

**Tax Compliance of
Multinationals
and Industry
Concentration in the
European Union**

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Abstract

We study whether stronger tax compliance among multinationals can reduce industry concentration. Exploiting the 2016 introduction of country-by-country reporting in the European Union as a natural experiment, we implement a difference-in-differences design comparing large multinational groups subject to the reform with unaffected firms. We find that increased tax compliance led to a significant decline in multinationals' consolidated global sales, with a one-percentage-point rise in effective tax rates associated with a 1.8% reduction in sales. Sales of the affected multinationals' subsidiaries also declined, and industry concentration fell in sectors where top firms were subject to the reform. The results suggest that curbing profit-shifting can reduce the competitive advantage of large multinationals and, consequently, industry concentration.

Keywords: tax compliance; tax avoidance; multinational; corporate tax; effective tax rate; industry concentration; European Union

JEL codes: F23, H26, L11

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1 Introduction

Industry concentration has risen sharply over recent decades, raising concerns about declining competition and productivity.¹ Explanations have focused on advances in information and communication technology (Bessen, 2020), the increasing importance of intangible assets (Crouzet and Eberly, 2023), weak antitrust enforcement (Gutiérrez and Philippon, 2018), a decline in technology diffusion (Akcigit and Ates, 2021) and low interest rates (Liu et al., 2022). Comparatively little attention has been paid to how international tax rules shape market structure, despite the high relevance of this link in the context of recent international efforts to increase tax compliance of multinationals.²

In this paper, we argue that profit shifting by multinationals can reinforce market concentration by lowering their effective costs relative to domestic firms. Large multinationals belong among the largest firms in many industries. At the same time, they often pay lower taxes than smaller competitors,³ and their tax avoidance is sizeable (Tørsløv et al., 2023) and has grown substantially over time (Wier and Zucman, 2022). If multinationals' sales have increased as a result of their tax-avoidance strategies, this may have also led to increased industry concentration. Using a simple model of monopolistic competition, we show that lower statutory tax rates alone do not affect firms' prices or sales. However, when multinationals can shift profits to low-tax jurisdictions, their ability to misprice intra-group transactions or to locate intangible assets abroad effectively reduces their production or investment costs. These mechanisms increase multinationals' sales relative to domestic firms and, since multinationals are typically among the largest firms, contribute to higher industry concentration.

To empirically test the relationship between tax avoidance by multinationals, multinationals' sales and industry concentration, we exploit the 2016 introduction of country-by-country reporting in a difference-in-differences approach to study the effect of taxes on sales of large multinationals and industry concentration in the European Union. The reform required multinationals with over €750 million in revenue to report their profits and taxes on a country-by-country basis, allowing tax authorities to conduct more informed assessments of tax avoidance risks and increasing the perceived detection risk for multinationals with aggressive tax strategies (Joshi, 2020). This, in turn, deterred multinationals from such strategies and improved tax compliance.

We conduct the analysis at three levels of aggregation. First, we compare the evolution of consolidated global effective tax rates and sales of the treated multinationals (revenues in excess of €750 million) to group-level sales and tax rates of companies below the

¹See Autor et al. (2020), Furman and Orszag (2018) and Grullon et al. (2019) for the US and Affeldt et al. (2021), Bajgar et al. (2023) and Bighelli et al. (2023) for Europe.

²See OECD (2015); Clausing (2020); Johannesen (2022); Hugger et al. (2024).

³See Bilicka (2019); Wier and Erasmus (2023); Bachas et al. (2023); Gaertner et al. (2025); Gallemore et al. (2024).

revenue threshold. This analysis has the advantage that the consolidated data abstracts from the relocation of sales, profits and taxes across countries in response to the reform. Second, we compare the evolution of sales of the treated multinationals' subsidiaries to those belonging to non-treated groups within each country-industry. Analysis at this level represents an intermediate link between the group-level and industry-level analyses. Third, we compare the evolution of industry concentration in country-industries where some of the top firms are multinationals treated by the reform, to country-industries where this is not the case. The analysis is based on consolidated and unconsolidated financial data from the Orbis database. We focus on companies headquartered in the European Union, both because Orbis offers better coverage in Europe than elsewhere (Bajgar et al., 2020), and because this ensures a more homogeneous institutional setting for the analysis.

We find that increased tax compliance following the country-by-country reporting reform is associated with reductions in the sales of large multinationals and in industry concentration. We begin by confirming the existing evidence on the reform's effect on tax compliance, which indicates that the effective tax rates of affected large multinationals rose by 1-2 percentage points as a result of the reform (Joshi, 2020; Tuinsma et al., 2023; Hugger, 2024). Moving to the core contribution of our paper, we estimate that business groups that had to report on the country-by-country basis saw a decrease in sales of 5% relative to the unaffected business groups in the same country-industries, with the effect taking a few years to materialize but increasing over time. The findings are robust to different data and methodology choices and they are economically important: we estimate a semi-elasticity of consolidated sales with respect to consolidated effective tax rates of -1.8. Hence, a 1-percentage-point rise in consolidated effective tax rates is associated with a 1.8% decrease in consolidated sales.

We further find that the reform was also associated with sales declines within individual subsidiaries of the affected multinationals. This is not guaranteed by the findings for consolidated sales; for example, if the higher effective tax rates made the affected multinationals sell some subsidiaries, we could observe negative effects on group-level sales but not on sales of individual subsidiaries. On average, we estimate that sales of treated subsidiaries fell by 2.1% as a result of the reform.

Finally, we document a decrease in concentration in industries where the top firms belonged to multinational groups subject to the reform. Our main difference-in-differences results indicate a decrease in industry concentration due to country-by-country reporting. Using the Herfindahl-Hirschman index to measure concentration, we estimate a significant reduction of this index by 0.018-0.028 on a scale from 0 to 1 for country-industries with treated firms after the introduction of country-by-country reporting. Measuring concentration as the sales share of the largest firms within a country-industry, we estimate a decrease of 2 percentage points in the sales share of the top 4 firms (significant

at the 5% level), or 2.6 percentage points in the sales share of the top 8 (significant at the 1% level) when the top firms in those industries have country-by-country reporting obligations.

Our findings highlight the potential of tax compliance reforms in reducing industry concentration. For example, the European Union has agreed to require large multinationals to make most of the information in their country-by-country reports publicly available, starting in 2024.⁴ The publication of the reports is likely to further deter multinationals from aggressive tax strategies and support tax compliance.⁵ Even more consequentially, 135 countries have agreed to require large multinationals to pay a global minimum tax of 15% from 2024 (Johannesen, 2022; Hugger et al., 2024). Existing studies suggest that these reforms will contribute to tax compliance of large multinationals. This paper argues that the reforms can also be expected to result in reduced industry concentration.

Related Literature. Our work contributes to the literature on the consequences of tax avoidance and compliance. While the determinants of tax avoidance have been intensively studied, research on its consequences constitutes a small but growing body of work (Bruehne and Jacob, 2019). Some documented firm-level consequences of tax avoidance include decreasing firm transparency (Ayers et al., 2009; Chen et al., 2018), higher cost of capital (Heitzman and Ogneva, 2018), and higher cost of debt (Hasan et al., 2014; Platikanova, 2017). Other studies show links between tax avoidance and firm value (Desai and Dharmapala, 2009; Hanlon and Slemrod, 2009). Li et al. (2021) additionally find that an anti-tax avoidance measure in the US decreased firm innovation. Our study contributes to this literature by highlighting that, besides the previously studied consequences, greater tax compliance among large multinationals is associated with lower industry concentration.

Our work is also related to studies analysing the drivers of the concentration increases observed in the United States and other parts of the world (e.g. Aghion et al., 2023; Crouzet and Eberly, 2023; Autor et al., 2020; Gutiérrez and Philippon, 2018; Akcigit and Ates, 2021; Liu et al., 2022). We complement these studies by analysing tax avoidance as a novel potential driver of the concentration increases.

Two of the most closely related studies are Martin et al. (2023) and Gauß et al. (2024). The former examine the impact of corporate tax avoidance on sales of US firms. Their results suggest that changes in tax avoidance of large relative to small firms can explain about 15% of the variation in concentration across U.S. industries between 1994 and 2017. Our paper reaches a similar conclusion but differs along several important dimensions. We focus specifically on large multinational enterprises, exploit a recent international reform rather than national US policy changes, and analyse the effects of stronger tax

⁴Previously the reports were only shared with national tax authorities.

⁵There is evidence that publishing such reports in the banking sector (Overesch and Wolff, 2021; Joshi et al., 2020) and in the extractive and logging sectors (Johannesen and Larsen, 2016) in 2010s affected tax compliance.

compliance rather than reduced tax burdens. Unlike Martin et al. (2023), we also directly link tax compliance to industry concentration in a regression framework. Gauß et al. (2024), in turn, document that tighter transfer-pricing rules in the EU increased effective taxation, reduced multinationals' sales and boosted domestic firms' performance. While closely related, they study a different reform and period and do not link tax compliance to industry concentration.

2 Theoretical Framework

This section develops a simple model illustrating two mechanisms through which profit shifting to low-tax jurisdictions can increase multinationals' sales and, since multinationals are typically among the largest firms, raise overall industry concentration.

2.1 Domestic Firms Only

Consider first a high-tax country with L consumers who have constant-elasticity-of-substitution (CES) preferences with elasticity $\sigma > 1$ over varieties ω supplied by a continuum of monopolistically competitive firms. Each consumer supplies one unit of labour inelastically and earns wage w . Initially, assume that all firms are purely domestic. Each firm produces one variety using labour l , hired at wage w , and a homogeneous intermediate input m , purchased on world markets at price r . All firms have the same production function $F(l, m)$ and have to pay the corporate income tax given by $t(p \cdot F(l, m) - w \cdot l - r \cdot m)$, where p is the output price and t is the local corporate income tax rate. Net profits are then given by

$$\pi^{DOM} = (1 - t)(p \cdot F(l, m) - w \cdot l - r \cdot m). \quad (1)$$

Minimising total costs $w \cdot l + r \cdot m$ subject to $F(l, m) \geq q$, where q is firm output, gives the total cost function $C(w, r, q)$.⁶ The net profits can then be written as

$$\pi^{DOM} = (1 - t)(p \cdot q - C(w, r, q)). \quad (2)$$

Under CES preferences, each firm sets a constant markup over marginal cost $c(w, r, q) := \frac{\partial C(w, r, q)}{\partial q}$,

$$p^{DOM} = \frac{\sigma}{\sigma - 1} c(w, r, q). \quad (3)$$

Let P denote the CES price index and $E = Lw$ aggregate expenditure. Then the optimal

⁶Gauß et al. (2024) work with net-of-tax total costs, which reflect the fact that the price of the inputs can be deducted from the tax base, but with gross marginal costs. We express both total and marginal costs in gross terms.

revenue⁷ is a decreasing function of the marginal costs,

$$(pq)^{DOM} = \left(\frac{\sigma - 1}{\sigma} \right)^{\sigma-1} \frac{EP^{\sigma-1}}{c(w, r, q)^{\sigma-1}}. \quad (4)$$

2.2 A Non-Mechanism: Multinationals Facing Lower Tax Rates

We begin by noting that a lower tax rate alone does not increase sales in this framework. Assume that some of the firms in the economy are multinationals and that they are offered a preferential tax rate $t^{PREF} < t$. Their net profits are then

$$\pi^{PREF} = (1 - t^{PREF}) (p \cdot q - C(w, r, q)). \quad (5)$$

However, notice above that prices and revenues do not depend on the tax rate. Intuitively, the optimisation of gross profits is not affected by the rate at which these are taxed. As a result, the price and revenues of multinationals are exactly the same as those of domestic firms, although multinationals retain higher after-tax profits.

We now present two mechanisms in which profit shifting can lead to increased sales by multinationals.

2.3 Mechanism 1: Multinationals and Transfer Pricing

The first mechanism arises when multinationals engage in transfer pricing. The mispriced input then serves a dual role: as a productive factor and as a vehicle for profit shifting. The latter role effectively subsidizes the input's use in production, leading transfer-pricing multinationals to employ more of it, charge lower prices, and earn higher revenues.⁸

Assume that the tax rate in the high-tax jurisdiction is the same for all firms, but multinationals differ from domestic firms by their ability to shift profits to a low-tax jurisdiction (tax haven) with tax rate $t^h < t$ by buying the input from their tax-haven subsidiary at price $r^h > r$. However, implementing and concealing the transfer pricing is associated with costs of $\gamma(\delta) \cdot m$ that increase with the transfer-pricing intensity $\delta = r^h - r$.

The net profits of transfer-pricing multinationals are

$$\pi^{TP} = (1 - t) (p \cdot F(l, m) - w \cdot l - (r + \delta) \cdot m) + (1 - t^h) \cdot \delta \cdot m - \gamma(\delta) \cdot m. \quad (6)$$

Multinationals' optimisation problem includes not only setting the inputs and price, but also setting the optimal transfer pricing intensity δ^* . The transfer pricing intensity

⁷We focus on revenues rather than physical sales here because the former is closer to the 'sales' variable used in the empirical analysis. However, all results derived here for revenues would similarly hold for physical sales.

⁸The discussion of this mechanism is based on Gauß et al. (2024).

is optimal when the tax-rate differential equals the marginal transfer pricing costs:

$$(t - t^h) = \frac{\partial \gamma(\delta^*)}{\partial \delta}. \quad (7)$$

The total costs of a transfer-pricing multinational, gross of the taxes in the high-tax country and taking into account the transfer pricing and the associated costs, can be written as

$$w \cdot l + (r - a(\delta^*)) \cdot m, \quad (8)$$

where $a(\delta^*) = \frac{(t-t^h) \cdot \delta^* - \gamma(\delta^*)}{1-t}$ represents the part of the price of input m that is offset by using the input as a vehicle for profit-shifting. Minimising the costs leads to the cost function $C(w, r - a(\delta^*), q)$ and to marginal costs $c(w, r - a(\delta^*), q)$ such that

$$c(w, r - a(\delta^*), q) < c(w, r, q), \quad \forall a(\delta^*) > 0. \quad (9)$$

The lower (effective) marginal costs imply that transfer-pricing multinationals charge lower prices and earn higher revenues than domestic firms.

2.4 Mechanism 2: Multinationals and Allocation of Intangibles

The second mechanism stems from multinationals' strategic allocation of intangible assets, a key channel of profit shifting to low-tax jurisdictions (Beer et al., 2020) and from the fact that many intangibles (such as R&D) can be modelled as fixed-cost investments that enhance firm productivity and lower marginal costs. Locating intangibles in low-tax jurisdictions reduces the effective fixed cost of R&D, raising the likelihood that multinationals invest in R&D even when domestic firms do not.⁹

Now assume that multinationals are not able to misprice the input m , but all firms have the option of undertaking an R&D project that carries a fixed cost I but leads to a Hicks-neutral productivity improvement such that the production function becomes

$$\tilde{F}(l, m) = \alpha F(l, m), \quad \alpha > 1. \quad (10)$$

The net profits of a domestic firm that undertakes the R&D project are

$$\pi^{I(dom)} = (1 - t) (p \cdot \alpha F(l, m) - w \cdot l - r \cdot m - I). \quad (11)$$

Minimising the total costs of a domestic firm investing in R&D, $w \cdot l + r \cdot m + I$, subject

⁹Several studies find intangible investments to be associated with greater industry concentration, see, e.g., Covarrubias et al. (2020) and Crouzet and Eberly (2023) for the US and Bajgar et al. (2025) for cross-country evidence.

to $\alpha F(l, m) \geq q$ leads to the cost function

$$\tilde{C}(w, r, q, I) = \frac{C(w, r, q)}{\alpha} + I \quad (12)$$

and to marginal costs

$$\tilde{c}(w, r, q, I) = \frac{c(w, r, q)}{\alpha} < c(w, r, q). \quad (13)$$

The lower marginal costs imply that if a firm decides to undertake the R&D project, it will charge a lower price and receive greater revenues than without implementing the R&D project.

Suppose further that multinationals can shift profits by assigning patents from their R&D projects to subsidiaries in low-tax jurisdictions (facing a tax rate of t^h) and paying a licensing fee ϕ to the subsidiary for the right to use the patented technology. As in the transfer-pricing case, implementing and defending the licensing arrangement entails costs $\gamma(\phi)$.¹⁰ The net profits of a multinational that undertakes the R&D project are then¹¹

$$\pi^{I(mne)} = (1 - t) (p \cdot \alpha F(l, m) - w \cdot l - r \cdot m - I - \phi) + (1 - t^h) \phi - \gamma(\phi). \quad (14)$$

As in the transfer-pricing case, multinationals set the optimal licensing fee ϕ^* such that the tax-rate differential equals the marginal transaction costs, $(t - t^h) = \frac{\partial \gamma(\phi^*)}{\partial \phi}$. The total costs of a multinational investing in R&D, gross of the taxes in the high-tax country and taking into account the licensing fee and the associated costs, are

$$w \cdot l + r \cdot m + I - b(\phi^*), \quad (15)$$

where $b(\phi^*) = \frac{(t - t^h) \cdot \phi^* - \gamma(\phi^*)}{1 - t}$ represents the part of the cost of the R&D offset by locating the patents in the tax haven. The total costs of a multinational investing in R&D differ from those of a domestic firm investing in R&D only by the fixed component $-b(\phi^*)$, so it follows that the cost function of a multinational investing in R&D is equal to $\frac{C(w, r, q)}{\alpha} + I - b(\phi^*)$ and, because ϕ is modelled as fixed, the marginal costs are the same as in the case of a domestic firm.

Define $g^\pi(\alpha)$ as the proportional increase in profits resulting from a productivity increase by a factor of α , not taking into account the R&D costs. If the tax advantage $b(\phi^*)$ is sufficiently large, then for some intermediate magnitudes of the productivity

¹⁰To simplify this exposition and more clearly distinguish the intangibles mechanism from the transfer-pricing mechanism, we assume that the licensing fee is set as a fixed amount rather than as a function of the output or of input use. A license fee proportional to firm output would further reduce the effective marginal costs of multinationals and increase their revenues in a similar way as mispricing of input m .

¹¹We assume that the R&D investment is undertaken and deducted from the tax base in the high-tax country, but this assumption could be reversed without substantially altering the analysis. In practice, multinationals commonly file patents in different countries than where the inventive activity took place (Baumann et al., 2020).

increase α , relative to the after-tax R&D investment costs I , such that

$$(1 - t)(I - b(\phi^*)) < g^\pi(\alpha)\pi^{DOM} < (1 - t)I, \quad (16)$$

multinationals will invest in R&D but domestic firms will not, and multinationals consequently will have lower marginal costs, charge lower prices and generate greater revenues than domestic firms.

2.5 Summary

In summary, while lower effective tax rates alone do not necessarily increase multinationals' sales, two mechanisms can do so. Profit shifting through transfer pricing effectively lowers input costs, and shifting intangibles to low-tax jurisdictions reduces the fixed costs of productivity-enhancing investments such as R&D. These mechanisms imply that policies tightening profit-shifting opportunities can reduce affected multinationals' sales and, consequently, industry concentration. We test this prediction in the empirical analysis that follows.

3 Data and Measurement

3.1 Orbis Microdata

We obtain yearly consolidated and unconsolidated financial data and ownership data from the Orbis Historical database, provided by Bureau van Dijk. We restrict the sample to firms in the EU28 because Orbis offers a much better coverage of firms in Europe than elsewhere (Bajgar et al., 2020) and because the country-by-country reform was implemented in a homogeneous way across all member states of the European Union.¹² While Orbis does provide a separate dataset for financial firms (i.e. banks and investment firms), we do not include these firms since their financial variable definitions are different and this sector was subject to a public country-by-country reporting regime already (the Capital Requirements Directive IV, effective from 2015) which may confound our analysis (Joshi et al., 2020).

We download all firms located in the EU with sales of at least €1 million, either consolidated or unconsolidated. As the treatment status of each business group (and its subsidiaries) is based on consolidated revenues, we further restrict the sample to business groups with a consolidated revenue between €10 million and €10 billion to

¹²We include firms in the 28 countries that were EU members before Brexit, since nearly our full sample period is pre-Brexit and the UK implemented country-by-country reporting simultaneously with the rest of the EU.

prevent comparing the very smallest with the very largest groups.¹³ Groups that switch treatment status during our post-reform period are dropped since the timing of treatment effects is inconsistent with the rest of the treated group.¹⁴ Loss-making groups are also dropped, consistent with most of the tax avoidance literature (Hanlon and Heitzman, 2010; Henry and Sansing, 2018).

To determine which firms belong to each business group, we identify the global ultimate owner (GUO) of each firm and define business groups as collections of firms with the same global ultimate owner, which may be a non-EU firm. Specifically, we use the Orbis GUO50 link which identifies the global ultimate owner anywhere in the world with over 50% ownership of the subsidiary, hence ensuring unique GUOs for each subsidiary. Where we do not identify a GUO, we assume the firm is independent, i.e. it is its own GUO. To clearly distinguish between the different levels of analysis, we henceforth use the term ‘group’ to refer to collections of firms with the same GUO and the term ‘subsidiary’ for unconsolidated firms, although both also include independent firms, provided these meet the size threshold.

3.2 Variable Definitions at Three Levels of Analysis

We use the Orbis microdata to build datasets on three different levels: the business group level, the subsidiary level, and the industry level.

In the group-level analysis, our main outcome variable is consolidated group sales. The advantage of this variable is that it is not directly affected by re-location of sales between different parts of the group (De Simone and Olbert, 2022; Doeleman et al., 2024). The country-by-country reporting requirements apply to multinational groups with consolidated revenues in excess of €750 million. Which business groups are considered treated in our difference-in-differences analysis is thus determined by interacting an indicator of whether a group exceeds the revenue threshold with an indicator of its multinational status, which we set to one if a given GUO owns at least one foreign subsidiary.

In the subsidiary-level analysis, we focus on unconsolidated sales of each group’s subsidiaries.¹⁵ This allows us to test whether the increased tax compliance reduced within-firm sales in the subsidiaries of the affected multinationals, as opposed to, for example, just making the multinationals divest some of these subsidiaries. Treatment at the subsidiary level is determined by the treatment status of each subsidiary’s GUO.

In the industry-level analysis, the outcome variable is industry concentration. We use several concentration measures, where the first are defined as the share of the top

¹³We test the robustness of our results to further reducing this interval, with the results in Figure 4 in the Appendix.

¹⁴This drops 4% of our observations. In a robustness test presented in Figure 4 in the Appendix, we show that our results are the same when treatment switchers are not excluded.

¹⁵These subsidiaries are located in the EU, but their GUO may be headquartered elsewhere.

1, 4 or 8 groups in the total sales in each country-industry. Alternatively, we measure concentration using the Herfindahl-Hirschman index (HHI) of the country-industry, which sums the squares of all group sales within the country-industry to obtain an index between 0 and 1. Following Bajgar et al. (2023), the numerator of the sales shares is calculated aggregating sales across all subsidiaries of each group within each country-industry.¹⁶ The denominator is based on country-industry sales, sourced from Eurostat Structural Business Statistics data.¹⁷ As a baseline, we use 2-digit NACE industries, but we also test the robustness of the results to using 1-digit and 3-digit industries. The treatment variable at the industry level is given by the share of the top 1, 4 or 8 groups in a given country-industry that are affected by the reform. Hence, it can attain several values between 0 and 1.

A limitation of our analyses at the subsidiary and industry levels, common to most studies measuring industry concentration,¹⁸ is that our data do not allow distinguishing between external sales and within-group transactions.¹⁹ In principle, this means that the observed changes in subsidiary sales and industry concentration might reflect changes in intra-group transactions rather than in ‘true’ external sales. This could be an issue for our analysis given that mispricing of intra-group trade is a key channel for profit-shifting to low tax jurisdictions (Heckemeyer and Overesch, 2017; Cristea and Nguyen, 2016; Davies et al., 2018). However, note that, to the extent that the country-by-country reporting reform led to increased tax compliance, it should be associated with less overvaluation of intra-group sales by subsidiaries located in low-tax jurisdictions and less undervaluation of intra-group sales by subsidiaries located in high-tax jurisdictions. As our sample consists of firms located in EU countries, most of which have relatively high tax rates, the latter effect is far more important in the context of our analysis. But the latter effect should lead to an *increase* in the value of intra-group sales by high-tax-jurisdiction subsidiaries of multinationals affected by the country-by-country reporting reform, which is the opposite of the expected effect of the country-by-country reporting on external sales of those subsidiaries. This means that, to the extent that the country-by-country reporting reform limited the undervaluation of intra-group sales by EU-based subsidiaries, our results based on total sales will underestimate the decline in the external sales by

¹⁶These country-industries and subsidiaries within them are in the EU, but those subsidiaries may be ultimately owned by non-EU groups.

¹⁷The coverage of smaller firms in Orbis tends to increase over time (Bajgar et al., 2020), so calculating the denominator of the concentration ratios by simply summing up across all firms observed in Orbis would create a spurious upward trend in such denominator and, consequently, a spurious downward trend in industry concentration. Denominators based on country-industry sales from Eurostat are not subject to a similar bias.

¹⁸E.g. Furman and Orszag (2018), Grullon et al. (2019), Kalemli-Özcan et al. (2024) and Furman and Orszag (2018).

¹⁹Consolidated accounts do not suffer from this issue (intra-group transactions cancel out) but consolidated sales are not apportioned to individual countries and industries and thus cannot be used to construct meaningful measures of concentration at the country-industry level.

subsidiaries of the treated multinationals in response to country-by-country reporting.

Effective tax rates at the group level are calculated as a ratio of consolidated taxes paid to consolidated profits before taxes of each business group. Additional variables used as covariates in robustness tests include the number of employees, return on assets, leverage, and intangibles share.

3.3 Final Sample and Summary Statistics

Since Orbis historical ownership coverage starts in 2007 and the latest information available to us is from 2021, our sample period is 2007–2021,²⁰ allowing us to include 9 pre-reform and 6 post-reform years. The descriptive statistics for our data are shown in Table 1, and they are complemented with descriptive graphs in Figure 1.

At the group level, we observe 28,651 unique corporate groups and 164,209 group-year observations, which are summarized in Panel A of Table 1. Average sales and revenue in our sample are just over €200 million, and the average effective tax rate is 25%. The time trend of effective tax rates within our sample is shown in Panel A of Figure 1: it declines from 27% at the start of our sample period to 23% at the end. Nearly 45% of observations correspond to groups with a multinational status, but most of these remain below the country-by-country reporting threshold of €750 million, so only about 3% of all observations correspond to treated groups. Panel B of Figure 1 shows the relationship between effective tax rates and group size within a country-industry-year. Effective tax rates are initially clearly positively related to group size. However, for larger groups this flattens out. At the top end of the size distribution, tax rates even become slightly regressive. Although this figure does not prove that large groups avoid more taxes, it motivates our research question by illustrating suggestive evidence for this phenomenon, in line with evidence of lower effective tax rates for the largest firms found by Bachas et al. (2023) and Wier and Erasmus (2023).

Panel B in Table 1 presents the summary statistics on the subsidiary level. Our panel dataset here includes 130,604 unique subsidiaries and 707,658 yearly observations. Average unconsolidated sales is €44 million. Over 70% of observations are subsidiaries owned by a multinational group, of which 31% have a consolidated revenue exceeding the revenue threshold for country-by-country reporting.

Finally, Panel C shows the descriptive statistics for the country-industry level. We selected only industries with at least two observed firms to an aggregate sales of at least €500 million to avoid our results being driven by very small industries in the control group. Our data includes observations on 1,213 unique country-industries, divided between 26 EU countries and 68 industries.²¹ The average industry size is between 20

²⁰The coverage of the Eurostat data restricts the industry-level analysis to years 2008–2020.

²¹Our data unfortunately does not contain unconsolidated financials for Great Britain or Ireland, hence these countries are not excluded in the industry-level (and subsidiary-level) analysis.

Table 1: Descriptive statistics

	(1) N	(2) Mean	(3) SD	(4) Min	(5) Max
<i>Panel A. Group-level dataset</i>					
Sales (consolidated, €million)	164,209	204.0	711.6	1.076	86,966
Revenue (consolidated, €million)	164,209	208.1	658.9	10.00	10,000
Effective tax rate (consolidated)	164,209	0.250	0.189	0	1
Revenue > €750 million	164,209	0.042	0.200	0	1
Multinational	164,209	0.447	0.497	0	1
Treated	164,209	0.032	0.176	0	1
Post-reform	164,209	0.403	0.490	0	1
Treated x post-reform	164,209	0.013	0.113	0	1
<i>Panel B. Subsidiary-level dataset</i>					
Sales (unconsolidated, €million)	707,658	44.28	179.6	1	17,399
GUO with revenue > €750 million	707,658	0.323	0.468	0	1
Multinational GUO	707,658	0.717	0.450	0	1
Treated	707,658	0.314	0.464	0	1
Post-reform	707,658	0.432	0.495	0	1
Treated x post-reform	707,658	0.134	0.340	0	1
<i>Panel C. Country-industry-level dataset</i>					
Turnover (million €, top 8)	10,688	25,302.2	70,717.6	501.1	1,251,471.8
Turnover (million €, top 4)	12,590	22,034.7	65,671.6	500.6	1,251,471.8
Turnover (million €, top 1)	14,065	19,946.4	62,441.2	500.6	1,251,471.8
Sales share (top 8)	10,688	0.248	0.213	0.003	1
Sales share (top 4)	12,590	0.208	0.205	0.001	1
Sales share (top 1)	14,065	0.118	0.156	0	1
Herfindahl-Hirschman Index	14,065	0.078	0.154	0	1
Treated share (top 8)	10,688	0.626	0.261	0	1
Treated share (top 4)	12,590	0.671	0.299	0	1
Treated share (top 1)	14,065	0.740	0.439	0	1

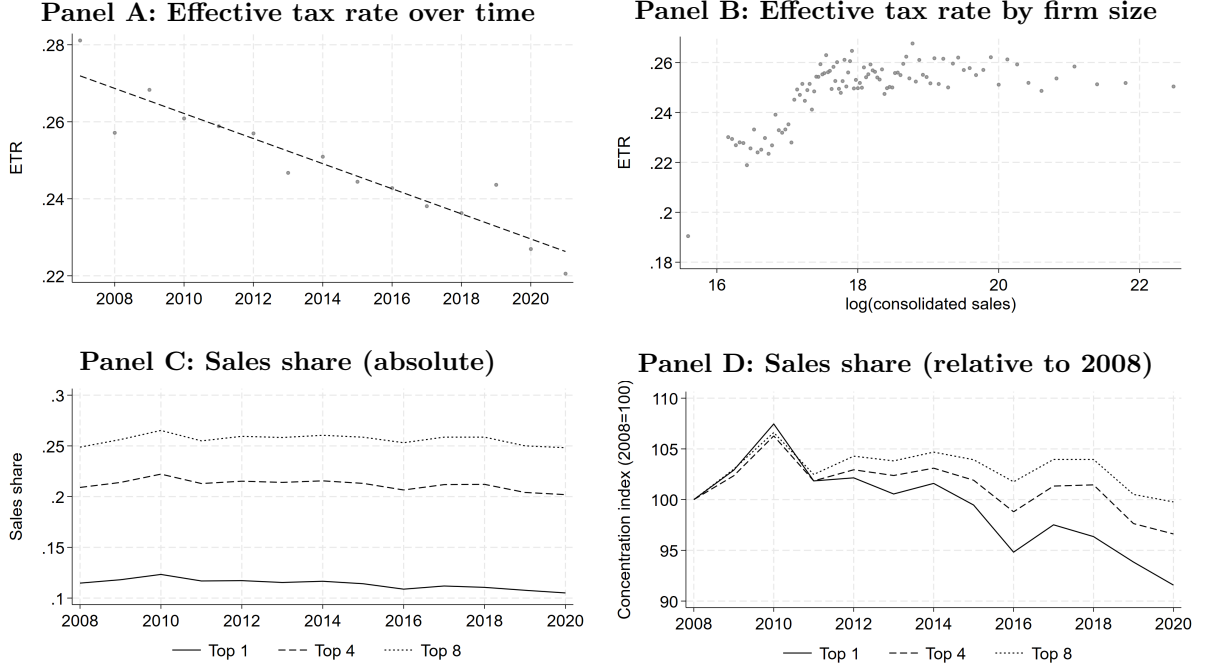
Notes: this table shows descriptive statistics for the datasets on three levels. Panel A describes the group-level dataset, with all financials on consolidated basis. We observe 28,651 unique corporate groups. Panel B describes the subsidiary-level dataset, with sales on unconsolidated basis. We observe 130,604 unique subsidiaries. Panel C describes the country-industry-level dataset. We observe 1,213 unique country-industry pairs.

and 25 billion euros. On average, the top 8 firms account for 25% of these sales, with the top 4 accounting for just over 20% and the largest firm alone for nearly 12%. The average Herfindahl Most firms entering the concentration ratios form part of the affected multinationals, as the average share of treated firms is 62% within the top 8, 67% within the top 4 and 74% for the largest firm.²²

The evolution of industry concentration during our sample period is shown in Panels

²²These figures may seem large at first. Note that treatment status is determined by consolidated group revenue, also taking into account revenue of the business group outside the country-industry unit. For example, the treated share of a country-industry with total turnover of €500 million may still be strictly positive if its largest business group with €250 million of sales *within* that country-industry additionally has €600 million of revenue in other country-industries.

Figure 1: Motivating figures



Notes: Panel A shows the time trend in average consolidated effective tax rates on the group level over time. All groups with positive profits are selected, yearly bins are plotted together with a linear fit. Panel B shows the relationship between effective tax rates and the natural logarithm of consolidated sales for the same sample. 100 equally-sized bins are plotted. Panels C and D show the time trend in industry concentration, respectively in absolute terms and relative to 2008 levels. Concentration is measured as the sales share of the top 1, top 4, or top 8 firms within a country and a 2-digit NACE industry. The sample is balanced and only country-industries with at least 8 firms in each year are included. Outlier country-industries for which the difference between the lowest and highest recorded concentration exceeds 75 percentage points are excluded from the sample.

C and D of Figure 1. Panel C confirms the averages in the summary statistics, which appear quite stable over time but the relative trends show important trends. Panel D shows the evolution of concentration relative to 2008 levels. It suggests that concentration rose during the financial crisis, which might be partly due to recessions hitting smaller firms more and larger firms recovering faster after the financial crisis (Crouzet and Mehrotra, 2020; Sahin et al., 2011). After 2010, concentration drops back to a few percent above 2008 levels and remains flat until 2015. From 2016 onwards, concentration starts decreasing. While many factors might explain this decrease, it is also consistent with our hypothesis that increased tax compliance due to the 2016 country-by-country reporting reform reduced industry concentration.

4 Empirical Strategy

To infer the effects of country-by-country reporting on tax compliance, sales, and industry concentration, we use a difference-in-differences (DiD) approach. Additionally, we use

event study estimates to test the plausibility of the identifying parallel trends assumption and to explore the time dynamics of the policy effect.

In the group level analysis, the treatment group consists of multinationals with a revenue above the threshold of €750 million, which have to report on the country-by-country basis from 2016 onwards (our post-treatment period). Non-multinationals (i.e. business groups operating only in a single country) and multinationals below the revenue threshold constitute the control group.²³ We estimate the following equations:

$$ETR_{g,c,s,t} = \alpha Treatment_g \cdot Post2016_t + FE_g + FE_{c,s,t} + \epsilon_{g,c,s,t}, \quad (17)$$

and

$$\log Sales_{g,c,s,t} = \beta_1 Treatment_g \cdot Post2016_t + FE_g + FE_{c,s,t} + \mu_{g,c,s,t}, \quad (18)$$

where $ETR_{g,c,s,t}$ and $\log Sales_{g,c,s,t}$ respectively denote the consolidated effective tax rates and the natural logarithm of consolidated sales of business group g headquartered in country c and operating in industry s in year t . α is the estimate for the effect of country-by-country reporting on effective tax rates and β_1 estimates its effect on consolidated sales.

To limit the possibility that our estimates are capturing confounding effects of factors other than the reform, we control for a rich set of fixed effects. Group fixed effects control for all time-invariant group characteristics. Country-industry-year fixed effects further control for all time-varying factors specific to particular countries (e.g. the business cycle), industries (e.g. international industry-specific demand and supply shocks) or country-industries (e.g. domestic industry-specific demand and supply shocks). We thus effectively compare proportional *changes* in sales between treated and untreated groups operating *within the same country-industries*. This also means that we cannot (and do not need to) to include any control variables varying at the level countries or country-industries (other than the fixed effects). Standard errors are clustered at the group level.

Similar to Martin et al. (2023), we also employ two-stage least squares to obtain a semi-elasticity of sales with respect to effective tax rates, using $Treatment_g \cdot Post2016_t$ as an instrument exogenously affecting effective tax rates. We also provide several robustness checks to show that our results hold using different sampling decisions, methodologies,

²³In a robustness test, we exclude non-multinationals with a revenue over €750 million from our sample. These are groups for which we cannot observe a foreign subsidiary, hence their multinational status is zero. However, if some ownership links are missing in Orbis, we could incorrectly identify some groups as non-multinationals because of a missing link to a foreign subsidiary. Our results are robust to dropping these groups (see Figure 4). Restricting our sample further to only include multinational groups, i.e. also excluding non-multinationals below the revenue threshold, does not alter our main results either (see Figure 4). The former sampling decision is the most similar to the one used by Hugger (2024), while the latter sample excluding non-multinationals from both treatment and control groups is used by Joshi (2020).

or definitions.

At the subsidiary level, we estimate equation (19), which is similar to equation (18) but with the outcome variable consisting of unconsolidated sales of subsidiary i of group g :

$$\log Sales_{i,g,c,s,t} = \beta_2 Treatment_g \cdot Post2016_t + FE_i + FE_{c,s,t} + \mu_{i,g,c,s,t}. \quad (19)$$

Here, the subsidiary i operates in country c and industry s . The unit fixed effects are now defined at the subsidiary level to remove any confounding time-invariant differences between subsidiaries, and we again control for country-industry-year fixed effects. Standard errors are clustered at the subsidiary level.

Finally, at the industry level, the outcome variable is the top 1, 4 or 8 concentration ratio for country c , industry s and year t and we estimate the following equation:

$$Concentration_{c,s,t,k} = \beta_3 TreatedShare_{c,s,t,k} \cdot Post2016_t + FE_{c,s} + FE_t + \nu_{c,s,t}, \quad (20)$$

Here, $Concentration_{c,s,t,k}$ is defined as the sales share of the top $k \in \{8, 4, 1\}$ within their country-industry c, s at year t , and $TreatedShare_{c,s,t,k}$ is defined as the share of those k firms that are treated (i.e. the number of groups out of the top k within a country-industry with the country-by-country reporting obligation, divided by k).²⁴ Alternatively, we use the Herfindahl-Hirschman Index: $HHI_{c,s,t} = \sum_i SalesShare_{c,s,t,i}^2$ where the summation is over all i firms in country-industry-year c, s, t as the dependent variable in equation (20). Country-industry-year fixed effects would be perfectly collinear with our explanatory and outcome variables, hence we now control for separate country-industry fixed effects and year fixed effects. Standard errors are clustered at the country-industry level.

5 Results

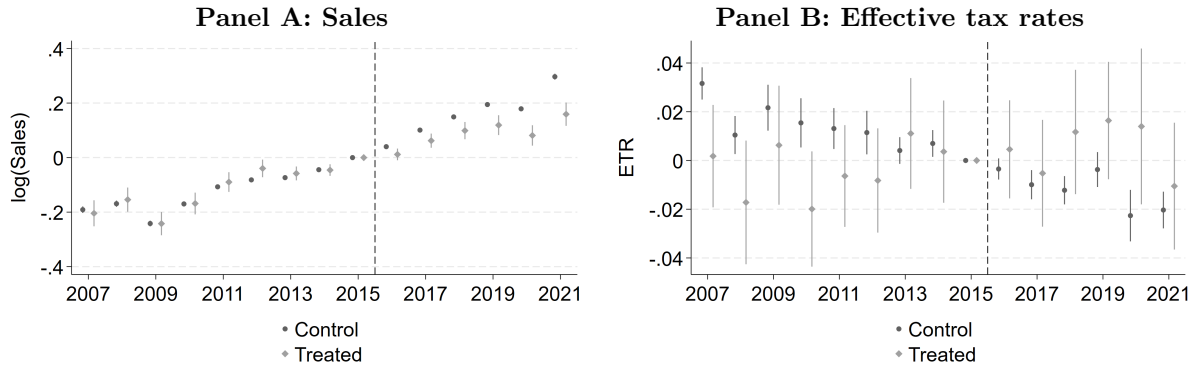
In this section, we first describe our results on the group level, subsidiary level, and industry level. We then do an additional regression discontinuity analysis to show that other policy reforms cannot explain our findings. Finally, we provide a host of robustness tests on all three analysis levels to show that our findings do not depend on methodological specification choices.

5.1 Group Level

We treat the introduction of country-by-country reporting as a quasi-experiment, exogenously changing tax compliance behaviour of treated firms, i.e. multinationals with a

²⁴Hence, in the case of the top 8 and the top 4, we estimate a DiD with a multi-valued ordered discrete treatment variable. In the case of the top 1, this simplifies to an ordinary DiD with binary treatment.

Figure 2: Consolidated effective tax rate and sales – parallel trends



Notes: this figure shows the time trends in consolidated sales and effective tax rates for the treatment group (firms with country-by-country reporting obligations) and the control group separately. Both trends are relative to 2015 base levels. Firm fixed effects are taken into account. 95% confidence intervals are depicted. The dotted vertical line represents the introduction of country-by-country reporting in 2016.

revenue of at least €750 million. The control group consists of non-multinational firms and multinationals with a revenue below the threshold.²⁵ Figure 2 shows the trends in terms of effective tax rates and sales of the control and treatment groups separately, relative to their 2015 base levels. Until 2015, before the reform was implemented, sales trends are parallel for the treatment and control group. However, from 2016 onwards the sales growth of treated firms is significantly lower relative to the control group. This is a first visual indication of the effect of country-by-country reporting on sales of treated (larger) business groups.

The treatment group's effective tax rate is somewhat volatile before the introduction of country-by-country reporting due to the smaller sample this group constitutes, but is overall not significantly different from the control group's effective tax rate trend (see column 1 of Table 6 for the event study estimates). However, from 2016 onwards, treated firms' effective tax rates rise slightly compared to its previous trend, and diverge even more significantly from the control group's effective tax rates which continues its downward trend. The pre-treatment trends may not be sufficiently parallel enough to confidently assert the point estimate of the effect on effective tax rates, instead we may be overestimating its size. In turn, this means that we may be *underestimating* the semi-elasticity of sales with respect to effective tax rates. The literature on country-by-country reporting shows that country-by-country reporting does indeed affect effective tax rates positively (Hugger, 2024; Joshi et al., 2020). In Section 5.4 we provide a regression discontinuity analysis to confirm such an effect and its direction. Additionally, event study estimates presented in columns 1 and 2 of Table 6 in the Appendix confirm

²⁵In robustness tests, we show that conclusions remain the same when non-multinationals are excluded from the control group.

Table 2: Main results – group and subsidiary level

	(1)	(2)	(3)	(4)
Outcome variable	ETR	log(Sales)	log(Sales)	log(Sales)
Analysis level	Group	Group	Group	Subsidiary
	2SLS			
	1st stage	2nd stage		
ETR		-1.783** (0.797)		
Country-by-country reporting	0.028*** (0.007)		-0.050*** (0.018)	-0.021*** (0.006)
Firm FE	Yes	Yes	Yes	Yes
Country $\times$ industry $\times$ year FE	Yes	Yes	Yes	Yes
Observations	164,209	164,209	164,209	707,658
Adjusted R^2	0.398		0.953	0.939
F-statistic		13.94		

Notes: *** 1%, ** 5%, * 10%. Standard errors, in parentheses, are clustered on the group level (columns 1–3) or the subsidiary level (column 4). This table summarizes the effects of country-by-country reporting on group-level consolidated effective tax rates and consolidated sales in columns 1–3 and on subsidiary-level unconsolidated sales in column 4. Country-by-country reporting is the interaction of post-CbCR and treatment. Industry classification is at the 2-digit level. In column (2), the Kleibergen-Paap Wald rk F-statistic is reported.

that until 2015, the year before country-by-country reporting became mandatory for our treated group, those firms’ effective tax rates and sales trends did not significantly differ to that of our control group.²⁶ Hence, the necessary parallel trends assumption for our difference-in-differences approach to be valid is not rejected.

Continuing with our main difference-in-differences estimates, column 1 in Table 2 shows that country-by-country reporting did increase tax compliance. We estimate a highly significant 2.8 percentage point increase in an effective tax rate, an effect size similar to but slightly larger than found in the literature (Hugger, 2024; Joshi et al., 2020). These studies estimate an increase in effective tax rates between 1 and 2 percentage points; our slightly larger treatment effect may be partly attributable to the fact that our data includes more and later post-reform years in which the effect has intensified, as seen in column 1 of Table 6. Column 3 in Table 2 shows that not only effective tax rates were affected, but sales were as well. Firms that had to report on the country-by-country basis saw a decrease in sales of 5% relative to the control group, statistically significant at the 1% level. We can observe the timing of this effect in our event study results in column 2

²⁶2012 appears to be a small outlier within the parallel sales trends, when sales were significantly higher in the treatment group. There is no clear explanation for this phenomenon in this specific year, but the overall picture of the 2007–2015 trend remains such that we are still confident in our parallel trends assumption.

of Table 6. It is clear that the tax compliance effect materializes slowly and only becomes significant from the third year after implementation. It is relatively constant (the larger estimate found in 2020 may be due to the coronavirus pandemic). The sales effect takes a year longer to become significant but increases over time.

In column 2 in Table 2 we decompose the effect of country-by-country reporting on sales to obtain the semi-elasticity of sales with respect to effective tax rates. We estimate that a one percentage point increase in effective tax rates decreases sales by 1.8%. Noting our potential overestimation of the first stage effect size, this estimation of the semi-elasticity may actually be an underestimation. The Kleibergen-Paap Wald rk F-statistic is 13.94, well above the commonly accepted benchmark of 10, indicating the strength of our instrument.

In Section 5.4, we additionally perform a regression discontinuity analysis to confirm that alternative reforms cannot explain our findings. We also show with a myriad of robustness tests that these results are robust to most alternative specifications and sample definitions (Section 5.5).

5.2 Subsidiary Level

In column 4 of Table 2, we present the difference-in-differences result of our analysis on the subsidiary level. Treatment is determined by the treatment status of the global ultimate owner of the subsidiary, the outcome is unconsolidated sales on the subsidiary level. Using country-industry-year fixed effects on the subsidiary level allows us to compare unconsolidated sales with other subsidiaries in the same country-industry at the same time. Including subsidiary fixed effects accounts for pre-existing differences in subsidiary characteristics. On average, we estimate that treated subsidiaries' sales dropped by 2.1% due to country-by-country reporting. This shows that our estimate on consolidated group sales is not due to affected multinationals simply divesting their subsidiaries, but rather the decline in size of those subsidiaries relative to untreated firms.

Event study results in column 3 of Table 6 in the Appendix do not give reason to reject the parallel trends assumption on the subsidiary level, although the financial crisis may explain small differences at the start of our sample period. In tests presented in Section 5.5, we show that our subsidiary-level results are not driven by potential non-parallel trends at the start of our sample period and further tests show additional robustness to different sample and methodological choices.

5.3 Industry Level

In this section, we investigate whether country-by-country reporting had a direct effect on industry concentration. We first measure concentration by the unconsolidated sales share of the top firms within every country-industry combination (2-digit industries).

Table 3: Main results – industry concentration

	(1)	(2)	(3)	(4)	(5)	(6)
Concentration	Sales share	Sales share	Sales share	HHI	HHI	HHI
Top N (shares)	Top 8	Top 4	Top 1	Top 8	Top 4	Top 1
Treated share	0.059*** (0.010)	0.042*** (0.008)	0.013*** (0.004)	0.014** (0.007)	0.013** (0.006)	0.008*** (0.002)
Post $\times$ treated share	-0.026*** (0.010)	-0.020** (0.008)	-0.005 (0.004)	-0.028*** (0.008)	-0.018** (0.007)	-0.007* (0.004)
Observations	10,688	12,590	14,065	10,688	12,590	14,065
Adjusted R^2	0.875	0.866	0.852	0.795	0.806	0.825
Country $\times$ industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** 1%, ** 5%, * 10%. Standard errors, in parentheses, are clustered on the country-industry level. The outcome variable is either the sales share of the top 8, top 4, or top 1 firm (columns 1–3), or the Herfindahl-Hirschman Index (HHI, columns (4–6), within the country-industry-year. Treated share is the share of treated firms in the top 8/4/1 (multinationals with a revenue above the CbCR threshold of €750 million). Small country-industries with aggregated sales under €500 million are excluded. Country-industries which include only one firm, for which the sales share or HHI equals 1 by definition, are excluded.

The treatment variable is the share of the top $k \in \{8, 4, 1\}$ firms with treated status. In the case of the top 8 and top 4, our treatment variable is multi-valued discrete but can be thought of as continuous and ranges between 0 and 1; in the case of the top 1, this simplifies to a classic difference-in-differences with binary treatment. Results are presented, for the top 8, top 4, and top 1 respectively, in columns 1–3 of Table 3. Assuringly, we find that industries with more treated firms (exceeding the €750 million revenue threshold) are more concentrated. Interacting this with a post-treatment indicator to obtain our difference-in-differences estimates, we find that an industry in which all of the top 8 firms are treated experienced a 2.6 percentage points drop in concentration after the introduction of country-by-country reporting relative to industries without treated firms (significant at the 1% level). Hence, assuming this effect is linear, every additional top 8 firm with reporting obligations leads to a decrease in concentration of $\frac{1}{8} \cdot 2.6 = 0.325$ percentage points. For the top 4, this effect is $\frac{1}{4} \cdot 2 = 0.5$ percentage point, statistically significant at the 5% level. For the top 1 firm, we do not find a statistically significant effect on their sales share.

We additionally present estimates where the Herfindahl-Hirschman Index (HHI) as a measure of industry concentration is the dependent variable. These results are in columns 4–6, using the same treatment variables as before (i.e. the treated share of the top 8, top 4, and top 1 firms within the country-industry-year). The first estimates – not interacted with the post-treatment indicator – again confirm the intuitive finding that country-industries in which more of the top firms exceed the €750 million reporting threshold

are more concentrated, as indicated by a larger HHI. However, the interaction estimates indicating the effect of country-by-country reporting are all negative and statistically significant. This strengthens our finding that the implementation of country-by-country reporting decreased concentration, also when measured in HHI, in industries in which the largest firms have reporting obligations. After the introduction of country-by-country reporting, the HHI of industries in which all of the top 8 were treated decreased on average by 0.028 relative to industries in which no firms had country-by-country reporting obligations (significant at the 1% level). For the top 4, we estimate a relative decrease by 0.018, significant at the 5% level, while the decrease we estimate for the top 1 is 0.007 and significant at the 10% level.

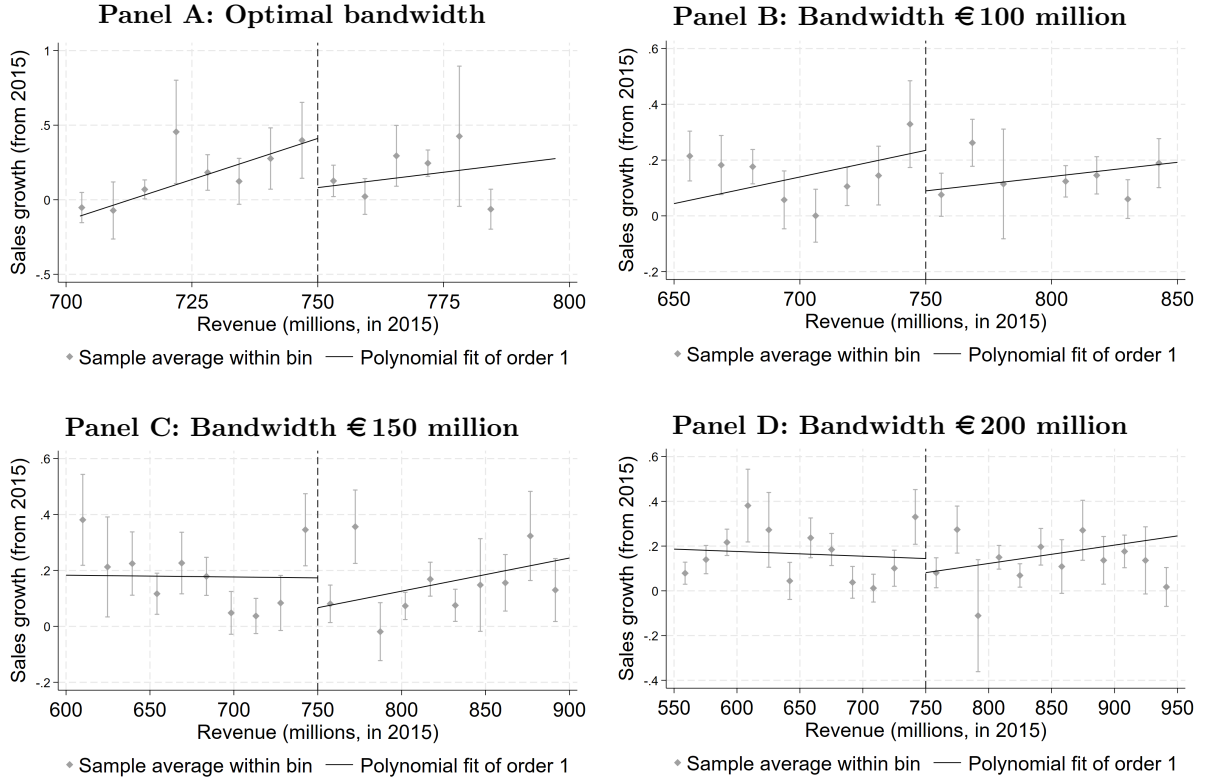
In Figure 7 in the Appendix we present event study estimates showing the yearly effect of the top firms of industries having country-by-country reporting obligations. These figures indicate that the decrease in concentration starts to materialize around two years after the introduction of country-by-country reporting. Pre-implementation estimates present some potential concern about the validity of the parallel trends assumption for the years before 2011 and confounding effects of the pandemic at the other end of our sample period. In Section 5.5 we present several tests alleviating these concerns and showing robustness to alternative methodological choices.

5.4 Alternative Explanations — Regression Discontinuity

Around the same time as the introduction of country-by-country reporting, other policies and political changes happened which may have affected large firms' sales more negatively than sales of smaller firms. Examples of such potentially confounding developments are Brexit, the EU Anti-Tax Avoidance Directive (ATAD), the introduction of GDPR, as well as significant antitrust cases against large multinationals including Google and Apple in the EU. For any of these, one could argue that the competitive position of large internationalized companies was affected differently than that of smaller firms.²⁷ How-

²⁷In the case of the ATAD, aimed at improving tax compliance especially for larger multinationals and partly the EU's way of implementing several of the BEPS recommendations agreed at the OECD, one could argue that even if some of the observed relative sales decrease among large firms is due to the ATAD rather than the introduction of country-by-country reporting, it still represents general evidence for a link between tax compliance and group sales. Moreover, the ATAD rules entered into force between 2019 and 2022; in contrast, our treatment starts already in 2016. In the case of GDPR, while Frey and Presidente (2024) show that ICT companies' profitability is negatively related to their exposure to GDPR, they do not find such a relationship with sales. Furthermore, their size heterogeneity test does not indicate a difference between small and large firms in their relationship between GDPR exposure and sales, hence providing a first confirmation that the introduction of GDPR did not affect industry sales concentration in the ICT sector, where it should have the largest impact. Nevertheless, we perform a test in which we exclude ICT sectors from our difference-in-differences strategy on the industry level, see Section 5.5 and Figure 4 in the Appendix. The results show findings very similar to those for our full sample. As for the significant antitrust cases against large multinationals including Google and Apple in the EU, they do not align with the country-by-country reporting's €750 million threshold and our threshold RDD helps rule out antitrust shocks as the driver of our discontinuity. A similar case holds for the broader

Figure 3: Regression discontinuity graphs



Notes: RDD figures for four different bandwidths. The running variable, revenue in 2015, is presented on the horizontal axes with the country-by-country reporting threshold indicated as a dashed line. Sales growth relative to 2015 in % is presented on the vertical axes. Observations are presented in same-sized bins together with their 95% confidence intervals, and linear fits are given separately below and above the country-by-country reporting threshold. Triangular kernel weighting is used, and country-industry-year fixed effects are taken into account. In Panel A, the mse-optimal bandwidth of €47.2 million is used (Cattaneo et al., 2019; Cattaneo and Titiunik, 2022). Panels B–D employ bandwidths of €100, €150, and €200 million respectively.

ever, country-by-country reporting is the only policy change that used a sharp revenue threshold at €750 million, above which the reporting obligation applies. We leverage this fact by using a regression discontinuity design (RDD) to confirm that while we cannot fully rule out effects of the previously mentioned changes, there is strong evidence for a discontinuity in group-level sales growth at this specific threshold which can only be due to country-by-country reporting.

In our main RDD specification, the outcome variable is yearly group-level consolidated sales growth in percentages in 2016–2021 relative to sales in 2015 (pre-country-by-country reporting). The running variable is consolidated revenue, with a sharp threshold at €750 million. Triangular kernel weighting is applied to give more weight to observations closer

transparency shift, spurred by, among others, LuxLeaks 2014 and Panama Papers 2016, which could raise detection risk generally, but our design leverages the country-by-country reporting-specific revenue threshold and pre-trends to separate this from slower, economy-wide shifts.

Table 4: Regression discontinuity estimates – sales growth

	(1)	(2)	(3)	(4)
	Sales growth	Sales growth	Sales growth	Sales growth
CbCR	-0.330*** (0.0178)	-0.145*** (0.0211)	-0.107*** (0.0334)	-0.0631 (0.0396)
Bandwidth	47.2	100	150	200
Country-industry-year FE	Yes	Yes	Yes	Yes
Observations	273	665	955	1,401

Notes: *** 1%, ** 5%, * 10%. Standard errors, in parentheses, are clustered on the group level. This table shows regression discontinuity estimates of the effect of CbCR on sales growth. Revenue in 2015 is the running variable with a sharp treatment discontinuity at €750 million, above which CbCR is mandatory. The outcome variable is sales growth relative to 2015. The local polynomial order is one (linear), triangular kernel weighting is used, and country-industry-year fixed effects are taken into account. In column 1, the mse-optimal bandwidth of €47.2 million is used (Cattaneo et al., 2019; Cattaneo and Titiunik, 2022). Columns 2–4 employ bandwidths of €100, €150, and €200 million respectively.

to the threshold, and a local linear polynomial is used since this minimizes specification bias if the bandwidth used is small enough. As in the difference-in-differences approach, we employ country-industry-year fixed effects to be able to compare sales growths of groups within the same country-industry at the same time. Standard errors are clustered on the group level to account for potential autocorrelation. Visual results are presented in four graphs in Figure 3, corresponding to four different bandwidths around the reporting threshold. The first threshold is the mean squared error-optimized threshold of €47.2 million following Cattaneo et al. (2019); Cattaneo and Titiunik (2022), see Panel A. In Panels B–D the same estimations are performed using hand-picked bandwidths of €100, €150, and €200 million around the threshold. All figures indicate a negative discontinuity in sales growths at the country-by-country reporting threshold, providing additional evidence for our argument that our findings are indeed driven by country-by-country reporting.

In Table 4 we present estimation results for our RDD, for the same four bandwidths. Point estimates show a negative discontinuity in sales growth at the country-by-country reporting threshold, statistically significant except for the largest bandwidth’s estimate which is just outside common significance levels ($p = 0.111$). Note that this methodology estimates a highly local effect, which is why the point estimates are relatively large (but converge towards more realistic effect sizes as the bandwidth increases). These are not generalizable however, and for more externally valid estimate sizes we keep referring to our difference-in-difference results.

In Table 5 we also provide the same estimations using effective tax rate differences with 2015 as the outcome variable. Point estimates for the optimal bandwidth and the

Table 5: Regression discontinuity estimates – ETR difference

	(1) ETR diff	(2) ETR diff	(3) ETR diff	(4) ETR diff
CbCR	0.0457*** (0.00941)	0.00749 (0.0112)	0.0217 (0.0152)	0.0566*** (0.0155)
Bandwidth	47.2	100	150	200
Country-industry-year FE	Yes	Yes	Yes	Yes
Observations	306	665	955	1,401

Notes: *** 1%, ** 5%, * 10%. Standard errors, in parentheses, are clustered on the group level. This table shows regression discontinuity estimates of the effect of CbCR on sales growth. Revenue in 2015 is the running variable with a sharp treatment discontinuity at €750 million, above which CbCR is mandatory. The outcome variable is the effective tax rate (ETR) difference relative to 2015. The local polynomial order is one (linear), triangular kernel weighting is used, and country-industry-year fixed effects are taken into account. In column (1), the mse-optimal bandwidth of €47.2 million is used (Cattaneo et al., 2019; Cattaneo and Titiunik, 2022). Columns (2)–(4) employ bandwidths of €100, €150, and €200 million respectively.

€200 million bandwidth show a positive discontinuity in the difference between post-country-by-country reporting effective tax rates and those in 2015, indicating a decrease in tax avoidance for multinationals affected by country-by-country reporting. The other two bandwidth also give positive point estimates, but are statistically not significant.

Taking the RDD results on sales growth and effective tax rate differences together, these findings provide further evidence for the argument that increased tax compliance due to country-by-country reporting drives our findings.

While local linear polynomial approximation is least biased for small RDD bandwidths, smoothing bias may get larger as bandwidth sizes increase (Cattaneo et al., 2019). As a robustness test, we thus perform the same RDD regressions but with local quadratic estimation. Results are presented in Table 7 in the Appendix, strengthening our evidence for a significant negative discontinuity in sales growth at the country-by-country reporting threshold for all four bandwidths. Another concern may be the bunching of firms just below the revenue threshold in order to avoid reporting obligations. Although most of the literature finds revenue manipulation implausible and most tests reject this hypothesis (Healy and Wahlen, 1999; Joshi, 2020; Tuinsma et al., 2023), Hugger (2024) does find evidence for bunching. A density test for treatment manipulation following McCrary (2008) and Cattaneo et al. (2018) does not provide a statistically significant indication for bunching below the threshold in our sample (Figure 5 in the Appendix). As another test for robustness, we use 2014 as the reference year to determine treatment status as well as the base for calculating sales growth. Since the country-by-country reporting revenue threshold was decided upon in 2015, anticipatory treatment manipulation in 2014 would not have been possible. Results for this estimation are given in Table 8 in the Appendix,

where negative estimates with strong statistical significance provide further robustness to our main RDD findings.

5.5 Robustness Tests

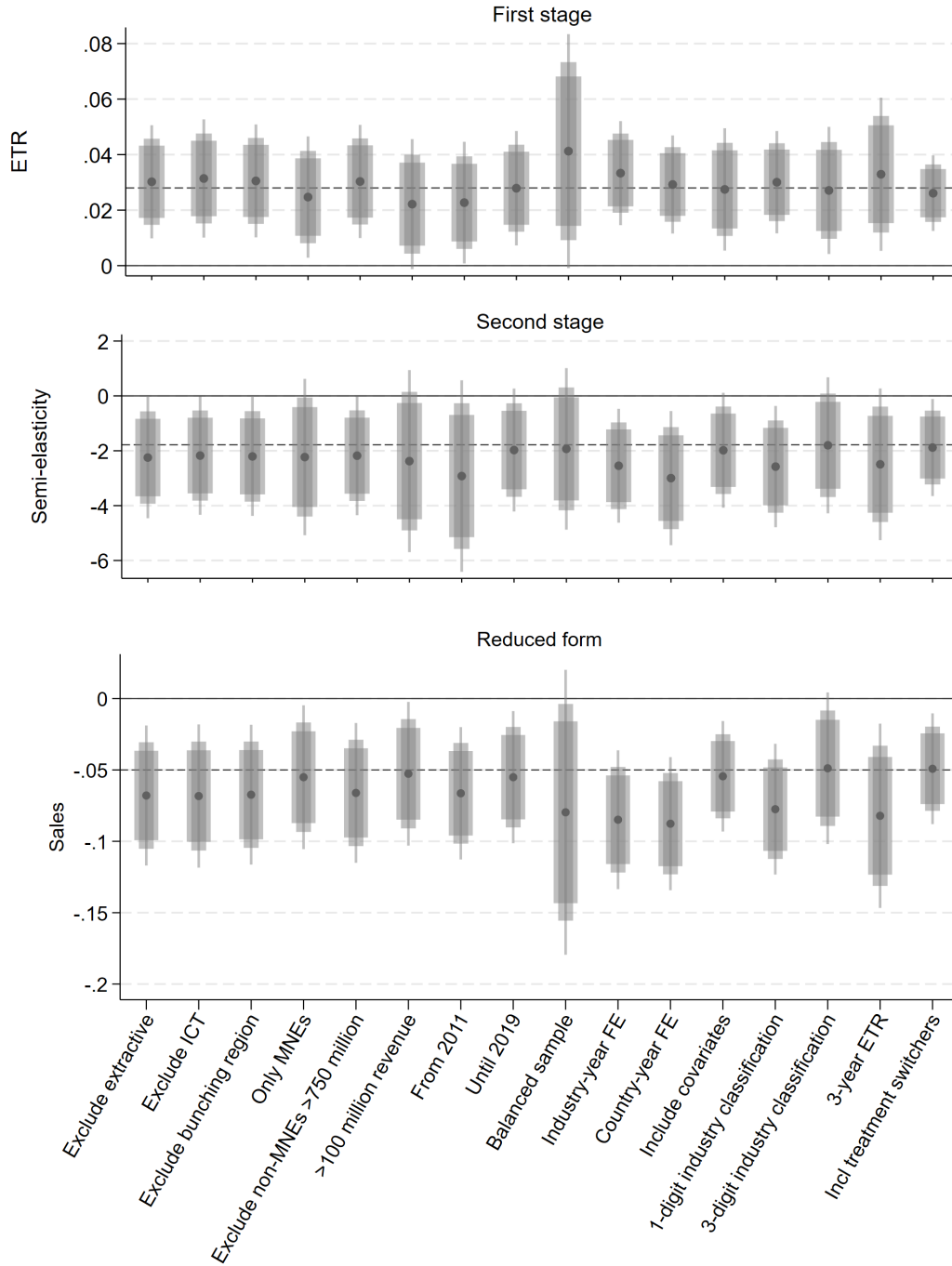
In this section we present a myriad of robustness tests, on all three analysis levels, to show that our main difference-in-differences results are robust to alternative methodological and sample selection choices.

On the group level, the results of these robustness tests for the first stage, the second stage, and the reduced form, are summarized in Figure 4. In almost all tests, the point estimates are similar to the baseline results (indicated by the dotted lines) and remain statistically significant. In the first test, we exclude the extractive industry since firms in this sector were already subject to a sector-specific country-by-country reporting regime which may confound our results (Johannesen and Larsen, 2016). The next test drops the ICT sector because of potential confounding effects of the introduction of GDPR, which especially affects the ICT sector (Frey and Presidente, 2024). Next, we drop firms that potentially manipulated their revenue to remain below the country-by-country reporting threshold and avoid the regulation. Although in Section 5.4 we do not find evidence for bunching below the threshold, conform findings of Joshi (2020) and Tuinsma et al. (2023), Hugger (2024) and De Simone and Olbert (2022) do find such evidence. Following Hugger (2024), we exclude firms in the bunching region of 90%-100% of the reporting threshold. Estimates are robust to these first tests and remain significant, with all point estimates slightly larger in size compared to our baseline estimates.

Next, we present results for three alternative sampling decisions. We exclude non-multinational firms, either from the full sample or only those exceeding the reporting threshold, and we exclude firms with a revenue below €100 million. Removing all non-multinationals improves the comparability of the treatment and control group at the cost of decreasing sample size and precision, resulting in a slightly smaller first-stage point estimate and a small drop in statistical significance for the second stage. Removing non-multinationals only when their revenue exceeds the reporting threshold similarly does not significantly differ from our main results. Excluding firms with a revenue below €100 million further improves comparability between the control and treatment group. Again, point estimates are robust, however the first stage estimate is slightly smaller compared to our baseline estimate and is significant at the 5% level. The second stage estimate is in this case only significant at the 10% level.

Since Figure 2 shows significant sales drops after 2008 due to the financial crisis and from 2020 due to the COVID-19 outbreak, we want to exclude the possibility that these crises affected the control and treatment group differently and bias our results. Hence,

Figure 4: Robustness tests – group level



Notes: these figures summarize the point estimates along with their confidence intervals at the 90%, 95%, and 99% level for our robustness tests at the group level. In the first stage, we estimate the effect of country-by-country reporting on effective tax rates. In the second stage, we estimate the effect of effective tax rates on sales. In the reduced form, we estimate the effect of country-by-country reporting on sales. In all three figures, the solid horizontal line indicates zero and the dashed horizontal line indicates our baseline estimates displayed in Table 2.

we exclude 2007–2010 and 2020–2021 from our sample in the following robustness tests.²⁸ Results are robust, although the second stage loses some statistical significance due to smaller sample size and lower precision.

Coverage of large firms in Orbis is generally good, but for small firms this is less the case (Bajgar et al., 2020). To alleviate concerns about the consistency of the treatment and control groups, we also perform our analyses on the balanced sample of firms for which all 15 years are observed. Although the results lose some statistical significance due to lower power, they are consistent with our baseline results and point estimates exceed our main estimates in size.

We also perform several robustness checks related to regression specifications and the definition of industries. First, we treat the EU as a single market, hence industries in which firms operate are only determined by their industry classification (country-industry-year fixed effects are dropped in favor of industry-year fixed effects). We also show results where country-year fixed effects are employed instead. Results and conclusions are robust to these specifications. We also add potential determinants of our outcomes and independent variables as covariates. Control variables included are size in terms of assets and employees (both in natural logarithms), return on assets, and leverage. Finally, we vary the granularity of our industry classification (1-digit and 3-digit), our effective tax rate definition (3-year average effective tax rate), and we include groups that switch treatment during our sample period. Results from these specifications confirm the robustness of our main findings.

On the subsidiary level, we perform several similar robustness tests. Their results are summarized in Figure 6 in the Appendix, showing estimates of a size and significance similar to our main subsidiary-level results. Our main result is robust to the exclusion of subsidiaries in the extractive sector (subject to another country-by-country reporting regime: Joshi et al. (2020)) or the ICT sector (most affected by GDPR: Frey and Presidente (2024)), those that are not owned by multinational firms, and to the exclusion of non-multinational owners with a revenue over the treatment threshold. Excluding self-owned subsidiaries increases our point estimate slightly but otherwise confirms the robustness of our main estimate, as do regressions using 1-digit or 3-digit industry classifications.

At the start of our sample, control subsidiaries may have been affected more by the financial crisis leading to statistically significant differences in sales with the treatment group in those years. Excluding the years until 2010 from the main difference-in-differences analysis does not significantly alter the estimate in column 4 of Table 2, see the result of this robustness test in Figure 6 in the Appendix. The event study estimates further show that subsidiaries of firms with country-by-country reporting obligations started losing sales relative to control subsidiaries immediately after the policy

²⁸The latter also takes care of concerns about potential confounding due to Brexit coming into effect.

was implemented. This effect appears to grow over time, but this effect size increase is coincident with the coronavirus pandemic which may confound this finding. Our main estimate is also robust to excluding the pandemic years from 2020 onward, although the effect size is slightly smaller and statistical significance is at the 5% level instead (see Figure 6 in the Appendix).

Finally, we provide results for our robustness tests on the industry level in Figure 8 in the Appendix. Excluding the extractive sector because of their separate country-by-country reporting regime or the ICT sector because of potential confounding due to the introduction of GDPR leads to estimates similar to our main industry-level results.

As our yearly event study estimates in Figure 7 in the Appendix present some potentially divergent trends before 2011, we remove these years from the estimation and show that our finding is robust to using a shorter pre-treatment period in which the parallel trends assumption more clearly holds. In an additional test, we drop 2020 due to potential distorting effects of the coronavirus pandemic, again confirming the robustness of our main results.

Unfortunately, we only have availability of Eurostat’s aggregate industry data on the two-digit level, so we cannot perform a robustness test using the three-digit industry level. The one-digit industry classification does not provide enough information to allow for meaningful estimation of concentration on this level. We do provide results of further robustness tests in Figure 8 in which we control for the size of industries and in which we use balanced samples. Our main industry-level estimates are robust to these specifications.

6 Conclusion

In this paper, we show that the 2016 introduction of country-by-country reporting decreased the sales of the largest multinationals in the EU by around 5%, relative to smaller business groups in the same country-industry. Our two-stage least squares analysis leads to a semi-elasticity indicating that a one percentage point increase in effective tax rates leads to a 1.8% decrease in sales. We also show that country-by-country reporting decreased the sales of subsidiaries of the affected multinationals, thereby showing that our estimate on consolidated group sales is not driven by divestment of subsidiaries but due to actual decline in size of the group’s subsidiaries. Finally, we provide evidence for a reduction in industry concentration in country-industries where a larger share of the top firms have the country-by-country reporting obligation. Industries in which the top eight firms had this obligation became 2.6 percentage points less concentrated relative to industries in which no firms had the reporting obligation. Measuring industry concentration as the sales share of the four largest firms, we estimate the effect at a decrease of 2 percentage points. Using the Herfindahl-Hirschman Index to measure concentration yields similar

results. As far as we are aware, these are the first estimates in the academic literature of the direct effect of multinational tax compliance on industry concentration.

Our findings suggest that, beyond boosting tax revenues, more effective corporate tax policy can have the additional benefits of levelling the playing field for competition among firms of different size and reducing industry concentration. The findings are particularly important in the context of ongoing debates about the causes and consequences of recent industry concentration trends, and in the context of recent advances in international corporate taxation, especially the publication of country-by-country reports in the EU from 2024 onwards and the implementation of a global minimum corporate tax rate of 15% implemented by over 135 countries at the same time.

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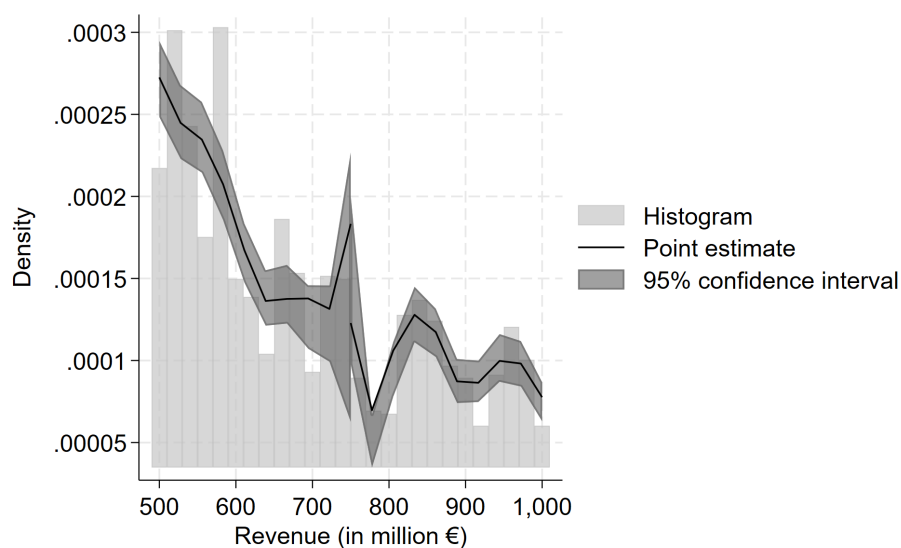
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Appendix

Figure 5: McCrary (2008) treatment manipulation test



Notes: this figure presents graphic results of the McCrary (2008) treatment manipulation test. A histogram of observations in the revenue range of €500 to €1,000 million is plotted, as well as the point estimates of the local polynomial density estimation and their 95% confidence intervals. The manipulation test value is $T = 0.0868$ with a p -value of $p = 0.9308$, rejecting the hypothesis of systemic manipulation of the treatment variable.

Table 6: Event study estimates – effective tax rate and sales

Variable Analysis level	(1) ETR Group	(2) log(Sales) Group	(3) log(Sales) Subsidiary
2007 $\times$ treatment	-0.017 (0.014)	-0.008 (0.028)	0.033*** (0.012)
2008 $\times$ treatment	-0.022 (0.015)	0.018 (0.027)	0.018* (0.010)
2009 $\times$ treatment	-0.005 (0.016)	0.010 (0.024)	0.007 (0.009)
2010 $\times$ treatment	-0.013 (0.013)	0.019 (0.023)	0.006 (0.008)
2011 $\times$ treatment	-0.002 (0.013)	0.009 (0.021)	0.008 (0.007)
2012 $\times$ treatment	-0.012 (0.012)	0.038** (0.019)	0.012* (0.007)
2013 $\times$ treatment	0.016 (0.012)	0.022 (0.015)	0.005 (0.006)
2014 $\times$ treatment	0.009 (0.012)	-0.007 (0.013)	-0.004 (0.005)
2016 $\times$ treatment	0.016 (0.011)	-0.021 (0.013)	-0.013*** (0.004)
2017 $\times$ treatment	0.015 (0.012)	-0.012 (0.016)	-0.005 (0.005)
2018 $\times$ treatment	0.034** (0.013)	-0.021 (0.018)	-0.015** (0.006)
2019 $\times$ treatment	0.025* (0.014)	-0.048** (0.021)	-0.007 (0.007)
2020 $\times$ treatment	0.043*** (0.015)	-0.067*** (0.022)	-0.029*** (0.007)
2021 $\times$ treatment	0.027* (0.015)	-0.100*** (0.026)	-0.035*** (0.009)
Observations	164,209	164,209	707,658
Adjusted R^2	0.398	0.953	0.939
Country $\times$ industry $\times$ year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Notes: *** 1%, ** 5%, * 10%. Standard errors, in parentheses, are clustered on the group level (columns 1–2) or the subsidiary level (column 3). This table summarizes the yearly differences between the control group and the treatment group in consolidated effective tax rate (column 1), consolidated sales (column 2), and unconsolidated sales (column 3), relative to the 2015 baseline. Industry classification is at the 2-digit level.

Table 7: Regression discontinuity estimates (local quadratic polynomial) – sales growth

	(1)	(2)	(3)	(4)
Local polynomial order	Sales growth 2	Sales growth 2	Sales growth 2	Sales growth 2
CbCR	-0.645*** (0.0160)	-0.356*** (0.0234)	-0.141*** (0.0417)	-0.118** (0.0562)
Bandwidth	47.2	100	150	200
Country-industry-year FE	Yes	Yes	Yes	Yes
Observations	273	665	955	1,401

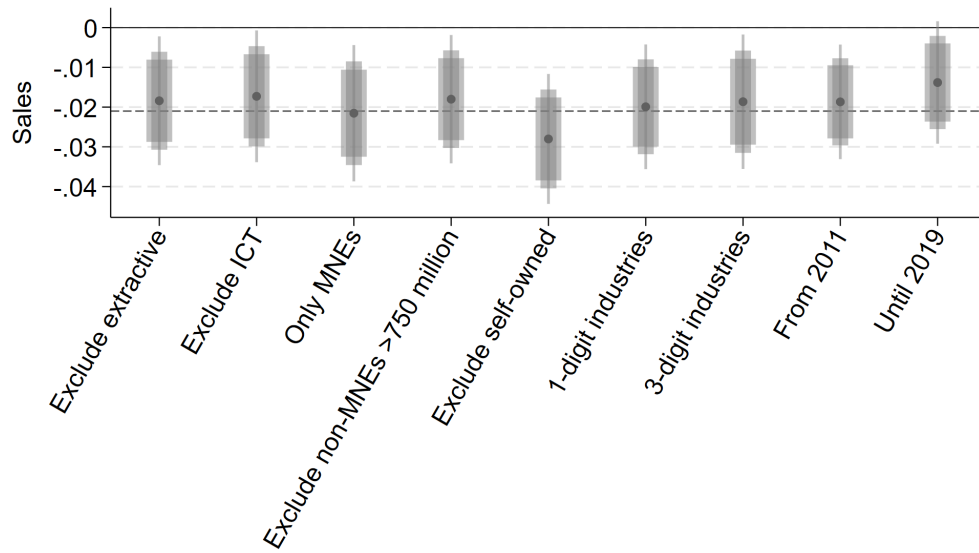
Notes: *** 1%, ** 5%, * 10%. Standard errors, in parentheses, are clustered on the group level. This table presents regression discontinuity estimates of the effect of CbCR on sales growth. Revenue in 2015 is the running variable with a sharp treatment discontinuity at €750 million, above which CbCR is mandatory. The outcome variable is sales growth relative to 2015. The local polynomial order is two (quadratic), triangular kernel weighting is used, and country-industry-year fixed effects are taken into account. In column 1, the original mse-optimal bandwidth of €47.2 million of the specification of Table 4 is used (Cattaneo et al., 2019; Cattaneo and Titiunik, 2022). Columns 2–4 employ bandwidths of €100, €150, and €200 million respectively.

Table 8: Regression discontinuity estimates – sales growth (relative to 2014)

	(1)	(2)	(3)	(4)
	Sales growth	Sales growth	Sales growth	Sales growth
CbCR	-0.369*** (0.0651)	-0.167*** (0.0509)	-0.179*** (0.0449)	-0.157*** (0.0398)
Bandwidth	47.2	100	150	200
Country-industry-year FE	Yes	Yes	Yes	Yes
Observations	257	547	840	1,230

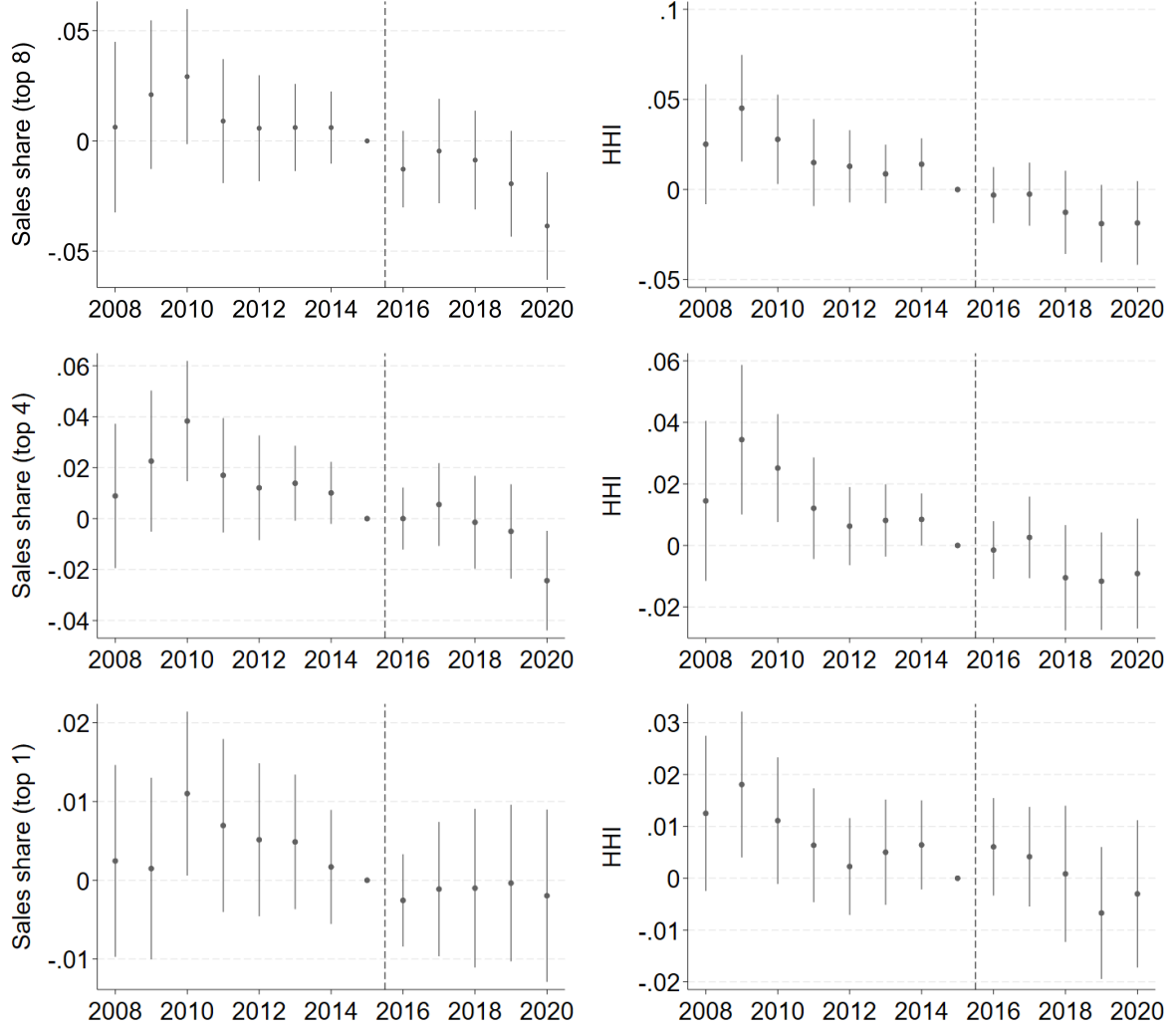
Notes: *** 1%, ** 5%, * 10%. Standard errors, in parentheses, are clustered on the group level. This table presents regression discontinuity estimates of the effect of CbCR on sales growth. Revenue in 2014 is the running variable with a sharp treatment discontinuity at €750 million, above which CbCR is mandatory. The outcome variable is sales growth relative to 2014. The local polynomial order is one (linear), triangular kernel weighting is used, and country-industry-year fixed effects are taken into account. In column 1, the original mse-optimal bandwidth of €47.2 million of the specification of Table 4 is used (Cattaneo et al., 2019; Cattaneo and Titiunik, 2022). Columns 2–4 employ bandwidths of €100, €150, and €200 million respectively.

Figure 6: Robustness tests – subsidiary level



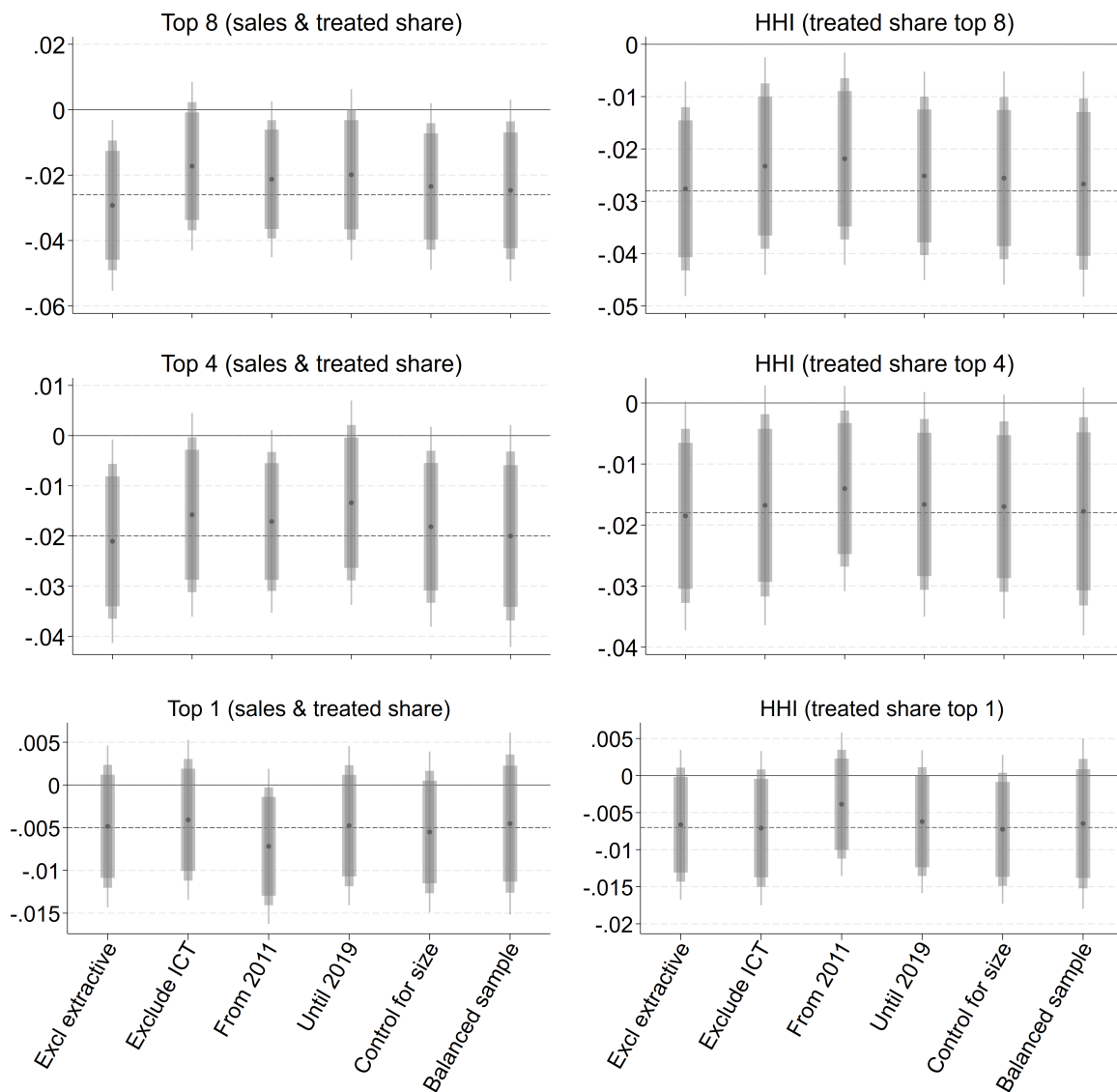
Notes: this figure summarizes the point estimates of the effect of country-by-country reporting on consolidated sales along with their confidence intervals at the 90%, 95%, and 99% level for our robustness tests at the subsidiary level. The solid horizontal line indicates zero and the dashed horizontal line indicates our main estimate in Table 2.

Figure 7: Event study estimates – industry level



Notes: these figures show event study estimates of the effect of country-by-country reporting, with treatment defined as the treated share of the top 8 (top two graphs), top 4 (middle two graphs), or top 1 firm (bottom two graphs), on industry concentration, measured as the sales share of the top 8/4/1 firms (graphs on the left) or with the Herfindahl-Hirschman Index (HHI, graphs on the right). Standard errors are clustered on the country-industry level. Small country-industries with aggregated sales under €500 million are excluded. Country-industries which include only one firm, for which the sales share and HHI equal one by definition, are excluded.

Figure 8: Robustness tests – industry level



Notes: these figures summarize the point estimates along with their confidence intervals at the 90%, 95%, and 99% level for our robustness tests at the industry level for the sales shares of the top 8, the top 4, and the top 1 largest firms (graphs on the left) and the HHI (graphs on the right, for the three different treatment share choices). Size is taken as the natural logarithm of aggregate sales in the industry. In all figures, the solid horizontal line indicates zero and the dashed horizontal line indicates our main estimates in Table 3.