Does Tax Transparency Improve or Impair Internal Control Quality?

Evidence from Country-by-Country Reporting

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ABSTRACT

We examine whether the U.S. non-public country-by-country reporting (CbCR) mandate affects multinationals' internal control quality. We argue that CbCR requires firms to change their information processing structures because the demanded information is not readily available, thus encouraging adjustments to internal controls but simultaneously increasing the risk of weakness exposure. We find that firms affected by CbCR have a significantly lower likelihood of having material weaknesses in their internal controls than control firms. The effect is limited to tax-related weaknesses but robust to falsification tests, sample balancing, and addressing the rarity of internal control weaknesses throughout the sample. We also show that our results are not driven by remediations of material weaknesses before the CbCR adoption. Finally, we observe that the response prevails in tax-aggressive firms and firms with low tax accrual quality. Our results are consistent with CbCR stimulating firms to improve their information-processing structures, resulting in better internal controls.

Keywords: tax transparency; country-by-country reporting; internal controls; financial reporting quality.

JEL Classification: G38, H25, H26, M41, M42, M48.

Data Availability: Data are available from the sources cited in the text.

I. INTRODUCTION

In light of growing public scrutiny about the justness of tax systems worldwide and the tax avoidance and income shifting activities of large multinational entities (MNEs), legislators, tax authorities, and researchers alike are discussing the feasibility and effectiveness of various measures to limit such harmful tax practices (Müller, Spengel, and Vay, 2020). At the center of this discussion is the OECD's and G20 member states' joint initiative on base erosion and profit shifting (BEPS). This project, aimed at harmonizing and optimizing international tax laws to stop such practices, proposes a set of instruments that countries shall introduce to prevent aggressive tax behavior. A central measure within this framework is to increase large corporations' tax transparency by disclosing a country-by-country (CbC) report. The motive for this disclosure requirement is to improve tax authorities' ability to audit firms' tax positions appropriately, simultaneously disincentivizing firms from continuing tax-aggressive practices. Prior research shows that CbC disclosure rules can help reduce tax avoidance and income shifting (Joshi, 2020) and lead firms to substantiate their economic activities in countries that may be perceived as being primarily used to optimize tax outcomes (De Simone & Olbert, 2021). Firms also appear to increase voluntary disclosures when facing tax transparency requirements under different disclosure schemes (Kays, 2022; Bilicka, Casi-Eberhard, Seregni, and Stage, 2022).

We conjecture that the adoption of non-public CbC reporting (CbCR), i.e., the obligation to annually disclose country-level information on taxes and economic substance to the local tax authorities, has implications for firms' information processing structures. Since CbCR requires firms to disclose information that is often not readily available at the required level, the regulation is likely accompanied by substantial procedural efforts required to maintain compliance (Hanlon, 2018). While the immediate objective of these efforts is to adjust information flows such that country-level data on operations and tax payments can be obtained,

the corresponding structural challenges are also relevant for firms' internal control systems. For instance, if existing documentation policies are revised or internal reporting obligations are updated, these changes do not only affect internal information processing structures but also have an impact on the effectiveness of these structures as instruments of the internal control system. However, when considering the implementation of CbCR legislation as a disruption to firms' financial reporting environments, the requirement to disclose CbC information *per se* renders the risk of exposure of material weaknesses. For instance, it reveals or channels existing deficiencies in information flows, thereby highlighting previously unobserved material weaknesses to managers and auditors. Moreover, tax authorities possibly use CbC data as a basis for improved risk-oriented tax audits (OECD, 2015), thereby altering the environment within which managers evaluate their internal controls. Given these opposing considerations and managerial discretion over their (pre-audit) assessment of the relevance of CbCR for financial reporting and the internal control system, it is ultimately an empirical question whether the newly introduced disclosures are associated with improvements or impairments of internal control quality.

For our analysis, we utilize disclosures of material weaknesses in internal controls under Section 404 of the Sarbanes-Oxley Act of 2002 (SOX Section 404) to proxy for internal control quality. This regulation requires managers of a firm to assess and report the effectiveness of its internal controls over financial reporting concerning specific objectives (SOX Section 404(a)). Management's assessment of internal control must then be attested by an external auditor (SOX Section 404(b)). We focus our analysis on audited internal control opinions to ensure that the assessment of internal control quality was conducted in accordance with SOX requirements and to mitigate concerns about managers' differing incentives resulting in underestimation of internal control weaknesses (Bedard & Graham, 2011). Thus, we define a firm-year's internal controls to be of low quality if the corresponding auditor report signals a

material weakness. We differentiate tax-related from non-tax-related material weaknesses, depending on whether disclosed internal control weaknesses are associated with income tax reporting. Since CbCR is primarily perceived as an instrument against tax avoidance (OECD, 2015; Joshi, 2020), we expect a stronger impact of the CbCR introduction on tax-related internal control quality than on non-tax internal control quality.

We employ a difference-in-differences analysis using logistic regression models to test our hypotheses empirically. Our sample comprises U.S.-based MNEs with sufficient data availability on internal control quality, auditor-client relationships, and firm characteristics. After controlling for a large set of variables identified by prior research as affecting the likelihood of material internal control weaknesses and including industry- and year-fixed effects, we find that the introduction of U.S. CbCR is associated with a significantly lower likelihood of reporting a tax-related material weakness in internal controls. Our analysis of marginal effects shows that this result is economically important. However, we do not find a significant effect of CbCR on the reporting of non-tax material weaknesses in internal controls. We interpret our finding as evidence for firms considering CbCR as relevant for internal controls related to the tax function.

We conduct a battery of robustness tests to reinforce the validity of our results. First, we examine the sensitivity of our findings to several falsification tests by exploiting both the size-based threshold of CbCR treatment and the enter-into-force date. None of the specifications yields significant results, reinforcing the presumption that our main tests reflect the introduction of CbCR in the U.S. instead of any broader trend. Second, to mitigate concerns about non-random treatment assignment biasing our results, we use entropy balancing and inverse-probability treatment weighting to reduce covariate imbalance in our sample. All main results continue to hold when using balanced samples. Finally, we explicitly address the eventuality of "rare event bias" stemming from the relatively rare occurrence of internal control

weaknesses throughout our sample and possibly affecting our conventional maximum likelihood estimates (King & Zeng, 2001). We re-estimate the logistic regressions under consideration of the penalized maximum likelihood estimation procedure by Firth (1993), which explicitly and effectively corrects bias stemming from rare dependent variable outcomes and/or small sample sizes. Results under Firth's Logit are similar to our main estimates, consistent with the latter not being the result of biased estimation. Overall, these tests support the view that our results are not driven by spurious treatment, a lack of covariate balance, or biased estimates stemming from rare outcomes in the dependent variable.

We also examine the sensitivity of our findings to important firm characteristics. First, we explore the role of recent remediations of internal controls before the introduction of CbCR rules to mitigate concerns about our results being mechanically linked to improvements in financial reporting quality over time caused by the need for internal control 'upgrading.' We address this issue both visually and by adding recent pre-treatment period remediations of internal controls to our primary model. We observe that firms with recent remediations can successfully improve their tax-related internal controls such that they do not report another tax-related internal control weakness in the post-CbCR period, irrespective of their CbCR treatment status. At the same time, controlling for recent remediations does not weaken our main results, consistent with the initial difference-in-differences estimates not merely reflecting this subgroup's CbCR-unrelated success in internal control improvement.

Next, we examine the sensitivity of our main results to companies' tax behavior in the years before CbCR treatment. Specifically, we expect firms with more aggressive tax outcomes and firms with lower-quality tax accruals to react differently to the CbCR mandate than others. To test this presumption, we split our sample into two groups of firms based on their pre-treatment levels of tax aggressiveness and tax accrual quality, respectively. Our results confirm that the effect of CbCR on tax-related internal control quality is concentrated in tax-aggressive

firms (based on the definition and measure developed by Balakrishnan, Blouin, and Guay, 2019) and firms with lower-quality tax accruals (using the measure developed by Choudhary, Koester, and Shevlin, 2016). This result is consistent with less tax-aggressive and higher-tax-accrual-quality firms requiring less internal adjustment following the regulatory challenges arising from the introduction of CbCR.

Our research makes several contributions to the literature. First, we contribute to the literature exploring the real effects of CbCR and other tax transparency regimes. Concurrent research shows that tax transparency alters corporate tax (Joshi, 2020) and (capital and labor) investment behavior (De Simone & Olbert, 2021) and decreases analysts' misalignment between tax and economic activities in their tax forecasts (Huang, Jiang, and Persson, 2021). Nessa, Persson, Song, Towery, and Vernon (2021) examine confidential U.S. tax administrative data (from CbC reports and other filings) and, although they do not find significant changes in tax outcomes after CbCR, show that the tax disclosure can provide tax authorities with incremental information about cross-border operations, consistent with firms collecting and disclosing additional financial information from within the group. Adams, Demers, and Klassen (2022) examine voluntary tax disclosures in sustainability reports, finding that a firm's likelihood to disclose general tax information increases in its effective tax rate (ETR), while the extent of disclosure of CbC data is lower when firms engage in tax-motivated income shifting. Examining the Australian public tax return disclosure scheme, Kays (2022) observes that firms voluntarily supplement mandated public disclosures with additional information to offset potential reputational costs, consistent with firms adjusting their financial reporting behavior in the light of mandatory disclosure. We add to this literature by showing that managers perceive CbCR as sufficiently relevant to adjust company-wide information flows to maintain compliance with tax regulations. By focusing on U.S. CbCR, we confirm that firms beyond the European Union (EU) respond to BEPS-based tax transparency rules.

Moreover, since the U.S., unlike most European countries, did not adopt the master and local file requirements recommended in BEPS Action 13 (KPMG, 2022), we isolate the effect of CbCR from these other transfer pricing reporting requirements. Thus, our inferences are explicitly attributable to the reportable information's country-level nature.

Second, we contribute to the literature on the determinants of the strength of internal controls. Prior research identifies a variety of firm characteristics, including firm size, multiple dimensions of complexity, financial constraints, and auditor type, that determine the likelihood of reporting a (material) internal control weakness identified under SOX Sections 302 and 404 (Ashbaugh-Skaife, Collins, and Kinney, 2007; Doyle, Ge, and McVay, 2007; Ogneva, Subramanyam, and Raghunandan, 2007). De Simone, Ege, and Stomberg (2015) shed light on the relevance of auditor engagement as they find that the likelihood of material weakness disclosure increases with audit fees but decreases with the provision of tax-related services by the auditor. Focusing on the characteristics of boards and audit committees, Chen, Knechel, Marisetty, Truong, and Veeraraghavan (2017) find that the number of independent board directors is associated with a lower likelihood of reporting an internal control weakness. Cheng, Felix, and Indjejikian (2019) show spillover effects of internal control quality stemming from audit committee members that previously experienced material weaknesses as directors of other firms. However, to the best of our knowledge, our study is the first to show that specific disclosure requirements (here, CbC disclosures on economic activities and tax payments) have implications for internal controls related to firms' tax functions.

Finally, our research yields implications for policymakers, authorities, and corporate decision-makers. We provide evidence that tax transparency regulation does not only affect companies' financial reporting and taxation behavior but also requires substantial in-house adaptions. While this form of indirect investment incentive possibly improves firms' internal information flows, enabling them to pursue more efficient business activities (Cheng,

Dhaliwal, and Zhang, 2013), adapting to these changed requirements is also costly for firms, making the new regulation an investment intervention aimed at increasing the alignment of interest with tax authorities. Considering the threshold-based nature of the CbCR mandate, this intervention possibly creates a competitive disadvantage for firms exceeding the threshold. This concern is particularly relevant in light of the ongoing debate about changing (i.e., lowering) the CbCR revenue threshold (e.g., OECD, 2020).[1] Moreover, our results are consistent with country-level information not always being readily available before CbCR is required (Hanlon, 2018). Therefore, firms not subject to the mandate possibly continue to operate without considering country-level data (esp. in the context of work performed by the tax function), making themselves vulnerable to transfer pricing and tax audits and transfer pricing-related operational challenges.

II. INSTITUTIONAL BACKGROUND, LITERATURE, AND HYPOTHESES

U.S. Country-by-Country Reporting

In 2013, the OECD and G20 member states jointly issued their action plan on BEPS, proposing 15 actions that countries shall adopt to prevent excessive multinational income shifting (OECD, 2013). In BEPS Action 13, one of the four minimum standards of the project, they recommend that countries implement rules mandating firms to annually disclose three types of data to the local tax authorities: (1) a master file that provides the authorities with general information about the entity, including a business overview, a description of transfer pricing policies, and the entity's financial and tax positions; (2) a local file supplementing the master file with more specific information regarding the local entity and its intercompany transactions; and (3) a CbC report that contains country-level information on the group's revenues, profits, and tax payments (OECD, 2015). While the first two are to be filed only

[1] The public comments on the 2020 Review of CbCR received by the OECD are available for download at: www.oecd.org/tax/beps/public-comments-received-on-the-2020-review-of-country-by-country-reporting-beps-action-13-minimum-standard.htm. A content analysis of these commects can be found in Müller, Schoenrock, and Spengel (2022).

directly with the local tax administrations to whose jurisdictions they apply, the CbC report must be filed by the global ultimate group parent company to its local tax authorities, who then forward the report to other tax jurisdictions.

With this tax transparency regime, the OECD intends to equip tax authorities with information that is relevant for making better-informed transfer pricing risk assessments and allows them to incorporate CbC information as a basis for firms' tax audits (OECD, 2015). As of 2021, more than 100 countries, including the G20 nations and all EU member states, have adopted CbCR rules in line with BEPS Action 13 (OECD, 2021). In the U.S., regulation in line with the OECD's CbCR recommendations has been adopted through 26 C.F.R. § 1.6038-4 (2017). However, the U.S. did not implement the proposed rules regarding the submission of master and local files.

The U.S. legislation requires the parent entity of a U.S.-resident MNE with prior-year consolidated group revenues of at least 850 M$ to file an annual CbC report. A group is defined as "multinational" if its U.S. parent company owns at least one firm that is tax resident in a tax jurisdiction other than the U.S. The disclosure requirement includes filing Form 8975 ("Country-by-Country Report"), containing identifying information of the MNE filing the report, and, for each tax jurisdiction in which the group operates, Schedule A to Form 8975 ("Tax Jurisdiction and Constituent Entity Information").[2] In Part I of Schedule A, the MNE shall report its global revenues (separated by payment to related and unrelated parties), pre-tax income, tax payments and expenses, capital, earnings, non-cash tangible assets, and the number of employees at the country-level. Moreover, the MNE shall disclose a list of constituent entities in Part II of Schedule A. Form 8975 must be filed for fiscal years that begin on or after June 30, 2016. The CbC information obtained by the IRS through the form is subject to an

[2] We provide the templates of Form 8975 and Schedule A to Form 8975 in their current revision (December 2020) as issued on the IRS website in Appendix A.

automated exchange of information with foreign tax authorities if (1) the respective authority's jurisdiction is named in the filed report and (2) the foreign jurisdiction and the U.S. have bilaterally agreed on a Competent Authority Arrangement to share such information.

Related Literature

Most prior research examining the tax consequences of CbC disclosure focuses on the realization of BEPS Action 13 by the EU through Council Directive 2016/881/EU. Joshi (2020) shows that European companies required to disclose a CbC report to the local authorities have, on average, higher ETRs and engage less in tax-motivated income shifting. Examining firms' real investment decisions, De Simone & Olbert (2021) find that firms substantiate their capital and labor investments in European countries with preferential tax regimes following the adoption of CbC disclosure mandates, consistent with MNEs making efforts to appear less tax-aggressive. These studies suggest strong company reactions to the introduction of CbCR in Europe concerning both taxation and investment behavior.[3] Using IRS data obtained from Form 8975, Nessa et al. (2021) explore company responses to U.S. CbCR requirements. While they do not find a significant change in income shifting or economic activities by firms that are subject to the CbCR mandate, they observe that CbC reports contain incremental information about international operations to the Internal Revenue Service (IRS), thereby highlighting the relevance of CbC information for tax authorities. Adams et al. (2022) use a multinational sample of firms voluntarily disclosing tax information (either in general or in the form of CbCR) within their sustainability reporting. They find that the propensity of a firm to voluntarily disclose any tax information increases in its ETR, but not in the use of cross-border income shifting activities, whereas the extent of CbC disclosure[4] is larger if firms conduct

[3] In a recent article, Fuest, Hugger, and Neumeier (2022) use data from individual CbC reports provided to them by the German tax authorities. They examine the extent of income shifting into tax havens by German companies and use the CbC data to evaluate the completeness of other databases relative to CbC reports. This is consistent with CbC disclosure yielding informative data beyond other (public) disclosures of global operations.

[4] The authors develop a scale for the extent of CbC disclosure based on the OECD's CbCR recommendations.

income shifting activities, but is not significantly affected by its ETR, consistent with different types of tax avoidance strategies promoting different types of voluntary tax disclosures.

Beyond the focus of CbCR as a central measure of curbing aggressive tax behavior, additional research exists on the broader implications of mandatory public and non-public tax transparency. Examining the Australian Tax Office's decision to publicly disclose tax return data from large Australian corporations, Hoopes, Robinson, and Slemrod (2018) do not find significant evidence of increased tax payments after the public disclosure of tax return data. However, Kays (2022) reports that firms affected by this public disclosure strategically increase their voluntary disclosures to explain their tax behavior if they expect high reputational costs from the mandatory disclosure. Bilicka et al. (2022) explore the consequences of the United Kingdom's tax strategy disclosure rule, finding that although firms voluntarily increase qualitative tax strategy information in the annual report, they do not change their overall tax behavior. Finally, several studies document that the introduction of CbCR mandates is associated with investors responding to the prospect of additional tax-related disclosures to the authorities (Dutt, Ludwig, Nicolay, Vay, and Voget, 2019) and the general public (Müller, Spengel, and Weck, 2022) as well as with analysts perceiving less misalignment when forecasting a firm's taxes and economic activities (Huang et al., 2021).

Overall, these studies report substantial company reactions to CbCR and similar tax transparency requirements since affected firms change their tax avoidance and income shifting behavior, alter their real investment activities and increase voluntary disclosures to appear less tax aggressive towards external parties. However, the implications of mandatory tax transparency disclosure for firms' underlying internal procedures remain unclear. Prior literature shows that the presence of material weaknesses in (tax-related or general) internal controls is strongly related to tax payments (Gallemore & Labro, 2015; Bauer, 2016; Laplante, Lynch, and Vernon, 2021) and income shifting (McGuire, Rane, and Weaver, 2018), thus

establishing a link between internal control quality and the effectiveness of corporate tax avoidance. Therefore, firms may also respond to additional tax disclosure requirements by investing in their internal controls to maintain the effectiveness of their tax function.

Further research shows both company- and auditor-based determinants of the quality of firms' internal controls. For example, several studies confirm that firms are less likely to report weak internal controls when they are older and/or exhibit higher market capitalization (Ashbaugh-Skaife et al., 2007; Doyle et al., 2007; Ogneva et al., 2007; De Simone et al., 2015). At the same time, firms are more likely to have weak internal controls when they are financially distressed, have more complex operations, and/or grow more rapidly. Turning to audit firm-related determinants of internal control quality, Ashbaugh-Skaife et al. (2007) establish that larger auditors are more likely to discover deficiencies in their clients' internal control systems. De Simone et al. (2015) directly investigate the effect of audit firm involvement in corporate tax matters, i.e., the usage of auditor-provided tax services, on the quality of internal controls. They find that increases in auditor-provided tax services are associated with a substantially lower propensity of reporting a material weakness in internal controls, incremental to the (propensity-increasing) effects of audit fees, the economic relevance of the client to the audit firm, and the degree of other auditor-provided services. The authors interpret this finding as evidence for more involved auditors having better and earlier available information about company operations and both tax and non-tax matters, thus being able to prevent or early detect reportable material weaknesses.

Collectively, these studies provide evidence consistent with internal control quality being determined not only by the shape of firms' operations but also by the availability of tax and non-tax information and the ability to evaluate such information jointly. Therefore, when firms are mandated to collect additional tax-related information (esp. when this information is

relevant to tax authorities as a basis for risk-oriented tax audits), it is unlikely that firms leave their internal controls unmodified.

Hypothesis Development

We expect that the requirement to issue a CbC report has substantial implications for firms' tax-related internal control quality. As Hanlon (2018) points out, the CbC data required under BEPS Action 13 is not necessarily readily available in MNEs. Therefore, collecting, processing, and aggregating such information may have substantial implications for firms' internal information flows as it requires them to adjust their information processing structures. These adjustments may include procedures relevant to the internal control environment of the firm, thereby inducing a change in the internal control system itself. At the same time, the presence of additional regulations requires firms to incorporate these into their risk assessment, possibly altering managers' and auditors' conclusions about the effectiveness of current internal controls. Thus, CbCR likely triggers (1) actions by managers that improve existing internal controls but also (2) an increased propensity of exposure to existing but not yet discovered material weaknesses. Consequently, it is ambiguous whether CbCR is beneficial or detrimental to internal control quality.

On the one hand, tax transparency may direct firms to improve the quality of their internal controls to maintain authority over their tax strategy. First, given that CbC data is intended to be used by the authorities as a basis for better-informed risk assessments and tax audits, this information provision will leave firms with the urge to safeguard their tax (avoidance) strategy from objections. To stay able to actively shape their tax outcomes without increased risk during the (tax) audit, firms might deem it necessary to build up their internal control systems by, for instance, updating documentation policies, decision-making and signatory authorities, and the design of compliance-oriented workflows.

Second, since the additional legal requirements stemming from CbCR are non-trivial to tax practitioners (Hanlon, 2018), it may be necessary for companies to invest in additional qualified personnel (through training or hiring), improved information technology systems, and/or further external expertise in the form of tax advisory services. Such investments may also translate into higher-quality internal control systems (Masli, Peters, Richardson, and Sanchez, 2010; De Simone et al., 2015; Guo, Huang, Zhang, and Zhou, 2016; Chen, Cheng, Chow, Liu, 2021; Renschler, Hoitash, and Hoitash, 2021). Even when they have no needs to defend any "aggressive" tax strategies, the CbCR mandate may have stimulated firms to improve their tax, accounting, and other departments, indirectly resulting in higher-quality internal control systems.

Third, to collect and consolidate the required country-level tax information, tax and accounting professionals within the firm may have to intensify their communication across departments. Given that information sharing between professionals from different areas is superior to an isolated view of transactions and operations from a tax or non-tax perspective (De Simone et al., 2015; Robinson, Sikes, and Weaver, 2010), being required to intensify cooperation across departments for CbCR purposes may have a positive effect on internal control quality.

On the other hand, the new CbCR mandate may also have adverse consequences. First, as stated above, the collection of CbC data requires active coordination between the domestic parent entity and international subsidiaries. If such intra-group information flows exhibit existing deficiencies before CbCR, these deficiencies are exacerbated by the disclosure mandate. Therefore, structural weaknesses that previously were less matter of concern will be evaluated differently by managers and auditors when the company first becomes subject to CbCR. Second, managers of some firms may assess the CbC disclosure mandate as not a relevant issue for their internal information procedures. Considering that the U.S. CbCR

mandate did not receive a strong company response in the form of reduced tax avoidance (Nessa et al., 2021), managers may perceive CbC disclosure rules as not important enough to require changes in internal information collection and processing structures. To the extent that this perception is incorrect (which would uncover during the preparation or after submission of the CbC report), the management would have (or be prompted by the auditor) to reconsider the effectiveness of those internal controls that failed to highlight the need for a timely internal response. Third, after firms file their CbC reports, the tax authorities can use the provided information as input for transfer pricing risk assessments, thereby improving the preparation and conduct of the risk-oriented tax audit (OECD, 2015). In this context, the CbCR mandate constitutes an external shock to the corporate environment within which managers assess whether their internal controls remain effective (COSO, 2013).

Collectively, it is unclear whether the CbC disclosure mandate will positively or negatively affect the quality of internal controls. However, we expect that any effect of CbCR on internal control quality will be the most pronounced in *tax-related* controls. Given that CbCR is primarily aimed at increasing the transparency of transfer prices to the tax authorities (OECD, 2015), the measure is often perceived as primarily an anti-tax avoidance instrument (Joshi, 2020). As the CbCR mandate mainly triggers firms to collect, evaluate, and consolidate country-level information relevant for tax purposes and considering that these compliance requirements are costly, we expect that firms concentrate any adjustments to internal controls on critical aspects of the disclosure, namely, tax issues. Thus, we conjecture that the new disclosure requirement will materialize the strongest in the quality of tax-related internal controls. We, therefore, state the following undirected hypothesis:

> ***H1a:*** *The probability of internal controls having a tax-related material weakness changes when the firm is subject to the CbCR mandate.*

Despite CbCR being mainly related to tax disclosures, non-tax departments may still benefit from tax professionals' increased demand for information-sharing. As argued before, different departments within an organization are likely to benefit from better communication. Therefore, it is well-possible that CbCR also improves the information environment in other departments and, thus, the quality of non-tax internal controls. Conversely, the same interdependencies may also extend to adverse reactions of CbCR to non-tax-related internal controls. We hypothesize the effect of CbC disclosure on non-tax internal controls undirectedly and as follows:

> ***H1b:*** *The probability of internal controls having a non-tax-related material weakness changes when the firm is subject to the CbCR mandate but less strongly than for tax-related material weaknesses.*

III. DATA AND METHODOLOGY

Sample Selection

To examine the effect of the U.S. CbCR mandate on internal control quality, we obtain data from Audit Analytics for the sample period 2014 through 2019. The database provides information on internal control weaknesses under SOX Section 404, fees paid to the auditor, and auditor changes for all SEC registrants required to report such information. We match this dataset with consolidated financial data from Compustat North America and Segments to obtain the data necessary to create all relevant control variables. Following other empirical studies in the field (e.g., Dyreng & Lindsey, 2009; Gallemore & Labro, 2015), we set pre-tax restructuring costs (*rcp*), tax loss carryforwards (*tlcf*), and sales in foreign jurisdictions (based on *sales* in Segments) to zero if missing in Compustat (mnemonics in parentheses). For all other variables, we require observations to have sufficient non-missing data to construct the variables relevant for our empirical analysis. If a firm reports multiple internal control opinions

for the same fiscal year, we keep only the most recent version to minimize concerns about material weaknesses that were initially not reported biasing our results.

Since CbC disclosures are only required for U.S.-headquartered firms with operations in at least two countries, we drop all firms not located in the U.S. as well as domestic-only firm-years. We classify an observation as being "domestic-only" if its value of pre-tax foreign income or foreign tax expenses is missing or equal to zero (Lampenius, Shevlin, and Stenzel, 2021; Laplante et al., 2021).[5] We also require firms to have at least one observation in both the pre- and post-implementation period. Our sample consists of 7,477 observations from 1,338 distinct firms. However, we exclude firms in industries without any internal control weakness throughout our sample period to reduce the variance of conventional maximum likelihood estimates (Bauer, 2016). Therefore, for our tests of tax-related (non-tax-related) internal control quality, our final sample size reduces to 6,412 (6,954) observations from 1,147 (1,247) distinct firms.[6] All continuous variables are winsorized at the 1st and 99th percentile. We summarize the sample selection procedure in Table 1.

Research Design

For our primary empirical analysis, we rely on a difference-in-differences design that exploits the introduction of CbCR for firms exceeding the 850 M$ revenue threshold in fiscal years starting on or after June 30, 2016. In selecting this strategy, we also follow prior research on the implications of CbCR (Joshi, 2020; Joshi, Outslay, and Persson, 2020; De Simone &

[5] While we note that our financials-based definition of a "multinational" firm may misspecify some firms as "domestic-only" despite them having cross-border subsidiaries, we believe that this approach ensures that our sample consists exclusively of multinational entities. However, to the extent that multinational companies do not report foreign pre-tax income or tax expenses strategically or as a result of weak financial reporting quality, we acknowledge that our findings may be biased downwardly, resulting in a more conservative estimate of the overall effect of CbCR requirements on internal control strength.

[6] Our results remain unaltered when re-estimating Equation (1) with a sub-sample of firm-years included in both samples. The same applies to re-assigning firm-years from industries without any tax- (non-tax-) related internal control weakness to their nearest neighboring two-digit SIC industry. Our results are also robust to excluding industry-fixed effects, which allows us to retain the full sample because the regression is not affected by quasi-separation.

Olbert, 2021; Huang et al., 2021; Overesch & Wolff, 2021).[7] We estimate the following logistic regression:

$$\text{Prob}(ICW = 1) = \text{F}\left(\beta_0 + \beta_1 CBCR_i + \beta_2 CBCR_i \cdot POST_t + \sum_{\text{k=3}}^{\text{K}} \beta_k X_{it}^k\right) \quad (1)$$

All variable definitions are presented in further detail in Appendix B. *ICW* represents either of the indicator variables TAX_ICW_{it} or $NON\_TAX\_ICW_{it}$. TAX_ICW_{it} is an indicator variable equal to one if firm *i*'s auditor reports a material weakness in tax-related internal controls, and zero otherwise. Equivalently, $NON\_TAX\_ICW_{it}$ is an indicator variable equal to one if firm *i*'s auditor reports a material weakness in internal controls unrelated to taxes and no tax-related internal control weakness, and zero otherwise. $CBCR_i$ is an indicator variable equal to one if the firm reported consolidated prior-year revenues of at least 850 M$ in the fiscal years following the introduction of CbCR, and zero otherwise. $POST_t$ is an indicator variable equal to one for fiscal years starting on or after June 30, 2016. The stand-alone variable $POST_t$ remains omitted as it is a linear combination of the vector of time-fixed effects. The primary coefficient of interest is β_2, which reports the difference in the likelihood of reporting a material weakness in the (tax or non-tax) internal controls of treatment firms after being treated through the CbCR mandate.

X_{it} is a vector of control variables that accounts for various factors prior research has identified to affect the strength of internal controls. First, we control for the relationship between the firm and its auditor to capture the influence of auditor expertise on the probability of weak internal controls (De Simone et al., 2015). Therefore, we include three variables, *LN_AUDITFEE*, *LN_TAXFEE*, and *LN_OTHERFEE*, defined as the natural logarithms of audit-related, tax-related, and other fees, respectively, paid to the auditor for a specific fiscal

[7] Unlike other studies examining the effects of CbCR also using regression discontinuity designs (Joshi, 2020; De Simone & Olbert, 2021; Huang et al., 2021), we cannot test our hypotheses accordingly because material weaknesses in internal controls are relatively infrequent across our sample. Therefore, empirical tests requiring a narrow range around the treatment threshold would lack the statistical power to produce meaningful results.

year. We also control for the relative economic importance of a client for the audit firm (*INFLUENCE*) to capture the possibility of impaired auditor independence or more conservative auditing of larger clients (Reynolds & Francis, 2001). Moreover, we include an indicator variable for the presence of a Big Four auditor (*BIG4*) and capture dissents between audit firms and their clients that lead to the auditor resigning from their mandate (*RESIGNATION*).

Furthermore, X_{it} comprises firm characteristics identified by Ashbaugh-Skaife et al. (2007), Doyle et al. (2007), and Ogneva et al. (2007) as relevant determinants of internal control quality. First, we control for a firm's size by including the natural logarithm of market capitalization at the fiscal year-end (*LN_MARKETCAP*) and the natural logarithm of total assets (*LN_ASSETS*). Considering that treatment is determined through a size-based measure (prior-year consolidated revenues), we include these variables to facilitate our interpretation that internal control quality is determined by the treatment itself instead of merely reflecting correlation with firm size.

Second, we control for financial distress by including an indicator variable equal to one if the sum of a firm's extraordinary items in years *t* and *t-1* is negative, and zero otherwise (*AGGLOSS*). We also include the reverse decile ranking of Altman's (1968) Z-score (*ALTMAN_DECILE*).

Third, to account for operational and geographic complexity, we control for the relative frequency of foreign sales to total sales (*REL_FORSALES*) and include the natural logarithm of the number of operating and geographic segments in which the firm operates (*LN_SEGCOUNT*). We also include an indicator variable capturing gains or losses through foreign currency translations (*FORTRANS*) to capture financial reporting complexity beyond the aforementioned variables. To control for changes in firms' organizational structure, we

include an indicator variable capturing acquisition or merger expenses in the current and/or preceding fiscal year (*MERGER*).

Finally, we control for restructuring expenses in the current and/or preceding fiscal year (*RESTRUCTURE*) and capture firms with extreme sales growth relative to year-industry-peers (*EXTREMESG*). We use industry-fixed effects to capture industry-specific differences in the propensity of having low-quality internal controls.[8] Finally, we include year-fixed effects to capture time-varying market trends. We cluster standard errors at the firm level.

Descriptive Statistics

We present descriptive statistics for the entire sample and separately for treatment and control firms in Table 2. In Panel A, we present the sample distribution per year. Observations in our sample appear to be relatively evenly distributed over time. The treatment group comprises about two-thirds of all firms in our sample, which is not surprising given that our sample consists of (at least relatively) large publicly listed U.S.-located MNEs, and that treatment is determined through the size-based threshold of prior-year revenues.

We provide summary statistics for all relevant variables, both for the entire sample (Panel B) and separated by CbCR treatment (Panel C). We note that treatment and control firms are not balanced on most control variables. This observation comes as no surprise given that treatment is determined by a firm's consolidated revenues, which is likely associated with other firm characteristics such as size and profitability (Joshi, 2020) and auditor-related characteristics such as audit firm type (Lawrence, Minutti-Meza, and Zhang, 2011) and fees paid (Whisenant, Sankaraguruswamy, and Raghunandan, 2003). To alleviate concerns about these variables confounding the effect observed in our primary analysis, we report all regression estimates with and without control variables.

[8] We do not include firm-fixed effects because of the relatively rare occurrence of material weaknesses in internal controls (consistent with He, Kothari, Xiao, and Zuo, 2021). Firms are assigned to industries using two-digit SIC codes.

Moreover, we observe that the average firm-year in the treatment group exhibits an unconditional probability of 1.1% (3.0%) to report a tax-related (non-tax) material weakness in their internal controls. For control firms, the a priori likelihood of a tax-related (non-tax) internal control weakness is 1.8% (6.6%). This difference is significant at the 5% (1%) level. We demonstrate the validity of the parallel trend assumption for our dependent variables visually in Figures 1 and 2, where we plot the mean values of *TAX_ICW* and *NON_TAX_ICW*, respectively, over time. We present averages per half-year in which a fiscal year begins to align the presentation format with the mid-year introduction date of CbCR regulation. While we continue to see a relatively larger overall proportion of firms in the control group reporting material weaknesses, the respective time trends of both tax-related and non-tax-related internal controls are similar before the introduction of CbCR, consistent with the parallel trend assumption. For fiscal years starting after June 30, 2016, we see that tax-related internal control weaknesses are steadily lower than in the control group, providing initial evidence supporting hypothesis H1a. Non-tax-related material weaknesses in internal controls show a less clear reaction to the policy treatment. However, the treatment group appears to maintain a stable likelihood of having internal control weaknesses relative to firms in the control group. We regard this finding neither as evidence for nor against hypothesis H1b. We note that these conclusions alone should be treated with caution given the relatively few internal control weaknesses throughout our sample and the potential influence of covariates.

IV. RESULTS

Hypotheses Tests

We test hypothesis H1a using Equation (1) with *TAX_ICW* as the dependent variable and present the results in Table 3. In column (1), we estimate Equation (1) without any control variables and find a coefficient estimate of -0.979 for the interaction between *CBCR* and *POST*. The estimate is statistically significant at the 5% level. However, this finding may be driven by

differences between treatment and control firms other than the obligation to file a CbC report. Therefore, we include a large set of control variables that capture both the relationship between sample firms and their auditor and relevant firm characteristics. In column (2), we present the re-estimation of Equation (1), including control variables that capture auditor-client relationships. We continue to find a negative coefficient β_2 that is slightly lower than the estimate in the base model. Our results also hold when including the full set of audit-related and firm-related control variables, as shown in column (3). In both settings, the results remain significant at the 10% level.

In order to assess the economic significance of our findings, we calculate the average marginal effect of the difference between pre- and post-treatment periods for the treatment group relative to the control group. We hold all continuous control variables at their means and all categorical control variables at their medians, and average over industry- and year-fixed effects. Our results translate into an average partial treatment effect of -0.603 percentage points (p-value: 0.080). Given that the unconditional probability of having a tax-related material weakness in internal controls is 1.8% for observations in the control group, we interpret the magnitude as substantial. To facilitate the interpretability of the treatment effect, we present the average marginal effect visually in Figure 3, which shows how the likelihood of having a tax-related internal control weakness remains relatively stable for the control group but decreases substantially for treated firms.

In Table 4, we test hypothesis H1b equivalently using *NON_TAX_ICW* as the dependent variable in Equation (1). In neither model specification (i.e., with and without control variables) do the reported coefficient estimates for *CBCR*POST* differ significantly from zero. The same applies to calculations of the average marginal effect of CbCR on non-tax internal control quality. For comparison with the average marginal effect calculated for the *TAX_ICW* model, we present the corresponding margins plot in Figure 3. We see that, unlike for treated firms'

reaction through *TAX_ICW*, the non-tax internal control response to the CbC disclosure rules is similar for treatment and control firms. We conclude that the introduction of CbCR did not affect the likelihood of reporting non-tax material weaknesses in internal controls. Instead, CbCR appears to affect only the quality of internal controls related to the firm's tax function. This does not contradict our predictions since we argue that CbCR is considered a measure primarily aimed at increasing the justness of cross-border tax practices, consequently mostly triggering firms' needs to improve internal control quality in areas that relate to taxation. Thus, we interpret this finding as reinforcing our previous finding of an adverse effect of CbCR treatment on *TAX_ICW*.

We report significant coefficients for several control variables in both settings (tax- and non-tax internal control quality). Central variables affecting the likelihood of tax- and/or non-tax internal control weaknesses include audit fees, the presence of a Big Four auditor, audit firm resignation, firm size, and rapid sales growth. The direction of most of these coefficients is generally as expected and consistent with prior literature (Ashbaugh-Skaife et al., 2007; De Simone et al., 2015). However, unlike De Simone et al. (2015), we fail to find a significant effect of auditor-provided tax services on internal control quality.[9] Moreover, we find a statistically significant negative effect of Big Four auditor presence on the likelihood of internal controls, which contradicts Ashbaugh-Skaife et al.'s (2007) finding. Both differences may be due to the additional restrictions exposed on our sample compared to theirs (i.e., focus on U.S.-located MNEs). We are careful not to generalize this finding for the post-BEPS project period at large.

[9] This also condenses in the calculation of average marginal effects (which we conduct as described for the effect of CbCR but keeping only non-fee control variables constant). We find, for the treatment and the control group and separately considering pre- and post-CbCR periods, a statistically significant effect (at the 5% level or better) for audit fees, but no significant effects for tax fees or other fees.

Falsification Tests

We conduct a battery of falsification tests to address the possibility of our inferences being driven by confounding policy changes. While the BEPS project has led to a series of policy changes worldwide, some of which may trigger firms to re-assess their internal control systems, we are not aware of any relevant legislative changes affecting our sample that use the same revenue threshold or become valid at the same time. Thus, inferences about the effect of CbCR based on the estimate of β_2 should be unbiased. Nevertheless, we conduct falsification tests that exploit the revenue threshold of CbCR treatment and/or the enter-into-force date of the regulation to reinforce the presumption that our results do reflect the effect of CbCR.

We define three placebo treatment dummies for counterfactual prior-year revenue thresholds. Moreover, we test the sensitivity of our findings to fictitious treatment periods, starting one year before and one year after the actual CbCR enter-into-force date, respectively. We present the results from these five falsification tests in Table 5. Panels A, B, and C show the estimated coefficients for the placebo thresholds at M$ 500, M$ 1,200, and M$ 2,500, respectively. In Panels D and E, we retain the actual treatment threshold but consider placebo enter-into-force dates on June 30, 2015, for Panel D and on June 30, 2017, for Panel E.[10] We estimate all models using the identical control variable specifications as for our primary analysis. As expected, none of these placebo treatments yields significant results, suggesting that our primary inferences are not spurious but do reflect the effect of the CbCR mandate on tax-related internal control quality.

Multivariate Balancing

As shown Table 2 Panel C, firms in the treatment group differ from control firms on many covariates. Although we control for these factors through a vector of control variables

[10] We also estimate models with both placebo threshold and enter-into-force dates. No combination except for the 'actual' CbCR treatment parameters yields significant estimates for the coefficient of interest.

included in our logit model, this procedure alone possibly does not sufficiently address potential bias stemming from non-random assignment to CbCR treatment (Joshi, 2020; Joshi et al., 2020; Overesch & Wolff, 2021). We address this issue by employing entropy balancing and inverse-probability weighting to improve covariate balance between the treatment and control group (Joshi, 2020; Joshi et al., 2020).[11]

First, we use entropy balancing, a re-weighting scheme that assigns an individual weight to each observation in the sample to achieve covariate balance between treatment and control firms for multiple moments (i.e., mean, variance, and skewness) while remaining as close as possible to uniform weights (Hainmueller, 2012). Second, we weight observations with the inverse probability of treatment using the procedure suggested by Stuart, Huskamp, Duckworth, Simmons, Song, Chernew, and Barry (2014). The idea behind the latter strategy is first to predict the propensity of each observation to be in the treatment vs. control group and the pre- vs. post-CbCR implementation period (i.e., in one of four groups) using multinomial logistic regression and then re-weight each observation such that the groups are similar on the selected covariates. For both techniques, we follow Gallemore et al. (2019) and calculate weights based on all out-of-balance control variables considered in the main tests.[12]

In Table 6, we present the results from re-estimating Equation (1) using entropy balancing (Panel A) and inverse probability of treatment weighting (Panel B), respectively. We conduct the difference-in-differences estimation both with and without control variables. In columns (1) to (3), we present the tests that use *TAX_ICW* as the dependent variable, whereas in columns (4) to (6), we use *NON_TAX_ICW*. Across all re-weighting schemes, the coefficient

[11] In untabulated tests, we also consider propensity score matching (Rosenbaum & Rubin, 1983) and coarsened exact matching (Iacus, King, and Porro, 2012) as performed, for instance, by Gallemore, Gipper, and Maydew (2019). However, both techniques lead to a substantial decline in sample size, hence exposing any inferences based on them to sample attrition bias.

[12] When using entropy balancing, we exclude *LN_ASSETS*, *LN_MARKETCAP*, and *RESTRUCTURE* because it is impossible to achieve convergence when including these variables. The reason is that since CbCR treatment is defined based on prior-year revenues, i.e., a size-based variable, other variables displaying size are inevitably larger in the treatment group, making it (near to) impossible to achieve convergence when attempting to balance on these variables.

on *CBCRxPOST* continues to be significant and negative. In fact, several specifications show strengthened statistical significance and economic magnitude, which is consistent with the treatment effect not being a spurious result of sample imbalance.

Non-Linear Estimation of Coefficients for Rare Events

One possible limitation of our logistic estimation procedures stems from the rarity of material weaknesses throughout our sample. We observe that 1.4% of observations in our sample report a tax-related material weakness in internal controls. For non-tax internal control weaknesses, the unconditional probability of a material weakness is 4.3%. As King & Zeng (2001) point out, such 'rare events' in the dependent variable can create substantial bias in the coefficient estimates of non-linear binary outcome models when using conventional maximum likelihood estimation.

To overcome this issue, Firth (1993) suggests a penalized maximum likelihood estimation method, the idea behind which is to penalize the score function through a modification that is sensitive to sample size and the number of events.[13] This procedure allows for an efficient logit estimation even when conventional maximum likelihood estimation would be prone to biased estimates. Another advantage of Firth's Logit is that it overcomes separation and quasi-separation in logistic regressions (Heinze & Schemper, 2002), allowing us to include the entire sample for the analysis.[14]

Therefore, we use Firth's Logit to confirm the robustness of our main findings to the possibility of rare-event-biased coefficient estimates. We present the estimated regression models in Table 7. In columns (1) to (3), we use *TAX_ICW* as the dependent variable, while

[13] While King & Zeng (2001) introduce bias correction using optional weights, Leitgöb (2013) shows that penalized maximum likelihood estimation, which not only *corrects* but *prevents* bias, provides superior (i.e., unbiased) coefficient estimates even for small samples with few events.

[14] In the setting at hand, quasi-separation arises from industry-fixed effects that would perfectly predict the outcome variable for some observations (because no observation in these industries exhibits a material internal control weakness). We therefore exclude these observations from our main samples. However, our results remain unaltered when using the same subsamples of firm-years as in the key tests.

we use *NON_TAX_ICW* in columns (4) to (6). We report the results with and without (the entire set of) control variables. The results remain significant at the 10% level or better for *TAX_ICW* and insignificant for *NON_TAX_ICW*. Comparing the estimates from Firth's Logit with the conventional logit results from Tables 3 and 4, we see that our primary analysis is largely unbiased as the estimated coefficient β_2 remains similar in magnitude and significance. The same applies to the set of control variables, none of which substantially declines in significance or changes in magnitude.

V. ADDITIONAL FINDINGS

Recent Upgrades to the Internal Control System

While our primary analysis shows that firms affected by the newly introduced CbCR mandate are less likely than other firms to report a tax-related material weakness in internal controls, this relationship may be caused by other sources of need for improvement than the new tax transparency rules. For instance, existing material weaknesses directly incentivize a firm to improve its internal controls. Firms that succeed in remediating internal control weaknesses show higher accrual quality, consistent with these firms improving their financial reporting quality (Ashbaugh-Skaife, Collins, and Kinney, 2008). Thus, firms that remediate existing material weaknesses in their internal controls recently before the CbCR adoption may be better equipped for the introduction of CbCR than others without having to conduct further adjustments to their internal control systems. Consequently, these firms would be less prone to experience tax-related internal control weaknesses in the post-treatment period, not because they had to adjust internal control features to meet CbCR requirements but because they had other (and, in the case of internal control weakness remediations, possibly fundamental) incentives to upgrade their internal control systems.[15]

[15] We acknowledge that firms may also have upgraded their internal control systems in fiscal years that are not preceded by a period with reportable internal control weaknesses. However, our strategy identifies a group of firms that is most likely to have recently gone through (required) improvements in internal controls. Therefore,

To empirically assess whether this mechanism drives our results, we first identify firms that have recently upgraded their internal control systems before the CbCR mandate if they have resolved all material internal control weaknesses in the two fiscal years before the enter-into-force date of CbCR.[16] We visualize the relative frequencies of firm-years with tax-related (non-tax related) internal control weaknesses in the groups of firms with versus without recently upgraded internal control systems in Figure 4. First, within the subgroup of firms that did not recently upgrade their internal control systems to resolve corresponding material weaknesses, we observe that the unconditional likelihood of having a tax-related material weakness in the pre-treatment period is similar across groups. While treated and control firms differ in the probability of having non-tax internal control weaknesses before treatment, this difference remains relatively constant in the treatment period. However, for tax-related material weaknesses, the likelihood of a treated firm reporting such is substantially lower, relative to the control group, after the CbCR mandate enters into force. This observation is related to and consistent with our previous findings regarding hypotheses H1a and H1b.

When focusing on the subsample of firms that have recently resolved an internal control weakness, we again observe similar pre-treatment trends for both types of weaknesses. We note that the magnitude of the unconditional probabilities in the pre-treatment periods is inevitably larger for this subsample than for the subsample of non-upgrading firms because of the mechanical relationship between our definition of a "recent upgrade" as a pre-treatment remediation and having an internal control weakness before the introduction of CbCR. Notable,

we note any differences between both groups (i.e., 'recently upgraded' versus 'not recently upgraded') are conservative estimates of the underlying mechanism.

[16] Our definition of an internal control remediation is consistent with Ashbaugh-Skaife et al. (2008). We define a remediation to have occurred *recently* before the CbCR mandate in terms of a two-year period before the first year of treatment. We do not focus on any specific type of internal control weakness (e.g., tax-related weaknesses) but require full remediation from any material weakness for two reasons: First, firms that were able to improve their internal controls in *all* faulty areas are more likely to have conducted relevant upgrades and not only minor improvements. Second, linking our definition of remediation to individual groups of material weaknesses would create stronger mechanical effects between remediation and future weaknesses because it would not allow the remediation to have occurred for other problems than in future periods.

however, is that while post-treatment observations continue to behave similarly as before the treatment (and as non-upgrading firms) with respect to non-tax internal control weaknesses, this does not extend to the likelihood of tax-related weaknesses. In fact, our sample does not contain any firm-year with tax-related internal control weaknesses in the treatment period when the controls have been upgraded recently. This observation is not limited to firms obliged to file a CbC report but applies to control firms as well. It is also consistent with firms having effectively improved their internal control systems to avoid (tax-related) material weaknesses, resulting in them being prepared to not only avoid 'common' future impairments of the internal control system (as indicated by the control group) but also to comply with the additional disclosure requirements stemming from the CbCR mandate (as indicated by the treatment group).

Since our descriptive findings indicate that firms with recent internal control improvements are better prepared for the additional tax transparency requirements, these recent upgrades may drive our main results. We address this concern by including an indicator variable that equals one if a firm has upgraded its internal control system within two fiscal years prior to the CbCR mandate, and zero otherwise, into Equation (1) with *TAX_ICW* as the dependent variable. We report the logit regression estimates from this expanded model in column (1) of Table 8.[17] We find that the CbCR requirement continues to significantly reduce the log-odds of reported tax-related material weaknesses in internal controls after controlling for recent internal control system remediations. Our results remain unaltered when allowing different coefficient estimates for treatment and control firms, as presented in column (2).[18] We also show that this finding is not limited to our definition of a *recent* weakness-induced

[17] We do not report on the results from re-estimating our test of hypothesis H1b because they do not add informative value to the main findings.

[18] We do not include a triple-difference (i.e., an interaction term of *CBCR*, *POST*, and the 'upgrade indicator variable') because upgraded firms in the post-period have no tax-related internal control weaknesses. Thus, adding an interaction term comprising both variables would add no explanatory power to our model.

upgrade before the CbCR mandate because a reduction to one year (columns (3) and (4)) or an expansion to three years (columns (5) and (6)) does not affect the significance, direction, or magnitude of the estimate.[19] Our inferences also remain significant when estimating Equation (1) for a subsample of firms that do not contain pre-treatment remediations (untabulated). We conclude that the effect of CbCR on the quality of tax-related internal controls is incremental to recent upgrades of the internal control system caused by prior internal control weaknesses.

Sensitivity to Corporate Tax Behavior

To better understand the characteristics of firms that respond to mandated tax transparency disclosures by improving their internal control quality, we examine potential heterogeneity in firm reactions to the policy change. Given that the effect of CbCR on internal control quality is limited to tax-related material weaknesses, which is consistent with managers perceiving the disclosure as mainly an issue for the tax function, we predict that firms' response to CbCR depends heavily on firms' broader tax behavior.

Specifically, we expect firms with lower-quality financial reporting environments and more aggressive tax positions to react stronger to the CbCR mandate. Gleason, Pincus, and Rego (2017) show that firms with weak tax-related internal controls reduce their ETRs from the third to the fourth quarter more strongly than firms with other material weaknesses to meet or beat earnings forecasts, suggesting that firms behave tax-aggressively in the presence of tax-related internal control weaknesses in the short term. However, Bauer (2016) finds that firms with tax-related internal control weaknesses are less able to successfully sustain broader long-run tax avoidance strategies, consistent with sustainably (but not necessarily aggressively) tax-avoiding firms already having implemented more effective internal controls. Relatedly, Chen, Yang, Zhang, Zhou (2020) find that internal control quality is associated with more tax

[19] The visual evidence provided in Figure 4, too, is not driven by our definition of a recent remediation. For brevity, we report only the visualization for upgrades within two years before the CbCR mandate.

avoidance for less tax-aggressive firms but with less tax avoidance for more tax-aggressive firms. This finding is consistent with Choudhary et al. (2016), who introduce a tax-specific measure of accrual quality and establish that tax accrual quality explains tax-related internal control weaknesses. They also find a negative relationship between unrecognized tax benefits – an established proxy for tax aggressiveness (Lisowsky, Robinson, and Schmidt, 2013) – and tax accrual quality. Finally, Balakrishnan et al. (2019) find that reporting relatively low ETRs (measured through comparison with industry-size peers) is associated with lower corporate transparency. Collectively, these studies suggest that firms with less aggressive tax positions and firms with higher-quality tax accruals should be better-equipped than others to handle additional challenges for tax-related internal controls, such as additional tax disclosure requirements.

We examine whether the observed effect of CbCR on tax-related internal control quality is concentrated in tax-aggressive firms and firms with lower tax accrual quality. For this, we split our sample based on their pre-treatment levels of tax aggressiveness and tax accrual quality, respectively. For each firm, we use the level of tax aggressiveness and tax accrual quality observed in the period immediately preceding the first year of the treatment period to mitigate endogeneity concerns (Iliev, 2010).[20] We measure tax aggressiveness (*TAX_AGGRESSIVE*) based on the difference between an observation's three-year average cash ETR and the average of its industry-year-size peers' (Balakrishnan et al., 2019). Tax accrual quality (*TAX_ACCR_QUAL*) is determined using the measure developed by Choudhary et al. (2016).[21] We split our sample depending on their status as being "tax-aggressive" and having "high tax accrual quality," respectively. We define firms as tax-aggressive if they have positive values of *TAX_AGGRESSIVE*, and as non-tax-aggressive otherwise. Firms with high-

[20] Splitting the sample based on firm-specific averages over all pre-treatment observations yields similar results.
[21] In untabulated tests, we perform sample splits based on firms' pre-treatment levels of unrecognized tax benefits and find qualitatively similar results to using the peer-based measure of tax aggressiveness at hand.

quality tax accruals are separated from low-quality firms based on the sample median. Table 9 reports the results from re-estimating Equation (1) using the respective subsamples with *TAX_ICW* as the dependent variable. Consistent with our expectations, the estimated coefficient for the interaction term is statistically significant for tax-aggressive firms. When splitting the sample based on tax accrual quality, the estimated coefficient of the interaction term is clearly insignificant for high-quality firms. Moreover, it falls slightly out of significance for low-quality firms using two-tailed tests. However, the differences between the interaction term coefficients in the split samples are statistically significant for both *TAX_AGGRESSIVE* and *TAX_ACCR_QUAL*, consistent with less aggressive firms and firms with higher-quality tax accruals requiring less adjustment to the new regulatory challenges.

VI. CONCLUSION

We examine whether the U.S. introduction of non-public CbCR rules for large MNEs affects the quality of firms' internal control systems. Aimed at increasing the tax transparency of globally operating companies, adopting CbCR regulations in line with the OECD's recommendations under BEPS action point 13 has evidently triggered firms to alter their tax strategies and investment activities. Studies focusing on tax transparency regimes in other countries find consistent evidence and further report changes in firms' voluntary reporting practices. However, the within-firm consequences of such disclosure rules have been understudied. We fill this void by providing evidence that the introduction of CbCR in the U.S. is associated with a significantly and economically substantially lower likelihood of reporting a material weakness in internal controls. This finding is relevant because CbC disclosures were not always readily available before the new disclosure requirement, inducing firms to modify their information processing structures to remain compliant. Our results appear to be limited to *tax-related* material internal control weaknesses, consistent with CbCR being perceived by managers as mainly an issue for their firm's tax function. Our findings are robust to a battery

of placebo tests, covariate balancing techniques, and alternative empirical strategies that address the low frequency of positive outcomes in the binary dependent variable.

Moreover, we examine the sensitivity of our findings to relevant firm characteristics. First, we show that although remediations of internal control weaknesses affect these firms' ability to maintain internal controls subsequently, our findings are not driven by firms experiencing such remediations recently before the adoption of CbCR. Next, we evaluate whether firms with different levels of pre-treatment tax aggressiveness and tax accrual quality respond differently to the tax transparency regulation. Our findings indicate that less tax-aggressive firms and firms with higher-quality tax accruals have less need to adapt to the new regulatory requirements arising from the introduction of CbCR.

We conclude that CbCR has important implications for firms' internal structures, processes, and workflows. Specifically, the changes carried out to meet the requirements positively affect the quality of internal controls. At the same time, our findings are consistent with affected firms incurring costs to remain compliant with CbCR requirements. These findings are relevant for (1) managers and tax and accounting professionals concerned with firms' intra-group information flows; (2) legislators and policymakers who assess the direct and indirect consequences of mandatory tax transparency regimes; and (3) the ongoing debates regarding the appropriateness of public and non-public CbCR and the currently set revenue thresholds exempting smaller firms from disclosure.

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APPENDIX

Appendix A: Form 8975

Form 8975

Form **8975**
(Rev. December 2020)
Department of the Treasury
Internal Revenue Service

Country-by-Country Report

For reporting period beginning _____ , 20 _____ , **and ending** _____ , 20 _____

▶ **Go to *www.irs.gov/Form8975* for instructions and the latest information.**

OMB No. 1545-2272

If this is an amended report, check here ☐

Enter the number of Schedules A (Form 8975) attached to this Form 8975 ▶

Part I Identification of Filer

1a Name of the reporting entity	**1b** Reporting role code	**1c** EIN
2 Number, street, and room or suite no. (if P.O. box, see instructions)		
3a City or town	**3b** State or province	**3c** Country, and ZIP or foreign postal code
4 Name of the U.S. Multinational Enterprise (MNE) group (if different from reporting entity)		

Part II Additional Information

Enter any additional information related to the U.S. MNE group

For Paperwork Reduction Act Notice, see separate instructions. Cat. No. 37798N Form **8975** (Rev. 12-2020)

Schedule A to Form 8975

SCHEDULE A (Form 8975)
(Rev. December 2020)
Department of the Treasury
Internal Revenue Service

Tax Jurisdiction and Constituent Entity Information

For reporting period beginning _____ , 20 ____ , **and ending** _____ , 20 ____

▶ **A separate Schedule A (Form 8975) is to be completed for each tax jurisdiction of the multinational enterprise group.**
▶ **Go to *www.irs.gov/Form8975* for instructions and the latest information.**

OMB No. 1545-2272

Name of the reporting entity | EIN

Part I Tax Jurisdiction Information. All financial amounts must be stated in U.S. dollars. See instructions.

Tax jurisdiction

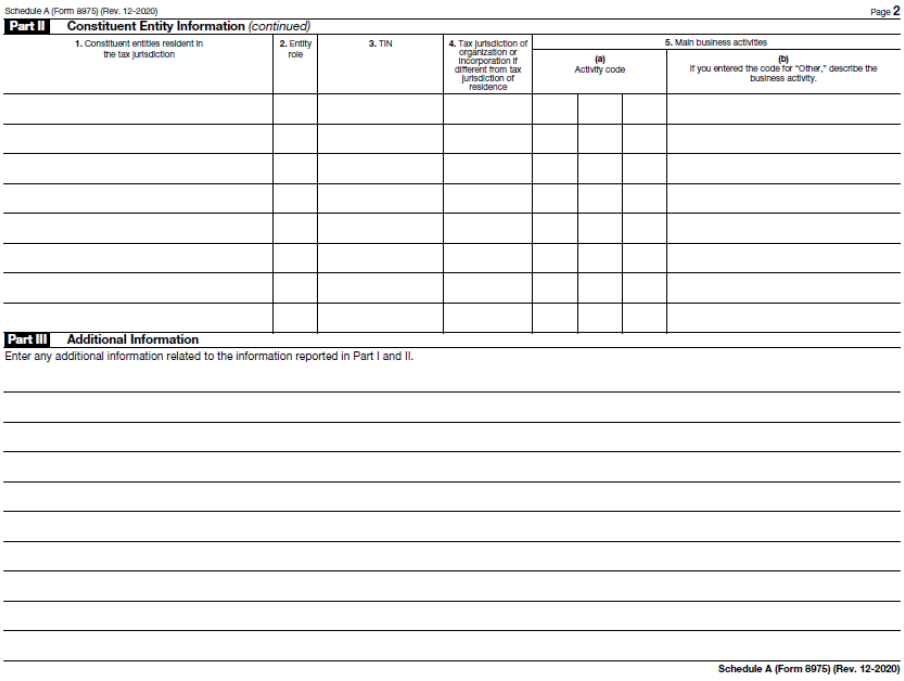
1. Revenues			2. Profit (loss) before income tax	3. Income tax paid (on cash basis)	4. Income tax accrued—current year	5. Stated capital	6. Accumulated earnings	7. Number of employees	8. Tangible assets other than cash and cash equivalents
(a) Unrelated party	(b) Related party	(c) Total							

Part II Constituent Entity Information

1. Constituent entities resident in the tax jurisdiction	2. Entity role	3. TIN	4. Tax jurisdiction of organization or incorporation if different from tax jurisdiction of residence	5. Main business activities	
				(a) Activity code	(b) If you entered the code for "Other," describe the business activity.

For Paperwork Reduction Act Notice, see separate instructions. Cat. No. 69310N **Schedule A (Form 8975) (Rev. 12-2020)**

Schedule A to Form 8975 (continued)



Schedule A (Form 8975) (Rev. 12-2020) Page 2

Part II **Constituent Entity Information** *(continued)*

1. Constituent entities resident in the tax jurisdiction	2. Entity role	3. TIN	4. Tax jurisdiction of organization or incorporation if different from tax jurisdiction of residence	5. Main business activities			
				(a) Activity code			(b) If you entered the code for "Other," describe the business activity.

Part III **Additional Information**

Enter any additional information related to the information reported in Part I and II.

Schedule A (Form 8975) (Rev. 12-2020)

Appendix B: Variable Definitions

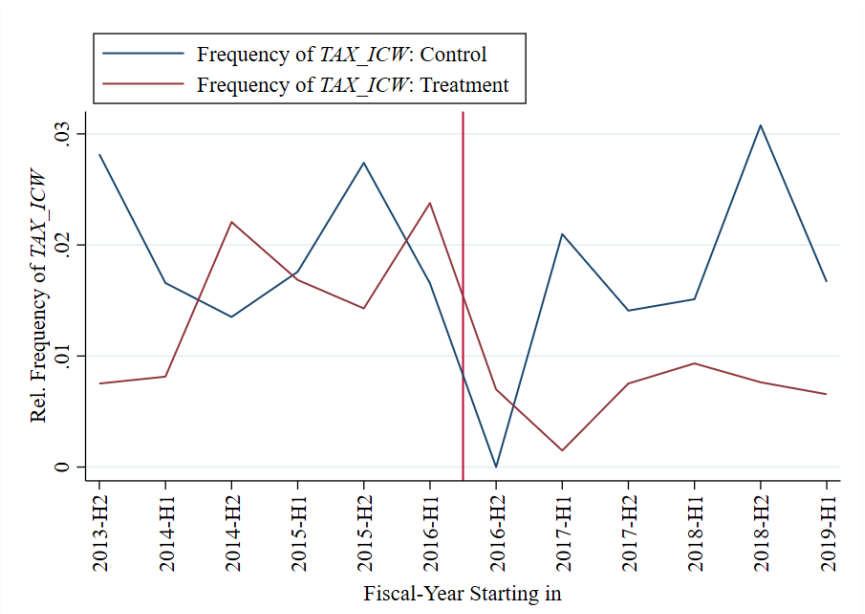
Variable Name	Variable Definition
Independent Variables	
TAX_ICW	Indicator variable equal to one if firm *i*'s audit firm reported a tax-related internal control weakness in year *t*, and zero otherwise.
NON_TAX_ICW	Indicator variable equal to one if firm *i*'s audit firm reported a non-tax internal control weakness in year *t* and no tax-related internal control weakness, and zero otherwise.
Dependent Variables in Primary Model	
CBCR	Indicator variable equal to one if firm *i* is subject to CbCR in fiscal years starting after June 30, 2016, and zero otherwise.
POST	Indicator variable equal to one for fiscal years starting after June 30, 2016, and zero otherwise.
LN_AUDITFEE	Natural logarithm of the sum of audit and audit-related fees paid by firm *i* to the auditor in year *t*.
LN_TAXFEE	Natural logarithm of the total tax-related fees paid by firm *i* to the auditor in year *t*.

LN_OTHERFEE	Natural logarithm of the total non-audit- and non-tax-related fees paid by firm *i* to the auditor in year *t*.
INFLUENCE	Share of total fees paid to the auditor by firm *i* in year *t* relative to the sum of all fees the audit firm has received from all audits conducted by the same practice office for the same calendar year. The audit firm and its practice office are identified from the audit opinion as reported in Audit Analytics.
BIG4	Indicator variable equal to one if firm *i* is audited by Deloitte, Ernst & Young, KPMG, or PricewaterhouseCoopers in year *t*, and zero otherwise.
RESIGNATION	Indicator variable equal to one if firm *i*'s auditor resigned in year *t*, and zero otherwise.
LN_MARKETCAP	Natural logarithm of firm *i*'s market capitalization at the end of year *t*.
LN_ASSETS	Natural logarithm of firm *i*'s total assets in year *t*.
AGGLOSS	Indicator variable equal to one if the sum of earnings before extraordinary items of firm *i* in years *t* and *t-1* is less than zero, and zero otherwise.
ALTMAN_DECILE	Reversed decile ranking of Altman's (1968) Z-score. Z-scores are calculated as follows: 1.2 · (Working Capital/Total Assets) + 1.4 · (Retained Earnings/Total Assets) + 3.3 · (EBIT/Total Assets) + .6 · (Market Capitalization/Total Liabilities) + .999 · (Sales/Total Assets).
REL_FORSALES	Fraction of firm *i*'s sales attributable to foreign segments in year *t*.
LN_SEGCOUNT	Natural logarithm of the sum of geographic and operating segments in which firm *i* operates in year *t*.
FORTRANS	Indicator variable equal to one if firm *i* reports non-missing and non-zero foreign exchange gains or losses in year *t*, and zero otherwise.
MERGER	Indicator variable equal to one if firm *i* reports non-missing and non-zero special items before taxes that correspond to merger or acquisition in years *t* or *t-1*, and zero otherwise.
RESTRUCTURE	Sum of firm *i*'s special items before taxes that correspond to restructuring expenses in years *t* and *t-1*, relative to *LN_MARKETCAP* and multiplied by -1.
EXTREMESG	Indicator variable equal to one if firm *i* experiences sales growth in year *t* in the top quintile of firms in the same industry and year, and zero otherwise.

Other Variables	
REVENUE	Consolidated prior-year revenues of firm *i* in year *t*; reported in M$.
UPGR_ONE	Indicator variable equal to one if firm *i* remediates all its material weaknesses in internal controls within one year before the first fiscal year starting on or after June 30, 2016, and zero otherwise.
UPGR_TWO	Indicator variable equal to one if firm *i* remediates all its material weaknesses in internal controls within two years before the first fiscal year starting on or after June 30, 2016, and zero otherwise.
UPGR_THREE	Indicator variable equal to one if firm *i* remediates all its material weaknesses in internal controls within three years before the first fiscal year starting on or after June 30, 2016, and zero otherwise.
TAX_AGGRESSIVE	Difference between firm *i*'s three-year cash ETR and mean three-year cash ETR of all firms in the same year, industry, and size quintile, where three-year cash ETRs are calculated as the sum of cash taxes paid over years *t-2* through *t* divided by the sum of pre-tax income over years *t-2* through *t*; divided by minus one.
TAX_ACCR_QUAL	Standard deviation of the residuals from firm-specific estimates of total tax accruals (i.e., total tax expense less cash taxes paid, scaled by total assets) on the tax accrual quality model described by Choudhary et al. (2016) using eight-year rolling averages.

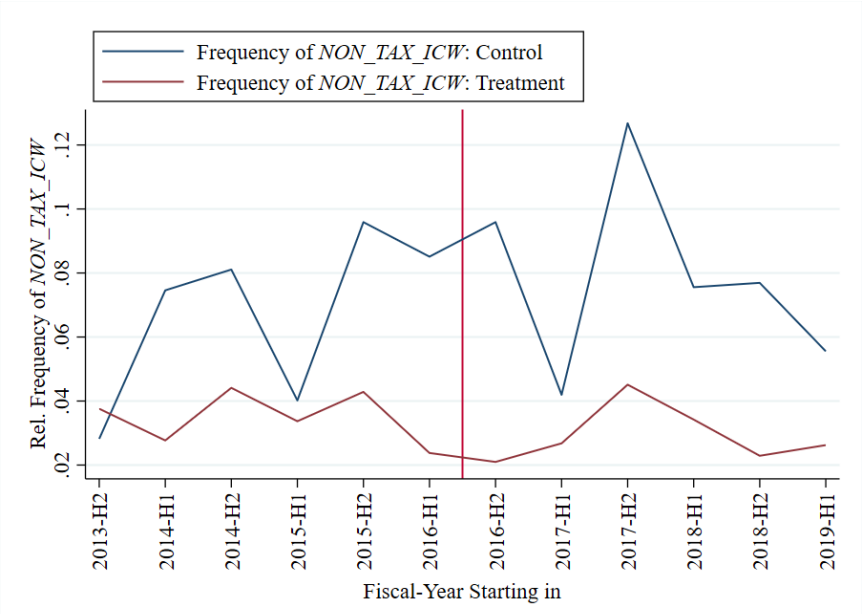
FIGURES AND TABLES

Figure 1: Time Trends in *TAX_ICW* by Treatment



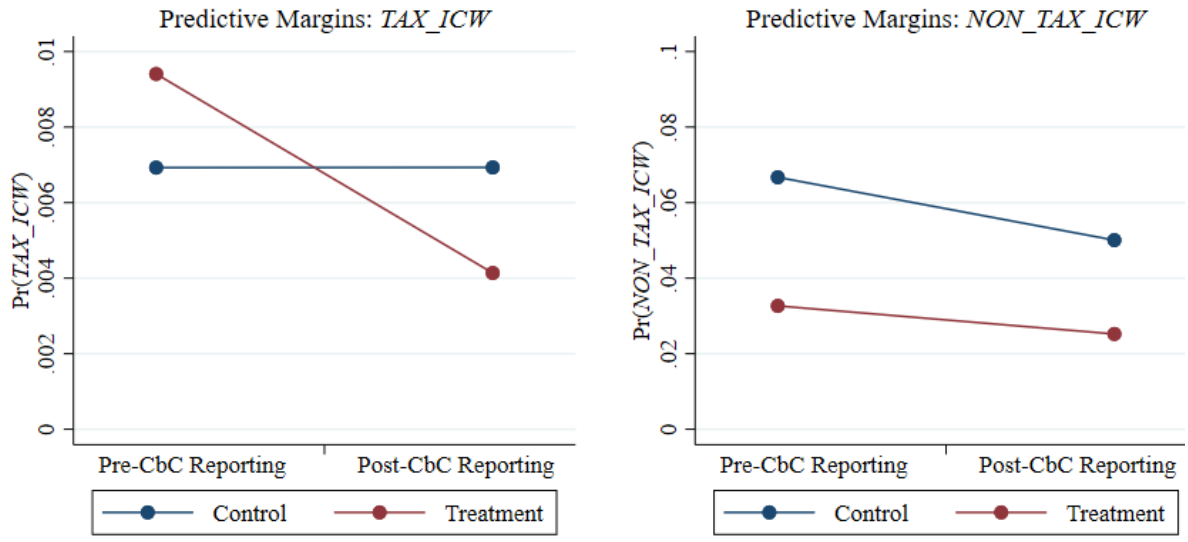
Notes: This figure visualizes the average *TAX_ICW* over time for the treatment group (red line) and the control group (blue line). Means are calculated and plotted by half-year in which the firm-year's fiscal-year starts in order to facilitate the interpretation with respect to the enter-into-force date of U.S. CbCR on June 30, 2016 (denoted by the vertical red line).

Figure 2: Time Trends in *NON_TAX_ICW* by Treatment



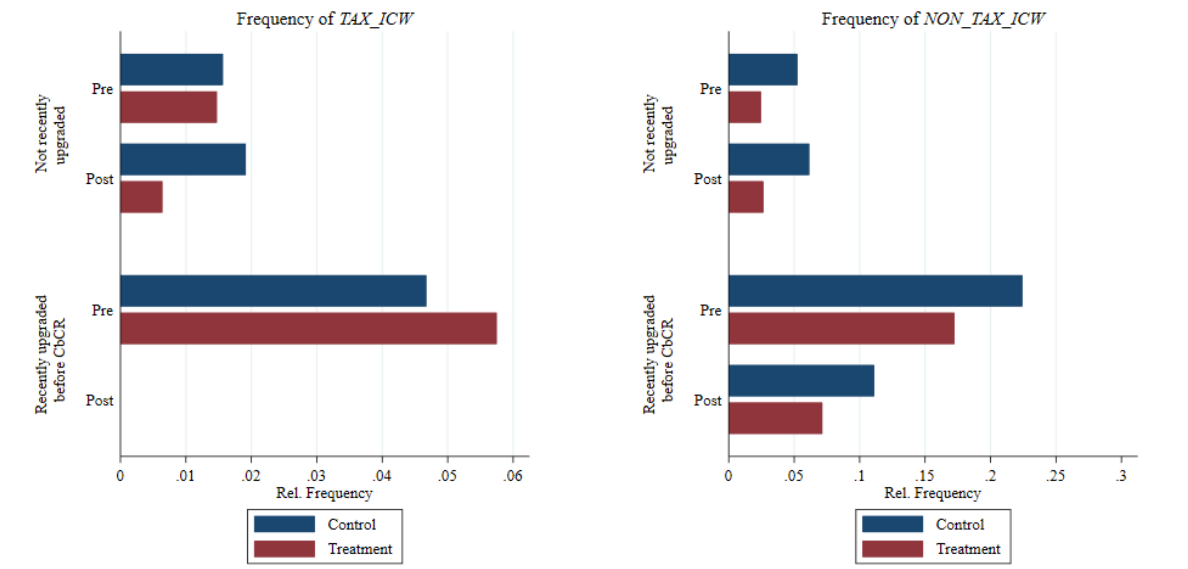
Notes: This figure visualizes the average *NON_TAX_ICW* over time for the treatment group (red line) and the control group (blue line). Means are calculated and plotted by half-year in which the firm-year's fiscal-year starts in order to facilitate the interpretation with respect to the enter-into-force date of U.S. CbCR on June 30, 2016 (denoted by the vertical red line).

Figure 3: Estimated Average Marginal Effects from Hypotheses Tests



Notes: This figure visualizes the marginal effects of CbCR pre- vs. post-treatment estimated in Tables (3) (for *TAX_ICW*) and (4) (for *NON_TAX_ICW*) using the full model specification described in Equation (1). The red (blue) dots represent the estimated likelihood of a material weakness before and after the introduction of CbCR for the treatment (control) group. Predictive margins are calculated by holding continuous control variables at their mean and categorical control variables at their median, averaging over industries and fiscal years.

Figure 4: Frequency of Internal Control Weaknesses Depending on Recent Upgrades to Internal Control Systems Before CbCR Mandate



Notes: This figure visualizes the relative frequencies of tax-related and non-tax-related material weaknesses in internal controls, separately for the treatment (red bars) and the control group (blue bars) before and after CbCR. Firms are subdivided into two groups, depending on their status as having remediated any existing internal control weakness within two years before the enter-into-force date of CbCR (consistent with the definition of *UPGR_TWO*).

Table 1: Sample Selection

Step	Distinct Firms	Observations
Intersection of firm-years available in Compustat and Audit Analytics from 2014 to 2019	4,673	20,728
Less firm-years without sufficient data to create relevant covariates	(-1,197) 3,476	(-5,609) 15,119
Less firm-years not identified as U.S.-located multinational entity	(-1,500) 1,976	(-6,464) 8,655
Less firms without sufficient data points before and after CbCR mandate	(-638) 1,338	(-1,178) **7,477**
Sample for main tests of H1a	(-191) 1,147	(-1,065) **6,412**
Sample for main tests of H1b	(-91) 1,247	(-523) **6,954**

Notes: This table summarizes the sample selection process. Financial firm data is obtained from Compustat North America and Segments; data on auditor-client relationships stems from Audit Analytics.

Table 2: Descriptive Statistics

Panel A: Sample Composition Over Time

		2014	2015	2016	2017	2018	2019	Total
Control Group		433	472	496	502	468	425	2,796
Treatment Group		747	789	813	815	776	741	4,681
N		1,180	1,261	1,309	1,317	1,244	1,166	7,477

Notes: This table reports the sample composition per year for both the treatment and the control group for the entire sample of firms. The sample composition remains similar when industries without sufficient variation in the dependent variable (i.e., *TAX_ICW* or *NON_TAX_ICW*) are excluded.

Panel B: Summary Statistics for Full Sample

	N	Mean	SD	Q1	Median	Q3
TAX_ICW	7,477	0.014	0.115	0	0	0
NON_TAX_ICW	7,477	0.043	0.203	0	0	0
REVENUE (M$)	7,477	7,221.611	26,516.61	455.41	1,532.186	4,405.6
LN_AUDITFEE	7,477	14.793	0.983	14.142	14.737	15.435
LN_TAXFEE	7,477	9.722	5.272	9.259	11.857	13.209
LN_OTHERFEE	7,477	4.249	4.926	0	0	8.517
INFLUENCE	7,477	0.156	0.226	0.024	0.066	0.177
BIG4	7,477	0.858	0.349	1	1	1
RESIGNATION	7,477	0.004	0.060	0	0	0
LN_MARKETCAP	7,477	7.827	1.783	6.606	7.753	9.006
LN_ASSETS	7,477	7.637	1.688	6.488	7.580	8.725
AGGLOSS	7,477	0.231	0.421	0	0	0
ALTMAN_DECILE	7,477	5.326	2.628	3	5	7
REL_FORSALES	7,477	0.328	0.265	0.100	0.294	0.503
LN_SEGCOUNT	7,477	1.234	0.708	0.693	1.386	1.792
FORTRANS	7,477	0.520	0.500	0	1	1
MERGER	7,477	0.603	0.489	0	1	1
RESTRUCTURE	7,477	4.706	14.573	0.000	0.000	2.293
EXTREMESG	7,477	0.162	0.368	0	0	0

Notes: This table descriptive statistics for all dependent and independent variables included in Equation (1) for the full sample of observations. Continuous variables are winsorized at the 1st and 99th percentile. All variables are as defined in Appendix B. The summary statistics remains similar when industries without sufficient variation in the dependent variable (i.e., *TAX_ICW* or *NON_TAX_ICW*) are excluded.

Panel C: Summary Statistics by Group

	Control Group						Treatment Group						Diff. in Means
	N	Mean	SD	Q1	Q2	Q3	N	Mean	SD	Q1	Q2	Q3	*t*-value
TAX_ICW	2,766	0.018	0.132	0	0	0	4,711	0.011	0.104	0	0	0	2.275
NON_TAX_ICW	2,766	0.066	0.249	0	0	0	4,711	0.030	0.17	0	0	0	6.830
REVENUE (M$)	2,766	397.79	324.55	140.82	336.59	583.29	4,711	11,228	32,750	1,686.	3,142.	7,803.	-22.714
LN_AUDITFEE	2,766	13.995	0.662	13.565	14.059	14.441	4,711	15.262	0.826	14.697	15.193	15.784	-72.738
LN_TAXFEE	2,766	7.368	5.567	0	10.445	11.936	4,711	11.103	4.555	10.933	12.612	13.782	-29.895
LN_OTHERFEE	2,766	3.327	4.383	0	0	7.601	4,711	4.79	5.143	0	0	9.21	-13.056
INFLUENCE	2,766	0.158	0.253	0.016	0.047	0.166	4,711	0.155	0.208	0.031	0.076	0.183	0.555
BIG4	2,766	0.7	0.459	0	1	1	4,711	0.951	0.216	1	1	1	-27.152
RESIGNATION	2,766	0.007	0.083	0	0	0	4,711	0.002	0.041	0	0	0	3.075
LN_MARKETCAP	2,766	6.409	1.191	5.519	6.411	7.276	4,711	8.659	1.529	7.602	8.555	9.634	-70.833
LN_ASSETS	2,766	6.083	1.005	5.402	6.137	6.82	4,711	8.55	1.295	7.614	8.38	9.349	-91.837
AGGLOSS	2,766	0.415	0.493	0	0	1	4,711	0.122	0.328	0	0	0	27.872
ALTMAN_DECILE	2,766	5.277	3.057	3	5	8	4,711	5.354	2.339	4	5	7	-1.142
REL_FORSALES	2,766	0.312	0.28	0.053	0.25	0.493	4,711	0.338	0.255	0.118	0.314	0.508	-3.932
LN_SEGCOUNT	2,766	1.102	0.71	0.693	1.099	1.609	4,711	1.312	0.696	0.693	1.386	1.792	-12.464
FORTRANS	2,766	0.514	0.5	0	1	1	4,711	0.524	0.499	0	1	1	-0.865
MERGER	2,766	0.564	0.496	0	1	1	4,711	0.625	0.484	0	1	1	-5.210
RESTRUCTURE	2,766	0.594	2.154	0	0	0.318	4,711	7.121	17.849	0	0	5.408	-24.795
EXTREMESG	2,766	0.2	0.4	0	0	0	4,711	0.139	0.346	0	0	0	6.624

Notes: This table descriptive statistics for all dependent and independent variables included in Equation (1) for the entire sample of observations, but separately for the treatment and the control group. Continuous variables are winsorized at the 1st and 99th percentile. All variables are as defined in Appendix B. The final column reports the t-statistics for differences in means across each variable. The summary statistics remain similar when industries without sufficient variation in the dependent variable (i.e., *TAX_ICW* or *NON_TAX_ICW*) are excluded.

Table 3: Difference-in-Differences Estimates of the Effect of CbCR on *TAX_ICW*

Dependent variable	*TAX_ICW* (1)	*TAX_ICW* (2)	*TAX_ICW* (3)
CBCR	0.079	-0.441	0.534
	(0.316)	(0.507)	(0.465)
CBCR x POST	**-0.979****	**-0.872***	**-0.831***
	(0.468)	**(0.463)**	**(0.472)**
LN_AUDITFEE		0.728***	1.509***
		(0.218)	(0.287)
LN_TAXFEE		-0.016	-0.003
		(0.027)	(0.027)
LN_OTHERFEE		-0.005	0.006
		(0.024)	(0.024)
INFLUENCE		0.499	0.666
		(0.460)	(0.456)
BIG4		-1.342***	-1.236***
		(0.388)	(0.383)
RESIGNATION		1.780**	1.159
		(0.741)	(0.753)
LN_MARKETCAP			-0.486***
			(0.145)
LN_ASSETS			-0.440*
			(0.225)
AGGLOSS			-0.477
			(0.394)
ALTMAN_DECILE			0.093
			(0.067)
REL_FORSALES			0.056
			(0.575)
LN_SEGCOUNT			0.122
			(0.257)
FORTRANS			-0.160
			(0.268)
MERGER			0.216
			(0.270)
RESTRUCTURE			0.002
			(0.009)
EXTREMESG			0.781***
			(0.245)
Intercept	-3.228***	-12.568***	-19.246***
	(0.677)	(2.826)	(3.341)
Industry-FE	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes
Pseudo R^2	0.070	0.111	0.180
N	6,412	6,412	6,412

Notes: This table presents the results from estimating Equation (1) using *TAX_ICW* as the dependent variable in a logit model. Column (1) presents the results for the baseline regression of *TAX_ICW* on the difference-in-differences of exposure to CbCR. In column (2), we include control variables capturing characteristics of the auditor-client relationship. In column (3), we further include firm-specific characteristics identified as relevant determinants of internal control quality. All regressions include industry- and year-fixed effects. We present firm-level-clustered standard errors in parentheses. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent, respectively, based on two-tailed tests. All continuous variables are winsorized at the 1st and 99th percentile. All variables are as defined in Appendix B.

Table 4: Difference-in-Differences Estimates of the Effect of CbCR on *NON_TAX_ICW*

Dependent variable	*NON_TAX_ICW* (1)	*NON_TAX_ICW* (2)	*NON_TAX_ICW* (3)
CBCR	-0.882***	-1.258***	-0.237
	(0.195)	(0.256)	(0.274)
CBCR x POST	**-0.001**	**0.036**	**0.037**
	(0.251)	**(0.252)**	**(0.262)**
LN_AUDITFEE		0.404***	1.392***
		(0.117)	(0.181)
LN_TAXFEE		-0.013	-0.004
		(0.015)	(0.015)
LN_OTHERFEE		-0.001	0.009
		(0.014)	(0.015)
INFLUENCE		0.148	0.278
		(0.318)	(0.291)
BIG4		-0.541**	-0.417*
		(0.251)	(0.236)
RESIGNATION		0.888	0.513
		(0.626)	(0.783)
LN_MARKETCAP			-0.447***
			(0.091)
LN_ASSETS			-0.610***
			(0.159)
AGGLOSS			-0.298
			(0.189)
ALTMAN_DECILE			0.000
			(0.036)
REL_FORSALES			0.214
			(0.343)
LN_SEGCOUNT			-0.031
			(0.135)
FORTRANS			0.192
			(0.151)
MERGER			0.294*
			(0.151)
RESTRUCTURE			0.002
			(0.006)
EXTREMESG			0.239
			(0.170)
Intercept	-1.744	-7.065***	-15.301***
	(1.079)	(2.004)	(2.033)
Industry-FE	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes
Pseudo R^2	0.049	0.060	0.137
N	6,954	6,954	6,954

Notes: This table presents the results from estimating Equation (1) using *NON_TAX_ICW* as the dependent variable in a logit model. Column (1) presents the results for the baseline regression of *NON_TAX_ICW* on the difference-in-differences of exposure to CbCR. In column (2), we include control variables capturing characteristics of the auditor-client relationship. In column (3), we further include firm-specific characteristics identified as relevant determinants of internal control quality. All regressions include industry- and year-fixed effects. We present firm-level-clustered standard errors in parentheses. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent, respectively, based on two-tailed tests. All continuous variables are winsorized at the 1st and 99th percentile. All variables are as defined in Appendix B.

Table 5: Falsification Tests

	TAX_ICW			*NON_TAX_ICW*		
Panel A: M$ 500	(1)	(2)	(3)	(4)	(5)	(6)
CBCR	-0.211	-0.531	0.409	-0.743***	-0.868***	0.217
	(0.356)	(0.451)	(0.471)	(0.203)	(0.261)	(0.269)
CBCR x POST	**-0.444**	**-0.332**	**-0.328**	**0.023**	**0.046**	**-0.001**
	(0.497)	**(0.489)**	**(0.492)**	**(0.254)**	**(0.256)**	**(0.269)**
Audit-related controls	No	Yes	Yes	No	Yes	Yes
Firm-related controls	No	No	Yes	No	No	Yes
Industry-and Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.066	0.106	0.177	0.042	0.049	0.137
N	6,412	6,412	6,412	6,954	6,954	6,954
	TAX_ICW			*NON_TAX_ICW*		
Panel B: M$ 1,200	(1)	(2)	(3)	(4)	(5)	(6)
CBCR	-0.337	-1.336***	-0.420	-0.925***	-1.330***	-0.254
	(0.313)	(0.480)	(0.466)	(0.198)	(0.266)	(0.274)
CBCR x POST	**-0.670**	**-0.548**	**-0.541**	**0.046**	**0.082**	**0.092**
	(0.471)	**(0.465)**	**(0.480)**	**(0.261)**	**(0.260)**	**(0.270)**
Audit-related controls	No	Yes	Yes	No	Yes	Yes
Firm-related controls	No	No	Yes	No	No	Yes
Industry-and Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.072	0.124	0.180	0.049	0.060	0.137
N	6,412	6,412	6,412	6,954	6,954	6,954
	TAX_ICW			*NON_TAX_ICW*		
Panel C: M$ 2,500	(1)	(2)	(3)	(4)	(5)	(6)
CBCR	-0.377	-1.444***	-0.518	-1.268***	-1.775***	-0.697**
	(0.324)	(0.532)	(0.503)	(0.254)	(0.320)	(0.315)
CBCR x POST	**-0.490**	**-0.382**	**-0.394**	**0.217**	**0.253**	**0.244**
	(0.569)	**(0.560)**	**(0.572)**	**(0.321)**	**(0.322)**	**(0.328)**
Audit-related controls	No	Yes	Yes	No	Yes	Yes
Firm-related controls	No	No	Yes	No	No	Yes
Industry-and Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.069	0.123	0.180	0.054	0.068	0.139
N	6,412	6,412	6,412	6,954	6,954	6,954

(continued on the next page)

	TAX_ICW			*NON_TAX_ICW*		
Panel D: June 30, 2015	(1)	(2)	(3)	(4)	(5)	(6)
CBCR	-0.088	-0.559	0.406	-0.573**	-0.885***	0.154
	(0.396)	(0.545)	(0.514)	(0.232)	(0.287)	(0.303)
CBCR x POST	**-0.264**	**-0.175**	**-0.076**	**-0.379**	**-0.355**	**-0.389**
	(0.471)	**(0.467)**	**(0.467)**	**(0.271)**	**(0.271)**	**(0.283)**
Audit-related controls	No	Yes	Yes	No	Yes	Yes
Firm-related controls	No	No	Yes	No	No	Yes
Industry-and Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.064	0.106	0.177	0.047	0.057	0.137
N	6,412	6,412	6,412	6,954	6,954	6,954
	TAX_ICW			*NON_TAX_ICW*		
Panel E: June 30, 2017	(1)	(2)	(3)	(4)	(5)	(6)
CBCR	-0.314	-0.808*	0.082	-0.853***	-1.112***	-0.151
	(0.299)	(0.444)	(0.434)	(0.179)	(0.228)	(0.248)
CBCR x POST	**-0.477**	**-0.404**	**-0.322**	**-0.042**	**-0.042**	**-0.055**
	(0.497)	**(0.492)**	**(0.505)**	**(0.258)**	**(0.258)**	**(0.269)**
Audit-related controls	No	Yes	Yes	No	Yes	Yes
Firm-related controls	No	No	Yes	No	No	Yes
Industry-and Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.068	0.111	0.177	0.049	0.058	0.137
N	6,412	6,412	6,412	6,954	6,954	6,954

Notes: This table presents the results from several falsification tests based on Equation (1) with either *TAX_ICW* (columns (1) through (3) or *NON_TAX_ICW* (columns (4) through (6)) as the dependent variable in a logit model. In Panel A, we create a placebo treatment for firms reporting prior-year revenues of M$ 500. Panel B shows the results from placebo treatment at prior-year revenues of M$ 1,200. In Panel C, placebo treatment is assumed at prior-year revenues of M$ 2,500. In Panels D and E, we retain the actual treatment threshold but consider alternative treatment periods. In Panel C, treatment is assumed on June 30, 2015, and in Panel D, it is assumed on June 30, 2017. Columns (1) and (4) present the results for the baseline difference-in-differences regressions. Columns (2) and (5) include control variables capturing characteristics of the auditor-client relationship, and columns (3) and (6) further include firm-specific characteristics identified as relevant determinants of internal control quality. All regressions include industry- and year-fixed effects. We present firm-level-clustered standard errors in parentheses. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent, respectively, based on two-tailed tests. All continuous variables are winsorized at the 1st and 99th percentile. All variables (except for those altered as indicated above) are as defined in Appendix B.

Table 6: Multivariate Balancing

Panel A: Entropy Balancing

Dependent variable	*TAX_ICW*			*NON_TAX_ICW*		
	(1)	(2)	(3)	(4)	(5)	(6)
CBCR	-1.599***	-2.101***	1.513**	-2.409***	-3.285***	-1.191***
	(0.615)	(0.752)	(0.709)	(0.424)	(0.502)	(0.423)
CBCR x POST	**-1.743***	**-2.294****	**-2.623*****	**0.060**	**0.742**	**0.575**
	(0.978)	**(0.906)**	**(0.834)**	**(0.594)**	**(0.499)**	**(0.453)**
LN_AUDITFEE		1.594***	1.452***		1.363***	2.011***
		(0.439)	(0.409)		(0.249)	(0.317)
LN_TAXFEE		0.150	0.091		-0.092***	-0.060**
		(0.110)	(0.076)		(0.032)	(0.028)
LN_OTHERFEE		-0.006	-0.022		-0.031	0.017
		(0.040)	(0.037)		(0.033)	(0.028)
INFLUENCE		-1.143	-0.252		-0.152	0.262
		(1.126)	(1.082)		(0.669)	(0.534)
BIG4		-2.603***	-1.310		-0.455	-0.370
		(0.917)	(0.863)		(0.539)	(0.563)
RESIGNATION		4.892***	3.750**		1.307	1.047
		(1.259)	(1.859)		(1.149)	(1.282)
LN_MARKETCAP			0.075			-0.171
			(0.287)			(0.272)
LN_ASSETS			-1.372***			-1.093***
			(0.448)			(0.381)
AGGLOSS			0.475			-0.256
			(0.670)			(0.380)
ALTMAN_DECILE			0.493***			0.017
			(0.137)			(0.103)
REL_FORSALES			-2.071**			1.622**
			(0.996)			(0.712)
LN_SEGCOUNT			2.030***			-0.549**
			(0.422)			(0.238)
FORTRANS			-1.980***			0.630*
			(0.399)			(0.371)
MERGER			-0.107			0.619**
			(0.508)			(0.272)
RESTRUCTURE			-0.008			0.006
			(0.015)			(0.012)
EXTREMESG			0.347			-0.144
			(0.420)			(0.709)
Intercept	-5.061***	-29.132***	-23.686***	-1.485	-19.851***	-21.889***
	(0.815)	(6.849)	(6.001)	(1.222)	(3.620)	(3.842)
Industry-FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	6,412	6,412	6,412	6,954	6,954	6,954
N	0.516	0.580	0.691	0.332	0.446	0.516

Panel B: Inverse-Probability of Treatment Weighting

Dependent variable	*TAX_ICW*			*NON_TAX_ICW*		
	(1)	(2)	(3)	(4)	(5)	(6)
CBCR	0.268	-0.124	0.743	-0.843***	-0.960***	-0.394
	(0.380)	(0.536)	(0.492)	(0.233)	(0.292)	(0.296)
CBCR x POST	**-1.066***	**-1.291****	**-1.038***	**-0.367**	**-0.432**	**-0.155**
	(0.554)	**(0.560)**	**(0.573)**	**(0.347)**	**(0.340)**	**(0.342)**
LN_AUDITFEE		0.709***	1.513***		0.197	1.107***
		(0.245)	(0.381)		(0.141)	(0.283)
LN_TAXFEE		-0.035	-0.021		-0.009	0.002
		(0.037)	(0.040)		(0.021)	(0.020)
LN_OTHERFEE		-0.012	0.008		-0.004	0.005
		(0.029)	(0.028)		(0.018)	(0.018)
INFLUENCE		-0.418	-0.216		0.025	0.220
		(0.617)	(0.578)		(0.603)	(0.550)
BIG4		-1.695***	-1.532***		-0.716	-0.544
		(0.440)	(0.481)		(0.445)	(0.395)
RESIGNATION		2.824***	2.554***		0.028	-0.777
		(0.786)	(0.789)		(0.883)	(1.276)
LN_MARKETCAP			-0.286			-0.452***
			(0.194)			(0.115)
LN_ASSETS			-0.700**			-0.445*
			(0.299)			(0.238)
AGGLOSS			-0.294			-0.237
			(0.414)			(0.270)
ALTMAN_DECILE			0.315***			0.006
			(0.091)			(0.054)
REL_FORSALES			0.275			-0.209
			(0.797)			(0.453)
LN_SEGCOUNT			0.306			-0.057
			(0.355)			(0.159)
FORTRANS			-0.668**			0.262
			(0.313)			(0.212)
MERGER			0.208			0.426**
			(0.329)			(0.203)
RESTRUCTURE			0.005			0.004
			(0.008)			(0.008)
EXTREMESG			0.988***			0.398*
			(0.293)			(0.240)
Intercept	-6.048***	-14.912***	-22.341***	-3.086***	-5.424**	-12.142***
	(1.201)	(3.449)	(4.624)	(0.957)	(2.111)	(2.992)
Industry-FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	6,412	6,412	6,412	6,954	6,954	6,954
N	0.092	0.125	0.230	0.076	0.081	0.147

Notes: This table presents the results from estimating Equation (1) using a balanced sample with either *TAX_ICW* (columns (1) through (3) or *NON_TAX_ICW* (columns (4) through (6)) as the dependent variable in a logit model. In Panel A, we use entropy balancing on the third moment of all out-of-balance covariates (except for *LN_MARKETCAP*, *LN_ASSETS,* and *RESTRUCTURE*). In Panel B, we re-weight observations using the inverse probability of treatment following Stuart et al. (2014). Columns (1) and (4) present the results for the baseline difference-in-differences regressions. Columns (2) and (5) include control variables capturing characteristics of the auditor-client relationship, and columns (3) and (6) further include firm-specific characteristics identified as relevant determinants of internal control quality. All regressions include industry- and year-fixed effects. We present firm-level-clustered standard errors in parentheses. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent, respectively, based on two-tailed tests. All continuous variables are winsorized at the 1st and 99th percentile. All variables are as defined in Appendix B.

Table 7: Logistic Regressions Using Firth's (1993) Penalized Maximum Likelihood Estimation

Dependent variable	*TAX_ICW*			*NON_TAX_ICW*		
	(1)	(2)	(3)	(4)	(5)	(6)
CBCR	0.064	-0.442	0.478	-0.872***	-1.240***	-0.238
	(0.267)	(0.348)	(0.377)	(0.170)	(0.207)	(0.222)
CBCR x POST	**-0.928****	**-0.811***	**-0.750***	**-0.000**	**0.036**	**0.042**
	(0.420)	**(0.422)**	**(0.431)**	**(0.230)**	**(0.231)**	**(0.237)**
LN_AUDITFEE		0.705***	1.429***		0.398***	1.358***
		(0.153)	(0.223)		(0.090)	(0.142)
LN_TAXFEE		-0.016	-0.003		-0.014	-0.004
		(0.022)	(0.022)		(0.012)	(0.013)
LN_OTHERFEE		-0.004	0.006		-0.001	0.009
		(0.022)	(0.022)		(0.013)	(0.013)
INFLUENCE		0.506	0.654		0.159	0.286
		(0.400)	(0.404)		(0.258)	(0.262)
BIG4		-1.312***	-1.195***		-0.536***	-0.413**
		(0.341)	(0.346)		(0.190)	(0.193)
RESIGNATION		1.883***	1.261		0.994*	0.605
		(0.712)	(0.901)		(0.591)	(0.670)
LN_MARKETCAP			-0.465***			-0.437***
			(0.141)			(0.086)
LN_ASSETS			-0.410**			-0.595***
			(0.201)			(0.123)
AGGLOSS			-0.448			-0.288*
			(0.298)			(0.168)
ALTMAN_DECILE			0.089			0.000
			(0.060)			(0.032)
REL_FORSALES			0.045			0.208
			(0.511)			(0.286)
LN_SEGCOUNT			0.114			-0.029
			(0.187)			(0.111)
FORTRANS			-0.149			0.188
			(0.228)			(0.135)
MERGER			0.186			0.280**
			(0.233)			(0.133)
RESTRUCTURE			0.003			0.003
			(0.007)			(0.006)
EXTREMESG			0.741***			0.236
			(0.243)			(0.156)
Intercept	-3.236**	-12.232***	-17.513***	-2.633*	-7.548***	-15.008***
	(1.484)	(2.559)	(2.858)	(1.468)	(1.879)	(2.138)
Industry-FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Log-Likelihood	-489.809	-456.178	-400.595	-1215.625	-1186.681	-1061.994
N	7,477	7,477	7,477	7,477	7,477	7,477

Notes: This table presents the results from estimating Equation (1) with either *TAX_ICW* (columns (1) through (3) or *NON_TAX_ICW* (columns (4) through (6)) as the dependent variable. Instead of using conventional maximum likelihood estimation, we apply Firth's (1993) penalized maximum likelihood estimation to account for the low number of occurrences of the dependent variable. Columns (1) and (4) present the results for the baseline difference-in-differences regressions. Columns (2) and (5) include control variables capturing characteristics of the auditor-client relationship, and columns (3) and (6) further include firm-specific characteristics identified as relevant determinants of internal control quality. All regressions include industry- and year-fixed effects. We present firm-level-clustered standard errors in parentheses. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent, respectively, based on two-tailed tests. All continuous variables are winsorized at the 1st and 99th percentile. All variables are as defined in Appendix B.

Table 8: Recent Upgrades to Internal Control Systems Before Introduction of CbCR

Dependent variable	*TAX_ICW*					
UPGR =	*UPGR_TWO*		*UPGR_ONE*		*UPGR_THREE*	
	(1)	(2)	(3)	(4)	(5)	(6)
CBCR	0.552	0.561	0.595	0.539	0.556	0.565
	(0.467)	(0.476)	(0.478)	(0.479)	(0.463)	(0.474)
CBCR x POST	**-0.834***	**-0.835***	**-0.841***	**-0.838***	**-0.824***	**-0.825***
	(0.474)	**(0.472)**	**(0.476)**	**(0.476)**	**(0.472)**	**(0.469)**
UPGR	**0.436**	**0.477**	**1.180*****	**0.879**	**0.046**	**0.072**
	(0.392)	**(0.517)**	**(0.400)**	**(0.561)**	**(0.355)**	**(0.461)**
CBCR x UPGR		**-0.087**		**0.826**		**-0.064**
		(0.738)		**(0.833)**		**(0.693)**
LN_AUDITFEE	1.483***	1.483***	1.442***	1.454***	1.501***	1.501***
	(0.292)	(0.292)	(0.297)	(0.295)	(0.291)	(0.291)
LN_TAXFEE	-0.002	-0.003	-0.003	-0.002	-0.004	-0.004
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
LN_OTHERFEE	0.004	0.004	0.004	0.002	0.005	0.005
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
INFLUENCE	0.644	0.648	0.654	0.618	0.645	0.647
	(0.464)	(0.467)	(0.473)	(0.475)	(0.455)	(0.455)
BIG4	-1.236***	-1.236***	-1.201***	-1.207***	-1.219***	-1.217***
	(0.381)	(0.382)	(0.389)	(0.386)	(0.383)	(0.385)
RESIGNATION	1.172	1.171	1.179	1.165	1.154	1.153
	(0.736)	(0.737)	(0.726)	(0.715)	(0.743)	(0.742)
LN_MARKETCAP	-0.478***	-0.480***	-0.478***	-0.467***	-0.492***	-0.493***
	(0.148)	(0.149)	(0.145)	(0.147)	(0.147)	(0.148)
LN_ASSETS	-0.427*	-0.426*	-0.406*	-0.425*	-0.434*	-0.434*
	(0.225)	(0.226)	(0.230)	(0.232)	(0.223)	(0.224)
AGGLOSS	-0.481	-0.479	-0.470	-0.489	-0.480	-0.479
	(0.392)	(0.393)	(0.387)	(0.391)	(0.393)	(0.392)
ALTMAN_DECILE	0.098	0.098	0.104	0.102	0.094	0.094
	(0.067)	(0.067)	(0.066)	(0.066)	(0.067)	(0.067)
REL_FORSALES	-0.009	-0.006	-0.048	-0.045	0.041	0.043
	(0.582)	(0.582)	(0.587)	(0.588)	(0.578)	(0.577)
LN_SEGCOUNT	0.135	0.136	0.171	0.163	0.127	0.128
	(0.256)	(0.256)	(0.261)	(0.263)	(0.256)	(0.256)
FORTRANS	-0.175	-0.176	-0.188	-0.177	-0.164	-0.165
	(0.271)	(0.270)	(0.275)	(0.275)	(0.269)	(0.268)
MERGER	0.216	0.217	0.206	0.188	0.229	0.231
	(0.269)	(0.270)	(0.269)	(0.271)	(0.270)	(0.271)
RESTRUCTURE	0.001	0.001	0.002	0.002	0.002	0.002
	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)
EXTREMESG	0.770***	0.771***	0.777***	0.774***	0.765***	0.766***
	(0.244)	(0.243)	(0.243)	(0.242)	(0.244)	(0.243)
Intercept	-19.068***	-19.072***	-18.757***	-18.788***	-19.178***	-19.179***
	(3.384)	(3.387)	(3.419)	(3.397)	(3.375)	(3.376)
Industry-FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.181	0.181	0.186	0.187	0.180	0.180
N	6,284	6,284	6,284	6,284	6,284	6,284

Notes: This table presents the results from estimating the following logit model: Prob(*TAX_ICW* = 1) = $F(\beta_0 + \beta_1 CBCR_i + \beta_2 CBCR_i \cdot POST_t + \beta_3 UPGR_i + \beta_4 CBCR_i \cdot UPGR_t + \sum_{k=5}^{K} \beta_k X_{it}^k)$, where *UPGR* is *UPGR_TWO* (columns (1) and (2)), *UPGR_ONE* (columns (3) and (4)), or *UPGR_THREE* (columns (5) and (6)). In columns (1), (3), and (5), we do not include the *CBCR x UPGR* interaction. All regressions include the full set of control variables and industry- and year-fixed effects. We present firm-level-clustered standard errors in parentheses. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent, respectively, based on two-tailed tests. All continuous variables are winsorized at the 1st and 99th percentile. All variables are as defined in Appendix B.

Table 9: Sensitivity to Firms' Tax Behavior

Dependent variable	*TAX_ICW*			
Splitting variable	*TAX_AGGRESSIVE*		*TAX_ACCR_QUAL*	
	High	Low	High	Low
	(1)	(2)	(3)	(4)
CBCR	0.874	0.225	-0.796	1.991***
	(0.664)	(0.741)	(0.946)	(0.653)
CBCR x POST	**-1.881*** **	**0.288**	**0.621**	**-2.000**
	(0.637)	**(0.775)**	**(0.878)**	**(1.241)**
LN_AUDITFEE	1.281***	1.943***	1.573**	1.130**
	(0.408)	(0.568)	(0.692)	(0.484)
LN_TAXFEE	-0.007	0.010	0.019	0.002
	(0.039)	(0.046)	(0.059)	(0.041)
LN_OTHERFEE	-0.016	0.027	0.010	-0.021
	(0.031)	(0.037)	(0.047)	(0.060)
INFLUENCE	0.712	0.698	-0.407	0.436
	(0.618)	(0.801)	(0.904)	(1.017)
BIG4	-1.104**	-1.228*	-1.643*	-1.054
	(0.441)	(0.698)	(0.880)	(0.670)
RESIGNATION	0.083	1.413*		
	(1.035)	(0.805)		
LN_MARKETCAP	-0.493***	-0.422	-0.963***	-0.455
	(0.188)	(0.288)	(0.312)	(0.332)
LN_ASSETS	-0.138	-0.955**	0.243	-0.768*
	(0.289)	(0.434)	(0.521)	(0.419)
AGGLOSS	-0.319	-0.206	0.136	-0.623
	(0.542)	(0.613)	(0.724)	(0.558)
ALTMAN_DECILE	0.095	0.057	-0.111	0.221*
	(0.079)	(0.119)	(0.157)	(0.126)
REL_FORSALES	0.129	0.166	0.644	-0.476
	(0.914)	(0.923)	(1.067)	(1.121)
LN_SEGCOUNT	0.430	-0.088	0.715*	-0.723*
	(0.392)	(0.353)	(0.430)	(0.382)
FORTRANS	-0.341	0.115	-0.291	0.662
	(0.343)	(0.424)	(0.505)	(0.688)
MERGER	0.023	0.341	0.510	0.027
	(0.411)	(0.411)	(0.623)	(0.600)
RESTRUCTURE	-0.002	0.009	-0.002	0.022
	(0.011)	(0.013)	(0.012)	(0.016)
EXTREMESG	0.893***	0.906**	1.317***	0.877**
	(0.290)	(0.389)	(0.473)	(0.441)
Intercept	-18.931***	-22.014***	-20.878***	-9.617*
	(5.418)	(5.763)	(7.602)	(5.283)
Chi^2	4.68**		2.98*	
Industry-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Pseudo R^2	0.174	0.270	0.259	0.269
N	3,167	2,177	1,259	1,653

Notes: This table presents the results from estimating Equation (1) using *TAX_ICW* as the dependent variable in a logit model. Tests in column (1) [(2)] are based on subsamples of firms with positive [negative] levels of pre-treatment *TAX_AGGRESSIVE*, and in column (3) [(4)] on having above-median [below-median] levels of pre-treatment *TAX_ACCR_QUAL*. Coefficients of *RESIGNATION* are omitted in column (4) because of lacking variation due to the smaller sample size. All regressions include the full set of control variables and industry- and year-fixed effects. The denoted Chi^2 are for tests of differences between the coefficients of the interaction terms of the respective subgroups. We present firm-level-clustered standard errors in parentheses. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent, respectively, based on two-tailed tests. All continuous variables are winsorized at the 1st and 99th percentile. All variables are as defined in Appendix B.