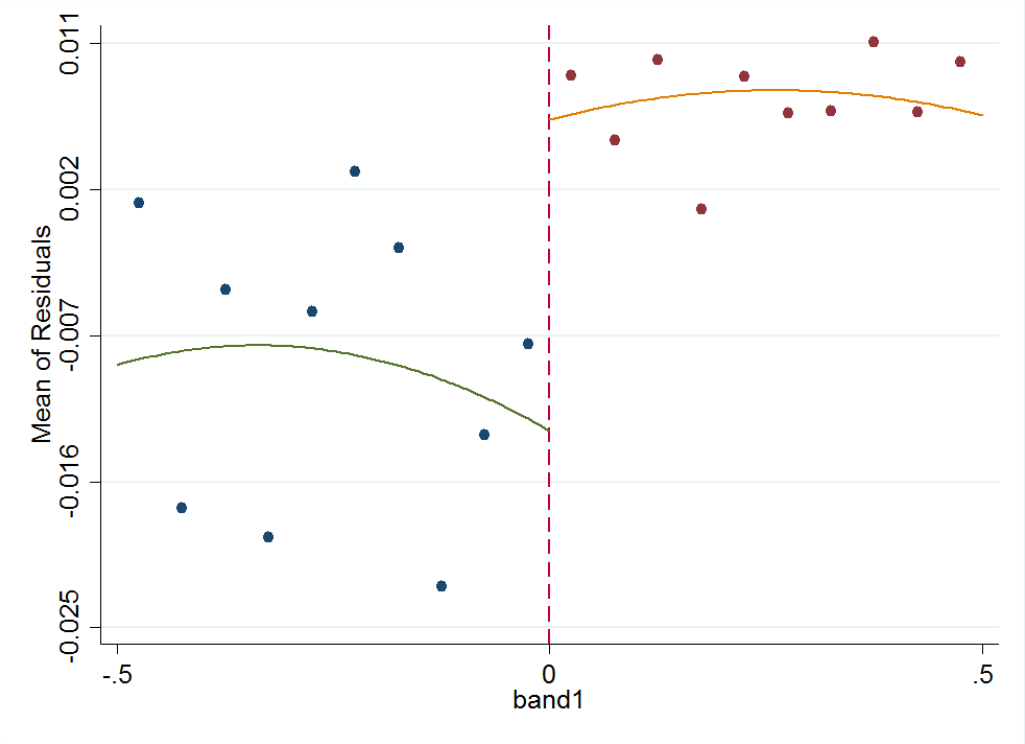
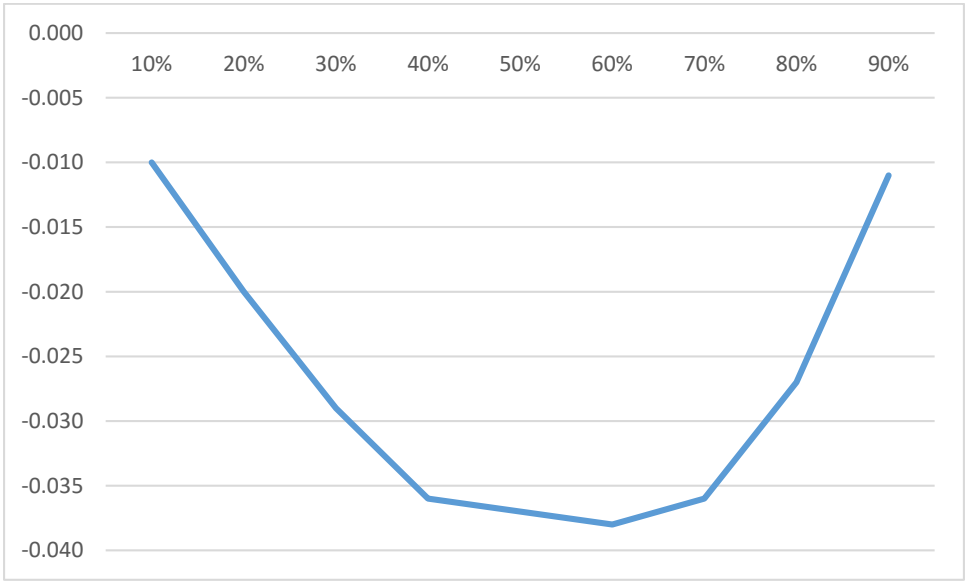


Figure 3. Quantile regression coefficients

Coefficients from quantile regressions of cash-effective tax rates on *Violation*, firm characteristics, High-order of covenant variables, firm fixed effects, and year fixed effects. The table reports the coefficient for each quantile and the figure plots the coefficients against the corresponding quantiles.

Quantile	10%	20%	30%	40%	50%	60%	70%	80%	90%
Coefficient	-0.010***	-0.020***	-0.029***	-0.036***	-0.037***	-0.038***	-0.036***	-0.027***	-0.011



Appendix A. Dealing with the issue of accounting manipulation

One may concern that the empirical results based on our RD setting would be plagued by potential accounting manipulations. If firms can precisely manipulate the accounting variable corresponding to a covenant and stay just above the threshold, the random assignment of treatment around the threshold would be breached. Prior accounting research suggests that firms tend to manage earnings *in advance* to avoid possible future technical default (Watts and Zimmerman 1986; DeFond and Jiambalvo 1994; Dichev and Skinner 2002). In contrast, recent finance studies argue that borrowers cannot *precisely* manipulate the financial figures to avoid violations, or, for the samples lying within the narrow band, the earnings manipulation is not statistically significant (Chava and Roberts 2008; Roberts and Sufi 2009b).

Our paper does not seek to reconcile these opposing findings. Instead, by a careful empirical design, we are able to pick out those covenants that are least likely to be subject to managerial manipulation. The idea is that we empirically examine the existence of manipulation for each covenant, and then include only those that show no evidence of manipulation in our sharp RD sample. First, based on Demerjian and Owens (2016), we pick those covenant types that generate the smallest measurement error if computed using Compustat items. This ensures that we can accurately identify the relative distance of firms' accounting variables to the corresponding threshold. Specifically, we require the accuracy rate of each type (if computed using Compustat items) to be higher than 80%. As shown in Demerjian and Owens (2016), this step leaves us six types of covenant.

Second, after determining the relative distance of each firm to the covenant threshold, we perform the McCrary test (McCrary, 2008) for each type of covenant. McCrary algorithm can empirically test the existence of manipulation for RD designs by looking at the density of observations at the threshold. If there is a discontinuity in the density of observations at

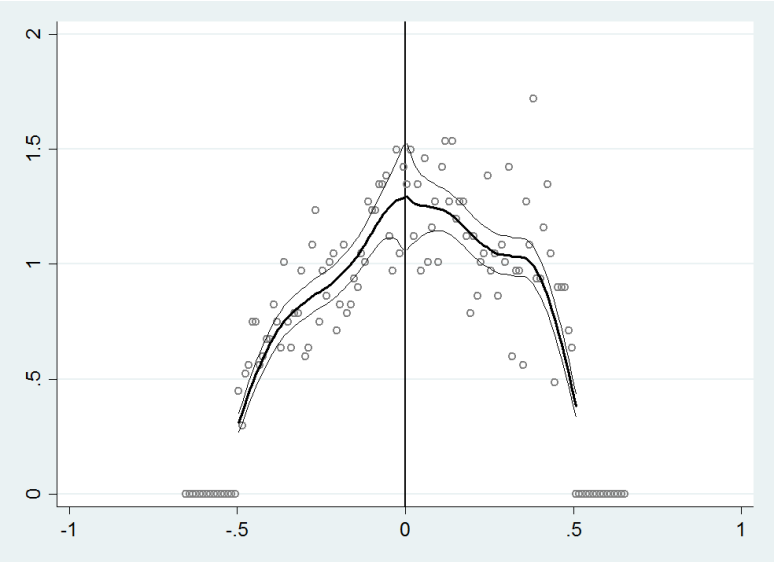
the threshold, it suggests that firms can precisely manipulate their treatment status. We conduct McCrary test for each type of covenant, and exclude firms that are bound by three types of covenant: minimum current ratio, maximum senior leverage, and maximum leverage. As a result, our main sample of sharp RD includes firms bound by the following types of covenants: minimum EBITDA, maximum debt to EBITDA, and maximum senior debt to EBITDA. We then conduct a McCrary test for the pooled sample of these three types in Panel A of Figure 1A. We can see that the density distribution of firms does not show any discontinuity at the threshold, implying an absence of precise manipulation in our selected sample.

Besides examining the density of observations, another way to show non-existence of perfect manipulation is to directly look at the discretionary accruals. We should observe a discontinuity of the level of abnormal accruals at the covenant threshold if firms above it are more likely (or able) to manipulate. To examine this, we plot the level of abnormal accruals of firms in the narrow band around the threshold against the relative distance to threshold (*Distance*) in the year of violation. To account for the effect of individual firms' inherent propensity of manipulation and the yearly fluctuation of the macro economy, we regress *Abnormal Accruals* on firm and year fixed effects and use the firm-level residual terms in our analysis. Next, we divide $Distance_{t\text{-}1}$ into ten bins on each side of the covenant threshold and calculate the average residual $Abnormal\ Accruals_{t\text{-}1}$ of firms within each bin. Year t-1 is the violation year. The result is shown in Panel B of Figure 1A. The abnormal accruals show no discontinuous change at the covenant threshold, which is not consistent with the existence of manipulation in our sample. In addition, we control for absolute abnormal accruals in all our regressions to further ensure that our results are not affected by the effect of earnings manipulation.

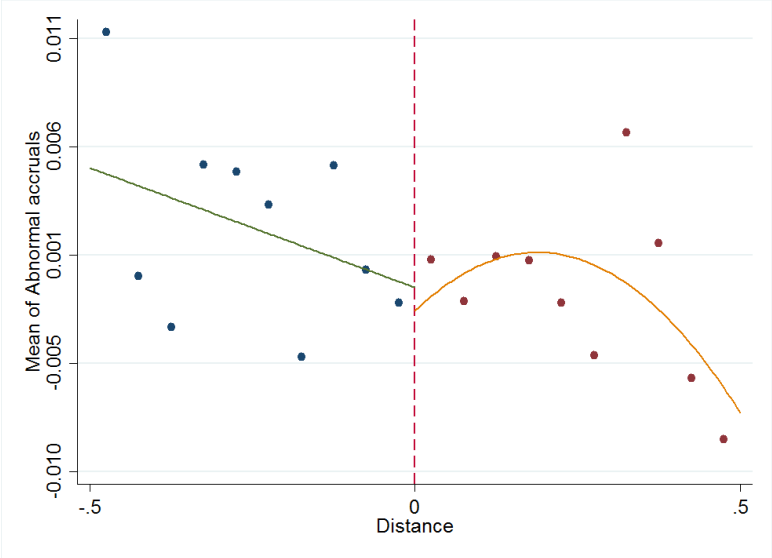
Figure 1A. Deal with potential manipulation

Panel A presents McCrary test for the pooled sample after excluding covenant types that fail the McCrary test. Panel B presents the abnormal accruals of firms surrounding the threshold in the violation year (year *t*-1). *Distance* is measured as *(r' – r)/abs(r')* for a minimum type of covenant and is *–(r' – r)/abs(r')* for a maximum type of covenant, where *r'* is the actual financial ratio and *r* is the covenant threshold. The absolute value of $Distance_{t-1}$ is restricted to a value less than 0.5. We divide each side of the covenant threshold into 10 bins based on $Distance_{t-1}$, and each dot represents the average residual $Abnormal\ accruals_{t-1}$ of firms within a bin. Residual *Abnormal accruals* is obtained from a regression of *Abnormal accruals* on firm and fiscal year dummies.

Panel A. McCrary test



Panel B. Abnormal accruals



Appendix B. Variable Definitions

Variable	Definition
Cash_ETR	Cash-effective tax rate, calculated as cash taxes paid divided by pre-tax book income less special items, or TXPD / (PI – SPI).
ETR volatility	The standard deviation of cash ETRs over the 5-year window from t to $t + 5$.
Distance	Distance between financial ratios and covenant thresholds, calculated as $(r' – r) / \sigma$ for a minimum type of covenant, and is $–(r' – r) / \sigma$ for a maximum type of covenant, r' is the actual financial ratio, r is the covenant threshold, and σ is standard deviation of r'.
Violation	New covenant violation, defined as a covenant violation in a single fiscal year (as indicated by negative distance) by a firm that has not experienced a violation in the previous fiscal year.
Size	Total assets, in log.
Market/book	Market value of assets divided by book value of assets, or (PRCC*CSHO + AT – CEQ) / AT.
ROA	Operating income before depreciation scaled by total assets, or OIBDP / AT.
Foreign assets	Foreign assets scaled by total assets, where foreign assets are estimated following Oler, Shevlin, and Wilson (2007).
Intangible assets	Intangible assets scaled by total assets, or INTAN / AT. When INTAN is missing, we use the value of Goodwill (GDWL).
PPE	Net property, plant, and equipment scaled by total assets, or PPENT / AT.
Cash holding	Cash and short term investments scaled by total assets, or CHE / AT.
New investment	New investments scaled by total assets, or (XRD + CAPX + AQC – SPPE – DPC) / AT.
Equity income in earnings	Equity income in earnings scaled by total assets, or ESUB /AT.
Leverage	The sum of current debt and long-term debt divided by total assets, or (DLC + DLTT) / AT.
Net operating loss	The change in net operating loss carryforwards (TLCF), scaled by lagged assets, or (TLCF – lag(TLCF)) / lag(AT)
Abnormal accruals	The absolute value of discretionary accruals, which are estimated from the performance-adjusted modified cross-sectional Jones model.
Cash flow	Net cash flows from operating activities scaled by total assets, or OANCF / AT.
Interest exp.	Interest expenses divided by total assets, or XINT / AT.
Current ratio	Current assets divided by current liabilities, or ACT / LCT.
Net worth	Net worth divided by total assets, or (AT – LT) / AT.
CAPEX	Capital expenditures divided by total assets, or CAPX / AT.
Relationship bank	An indicator variable, which equals 1 if a bank has lent to the same borrower at least once during the past 3 years and 0 otherwise.
Economic policy uncertainty index	An index of economic policy uncertainty based on newspaper coverage frequency in 10 leading U.S. newspapers (Baker, Bloom, and Davis 2016). Economic policy uncertainty index (taxes) is the economic policy uncertainty index specific to tax policies of the Federal Reserve.
Analysts coverage	Number of financial analysts following a firm in a fiscal year, as computed from the IBES database.
Largest inst. own.	The percent of shares owned by the institutional investor that hold the largest ownership in a firm.