# Real Effects of Private Country-by-Country Disclosure

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# Real Effects of Private Country-by-Country Disclosure


## Abstract

We investigate the effects of mandatory private Country-by-Country Reporting (CbCR) to European tax authorities on multinational firms' capital and labor investments as well as their organizational structures. We exploit the threshold-based application of this 2016 disclosure rule to conduct difference-in-differences and regression discontinuity tests. We document increases in capital and labor expenditures in Europe, but these effects are more pronounced in countries with preferential tax regimes. Cross-sectional tests and analysis using consolidated financial data provide evidence consistent with multinational firms reallocating capital across Europe to mitigate increased tax enforcement risk, as well as with CbCR hindering capital investment efficiency. We also find evidence consistent with firms responding to CbCR by reducing organizational complexity. Collectively, our results support the conclusion that mandatory private CbCR causes firms to change real investment activities to substantiate their tax avoidance activities in Europe while reducing the appearance of aggressive tax practices.



**Keywords:** Real Effects, Disclosure Regulation, Private Disclosure, Mandatory Disclosure, Country-by-Country Reporting, Tax Transparency, Tax Avoidance, Tax Havens, Organizational Complexity

**JEL Classifications:** H20, H25, H26, H32, K22, L51, M41, M48, O47

## I. INTRODUCTION

There is widespread interest in the effectiveness and consequences of regulations stipulating the amount, content, and format of disclosure. However, there is limited evidence on the real effects of disclosure, particularly regarding *private* (i.e., confidential) disclosures made to regulators but not made available to the public (Leuz and Wysocki, 2016; Roychowdhury et al., 2019). We begin to fill that void by examining real investment and organizational effects of private Country-by-Country reporting (CbCR) disclosures to tax authorities. In Europe, regulations effective in 2016 require multinational entities (MNEs) active in the European Union (EU) to disclose privately to tax authorities subsidiary ownership and economic activity by jurisdiction. Although the EU adopted CbCR to increase transparency and curb perceived harmful tax practices of MNEs, the response by MNEs can extend beyond observed changes in effective tax rates and tax avoidance. We study the effects of mandatory private CbCR disclosure on affected MNEs' disclosed tangible investment, employment, and organizational structures.

The EU adopted CbCR to provide tax authorities with indicators of country-level economic activity for every tax jurisdiction MNEs operate in (OECD, 2013; OECD, 2015a; Financial Times, 2016). Although tax authorities previously received corporate tax return information about operations within their own jurisdiction, they had only limited visibility outside their jurisdiction.[1] Further, some information required by the reports was typically not available at all (OECD, 2015a). Starting in 2016, MNEs with a parent or subsidiary in the EU and consolidated revenues greater than €750 million were required to report aggregated country-level financial information to tax

[1] Separate entity financial reports and beneficial ownership information are publicly available, but aggregating these data by MNE and country is costly. As summarized by the OECD, "This [CbCR] information has never previously been available to tax authorities and represents a great opportunity for tax authorities to understand the structure of a group's business in a way that has not been possible before" (OECD, 2017a).

authorities annually, including a list of all subsidiaries, revenues, pre-tax profits, cash income taxes paid, number of employees, and tangible assets (Council of the European Union, 2016).

CbCR disclosures could alter corporate decisions if firms believe the reports contain new, proprietary information to recipients (e.g., Kanodia and Sapra, 2016). If CbCR disclosures are informative to tax authorities, reporting firms face increased tax enforcement risk in any country in which the MNE operates. Even if the reports are not informative, MNEs could alter their behavior if they believe the information will lead to increased enforcement. MNEs could respond to CbCR by reducing tax avoidance, consistent with evidence of higher effective tax rates and reduced cross-jurisdictional income shifting among treated firms (Joshi, 2020; Hugger, 2020). Higher overall tax burdens resulting from reduced tax avoidance could lead to reduced investments in all EU jurisdictions (Hines and Rice, 1994; Suarez Serrato, 2019; Jacob, 2021).

Alternatively, MNEs could respond to CbCR by making investments that better align their taxation with economic activity, as that is a central policy goal of CbCR (OECD, 2015a,b). Increased investments in jurisdictions offering preferential tax regimes could better substantiate profits shifted to these jurisdictions (Grubert and Slemrod, 1998; Hanlon, 2018), especially in the presence of informed tax authorities from other jurisdictions newly able to observe discrepancies across countries in the ratio of profits to real activities reported by an MNE. MNEs could also liquidate obsolete entities and close tax haven subsidiaries, which are likely to be (at least perceived as) lacking economic substance (Dyreng et al., 2020). Because European countries have different strategies for taxing MNEs, as evidenced by their diverging views on tax transparency, we study tangible investment, employment, and organizational complexity in European countries offering preferential tax regimes versus all other European countries.[2]

[2] For example, Ireland, Malta, and Cyprus opposed making CbCR disclosures public (e.g., Kirwin, 2019).

Our empirical approach leverages both a difference-in-differences (DiD) and a regression discontinuity (RD) design. The CbCR mandate's size-based threshold of €750 million was chosen without considering characteristics specific to European firms and is thus unlikely to be associated with their tax avoidance or investment decisions. Further, the historical lack of corporate tax transparency suggests (i) the CbCR intervention could be sufficient to generate an economically meaningful corporate response, and (ii) our tests are unlikely to be contaminated by voluntary tax disclosures, either to tax authorities or to other corporate stakeholders. We combine several sources of detailed financial statement and ownership data from 2012 to 2018 on MNEs operating in the EU from Bureau van Dijk (BvD). We also exploit extensive BvD annual ownership data for legal entities worldwide to construct several organizational structure measures.[3]

We examine capital and labor investments using unconsolidated financial accounts of subsidiaries in Europe, whose tax authorities are the primary recipients of the private CbCR disclosures. Our DiD evidence is consistent with subsidiaries of affected MNEs increasing their economic activity in European jurisdictions blacklisted for having preferential tax regimes: Switzerland, Cyprus, Ireland, Luxembourg, Malta, and the Netherlands. Investments in tangible assets as a percentage of total assets for subsidiaries of CbCR MNEs in these countries increase 1.9-2.2 percentage points more after CbCR relative to subsidiaries of MNEs below the threshold. Similarly, CbCR MNEs' investments in labor expense in subsidiaries in European countries with preferential tax regimes increases 5.2 percentage points more after CbCR relative to MNEs below the threshold. These estimates imply differential subsidiary-level investments of €13.9-16.1 million in tangible fixed assets and of €4.6 million in labor expenses by subsidiaries of CbC firms

[3] We refer to the multinational group as the "MNE" or "firm" and its majority-owned entities as the "subsidiaries."

following CbCR. In contrast, we find smaller differential investments in other European countries without preferential tax regimes.

We conduct cross-sectional tests to support the validity of our main results. Because one explanation for our results is that MNEs may be shifting asset and labor investments to European countries with preferential tax regimes from other countries within Europe, we exploit variation in exit taxes imposed on MNEs by these other European countries. Consistent with expectations, we find qualitatively larger increases in capital and labor investments in countries with preferential tax regimes in the sub-sample of firms with greater exposure to exit taxes, relative to other firms. We also exploit variation in MNE tax avoidance before the CbCR mandate. Consistent with tax enforcement risk after CbCR likely increasing the most for firms with high ex-ante tax avoidance, we estimate larger capital and labor investment effects in the sub-samples exhibiting greater ex-ante tax avoidance. Collectively, these results suggest those MNEs anticipating greater risks related to increased tax enforcement substantiate their tax avoidance the most by locating real functions in countries offering preferential tax regimes.

To further validate our main inferences, we study capital and labor investments after aggregating data across all MNE subsidiaries in the same country. This aggregated approach is unaffected by restructurings that could induce large balance sheet changes in individual subsidiaries and thus contaminate the DiD results. It also allows us to more explicitly proxy for the aggregated firm-country amounts newly reported in the CbCR disclosures. We employ a local RD design to mitigate concerns about systematic differences between MNEs well above versus below the CbCR threshold. These results support that CbCR causes affected firms to increase capital and labor investments primarily in jurisdictions with preferential tax regimes. In additional RD tests using consolidated financial statement data, we fail to reject the null hypothesis of no

change in the level of total MNE investment. These results are consistent with our observed increases in capital and labor investments in European countries with preferential tax regimes coinciding with decreases in capital and labor investments in other countries. We further find MNEs affected by CbCR exhibit a lower sensitivity of capital – but not labor – investment to investment opportunities. These results suggest tax-induced capital reallocation decisions are not aligned with global investment opportunities, highlighting a potential cost of CbCR.

Because CbCR newly provides information on MNEs' organizational charts to many tax authorities, we also examine the effects of CbCR on organizational structures. Although MNEs can use complex corporate structures to facilitate tax avoidance, several studies suggest non-tax motivations (Dyreng et al., 2015; Bennedsen and Zeume 2018; Balakrishnan et al. 2019). Adjustments to organizational structures are an important real effect because organizational complexity is associated with control weaknesses, costly governance responses, and lower transparency (Bushman et al., 2004; Ashbaugh-Skaife et al., 2007; Balakrishnan et al., 2019). We find evidence consistent with MNEs reducing their number of subsidiaries in European countries with preferential tax regimes, in tax havens worldwide, across all countries worldwide, and particularly at lower hierarchical levels. MNEs thus reduce organizational complexity by at least "winding down" obsolete entities worldwide. These results suggest that CbCR firms aim to avoid the appearance of being overly tax aggressive in tax audits.

Collectively, our results suggest that in response to CbCR MNEs increase capital and labor investments European countries that offer preferential tax regimes. Although not likely the intent of CbCR, these results are consistent with ex-ante survey evidence of MNE reactions to CbCR (Thomson Reuters, 2017) as well as a spike in corporate tax revenues following CbCR in Ireland, one of the European countries with preferential tax regimes (Campbell, 2021). Several tests

validate identifying assumptions and the robustness of results, including addressing potential manipulation of treatment status, evaluating the parallel trends assumption, testing for covariate balance, conducting falsification tests around placebo reporting thresholds and event years, varying controls and corrections for correlations in error terms, using alternative RD estimation procedures, and evaluating anticipatory effects (e.g., Lee and Lemieux, 2010; Atanasov and Black 2016; Cattaneo et al., 2018). Several tax enforcement and whistleblowing events that may have disproportionately impacted firms well above the CbCR threshold occurred around this time (e.g., the European Commission's investigations into illegal state aid (Fox et al., 2021), the OECD/G20's Base Erosion and Profits Shifting (BEPS) initiative, and leaks about Luxembourg's corporate tax arrangements). While our RD results lend validity to our inferences because these events should not differentially apply to firms just above versus below the CbCR threshold, these events could contribute to our estimated magnitudes. Supplemental tests suggest our results are not driven by firms facing the greatest reputational costs, confirming private mandatory disclosures explain these real effects. However, a limitation of our setting is that firms are unlikely to disclose the mandated information to other stakeholders, so our results may not generalize to settings like mandatory public corporate social responsibility or financial reporting disclosures.

Our study contributes to the nascent literature on the real effects of disclosure (e.g., Christensen et al., 2017; Chen et al., 2018; Rauter, 2020) and responds to the call for research on the effects of tax disclosures on firm decisions (Dyreng and Maydew, 2018). Concurrent work finds higher corporate effective tax rates and some evidence of less income shifting among MNEs impacted by CbCR (Joshi 2020; Hugger 2020). However, MNEs could increase effective tax rates and reduce income shifting by altering their transfer prices (i.e., accounting manipulations) with little to no changes to their global economic footprint (i.e., the location of real factors of

production) (Beer, de Mooij, and Li, 2020). We extend this literature by examining real changes in capital investments, employment, investment efficiency, and organizational complexity, as well as by documenting heterogeneous effects across country-level regulations and MNE tax avoidance. Because tax policy often aims to incentivize economic growth of real factors of production, capital and labor investments are first-order outcomes for policymakers. Our estimated effect sizes of approximately 2 percent of total assets and 5 percent of labor expense fall within the range of other estimated impacts of mandatory reporting in the tax and CbCR settings, such as a reduction in financial reporting reserves for unrecognized tax benefits of 1 percent of pre-tax income (Towery, 2017), a reduction in tax avoidance of 5-9 percent of pre-tax income (Joshi, 2020), a delay in capital investments of 3.5 percent of the average amount of time between investment spikes (Jacob et al., 2021), and a reduction in capital expenditures of 7.1 percent of total assets (Rauter, 2020).

We also contribute to the tax avoidance literature by leveraging a unique setting to examine the role of the tax authority as a monitor of the firm. Our findings suggest increased monitoring has heterogenous effects across countries, potentially creating relative winners and losers of capital and labor investments resulting from increased transparency. We document potentially unexpected effects of private disclosures to tax authorities: more transparency for all EU tax authorities has positive capital allocation effects only for those countries with preferential regimes. We thus complement prior work suggesting that the tax authority may be a powerful monitor (e.g., Hoopes et al., 2012). This interpretation is consistent with evidence from the U.S. that private tax disclosures can be incrementally informative (Bozanic et al., 2017). Our study is also consistent with private tax disclosures having (potentially unintended) consequences with respect to both disclosed and undisclosed information, such as reduced magnitudes of unrecognized tax benefits

and capital investments after rules requiring firms to privately disclose uncertain tax positions to U.S. tax authorities on Schedule UTP (Towery, 2017; Jacob et al., 2021). At a broader level, our insights inform the effectiveness of disclosure regulation aimed at mitigating information asymmetry between MNEs and stakeholders (Roychowdhury et al., 2019).

Our study should be of interest to tax authorities and policymakers worldwide for several reasons. First, our examination of real effects suggests additional mandates to increase private reporting, such as for intermediaries of cross-border transactions effective July 2020, might have real consequences. Second, making some CbCR disclosures public, for example as proposed by the Global Reporting Initiative (GRI, 2019) or agreed by the Council of the EU in June 2021 (Schoen, 2021), is not required to elicit a corporate response and may even exacerbate our documented effects. Third, our results suggest transparency initiatives could worsen the "race to the bottom" by pressuring jurisdictions with less preferential tax regimes to reduce their tax rates to mitigate a shift of economic activity to jurisdictions with attractive tax rules following increased transparency. As a result, there could be greater motivation for global coordination on a minimum tax rate or formulary approach, such as those backed by the world's richest countries (Milliken and Holton, 2021; OECD/G20 2021).

## II. INSTITUTIONAL BACKGROUND AND RELATED LITERATURE

### Country-by-Country Disclosures

CbCR disclosures to tax authorities arose as a recommendation from the OECD/G20's BEPS initiative. The initiative is a multinational approach to combatting perceived harmful tax practices of MNEs and formally started with a first report in 2013. The BEPS efforts divided research and recommendations into 15 separate topics, with Action 13 devoted to transfer pricing documentation and CbCR. The EU formally adopted the BEPS CbCR recommendations on May 25, 2016, with an effective date of January 1, 2016. Worldwide, some 73 countries had adopted

some form of CbCR as of 2019 (Spengel et al., 2019). The OECD's Multilateral Competent Authority Agreement provides an automatic exchange mechanism for tax authorities to share these reports across jurisdictions, such that CbCR disclosures could be shared with tax authorities in all countries in which a firm operates.

Within the EU, the CbCR requirement applies to MNEs whose parent firm is a tax resident of an EU country or whose subsidiaries are incorporated in the EU. Additionally, MNE consolidated revenues must exceed €750 million in the fiscal year preceding the reporting year, with the first reporting year being 2016 (Council of the European Union, 2016).[4] MNEs meeting these criteria must report to the parent country's tax authority indicators of country-level economic activity by each tax jurisdiction: unrelated party revenues, related party revenues, total revenues, profit before income tax, income tax paid (on a cash basis), current year income tax accrued, stated capital, accumulated earnings, number of employees, tangible assets other than cash and cash equivalents. Reporting firms must also list each affiliated legal entity by tax resident jurisdiction, its country of incorporation (if different from the tax residence), and its main business activities. Figure 1 depicts the template for a CbCR disclosure published in European Council Directive 2016/881/EU. The EU directive allows firms to use their consolidated and unconsolidated financial accounting (book) numbers as well as managerial accounting systems as data sources.

CbCR could provide incremental information to tax authorities. Pre-CbCR disclosures to tax authorities only typically include information on legal entities (i.e., taxpayers) located in the tax authority's own jurisdiction and the foreign entities that engage in intrafirm transactions with

[4] U.S. MNEs with a subsidiary in the EU and consolidated revenues above the threshold are required to disclose CbCR to the EU in 2016, which is also the first year U.S. MNEs could voluntarily disclose CbCR to the U.S. tax authority (but before a U.S. mandate). The EU adopted the OECD's recommended revenue threshold, which was intended to maximize the share of corporate revenues subject to CbCR while limiting the share of MNEs required to report (OECD, 2015a).

them. CbCR expands this information set to newly provide local authorities with country-level information on all of the MNE's operations in foreign countries. Similarly, tax authorities outside the parent home country also may newly receive information about the MNE's global activities via information exchange. Proponents of CbCR argue these increased disclosures allow tax authorities to better evaluate an MNE's international tax arrangements and identify profit shifting and base erosion, potentially increasing enforcement effectiveness (Schoen, 2021). Scrutiny could increase even if MNEs comply with tax regulations because many transactions fall in the grey area between explicitly legal versus illegal tax avoidance (OECD, 2017b).[5]

On the other hand, tax authorities may already have obtained separate entity financials for affiliates operating within the EU due to relatively expansive public reporting requirements.[6] Further, critics argue CbCR may not help tax authorities assess transfer pricing arrangements but instead will lead to a disconnect between taxation and transfer pricing rules, which do not strictly tie the location of reported profits to the location of economic activities, in favor of formulary apportionment (Hanlon, 2018; Spengel, 2018). Another possible consequence of CbCR is increased double taxation resulting from the potentially increased enforcement of multiple tax authorities in different countries.

**Related Literature**

[5] The reporting requirement introduces compliance costs and the risk of divulging proprietary information (Hoopes et al., 2018). Further, LuxLeaks and other recent whistleblowing events suggest the possibility CbCR information could be leaked to the public (Hanlon, 2018; Huesecken et al., 2018; O'Donovan et al., 2019). Thus, CbCR could have real effects through managers' rational anticipation of reputational costs and consumer backlash. In untabulated tests, we find results are generally robust to excluding observations from firms that are publicly listed, own widely recognized brands, or are subject to additional public scrutiny as a result of being named in the LuxLeaks whistleblowing event. Similarly, Joshi (2020) fails to find evidence supporting that MNEs responded to the fear CbCR disclosures would be leaked publicly. Nonetheless, in all tests we either include controls for public listing status and ownership of valuable consumer-facing brands or (because these characteristics are generally time-invariant) MNE fixed effects so that estimated treatment effects are incremental to the effects of reputational costs.

[6] These reports exclude partnerships and hybrid companies (often used in more aggressive tax planning strategies) and subsidiaries outside the EU. In some European jurisdictions, firms must inform tax authorities in the year they acquire or establish a foreign corporation, but updates on the owned foreign subsidiary are not required.

Prior research generally provides evidence that public tax disclosures create incentives for firms to reduce behavior that could give rise to tax risk. Dyreng et al. (2016) examine newly mandated public subsidiary disclosures in the U.K. They find that many firms were initially non-compliant with the disclosure rule. When their non-compliance gained public attention, these originally non-compliant firms exhibited a larger decrease in tax haven subsidiaries in the years following compliance relative to other firms.

Focusing on market reactions to public tax disclosures, Hoopes et al. (2018) find negative reactions around Australian legislation mandating tax disclosures for firms that paid no tax in Australia, and Johannesen and Larsen (2016) find negative reactions for oil and gas firms to an EU law requiring public disclosures of payments to governments.[7] Collectively, these studies suggest that investors respond to increased public disclosures. However, Dutt et al. (2019) do not document significant market reactions after the introduction of public CbCR disclosures in the EU banking sector. Our setting deviates from this literature by examining the effects of disclosures only reported to regulating tax authorities.

Two related papers study the private CbCR setting and find higher effective tax rates (Joshi, 2020) but no change in tax payments for affected firms (Hugger, 2020). Both papers also provide limited evidence that MNEs reduced tax-motivated profit shifting following CbCR. We extend this literature by examining whether these effects of private CbCR disclosures also reflect changes in the factors of production. Evaluating the capital and labor investment effects of CbCR is

[7] A separate literature examines the reporting effects of public tax disclosures, generally finding limited compliance with required tax disclosures (e.g., Belnap, 2019) and a positive association between tax avoidance and the aggregation of financial reporting by segment (Brown et al., 2019) or geography (Hope et al., 2013; Akamah et al., 2018). Overesch and Wolff (2021) and Joshi et al. (2020) study multinational banks' responses to public CbCR and document mixed results, overall suggesting a modest mitigating effect of public CbCR on banks' international corporate tax avoidance.

important for understanding the economic growth implications of transparency initiatives worldwide.

## III. METHODOLOGY AND DATA

### Difference-in-Differences Design

Our first research design compares changes over time in economic activity (first difference) between MNEs affected by the CbCR mandate and those unaffected by the mandate (second difference). We use MNEs' individual subsidiaries as the units of observation to circumvent mechanical concerns of comparing consolidated firm-country outcomes of larger (treatment) firms to smaller (control) firms. We focus these tests on subsidiaries in Europe, where the CbCR mandate was introduced and unconsolidated financial statement information is widely required by statutory requirements. We are unable to examine the economic activities of subsidiaries outside Europe due to relatively incomplete unconsolidated financial statement information.

We present results across three groups of countries. First, we examine all European countries (full sample). Second, we separately examine the sub-sample of European countries with preferential tax regimes. We identify these regimes using a blacklist produced by the Tax Justice Network – an independent, non-governmental organization – by strictly applying the EU's published tax haven blacklist criteria for identifying non-cooperative regimes (Lips and Cobham, 2017). These criteria consider perceived harmful and abusive tax practices beyond tax rates, such as a lack of tax transparency or information sharing, not implementing the minimum BEPS recommendations, and facilitating offshore structures aimed at attracting profits without real economic activity (Council of the European Union, 2016). The preferential tax regimes of these countries – Switzerland, Cyprus, Ireland, Luxembourg, Malta, and the Netherlands – are considered to facilitate aggressive tax planning. Finally, for completeness, we separately examine the sub-sample of all other European countries.

We exploit time series data from 2012 to 2018. The post-CbCR period is three years, which is a reasonable window over which to expect to see real investment responses (e.g., McNichols and Stubben, 2008; Badertscher, Shroff, and White, 2013; Suarez Serrato and Zidar, 2016; Zwick and Mahon, 2017; Lester, 2019). We test several specifications based on the following model.

$$Outcome_{i,t} = \gamma Treated_f * Post_t + YearFE_t + FirmFE_f + \varepsilon_{i,t} \quad (1)$$

We define all variables in Appendix A. $Outcome_{i,t}$ is the outcome of interest of subsidiary *i* in year *t*. We focus our outcomes on measures typically studied as proxies for investment that are also choice variables reported in the CbCR disclosures: fixed tangible assets and labor expenses.[8] Consistent with the literature examining investment, we construct a flow measure approximating capital expenditures.[9] *Capital Investment*$_{i,t}$ *(%)* is the year-on-year change in unconsolidated subsidiary tangible fixed assets scaled by lagged total assets. Similarly, *Labor Investment*$_{i,t}$ *(%)* is the change in unconsolidated subsidiary labor expense scaled by lagged labor expense. $Treated_f$ is an indicator variable equal to one if the subsidiary belongs to a firm that reported consolidated revenues of at least €750 million in fiscal year 2016, and zero otherwise. $Post_t$ is an indicator variable taking on the value of one for the years 2016 to 2018. Firm (i.e., parent or MNE) fixed effects ($FirmFE_f$) control for time-invariant characteristics such as home country, industry, size, reputational costs, and the MNE's propensity to engage in tax avoidance; these MNE fixed effects absorb the baseline effect of $Treated_f$. Year fixed effects ($YearFE_t$) control for macroeconomic trends; these year fixed effects absorb the baseline effect of $Post_t$. In additional specifications, we

[8] Our analyses assume that numbers reported in the separate entity financial statements collected by BvD reflect real activities, consistent with other studies (e.g., Huizinga and Laeven, 2008; Badertscher et al., 2013; Shroff et al., 2014; Markle, 2016; De Simone, 2016; De Simone et al., 2017; Joshi, 2020). Fuest et al. (2021) compare figures from BvD to those reported on CbCR disclosures, providing evidence that these financial figures reflect real activities reported in CbCR disclosures.

[9] There is no separate entity public reporting requirement for capital expenditures. We follow the literature to construct an approximation of capital expenditures (e.g., Asker, Farre-Mensa, and Ljungqvist. 2015; Badertscher, Shroff, and White, 2013; Shroff, Verdi, and Yu, 2014; Bethmann et al. 2018; Jacob et al. 2021).

add (i) time-varying MNE controls for consolidated total assets, number of employees, leverage, cash, return on assets, intangibles, and tax avoidance of firm *f* in year *t*; (ii) time-varying subsidiary controls for total assets, number of employees, and leverage; (iii) time-varying country controls for GDP and the corporate income tax rate of firm *f;* and (iv) country-year fixed effects for subsidiary country *c* and year *t* to control for country-specific macroeconomics or unrelated changes in regulations or enforcement.[10]

**Regression Discontinuity Design**

MNEs might respond to CbCR by changing their organizational structure, potentially introducing noise and downward bias to tests using individual subsidiaries as the unit of observation such that estimated magnitudes from those tests may be understated. We therefore also examine capital and labor investment outcomes after aggregating unconsolidated financial statement data across all subsidiaries in the same country and same MNE. By doing so, we capture an MNE's total economic activity in a given country and thus more explicitly proxy for amounts reported in the new CbCR disclosures. We also examine firms' investment outcomes using their (worldwide) consolidated financial statement information. Finally, we examine organizational structures to test our prediction that firms aim to reduce the appearance of being overly tax aggressive. However, in our setting, where treatment is based on firm size, moving closer towards the firm (MNE) as the unit of observation introduces concerns about systematic differences across very large MNEs versus small MNEs in a DiD design. We thus exploit a sharp regression discontinuity design around the €750 million revenue threshold for CbCR.

[10] Country-year fixed effects subsume country fixed effects in this specification. Because time-varying firm characteristics may also be impacted by treatment, thus introducing bias (Angrist and Pischke 2009), we start by presenting specifications that do not include these controls.

Because the revenue threshold determines CbCR treatment, consolidated revenues are the "running" variable in our RD tests. We employ a non-parametric estimation method that implicitly assumes that the characteristics of treated firms are the same just below and above the threshold. Untabulated tests provide evidence supporting a quasi-random treatment of firms with respect to the CbCR mandate, such that the RD design identifies plausibly causal treatment effects by comparing outcomes of control firms just below the threshold with outcomes of treated firms just above the threshold (Lee and Lemieux, 2010; Cattaneo et al., 2018).[11]

We follow several recommendations from the literature when designing our RD tests (e.g., Imbens and Lemieux, 2008; Cattaneo et al. 2018). In particular, we run both local linear and third-order polynomial regressions as the functional form of the relation between the running variable and the outcome variable is important for generating an unbiased estimate of the treatment effect. We select optimal bandwidths that minimize the mean squared error according to Imbens and Kalyanaraman (2012). We use a triangular kernel function to construct the local estimators, such that observations closer to the threshold receive greater weight (Lee and Lemieux, 2010), and symmetric bandwidths above versus below the threshold. We test several specifications based on the following model.

$$Outcome_{f,(c),t} = \alpha\, Treated_f + \sum_{p=1}^{P} \beta_p RV_f^j + \sum_{p=1}^{P} \gamma_p RV_f^j * Treated_f \quad (2)$$

[11] As the EU-wide mandate of CbCR was released May 2016 (i.e., before fiscal year end 2016), we test for revenue manipulation by inspecting the density of firm observations around the threshold (McCrary, 2008). Overlapping 95 percent confidence intervals at both sides of the threshold are consistent with no evidence of manipulations, also documented by Joshi (2020). However, we acknowledge that there is a slight discontinuity in the distribution and Hugger (2020) provides evidence suggesting that some private firms reduced reported revenues to avoid CbCR. To address potential manipulation, we exclude firms with revenues just below the threshold in robustness tests. We also validate that firms just below versus above the threshold are comparable by plotting polynomial mean-smoothed values of pre-treatment covariates along with consolidated revenues separately from the left to the threshold and from the threshold to the right (see, e.g., Cattaneo et al. (2018)). Across all covariates, we fail to find a statistically significant discontinuity around the threshold, which also supports our identifying assumptions in the DiD analysis (Atanasov and Black, 2016).

$Treated_f$ is an indicator variable equal to one if the firm reported consolidated revenues of at least €750 million in fiscal year 2016, and zero otherwise. $RV_f$ is the running variable and is measured as the difference between a firm's consolidated revenues in 2016 and the threshold of €750 million.[12] $P$ denotes the number of polynomials included (three for third-order polynomials and one for the local linear regressions). Our baseline specification does not include covariates and thus relies on the RD identifying assumptions and its design implying unconfoundedness (Lee and Lemieux, 2010). Other specifications include some combination of (i) pre-treatment aggregated firm-country control variables for total assets, number of employees, and leverage, (ii) pre-treatment MNE controls for consolidated total assets, number of employees, leverage, cash, return on assets, intangibles, tax avoidance, and indicators for public listing status and inclusion in the Global 500 list of valuable consumer brands published by Brand Finance in February 2016, (iii) country controls for GDP and the corporate income tax rate of firm *f*, all measured as of 2015; and (iv) year indicators.[13]

We look at three types of outcome variables. First, to validate our DiD results presented in Table 4, we re-examine subsidiary capital and labor investment in a RD setting. In these tests, $Outcome_{f,c,t}$ is capital and labor investment aggregated to the firm-country level. Specifically, $Capital\ Investment_{f,c,t}$ *(%)* is the aggregate change in fixed tangible assets since 2015 scaled by 2015 total assets for all subsidiaries of firm *f* in country *c* and year *t*. Similarly, $Labor\ Investment_{f,c,t}$

[12] We use consolidated revenue in 2016 as we are interested in firms' responses to the CbCR mandate from 2016 to 2018 conditional on knowing that they will be required to disclose their 2016 operations. In untabulated robustness tests, we use an instrumental variables approach to isolate the exogenous portion of 2016 consolidated revenues, finding consistent results.

[13] In untabulated tests, we include indicators for each subsidiary country and MNEs' industries. Inferences remain unchanged. When we include covariates in the RD analyses, we do not include their interactions with $Treated_f$ because doing so requires more rigorous assumptions to produce a consistent RD estimate (Cattaneo et al. 2018). Indicators for public listing status and inclusion in the Global 500 list of valuable consumer brands in our DiD regressions are subsumed by MNE fixed effects.

*(%)* is the aggregate change in labor expense since 2015, scaled by 2015 labor expenses for all subsidiaries of firm *f* in country *c* and year *t*.

Second, we evaluate whether total consolidated investments in capital and labor also change using consolidated financial statement data from BvD. We construct consolidated outcome variables *Cons. Capital Investment*$_{f,t}$ *(%)* and *Cons. Labor Investment*$_{f,t}$ *(%)* that are analogous to those used in the main RD analysis: the change in firm-year tangible fixed assets (labor expense) since 2015 scaled by 2015 firm-year tangible fixed assets (labor expense).

Third, we study several characteristics of an MNE's organizational structure. We count the number of subsidiaries in European countries with preferential tax regimes (*Log. Preferential Tax Regime Subs.*$_{f,t}$) or in any global tax haven (*Log. Tax Haven Subs. (any list)*$_{f,t}$). We classify a country as a tax haven if it is on any of the tax haven lists used in Bennedsen and Zeume (2018) (see Appendix B). We also count the total number of majority-owned subsidiaries (*Log. Subs.*$_{f,t}$). Finally, organizational complexity is the sum of the hierarchical subsidiary levels of majority-owned subsidiaries (*Log. Org. Complexity*$_{f,t}$). For example, a parent firm (first tier) with one subsidiary one tier down (second tier) and two subsidiaries in the third tier has an organizational complexity measure of 9 (= 1*1+1*2+2*3). For all measures, we compute the natural logarithms.[14]

**Data and Sample Selection**

To analyze the effects of the CbCR regime at the subsidiary, firm-country, and MNE levels, we access three different products of the Orbis database provided by BvD. We detail the sample construction process in Table 1. First, we download information on consolidated financial

[14] We add one before taking the natural logarithm to circumvent significant sample loss for values of zero, which includes more than half of the sample when measuring the number of subsidiaries in European preferential tax regimes or in any tax haven. This approach also mitigates the effect of outliers and models changes from zero to one subsidiary as a 100 percent increase and changes from one to zero as a 100 percent decrease. Inferences are generally robust to not adding one before taking the logarithm (untabulated).

statements for firms with (i) at least €100 million in revenue in any of the years 2012 to 2018 and (ii) at least one subsidiary located in a country other than the firm's country of incorporation. Based on BvD's database as of March 2020, this download yields 53,575 unique firms.

Second, we access historical versions of the Orbis database from 2012 to 2018 to link unique MNEs to their global subsidiaries in an iterative process consistent with Olbert (2021). This approach allows us to track MNEs' global subsidiary ownership over time.[15] Because observing the existence of subsidiaries is not dependent on the availability of financial reports, we likely capture the majority of all controlled legal entities. Dutt et al. (2021) confirm the ownership data in Orbis mirrors what is reported by firms in their CbCR disclosures, which validates the reliability and quality of coverage of the Orbis ownership data. We exclude MNEs in financial services industries already subject to public CbCR mandates. We identify 29,182 MNEs as ultimate owners of 912,452 unique subsidiaries listed in the Orbis database where either the MNE parent firm or at least one subsidiary is located in the EU. 344,550 of these unique subsidiaries are incorporated in Europe; we observe non-missing information required for our DiD regressions for 687,406 subsidiary-years in the period 2012-2018. We limit the RD sample to post-CbCR years 2016 to 2018, resulting in 88,269 firm-years and 52,316 firm-country-years. We match unconsolidated financial information for all firms' subsidiaries located in Europe. We download this information from the Orbis Generics Flatfile as of January 2020. We report the number of subsidiary observations by year and country 2012 to 2018 in Table 2.

[15] BvD collects information on beneficial ownership status (both for shareholders and subsidiaries) through various sources, such as official national registers, annual reports of separate legal entities, private correspondence, telephone research, and M&A intelligence (BvD, 2021). The data should thus not be contaminated by non-compliance with disclosure rules. Our approach is similar to recent studies (Shroff et al., 2014; Beaver et al., 2019; Beuselinck et al., 2019) but integrates more tiers to better identify lower tier subsidiaries. Manually constructing the corporate hierarchy is superior to using BvD's designation of a subsidiary's ultimate parent as this designation often includes non-corporate entities with greater than 25 percent ownership, such institutional investors, families, trusts, or individuals.

Finally, we collect information on countries' annual gross domestic product from the WorldBank and the OECD and corporate income tax rates (CITs) from the European Commission. We also hand-collect data on exit-tax regimes from the IBFD legal database and brand awareness indicators from Brand Finance ® as of February 2016.

**Descriptive Statistics**

Table 3 describes variables used in the regressions for our different samples. To mitigate the impact of outliers when using a small set of observations in the local RD, we winsorize financial variables at the 1 and 99 percent level and the ETR at 0 and 100 following the tax avoidance literature.[16] We multiply the growth measures by 100 to aid interpretation in percentage terms. Panel A reports average subsidiary-year capital (labor) investment growth of 1.03 (9.38) percent and reports (untabulated) €40.3 million of tangible fixed assets and €27.8 million of labor expenses. Consolidated firm capital (labor) investment is approximately 1.21 (10.0) percent on average. Panel B reports average firm-country-year capital investment of approximately 1.87 percent, and labor investment of approximately 11.4 percent. Median values suggest our outcome variables exhibit significant skewness; we discuss steps we take to address this skewness in Section 4.

To facilitate interpretation, we also describe the unlogged number of entities of the average observation in our organizational complexity tests (untabulated). The average firm-year has 2.03 subsidiaries in European countries with preferential tax regimes, and 2.14 tax haven subsidiaries across the globe according to the classification of jurisdictions in Bennedsen and Zeume (2018). The average total number of subsidiaries is 34, and the average unlogged measure of organizational complexity is 110.

[16] We winsorize dependent financial variables in the RD at the 5 and 95 percent level given large outliers in positive and negative growth since 2015 for some firm-years introducing substantial noise.

We also inspect summary statistics for observations reporting consolidated 2016 revenue within the bandwidth of +/- €250 million surrounding the €750 million threshold for CbCR (untabulated). Overall, firms in the full sample are similar on average to firms in the RD bandwidth, particularly with respect to the outcome variables of interest.

## IV. EMPIRICAL RESULTS

### Economic Activity Using Unconsolidated Subsidiary Financial Accounts

#### *Main DiD Analyses of Capital and Labor Investment at the Subsidiary Level*

We begin the empirical analysis with a DiD design. We estimate differences in *Capital Investment*$_{i,t}$ *(%)* and *Labor Investment*$_{i,t}$ *(%)* between subsidiaries belonging to treated versus control multinationals (based on their parent firms' 2016 revenues), always relative to the year before the CbCR mandate became effective (2015).

To provide evidence on the validity of the parallel trends assumption for our DiD analysis and test for effect dynamics, we first plot annual difference-in-difference coefficients for capital and labor investment in Figure 2. To do so, we re-estimate Equation (1) after replacing $Post_t$ with annual indicator variables that we interact with $Treated_f$. We present the estimated annual coefficients, along with their 95 percent confidence intervals, for all European countries (Panels (1) and (2)), the sub-sample of European countries with preferential tax regimes (Panels (3) and (4)), and the sub-sample of all other countries (Panels (5) and (6). The graphs suggest that treated and control subsidiaries did not develop differently in their economic activities in the years leading up to the CbCR mandate, providing evidence supportive of the parallel trends assumption. The graphs further provide evidence of a differential response to CbCR by treated firms relative to control firms, particularly in European countries with preferential tax regimes with an immediate response in capital investment (4 percent in 2016 and 2 percent in 2017 and 2018) and a steadily increasing response in labor investment of up to approximately 10 percent in 2018.

We report regression results of our DiD analysis in Table 4. Columns (1) and (2) present results for all European countries. Columns (3) and (4) present results for European countries with preferential tax regimes considered to facilitate aggressive tax planning: Switzerland, Cyprus, Ireland, Luxembourg, Malta, and the Netherlands. Columns (5) and (6) present results for all other European countries. Specifications vary by the inclusion of time-varying MNE and subsidiary controls, as well as by the inclusion of firm and year fixed effects versus firm and country-year fixed effects. As assignment to treatment varies by firm, we cluster standard errors by MNE to account for correlations across subsidiaries of the same firm.[17]

Subsidiaries of treated firms increased their economic activity on average across all European countries following CbCR, leading to up to 0.5 (1.4) percentage point larger increases in capital (labor) investment from 2015 to 2018 relative to subsidiaries of control firms. Columns (3)-(4) suggest much larger differences in capital and labor investments in European countries with preferential tax regimes, with effect sizes of 1.9-2.2 and 5.2 percentage points, respectively.[18] Using sub-sample average total assets of €729.9 million and labor expenses of €89.2 million for 2015 in countries with preferential tax regime countries (untabulated), these magnitudes suggest the average treated subsidiary increased tangible fixed assets by €13.9-16.1 million and labor expenses by €4.6 million, relative to control subsidiaries.

In untabulated tests, we confirm these inferences are robust to three alternative ways of identifying countries in Europe with favorable tax regimes: (i) countries identified as non-cooperative preferential regimes by the European Commission (Ireland, the Netherlands, and

[17] See also Joshi (2020). In untabulated tests, we confirm inferences remain largely unchanged if we alternatively cluster standard errors by subsidiary country, MNE parent country, MNE parent country-year, or subsidiary country-industry.

[18] Inferences and magnitudes are similar when additionally including subsidiary fixed effects except when testing labor investment in European countries with preferential tax regimes (untabulated).

Luxembourg); (ii) our main set plus sample countries with low tax rates (Bulgaria and Romania); and (iii) our main set plus the United Kingdom, which is increasingly considered by some to be a preferential regime post-Brexit (KPMG, 2016). We observe larger DiD coefficients for each set of preferential countries relative to the rest of Europe for both capital and labor investment. Inferences also remain unchanged if we restrict the sample to unconsolidated subsidiaries with assets greater than local regulatory thresholds to hold constant the regulatory burden of public financial reporting requirements. Finally, to address skewness in the outcome variables, we evaluate the robustness of our results to jackknife estimation. We continue to observe significantly larger increases in capital and labor investments for CbCR firms relative to control firms in both European countries with preferential tax regimes and all other European countries. Further, the magnitudes are larger in European countries with preferential tax regimes, consistent with our main results.

Taken together, the robust set of economically larger results for European countries with preferential tax regimes are consistent with reports that, for example, "Ireland appears to have been a major beneficiary of … the BEPS project" (Campbell, 2021) based on soaring tax receipts. Seamus Coffey, Chairperson of the Irish Fiscal Advisory Council (IFAC) and one of the leading economists on the impact of taxes and incentives on the Irish economy, attributes Ireland's windfall to the fact that it is "actually one of the countries where … companies [can] have substance," thus conforming to BEPS' "objective … to align profit with substance" (Campbell, 2021). The overall effect of these responses could be a reduction in tax avoidance, as documented by Joshi (2020) and Hugger (2020). Alternatively, our results could indicate inferences in these studies are attributable to CbCR-induced changes in the location of assets and employment.

***Cross-Sectional DiD Analyses of Capital and Labor Investment at the Subsidiary Level***

We next conduct heterogeneity analyses. One explanation for observing larger capital and labor investments in European countries with preferential tax regimes is that MNEs shift some assets and employees from other countries in Europe. If true, we would observe stronger effects if the country of origin does not impose exit taxes on MNEs shifting out assets and employees. We thus exploit variation in exit taxes across the set of non-preferential European countries. We define an MNE's exposure to exit taxes (*Exit Taxes*$_{f,2015}$) as high if the MNE has an above-sample-median share of subsidiary tangible fixed assets located in non-preferential European countries with exit taxes as of 2015. These countries include Austria, Denmark, Finland, France, Germany, Italy, Portugal, Spain, and the United Kingdom, and the median subsidiary belongs to an MNE with 92 percent of fixed tangible assets in these countries.

Table 5, Columns (1) and (2) report results from parsing the sample based on high versus low MNE exposure to exit taxes and re-estimating our difference-in-differences analysis on each sub-sample. Consistent with expectations, we find that subsidiaries of treated firms in countries with preferential tax regimes increased capital and labor investments relative to subsidiaries of control firms, but only in the sub-sample of firms with lower exposure to exit taxes. When examining capital investment, we document a treatment effect of 3.02 for firms with lower exposure to exit taxes and -0.04 for firms with greater exposure. The difference in coefficients is statistically significant at conventional levels (t-statistic of 3.42). When examining labor investment, we document a treatment effect of 7.17 for firms with lower exposure to exit taxes and -2.23 for firms with greater exposure. However, the difference in coefficients is not statistically significant at conventional levels (t-statistic of 1.37).

We also expect that MNEs with greater ex ante tax avoidance have more incentives to substantiate their tax avoidance following CbCR via capital and labor investments in European

countries with preferential tax regimes. This is because tax enforcement risk from other countries after CbCR is likely to increase the most for firms with high tax avoidance. We measure ex ante tax avoidance using the MNE's GAAP effective tax rate (ETR) and tax haven subsidiary ownership (outside the studied countries), both measured in 2015 (i.e., prior to CbCR). We then parse the sample on the MNE industry-median value of each measure and re-estimate our DiD analysis on each sub-sample.[19]

Table 5, Columns (3)-(6) report results of these heterogeneity analyses based on ex ante MNE tax avoidance. Consistent with expectations, we estimate larger capital and labor investment effects in the sub-sample with below-industry-median GAAP ETRs, relative to the sub-sample with above-industry-median GAAP ETRs. We also estimate larger capital investment effects in the sub-sample with of firms with tax haven subsidiaries, relative to the sub-sample without tax haven subsidiaries. Except for the sample split in columns (5) and (6) of Panel B, the differences in treatment effects across partitions are statistically significant at conventional levels. Collectively, these results are consistent with those MNEs anticipating greater increases in tax enforcement risk substantiating their tax avoidance activities the most by locating real functions in countries offering preferential tax regimes.

***Regression Discontinuity Analysis of Capital and Labor Investment at the Firm-Country Level***

As additional validation of our DiD results on capital and labor investment, we also examine capital and labor investment using an RD approach. As suggested by Lee and Lemieux (2010), we begin the evaluation of RD results by graphically examining discontinuities in outcome variables in Figure 3. All panels plot average values of the outcome variable for evenly spaced

[19] See Appendix A for a definition of these variables. The subsidiaries' sample median MNE GAAP ETR in 2015 is 25.29 percent. The mean (median) subsidiary belongs to a MNE with two (eleven) tax haven subsidiaries outside the European countries with preferential tax regimes.

bins of 2016 consolidated revenue, the 95 percent confidence interval, and fitted linear trends.[20] In Panels (1), (3), and (5), we observe linear discontinuities in *Capital Investment$_{f,c,t}$ (%)*. These results indicate firms increase capital investments in countries with preferential tax regimes following CbCR disclosure, consistent with our DiD results. In contrast, we observe reductions in capital investments in the rest of Europe and across all European countries. We observe the same pattern in Panels (2), (4), and (6) for *Labor Investment$_{f,c,t}$ (%)*.

We present results of RD regression analysis on firm-country-level outcomes in Table 6. We report results using a parsimonious local linear regression. Columns (2), (4), and (6) include MNE controls measured as of 2015 and aggregated firm-country controls as well as year fixed effects. In Panel A, we find no evidence of an impact of CbCR on capital investments across all European countries. However, when we restrict the sample to European countries with preferential tax regimes in Columns (3) and (4), we estimate positive and significant coefficients across both specifications (1.43 and 1.55). We find consistent evidence for labor investments in Panel B, estimating positive, economically larger and statistically significant coefficients in European countries with preferential tax regimes (17.6-18.9 percent in Columns (3) and (4)). Using sub-sample average total assets and labor expense in 2015 (untabulated), these magnitudes suggest relative increases of €29.1-31.6 million in tangible fixed assets investment and €10.0-10.7 million in labor expense by subsidiaries of CbCR firms following CbCR. Together with the DiD results, these results suggest affected firms increase real investment activities in subsidiaries in European countries with preferential tax regimes.[21]

[20] We use a data-driven procedure recommended by Cattaneo et al. (2018) to produce the optimal number of bins. This procedure chooses the number of bins such that the overall variability of the binned means is similar to the overall variability in the raw scatter plot of data. Because we have two outcome variables with different variances, the optimal number of bins differs. Inferences strengthen if we instead fix the number of bins at each side of the threshold for both outcomes (untabulated).

[21] These inferences do not change if we instead use local polynomial regressions (untabulated).

In contrast, when we restrict the sample to non-preferential European countries in Columns (5) and (6), we find some evidence of negative capital and labor investments across both panels. We also observe this negative effect for labor investments in the full sample in Panel B, Column (1). However, these negative coefficients are not statistically distinguishable from zero once we include controls that account for differences across subsidiaries, MNEs, MNE home countries, and time. Differences between these results and those reported in Table 4 could be attributable to the different estimation technique, aggregation of subsidiary financial data to the firm-country level, and a significantly smaller sample due to dropped pre-treatment years and the narrow bandwidths around the consolidated revenue reporting threshold.

***Analysis of Capital and Labor Investment and Investment Efficiency at the Firm Level***

Our DiD and RD results thus far provide evidence consistent with CbCR firms increasing capital and labor investments in European countries with preferential tax regimes following CbCR. A natural question is whether total consolidated investments in capital and labor – as well as the firm's consolidated investment efficiency – also change. To answer these questions, we examine analogous capital and labor investment measures using consolidated financial statement data from BvD. We present results in Table 7. Panel A presents results for capital investment, and Panel B presents results for labor investment, both using an RD design. Across all specifications, we fail to find evidence of a change in consolidated capital or labor investments; effect sizes are small and have mixed signs across specifications. These tests are consistent with the increases in capital and labor investments in European countries with preferential tax regimes that we observe in our prior tests coinciding with decreases in capital and labor investments in other countries. Further, these findings suggest that firms shift investments to countries with preferential tax regimes rather than

altering overall investment. Thus, our collective evidence points to resource reallocation that should be of interest to policymakers.

In Panel C, we examine consolidated firms' responsiveness of capital and labor investments to investment opportunities by adding the interaction of a proxy for investment opportunities with the variables of interest in a DiD specification following prior literature (e.g., Jayaraman and Wu (2019)). We use median *Q* as our measure of investment opportunities. Median (mean) *Q* across MNE-years in our sample is 1.36 (1.66), similar to the values in Jayaraman and Wu (2019). We present results for consolidated capital investment in columns (1)-(2) and consolidated labor investment in columns (3)-(4). Columns (1) and (3) present results for the full sample, and columns (2) and (4) restrict the sample to firms within a €500 million bandwidth to better facilitate a comparison of these DiD results to the consolidated investment RD results reported in Panels A and B. In columns (1)-(2), we observe a negative and significant coefficient on the interaction of *Treated*, *Post*, and *Q*, consistent with CbCR firms exhibiting a lower sensitivity of capital investment to investment opportunities. These results are robust to alternatively using mean *Q* as the measure of investment opportunities. However, we observe mixed and insignificant results for labor investment in columns (3)-(4). We interpret these results as suggestive that the capital investment response to investment opportunities is constrained by CbCR.

**Organizational Structure and Complexity**

Finally, we study whether firms respond to CbCR by altering organizational structures. As these tests are conducted at the firm level, we again rely on RD for these analyses to mitigate the influence of stark differences in firm size across treated and control firms. When graphically examining discontinuities in outcome variables, we observe linear discontinuities at the CbCR

threshold, indicating a lower number of subsidiaries located in European countries with preferential tax regimes and in tax havens, and smaller negative discontinuities in the total number of subsidiaries and overall organizational complexity (untabulated).

We present results of the regression analysis in Table 8. Panel A presents results for *Log. Preferential Tax Regime Subs.*, Panel B presents results for *Log. Tax Haven (any list)*, Panel C presents results for *Log. Subs.*, and Panel D presents results for *Log. Org. Complexity.* Columns (1)-(2) use non-parametric local linear regressions, and columns (3)-(4) use non-parametric local third-order polynomial regressions. Columns (2) and (4) add year fixed effects and pre-treatment MNE controls. All columns use mean-squared-error optimal bandwidths according to the procedure from Calonico et al. (2014).

Results are generally consistent with CbCR inducing treated firms to reduce their number of subsidiaries and organizational complexity. The estimates suggest the average firm in the bandwidth just above the CbCR disclosure threshold closed 11.3 to 15.6 percent of subsidiaries in European countries with preferential tax regimes and 18.1 to 29.5 percent of tax haven subsidiaries worldwide.[22] Overall, these results provide some indication that even private CbCR disclosure deters tax haven operations. However, the magnitudes of significant coefficients for the total number of subsidiaries appear larger than those estimated for the number of tax haven subsidiaries.[23] Combined with reduced organizational complexity and anecdotal evidence from tax executives, these results suggest the "unwinding" of obsolete entities and a simplification of legal entity charts in response to CbCR.[24]

[22] To obtain these estimated percentages, we apply the exponential function to the estimated coefficient and subtract one, then multiply the quantity by 100.
[23] Results are robust to excluding 2018 or U.S.-based MNE parents, suggesting they are not driven by the U.S.' Tax Cuts and Jobs Act. Inferences for the number of subsidiaries in European countries with preferential tax regimes or tax havens are unchanged if we use alternative bandwidth selection procedures and kernel functions to construct the local polynomial estimators.
[24] We thank participants at the February 2020 International Tax Policy Forum meeting for helpful comments.

## V. FALSIFICATION TESTS AND ADDITIONAL VALIDATION

We conduct several additional tests to validate our results. We report results of these tests using our DiD design to examine capital and labor investment outcomes in Table 9. We report results of these tests using RD to examine changes in organizational structure in Table 10.

We begin with several falsification tests. First, we use the true €750 million threshold but 2013 as the year of treatment (Table 9, Panel A) or revenues measured in 2013 as the running variable (Table 10, Column (1)). Across all specifications, we document statistically insignificant estimates near zero. Second, we select €375 million as an alternative threshold for CbCR in the year of implementation in Table 9, Panel B and Table 10, Column (2).[25] We observe coefficients statistically distinguishable from zero in three out of ten specifications across the two tables. Upon closer examination, the positive and significant coefficients appear driven by a statistically significant decline in investment among control firms relative to a positive but insignificant change in investment for pseudo-treated firms. It is possible that splitting the sample on EUR 375 million in consolidated revenues (relative to the sample mean of EUR 1.6 billion) results in a control sample of, on average, less successful firms or those with fewer growth opportunities. We do not observe statistically significant results in European countries with preferential tax regimes, where we find the most economically and statistically significant results in our main tests. Third, for the DiD analysis, we examine whether firms that did not exceed the €750 million threshold until 2018 exhibit any response to CbCR prior to 2018 (Table 9, Panel C). We do not find a statistically significant anticipatory effect in the direction of our results. For the RD analysis, we use the €750 million threshold based on revenues in 2016 but examine outcome variables measured in 2015,

[25] In untabulated tests, we also use a larger placebo threshold of €1,500 million for the RD to address concerns that other events occurring around the same time as CbCR (e.g., OECD/G20 BEPS, European Commission's illegal state aid investigations, LuxLeaks) more heavily impact larger firms with more aggressive tax planning strategies and (at least partially) contribute to the effects documented in our study. We do not find a significant discontinuity between "treated" and control firms using this larger threshold.

one year before the first disclosures were required (Table 10, Column (3)). Across all outcomes, we do not observe a statistically significant effect.

We conduct two additional tests for the RD analysis. First, although untabulated tests suggest no manipulation of the forcing variable of our RD analysis in 2016, we conduct a formal test in Table 10 to determine if a systematic (non-)assignment of firms to the treatment group is a threat to our identification approach.[26] Specifically, we replicate our main RD analysis after excluding observations just below the threshold, as these observations may have manipulated revenues to fall just below the CbCR threshold. Inferences remain unchanged across all outcomes (Column (4)). Second, we confirm RD inferences are unchanged if we use alternative bandwidth sizes that are 75 or 150 percent of those reported in Table 7 (Columns (5)-(6)).

## VI. Conclusions

This study examines the real investment and organizational effects of the private country-by-country reporting requirement implemented in the EU in 2016. Using both a difference-in-differences and a regression discontinuity design, we find evidence of larger capital and labor investments made by MNEs subject to CbCR after implementation of the disclosure regime in European countries with preferential tax regimes, relative to MNEs not subject to CbCR disclosures. These results are consistent with the goal of better substantiating tax avoidance activities to European tax authorities, who are the primary initial recipients of the private disclosures. Notably, these capital and labor investment effects of CbCR most positively impact the same countries within Europe that likely had benefited the most from the MNE tax avoidance

[26] This test is motivated by the evidence in Hugger (2020), which suggests some bunching below the threshold for private firms, particularly in later reform years. We inspect a (untabulated) histogram of observations closely around the threshold and observe a relatively large fraction of observations precisely at the threshold (i.e., treated firms). Yet, we also note a larger fraction of firms within the bandwidth of €10 million below the threshold compared to the bandwidth of €10 million above the threshold.

activities that CbCR was intended to target. In contrast, CbCR provides less positive (or even negative) capital and labor investment effects for other countries.

We do not estimate statistically significant evidence to support the conclusion that CbCR impacts total consolidated asset or labor investment, though we do estimate a reduced responsiveness of capital investment to investment opportunities among CbCR firms. We further find evidence consistent with firms subject to CbCR disclosures significantly reducing their number of subsidiaries worldwide – including those located in European countries with preferential tax regimes and worldwide tax havens – and organizational complexity following implementation of CbCR relative to firms not required to disclose, consistent with the goal of reducing (at least the appearance of) aggressive tax practices. Our simultaneous examination of real investment, investment efficiency, and organizational changes in response to CbCR is novel and suggests tax aggressiveness and firm complexity do not necessarily capture the same construct (Balakrishnan et al., 2019). Our results are robust to an array of sensitivity analyses and falsification tests. We acknowledge there could be heterogenous effects over time as well as across different types of firms not examined in this study. We look forward to future research that can leverage larger samples and novel data to examine additional outcomes and cross-sectional drivers of corporate responses to CbCR.

Our study bridges research on the consequences of mandatory private disclosure. We provide evidence on the real investment and organizational effects of private disclosures to tax authorities made by a large number of MNEs. Our evidence suggests that disclosures mandated by one regulator with a vested interest in curbing tax avoidance can impact firms' real decisions without these disclosures being publicly available to other stakeholders. By investigating real effects of these tax disclosures, we extend the tax avoidance literature. We interpret our results as

indicative of firms substantiating their tax avoidance by increasing economic activities in jurisdictions with preferential tax regimes. At the same time, firms take steps to reduce the appearance of overly aggressive tax avoidance through complicated organizational structures. Our results suggest increased monitoring by tax authorities may not achieve intended results. Increased transparency may reduce MNE tax avoidance perceived to negatively impact jurisdictions with fewer tax preferences, but potentially at the same time incentivizing investment in jurisdictions with relatively preferential tax regimes. Thus, our study has clear policy implications.

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## Appendix A: Variable Definitions

| Variable | Definition |
|---|---|
| Panel A: Variables for Difference-in-Differences Analysis (2012-2018) | |
| ***Outcome variables*** | |
| *Capital Investment$_{i,t}$ (%)* | Change in unconsolidated fixed tangible assets scaled by lagged total assets of subsidiary i in year t, multiplied by 100. |
| *Labor Investment$_{i,t}$ (%)* | Change in unconsolidated labor expense scaled by lagged labor expense of subsidiary i in year t, multiplied by 100. |
| *Cons. Capital Investment$_{f,t}$ (%)* | Change in consolidated fixed tangible assets scaled by lagged total assets of firm f in year t, multiplied by 100. |
| *Cons. Labor Investment$_{f,t}$ (%)* | Change in consolidated labor expense scaled by lagged labor expense of firm f in year t, multiplied by 100. |
| ***Independent Variables*** | |
| *Treated$_f$* | Binary variable equal to one if the consolidated revenue of firm f owning subsidiary i was at least EUR 750 million in 2016. |
| *Log. Total Assets$_{i,t-1}$* | Natural logarithm of unconsolidated total assets of subsidiary i in year t-1. |
| *Log. Employees$_{i,t-1}$* | Natural logarithm of unconsolidated number of employees of subsidiary i in year t-1. |
| *Leverage$_{i,t-1}$* | Unconsolidated long-term debt scaled by total assets of subsidiary i in year t-1, multiplied by 100. |
| *Log. Total Assets$_{f,t}$* | Natural logarithm of consolidated total assets of firm f in year t. |
| *Log. Employees$_{f,t}$* | Natural logarithm of consolidated number of employees of firm f in year t. |
| *Log. Cash$_{f,t}$* | Natural logarithm of consolidated cash and cash equivalents of firm f in year t. |
| *ROA$_{f,t}$* | Consolidated return on assets, measured as earnings before taxes scaled by total assets, of firm f in year t, multiplied by 100. |
| *Leverage$_{f,t}$* | Consolidated long-term debt scaled by total assets of firm f in year t, multiplied by 100. |
| *Intangible Ratio$_{f,t}$* | Consolidated fixed intangible assets scaled by total assets of firm f in year t, multiplied by 100. |
| *ETR$_{f,t}$* | Consolidated GAAP effective tax rate, measured by dividing total tax expense by earnings before taxation, of firm f in year t, multiplied by 100. |
| *Log. GDP$_{f,t}$* | Natural logarithm of total GDP of the country of incorporation of firm f in year t. |
| *CIT$_{f,t}$* | Corporate income tax rate of the country of incorporation of firm f in year t. |
| ***Cross-sectional Variables*** | |
| *Low Exit Taxes$_{f,2015}$* | Indicator variable equal to one if firm f has a low pre-CbCR exposure to countries with exit tax regimes outside the the set of preferential tax regimes countries, and zero otherwise. Exposure to exit tax regime is measured as the share of a firm's subsidiaries' fixed tangible assets located in these countries. Low exposure is defined as a below median value in the sample of subsidiaries in 2015. |
| *Low ETR$_{f,2015}$* | Indicator variable equal to one if firm f has a low pre-CbCR ETR relative to other multinationals with positive pre-tax income from the same country in the same industry, and zero otherwise. Low ETR is defined as a below median value in the sample of subsidiaries in 2015. |
| *Tax Haven Firm$_{f,2015}$* | Indicator variable equal to one if firm f owns at least one tax haven subsidiary according to any of the classifications used in Bennedsen and Zeume (2018) but outside the set of preferential tax regime countries in 2015, and zero otherwise. |
| *Q(median)$_{f,t}$* | Tobin's Q of firm f in year t. Q is defined as the median country-industry-year ratio of market value of equity (PRCC_F*CSHO) plus consolidated total assets (AT) less the book value of equity (CEQ) to consolidated total assets (AT) using stock market and financial data from Compustat and two-digit SIC codes to define industries. |

| Variable | Definition |
|---|---|
| Panel B: Variables for Regression Discontinuity Analysis (2016-2018) | |
| ***Outcome variables*** | |
| *Capital Investment*$_{f,c,t}$ *(%)* | Change in the sum of unconsolidated tangible fixed assets since 2015 scaled by the sum of unconsolidated total assets in 2015 of all subsidiaries of firm f in country c in year t, multiplied by 100. |
| *Labor Investment*$_{f,c,t}$ *(%)* | Change in the sum of unconsolidated labor expenses since 2015 scaled by the sum of unconsolidated labor expenses in 2015 of all subsidiaries of firm f in country c in year t, multiplied by 100. |
| *Cons. Capital Investment*$_{f,t}$ *(%)* | Change in consolidated tangible fixed assets since 2015 scaled by consolidated total assets in 2015 of firm f in year t, multiplied by 100. |
| *Cons. Labor Investment*$_{f,t}$ *(%)* | Change in consolidated labor expenses since 2015 scaled by consolidated labor expenses in 2015 of firm f in country c in year t, multiplied by 100. |
| *Log. Preferential Tax Regime Subs.*$_{f,t}$ | Natural logarithm of the absolute number of subsidiaries located in a European country with a preferential tax regime (see Appendix B) of firm f in year t. |
| *Log. Tax Haven Subs. (any list)*$_{f,t}$ | Natural logarithm of the absolute number of tax haven subsidiaries according to any of the classifications used in Bennedsen and Zeume (2018) (see Appendix B) of firm f in year t. |
| *Log. Subs.*$_{f,t}$ | Natural logarithm of the total number of worldwide subsidiaries with direct equity ownership links above 50% of firm f in year t. |
| *Log. Org. Complexity*$_{f,t}$ | Natural logarithm of the sum of the number of subsidiaries at each hierarchical level in the organizational structure multiplied by the hierarchical level of firm f in year t. For example, if firm has one parent entity, one level 2 subsidiary, and one level 3 subsidiary directly owned by the level 2 subsidiary, the sum of the product is 1*1+1*2+1*3=6. |
| ***Pre-treatment Control Variables*** | |
| *Log. Total Assets*$_{f,c,2015}$ | Natural logarithm of the sum of unconsolidated total assets of all subsidiaries of firm f in country c in 2015. |
| *Log. Employees*$_{f,c,2015}$ | Natural logarithm of the sum of unconsolidated employees of all subsidiaries of firm f in country c in 2015. |
| *Leverage*$_{f,c,2015}$ | The sum of unconsolidated long-term debt scaled by the sum of unconsolidated total assets of all subsidiaries of firm f in country c in 2015, multiplied by 100. |
| *Log. Total Assets*$_{f,2015}$ | Natural logarithm of consolidated total assets of firm f in 2015. |
| *Log. Employees*$_{f,2015}$ | Natural logarithm of the consolidated number of employees of firm f in 2015. |
| *Log. Cash*$_{f,2015}$ | Natural logarithm of consolidated cash and cash equivalents of firm f in 2015. |
| *ROA*$_{f,2015}$ | Consolidated return on assets, measured as earnings before taxes scaled by total assets, of firm f in 2015, multiplied by 100. |
| *Leverage*$_{f,2015}$ | Consolidated long-term debt scaled by consolidated total assets of firm f in 2015, multiplied by 100. |
| *Intangible Ratio*$_{f,2015}$ | Consolidated fixed intangible assets scaled by consolidated total assets of firm f in 2015, multiplied by 100. |
| *ETR*$_{f,2015}$ | Consolidated GAAP effective tax rate, measured by dividing total tax expense by earnings before taxation, of firm f in 2015, multiplied by 100. |
| *Publicly Listed*$_{f,2015}$ | Indicator variable equal to one if firm f was pubilcly listed in 2015, and zero otherwise. |
| *Brand Awareness*$_{f,2015}$ | Indicator variable equal to one if firm f appeared in the Global 500 list of valuable consumer brands published by Brand Finance ® in February 2016, and zero otherwise. |
| *Log. GDP*$_{f,2015}$ | Natural logarithm of total GDP of the country of incorporation of firm f in 2015. |
| *CIT*$_{f,2015}$ | Corporate income tax rate of the country of incorporation of firm f in 2015. |

*Notes:* We take GDP data from The World Bank and the OECD, CIT data from the European Commission, and brand awareness data from Brand Finance ®.

**Appendix B: Classification of Tax Haven Countries**

| Country | Tax Haven (any list) | Preferential Tax Regime (Europe) | Big 8 | Dot Havens |
|---|---|---|---|---|
| Andorra | x | | | x |
| Antigua and Barbuda | x | | | x |
| Anguilla | x | | | x |
| Aruba | x | | | x |
| Barbados | x | | | x |
| Bahrain | x | | | x |
| Bermuda | x | | | x |
| Bahamas | x | | | x |
| Belize | x | | | x |
| Switzerland | | x | x | |
| Costa Rica | x | | | x |
| Cyprus | x | x | | |
| Dominica | x | | | x |
| Grenada | x | | | x |
| Gibraltar | x | | | x |
| Hong Kong | x | | x | |
| Ireland | x | x | x | |
| Jordan | x | | | x |
| Saint Kitts and Nevis | x | | | x |
| Cayman Islands | x | | | x |
| Lebanon | x | | x | |
| Saint Lucia | x | | | x |
| Liechtenstein | x | | | x |
| Liberia | x | | x | |
| Luxembourg | x | x | | |
| Monaco | x | | | x |
| Marshall Islands | x | | | x |
| Macau | x | | | x |
| Malta | x | x | | |
| Mauritius | x | | | x |
| Maldives | x | | | x |
| Netherlands | | x | | |
| Nauru | x | | | x |
| Panama | x | | x | |
| Puerto Rico | | | x | |
| Seychelles | x | | | x |
| Singapore | x | | x | |
| San Marino | x | | | x |
| Tonga | x | | | x |
| Saint Vincent and the Grenadines | x | | | x |
| British Virgin Islands | x | | | x |
| Vanuatu | x | | | x |
| Samoa | x | | | x |

*Notes:* This table lists the countries coded as tax havens or those with preferential tax regimes. The comprehensive tax haven list (*Tax Haven (any list)*) subsumes all countries listed in any of the categories in Bennedsen and Zeume (2018). We do not include countries labelled as tax havens according to Bennedsen and Zeume (2018) if no multinational firm in our sample has a subsidiary in the respective country. *Preferential Tax Regime (Europe)* refers to countries in Europe blacklisted by the Tax Justice Network. *Big 8* refers to the "big seven" tax havens in Hines and Rice (1994) plus Puerto Rico. *Dot Haven* are tax haven countries that are not European countries or *Big 8* havens.

## Figure 1: Example of Country-by-Country Disclosure

Table 1. Overview of allocation of income, taxes and business activities by tax jurisdiction

Name of the MNE Group:
Fiscal Year concerned:
Currency used:

| Tax jurisdiction | Revenues | | | Profit (loss) before income tax | Income tax paid (on cash basis) | Income tax accrued — current year | Stated capital | Accumulated earnings | Number of employees | Tangible assets other than cash and cash equivalents |
|---|---|---|---|---|---|---|---|---|---|---|
| | Unrelated party | Related party | Total | | | | | | | |
| | | | | | | | | | | |
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Table 2 List of all the Constituent Entities of the MNE Group included in each aggregation per tax jurisdiction

Name of the MNE Group:
Fiscal Year concerned:

| Tax Jurisdiction | Constituent Entities Resident in the Tax Jurisdiction | Tax Jurisdiction of Organisation or Incorporation if Different from Tax Jurisdiction of Residence | Main Business Activity(ies) | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Research and Development | Holding or Managing Intellectual Property | Purchasing or Procurement | Manufacturing or Production | Sales, Marketing or Distribution | Administrative, Management or Support Services | Provision of Services to Unrelated Parties | Internal Group Finance | Regulated Financial Services | Insurance | Holding Shares or Other Equity instruments | Dormant | Other (1) |
| | 1. | | | | | | | | | | | | | | |
| | 2. | | | | | | | | | | | | | | |
| | 3. | | | | | | | | | | | | | | |
| | 1. | | | | | | | | | | | | | | |
| | 2. | | | | | | | | | | | | | | |
| | 3. | | | | | | | | | | | | | | |

(1) Please specify the nature of the activity of the Constituent Entity in the "Additional information"

Table 3: Additional information

Name of the MNE Group:
Fiscal Year concerned:

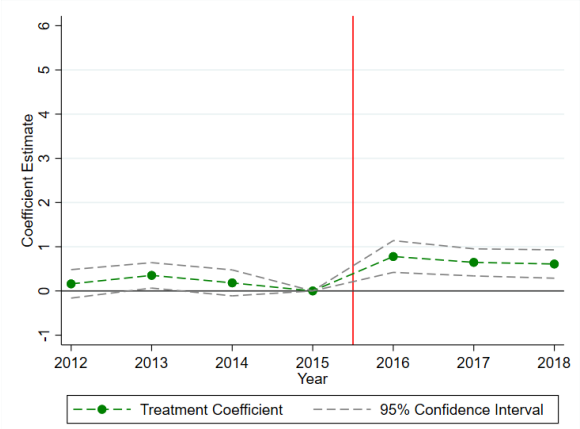
Please include any further brief information or explanation you consider necessary or that would facilitate the understanding of the compulsory information provided in the country-by-country report

*Notes:* This figure depicts the template report firms are required to submit to local tax authorities under the CbCR mandate as of 1 January 2016 that was published as part of the European Council Directive 2016/881/EU. Source: https://eur-lex.europa.eu/legal-content/EN/TXT/PDF/?uri=CELEX:32016L0881&from=EN.

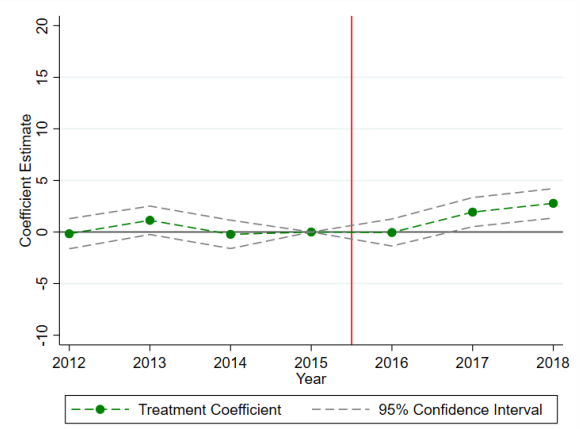
Figure 2: CbCR Effect Dynamics on Subsidiary-Level Economic Activity

**All European Countries**

(1) *Capital Investment*$_{i,t}$ *(%)*

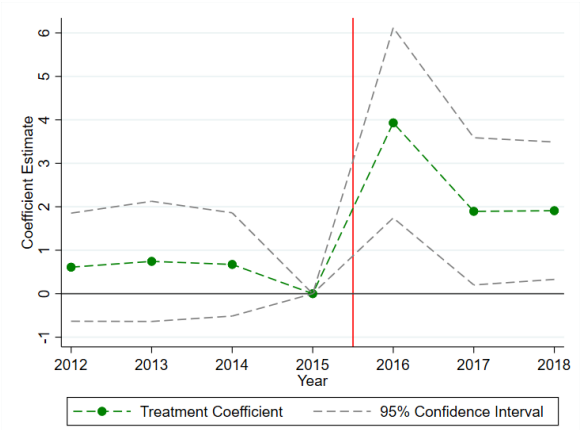



(2) *Labor Investment*$_{i,t}$ *(%)*

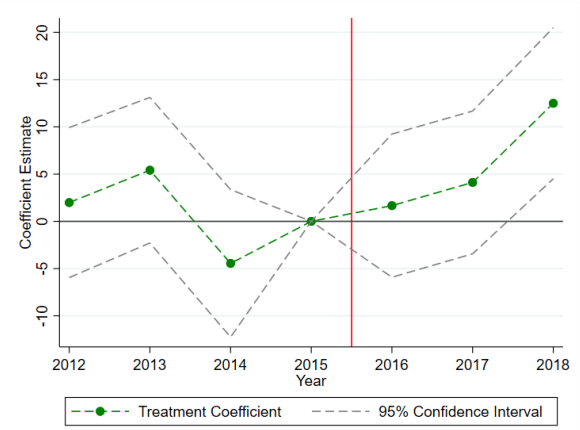



**European Countries with Preferential Tax Regimes**

(3) *Capital Investment*$_{i,t}$ *(%)*

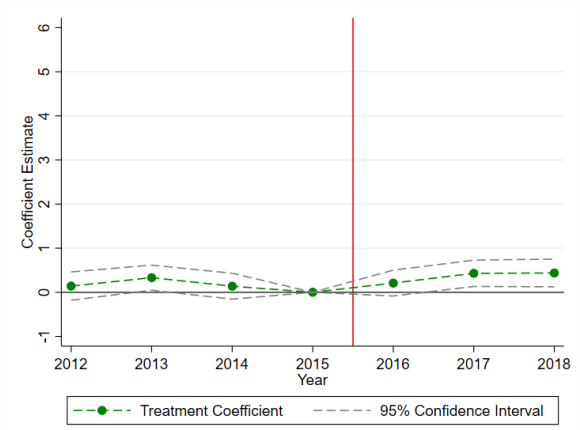



(4) *Labor Investment*$_{i,t}$ *(%)*

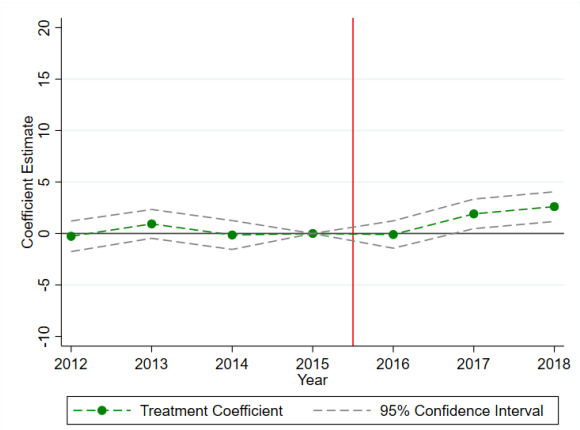



**Rest of Europe**

(5) *Capital Investment*$_{i,t}$ *(%)*




(6) *Labor Investment*$_{i,t}$ *(%)*

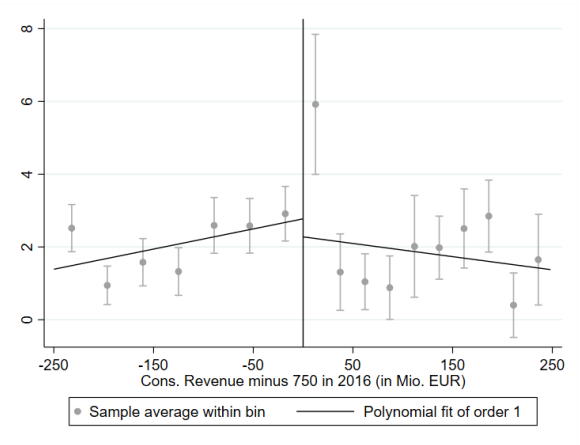



*Notes:* These graphs plot the difference-in-difference coefficients and their 95% confidence intervals from regressions of subsidiary-level *Capital Investment*$_{i,t}$ *(%)* or *Labor Investment*$_{i,t}$ *(%)* on indicators for each year in the sample period interacted with a treatment indicator taking on the value of 1 for treated CbCR firms (firms reporting more than €750 million in 2016 revenue). Graphs (1)-(2) are based on the full sample of subsidiaries in Europe, graphs (3)-(4) are based on the sub-sample of European countries with preferential tax regimes (Cyprus, Ireland, Luxembourg, Malta, Netherlands, Switzerland), and graphs (5)-(6) are based on the sub-sample of all other European countries. Regressions are based on the specifications used in columns (1), (3), and (5) of Table 4, respectively.

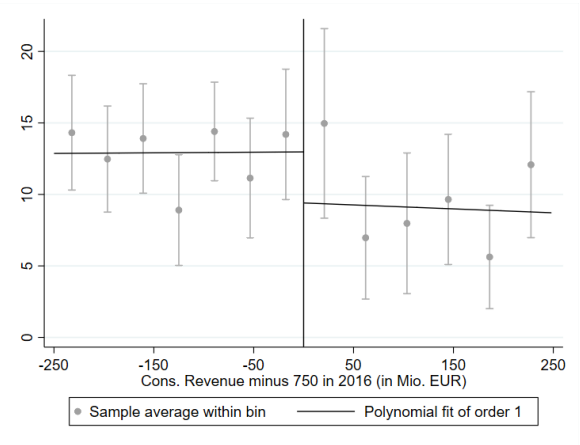
Figure 3: Unconsolidated Firm-Country-Level Economic Growth Discontinuities around €750 million Threshold

**All European Countries**

(1) *Capital Investment*$_{f,c,t}$ *(%)*

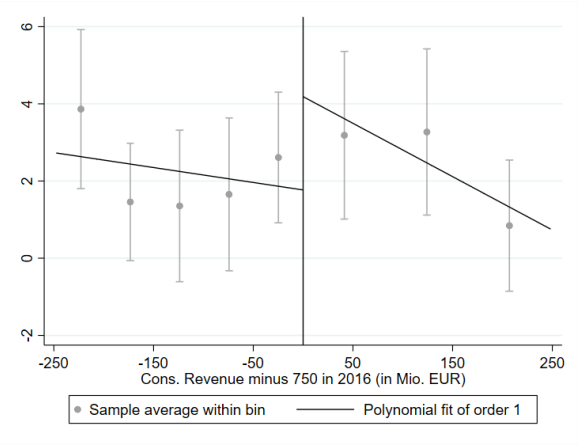



(2) *Labor Investment*$_{f,c,t}$ *(%)*

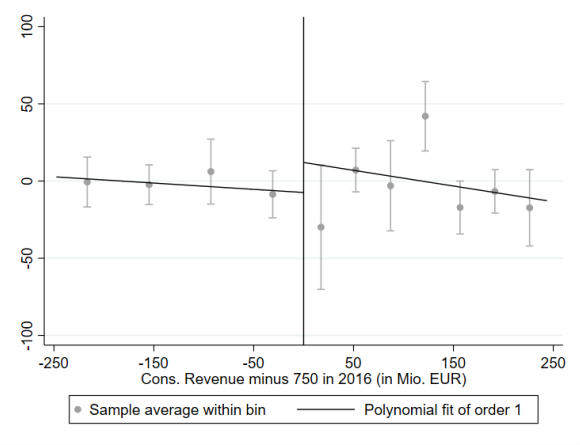



**European Countries with Preferential Tax Regimes**

(3) *Capital Investment*$_{f,c,t}$ *(%)*

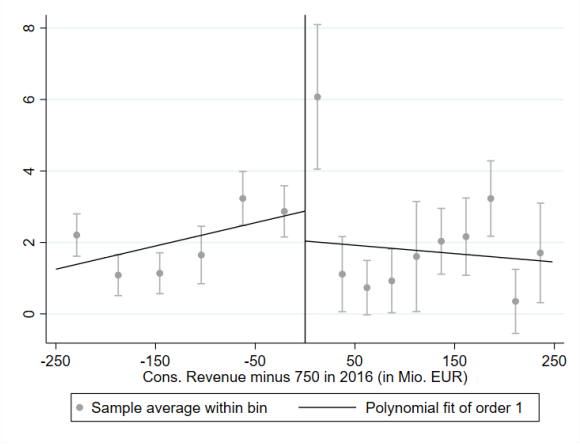



(4) *Labor Investment*$_{f,c,t}$ *(%)*

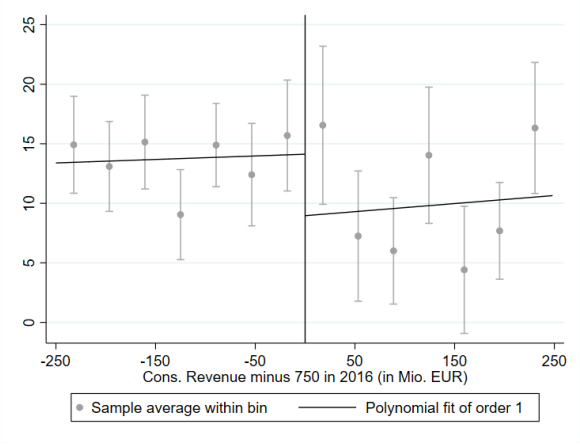



**Rest of Europe**

(5) *Capital Investment*$_{f,c,t}$ *(%)*




(6) *Labor Investment*$_{f,c,t}$ *(%)*




*Notes:* These graphs plot average values and their 95% confidence intervals of different firm-country-level outcome variables based on unconsolidated financial information of subsidiaries for evenly spaced bins of the firm's consolidated 2016 revenue. The graphs show linear trends. Graphs (1)-(2) are based on the full sample of subsidiaries in Europe, graphs (3)-(4) are based on the sub-sample of European countries with preferential tax regimes (Cyprus, Ireland, Luxembourg, Malta, Netherlands, Switzerland), and graphs (5)-(6) are based on all other European countries.

Table 1: Sample Construction

| Panel A: Sample Selection Steps | | Unique Firms (MNEs) | | Unique Subsidiaries | |
|---|---|---|---|---|---|
| | | Obs. | % Loss | Obs. | % Loss |
| (1) | *Firms with consolidated financial statements, at least EUR 100 million revenue in any year 2012-2018, at least one foreign subsidiary according to BvD Orbis database as of March 2020* | 53,575 | | | |
| (2) | *Matching historical subsidiary information to multinational firms identified as ultimate corporate owners incorporated in the EU or with at least one subsidiary in the EU in 2016* | 29,182 | 45.53% | 912,452 | |
| (3) | *Keeping subsidiaries located in Europe in 2016* | 19,553 | 33.00% | 344,550 | 62.24% |

| Panel B: Regression Samples | | |
|---|---|---|
| Difference-in-Differences Subsidiaries Sample (2012-2018) | | Subsidiary-Year Obs. |
| (4) | *Non-missing unconsolidated financial information* | 687,406 |
| Regression Discontinuity Sample (2016-2018) | | Firm-Country-Year Obs. |
| (5) | *Aggregating unconsolidated subsidiary information* | 88,269 |
| | | Firm-Year Obs. |
| (6) | *Consolidated firm-level and ownership information* | 52,316 |

*Notes:* This table presents the procedure to construct the final regression samples. Sample selection steps (1) and (2) rely on information on multinational firms with consolidated statements and their worldwide subsidiaries according to ownership information taken on an annual basis from the Bureau van Dijk Orbis database. Step (3) reports how many subsidiaries are located in Europe. Step (4) uses unconsolidated financial statement information on subsidiaries in Europe in the period 2012-2018. Step (5) uses this information on an aggregate basis by multinational firm and European country where one or more subsidiaries of that firm are incorporated. Step (6) uses information at the multinational firm-year level on multinationals from all over the globe that are potentially affected by CbCR due to being incorporated in the EU or having at least one subsidiary in the EU.

Table 2: European Subsidiary Observations by Year

| *Year* | 2012 | | 2013 | | 2014 | | 2015 | | 2016 | | 2017 | | 2018 | | Total | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *Country* | No. | % | No. | % | No. | % | No. | % | No. | % | No. | % | No. | % | No. | % |
| Austria | 2,764 | 3.1 | 2,639 | 2.8 | 2,546 | 2.7 | 2,773 | 2.8 | 2,822 | 2.8 | 2,889 | 2.7 | 2,640 | 2.6 | 19,073 | 2.8 |
| Belgium | 3,168 | 3.6 | 3,446 | 3.6 | 4,072 | 4.3 | 4,135 | 4.1 | 4,267 | 4.2 | 4,319 | 4.1 | 4,129 | 4.1 | 27,536 | 4.0 |
| Bulgaria | 366 | 0.4 | 508 | 0.5 | 490 | 0.5 | 515 | 0.5 | 526 | 0.5 | 476 | 0.5 | 630 | 0.6 | 3,511 | 0.5 |
| Croatia | 409 | 0.5 | 525 | 0.5 | 545 | 0.6 | 578 | 0.6 | 531 | 0.5 | 563 | 0.5 | 474 | 0.5 | 3,625 | 0.5 |
| Cyprus | 30 | 0.0 | 58 | 0.1 | 63 | 0.1 | 62 | 0.1 | 57 | 0.1 | 56 | 0.1 | 37 | 0.0 | 363 | 0.1 |
| Czech Republic | 1,813 | 2.1 | 1,779 | 1.9 | 1,850 | 1.9 | 2,074 | 2.1 | 2,050 | 2.0 | 2,010 | 1.9 | 1,617 | 1.6 | 13,193 | 1.9 |
| Denmark | 1,996 | 2.3 | 2,102 | 2.2 | 1,919 | 2.0 | 2,093 | 2.1 | 2,436 | 2.4 | 2,501 | 2.4 | 2,457 | 2.4 | 15,504 | 2.3 |
| Estonia | 548 | 0.6 | 546 | 0.6 | 527 | 0.6 | 535 | 0.5 | 536 | 0.5 | 524 | 0.5 | 508 | 0.5 | 3,724 | 0.5 |
| Finland | 1,870 | 2.1 | 2,098 | 2.2 | 2,124 | 2.2 | 2,137 | 2.1 | 2,175 | 2.1 | 2,158 | 2.1 | 2,281 | 2.3 | 14,843 | 2.2 |
| France | 12,882 | 14.6 | 13,236 | 13.8 | 14,385 | 15.1 | 15,219 | 15.3 | 14,230 | 13.9 | 14,081 | 13.4 | 12,370 | 12.2 | 96,403 | 14.0 |
| Germany | 8,736 | 9.9 | 8,968 | 9.4 | 8,393 | 8.8 | 9,193 | 9.2 | 9,419 | 9.2 | 9,652 | 9.2 | 8,633 | 8.5 | 62,994 | 9.2 |
| Greece | 246 | 0.3 | 307 | 0.3 | 283 | 0.3 | 307 | 0.3 | 318 | 0.3 | 302 | 0.3 | 659 | 0.7 | 2,422 | 0.4 |
| Hungary | 651 | 0.7 | 724 | 0.8 | 848 | 0.9 | 964 | 1.0 | 875 | 0.9 | 1,025 | 1.0 | 1,006 | 1.0 | 6,093 | 0.9 |
| Iceland | 86 | 0.1 | 113 | 0.1 | 113 | 0.1 | 109 | 0.1 | 117 | 0.1 | 112 | 0.1 | 120 | 0.1 | 770 | 0.1 |
| Ireland | 1,102 | 1.3 | 1,380 | 1.4 | 1,188 | 1.2 | 1,399 | 1.4 | 1,568 | 1.5 | 1,618 | 1.5 | 1,601 | 1.6 | 9,856 | 1.4 |
| Italy | 7,112 | 8.1 | 9,059 | 9.5 | 8,509 | 8.9 | 9,095 | 9.1 | 9,469 | 9.2 | 9,585 | 9.1 | 9,018 | 8.9 | 61,847 | 9.0 |
| Latvia | 392 | 0.4 | 444 | 0.5 | 567 | 0.6 | 615 | 0.6 | 581 | 0.6 | 555 | 0.5 | 528 | 0.5 | 3,682 | 0.5 |
| Lithuania | 282 | 0.3 | 282 | 0.3 | 288 | 0.3 | 294 | 0.3 | 316 | 0.3 | 326 | 0.3 | 315 | 0.3 | 2,103 | 0.3 |
| Luxembourg | 413 | 0.5 | 497 | 0.5 | 508 | 0.5 | 657 | 0.7 | 722 | 0.7 | 719 | 0.7 | 720 | 0.7 | 4,236 | 0.6 |
| Malta | 159 | 0.2 | 194 | 0.2 | 238 | 0.2 | 257 | 0.3 | 295 | 0.3 | 316 | 0.3 | 120 | 0.1 | 1,579 | 0.2 |
| Netherlands | 4,781 | 5.4 | 5,414 | 5.7 | 5,917 | 6.2 | 6,198 | 6.2 | 6,748 | 6.6 | 6,537 | 6.2 | 5,749 | 5.7 | 41,344 | 6.0 |
| Norway | 2,646 | 3.0 | 4,453 | 4.7 | 4,016 | 4.2 | 4,129 | 4.1 | 4,139 | 4.0 | 4,202 | 4.0 | 4,759 | 4.7 | 28,344 | 4.1 |
| Poland | 2,694 | 3.1 | 2,944 | 3.1 | 3,164 | 3.3 | 3,377 | 3.4 | 3,370 | 3.3 | 3,394 | 3.2 | 3,413 | 3.4 | 22,356 | 3.3 |
| Portugal | 1,091 | 1.2 | 1,041 | 1.1 | 1,236 | 1.3 | 1,277 | 1.3 | 1,282 | 1.3 | 1,325 | 1.3 | 1,032 | 1.0 | 8,284 | 1.2 |
| Romania | 1,476 | 1.7 | 1,405 | 1.5 | 1,579 | 1.7 | 1,695 | 1.7 | 1,715 | 1.7 | 1,776 | 1.7 | 1,690 | 1.7 | 11,336 | 1.6 |
| Slovak Republic | 657 | 0.7 | 660 | 0.7 | 925 | 1.0 | 1,042 | 1.0 | 1,083 | 1.1 | 1,101 | 1.0 | 1,076 | 1.1 | 6,544 | 1.0 |
| Slovenia | 263 | 0.3 | 266 | 0.3 | 310 | 0.3 | 304 | 0.3 | 294 | 0.3 | 311 | 0.3 | 295 | 0.3 | 2,043 | 0.3 |
| Spain | 5,994 | 6.8 | 5,941 | 6.2 | 5,972 | 6.3 | 6,091 | 6.1 | 6,503 | 6.4 | 6,736 | 6.4 | 6,199 | 6.1 | 43,436 | 6.3 |
| Sweden | 5,243 | 6.0 | 5,564 | 5.8 | 5,786 | 6.1 | 6,169 | 6.2 | 6,739 | 6.6 | 6,984 | 6.6 | 6,884 | 6.8 | 43,369 | 6.3 |
| Switzerland | 123 | 0.1 | 128 | 0.1 | 131 | 0.1 | 130 | 0.1 | 133 | 0.1 | 127 | 0.1 | 113 | 0.1 | 885 | 0.1 |
| United Kingdom | 17,950 | 20.4 | 18,872 | 19.7 | 17,008 | 17.8 | 16,306 | 16.4 | 17,088 | 16.7 | 18,878 | 18.0 | 20,006 | 19.8 | 126,108 | 18.3 |
| Total | 87,943 | 100.0 | 95,593 | 100.0 | 95,500 | 100.0 | 99,729 | 100.0 | 102,404 | 100.0 | 105,158 | 100.0 | 101,079 | 100.0 | 687,406 | 100.0 |

*Notes:* This table presents the distribution of observations of firms by their subsidiaries' countries of incorporation and year in Europe conditional on these subsidiaries' unconsolidated financial information being available in the Orbis database.

Table 3: Summary Statistics

| | Obs. | Mean | SD | P25 | Median | P75 |
|---|---|---|---|---|---|---|
| Panel A: Variables for Difference-in-Differences Analysis (2012-2018) | | | | | | |
| ***Outcome variables*** | | | | | | |
| *Capital Investment$_{i,t}$ (%)* | 687,406 | 1.03 | 11.10 | -0.84 | 0.00 | 0.16 |
| *Labor Investment$_{i,t}$ (%)* | 413,073 | 9.38 | 49.79 | -5.20 | 3.04 | 12.60 |
| *Cons. Capital Investment$_{f,t}$ (%)* | 49,441 | 1.21 | 6.50 | -0.90 | 0.14 | 2.11 |
| *Cons. Labor Investment$_{f,t}$ (%)* | 41,892 | 10.00 | 34.95 | -1.67 | 5.01 | 13.53 |
| ***Independent Variables*** | | | | | | |
| *Treated$_{f}$* | 687,406 | 58.05 | 49.35 | 0.00 | 100.00 | 100.00 |
| *Log. Total Assets$_{i,t-1}$* | 687,406 | 15.83 | 2.77 | 14.36 | 15.86 | 17.47 |
| *Log. Employees$_{i,t-1}$* | 424,817 | 3.67 | 1.94 | 2.30 | 3.64 | 4.91 |
| *Leverage$_{i,t-1}$* | 678,298 | 81.43 | 166.49 | 32.56 | 62.99 | 88.65 |
| *Log. Total Assets$_{f,t}$* | 671,779 | 21.27 | 2.52 | 19.28 | 21.42 | 23.25 |
| *Log. Employees$_{f,t}$* | 617,375 | 8.57 | 2.38 | 6.76 | 8.90 | 10.71 |
| *Log. Cash$_{f,t}$* | 670,614 | 18.60 | 2.59 | 16.76 | 18.78 | 20.78 |
| *ROA$_{f,t}$* | 664,170 | 4.73 | 54.65 | 2.05 | 4.98 | 8.59 |
| *Leverage$_{f,t}$* | 666,803 | 19.37 | 17.91 | 7.00 | 16.98 | 27.48 |
| *Intangible Ratio$_{f,t}$* | 670,608 | 19.73 | 20.15 | 2.55 | 12.34 | 33.17 |
| *ETR$_{f,t}$* | 662,406 | 26.12 | 20.90 | 14.04 | 24.39 | 32.76 |
| *Log. GDP$_{f,t}$* | 685,298 | 28.26 | 1.32 | 27.09 | 28.58 | 28.87 |
| *CIT$_{f,t}$* | 687,390 | 28.34 | 7.69 | 22.00 | 29.58 | 34.43 |
| Panel B: Variables for Regression Discontinuity Analysis (2016-2018) | | | | | | |
| ***Outcome variables*** | | | | | | |
| *Capital Investment$_{f,c,t}$ (%)* | 88,269 | 1.87 | 9.58 | -1.41 | -0.05 | 2.01 |
| *Labor Investment$_{f,c,t}$ (%)* | 77,041 | 11.40 | 56.70 | -9.64 | 5.43 | 23.57 |
| *Cons. Capital Investment$_{f,t}$ (%)* | 46,405 | 2.10 | 9.87 | -1.31 | 0.26 | 3.59 |
| *Cons. Labor Investment$_{f,t}$ (%)* | 37,200 | 17.49 | 52.04 | -3.55 | 7.37 | 23.21 |
| *Log. Preferential Tax Regime Subs.$_{f,t}$* | 52,316 | 0.42 | 0.80 | 0.00 | 0.00 | 0.69 |
| *Log. Tax Haven Subs. (any list)$_{f,t}$* | 52,316 | 0.43 | 0.81 | 0.00 | 0.00 | 0.69 |
| *Log. Subs.$_{f,t}$* | 52,316 | 2.16 | 1.43 | 0.69 | 1.95 | 3.04 |
| *Log. Org. Complexity$_{f,t}$* | 52,316 | 2.49 | 2.06 | 0.00 | 2.48 | 3.89 |
| ***Pre-treatment Control Variables*** | | | | | | |
| *Log. Total Assets$_{f,c,2015}$* | 88,269 | 16.99 | 2.48 | 15.24 | 16.87 | 18.57 |
| *Log. Employees$_{f,c,2015}$* | 88,269 | 4.07 | 2.34 | 2.48 | 4.14 | 5.67 |
| *Leverage$_{f,c,2015}$* | 88,269 | 64.97 | 55.97 | 37.94 | 58.19 | 78.87 |
| *Log. Total Assets$_{f,2015}$* | 47,890 | 18.89 | 2.17 | 17.37 | 18.62 | 20.28 |
| *Log. Employees$_{f,2015}$* | 40,924 | 6.42 | 2.07 | 5.01 | 6.26 | 7.78 |
| *Log. Cash$_{f,2015}$* | 47,674 | 16.39 | 2.39 | 14.76 | 16.27 | 18.01 |
| *ROA$_{f,2015}$* | 47,491 | 2.40 | 17.56 | 0.31 | 4.57 | 9.48 |
| *Leverage$_{f,2015}$* | 45,769 | 14.96 | 17.19 | 0.71 | 9.60 | 22.50 |
| *Intangible Ratio$_{f,2015}$* | 47,630 | 12.21 | 17.90 | 0.50 | 3.47 | 16.70 |
| *ETR$_{f,2015}$* | 46,702 | 23.92 | 21.75 | 6.17 | 22.49 | 32.42 |
| *Publicly Listed$_{f,2015}$* | 50,969 | 43.27 | 49.54 | 0.00 | 0.00 | 100.00 |
| *Brand Awareness$_{f,2015}$* | 52,191 | 2.20 | 14.68 | 0.00 | 0.00 | 0.00 |
| *Log. GDP$_{f,2015}$* | 50,745 | 27.95 | 1.35 | 26.95 | 28.24 | 28.71 |
| *CIT$_{f,2015}$* | 50,966 | 26.96 | 7.43 | 20.00 | 28.00 | 31.29 |

*Notes:* This table presents summary statistics for all variables included in the regression analyses. All variables are defined in Appendix A. We multiply growth measures and ratios by 100 to allow an interpretation in percentage terms.

Table 4: CbCR and Subsidiary-Level Economic Activity - Difference-in-Differences Analysis

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | All Europe | | Preferential Tax Regimes | | Rest of Europe | |
| Panel A | *Capital Investment$_{i,t}$ (%)* | | | | | |
| *Treated$_f$ * Post$_t$* | 0.51*** | 0.29*** | 2.22*** | 1.85*** | 0.21** | 0.22*** |
| | (5.35) | (3.40) | (3.86) | (3.85) | (2.36) | (2.60) |
| Obs. | 687,406 | 374,077 | 57,873 | 26,176 | 629,027 | 347,441 |
| Adj. R2 | 0.026 | 0.035 | 0.055 | 0.058 | 0.027 | 0.036 |
| Panel B | *Labor Investment$_{i,t}$ (%)* | | | | | |
| *Treated$_f$ * Post$_t$* | 1.33*** | 1.44*** | 5.15** | 5.18* | 1.30*** | 1.44*** |
| | (3.16) | (3.21) | (1.99) | (1.90) | (3.07) | (3.17) |
| Obs. | 413,073 | 313,679 | 11,733 | 9,766 | 400,968 | 303,583 |
| Adj. R2 | 0.034 | 0.051 | 0.079 | 0.103 | 0.033 | 0.050 |
| Sub. Controls | No | Yes | No | Yes | No | Yes |
| Firm Controls | No | Yes | No | Yes | No | Yes |
| Firm Ctry Controls | No | Yes | No | Yes | No | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | No | Yes | No | Yes | No |
| Country-Year FE | No | Yes | No | Yes | No | Yes |

*Notes:* This table presents the results of estimating the effect of CbCR on subsidiary-level economic activity based on unconsolidated financial information of subsidiaries located in Europe using a difference-in-differences OLS regression design. The sample includes European subsidiaries of multinational firms outside the financial services sector from parent countries around the world. The outcome variables are the change in subsidiary unconsolidated fixed tangible assets scaled by lagged total assets in Panel (A) and the change in subsidiary unconsolidated labor expenses scaled by lagged labor expenses in Panel (B). In Columns (1) and (2), we use the full sample of subsidiary observations in all European countries. In Columns (3) and (4), we only include observations from European countries with preferential tax regimes (Switzerland, Cyprus, Ireland, Luxembourg, Malta, and the Netherlands). In Columns (5) and (6), we only include observations from all other European countries. *Treated$_f$* is an indicator variable taking on the value of 1 for subsidiaries of firms reporting more than €750 million in 2016 revenue. *Post$_t$* is an indicator variable taking on the value of 1 for the years 2016 to 2018. Subsidiary controls include the natural logarithms of lagged total assets and employees as well as lagged leverage. Multinational firm controls include the natural logarithms of total assets, the number of employees, and cash, return on assets, leverage, intangible ratio, and the effective tax rate. Firm country controls include the natural logarithm of GDP as well as the corporate income tax rate in the multinational firm's country of incorporation. Robust standard errors are clustered at the multinational firm level. T-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Table 5: Cross-sectional Tests Exploiting Heterogeneity in Country-Specific Tax Rules and Levels of Tax Avoidance

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Preferential Tax Regimes | | | | | |
| Panel A | *Capital Investment$_{i,t}$ (%)* | | | | | |
| | *Low Exit Taxes$_{f,2015}$* | | *Low ETR$_{f,2015}$* | | *Tax Haven Firm$_{f,2015}$* | |
| | Yes | No | Yes | No | Yes | No |
| *Treated$_f$ * Post$_t$* | 3.02*** | -0.40 | 3.73*** | 1.95** | 2.44*** | -0.60 |
| | (4.78) | (-0.58) | (4.97) | (2.53) | (3.71) | (-0.58) |
| $\Delta$ Treated$_f$ * Post$_t$ | *3.42**** | | *1.78** | | *3.04*** | |
| | *(3.66)* | | *(1.65)* | | *(2.48)* | |
| Obs. | 44,760 | 10,035 | 20,635 | 27,734 | 43,227 | 11,668 |
| Adj. R2 | 0.053 | 0.057 | 0.066 | 0.040 | 0.055 | 0.040 |
| Panel B | *Labor Investment$_{i,t}$ (%)* | | | | | |
| | *Low Exit Taxes$_{f,2015}$* | | *Low ETR$_{f,2015}$* | | *Tax Haven Firm$_{f,2015}$* | |
| | Yes | No | Yes | No | Yes | No |
| *Treated$_f$ * Post$_t$* | 7.17** | -2.23 | 8.82* | -1.25 | 6.68* | 0.39 |
| | (2.48) | (-0.36) | (1.78) | (-0.42) | (1.85) | (0.08) |
| $\Delta$ Treated$_f$ * Post$_t$ | *9.40* | | *10.07** | | *6.29* | |
| | *(1.37)* | | *(1.74)* | | *(1.05)* | |
| Obs. | 8,728 | 2,480 | 4,383 | 5,637 | 8,989 | 2,262 |
| Adj. R2 | 0.080 | 0.073 | 0.042 | 0.081 | 0.053 | 0.196 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* This table presents the results of estimating cross-sectional differences in the effect of CbCR on subsidiary-level economic activity using a difference-in-differences OLS regression design. The sample includes subsidiaries located in European countries with preferential tax regimes (Cyprus, Ireland, Luxembourg, Malta, Netherlands, Switzerland) owned by multinational firms outside the financial services sector from parent countries around the world. The outcome variables are the change in subsidiary unconsolidated fixed tangible assets scaled by lagged total assets in Panel (A) and the change in subsidiary unconsolidated labor expenses scaled by lagged labor expenses in Panel (B). We split the sample into different sub-samples based on the MNE firms' characteristics as of 2015 (i.e., the year before CbCR became effective). In Columns (1) and (2), we partition the sample by sample median exposure to countries with exit tax regimes outside the set of preferential tax regime countries based on the share of a firm's subsidiaries' fixed tangible assets located in these countries in 2015. In Columns (3) and (4), we partition the sample by country-industry median ETR in 2015. In Columns (5) and (6), we partition the sample by ownership of at least one tax haven subsidiary according to any of the classifications used in Bennedsen and Zeume (2018) but outside the set of preferential tax regime countries in 2015. *Treated$_f$* is an indicator variable taking on the value of one for subsidiaries of firms reporting more than €750 million in 2016 revenue. *Post$_t$* is an indicator variable taking on the value of 1 for the years 2016 to 2018. We obtain the differences in coefficients in *Treated$_f$ * Post$_t$* across the sample splits from auxiliary regressions based on the full sample and report the triple differences coefficient below the sample splits. Robust standard errors are clustered at the multinational firm level. T-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Table 6: CbCR and Firm-Country-Level Economic Activity - Regression Discontinuity Analysis

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | All Europe | | Preferential Tax Regimes | | Rest of Europe | |
| Panel A | *Capital Investment$_{f,c,t}$ (%)* | | | | | |
| RD estimate | -0.71 | -0.30 | 1.43* | 1.55* | -1.05** | -0.61 |
| | (-1.56) | (-0.62) | (1.68) | (1.77) | (-2.12) | (-1.17) |
| Orig. Obs. | 88,269 | 76,390 | 7,983 | 7,008 | 80,286 | 69,382 |
| Bandwidth | 275 | 275 | 782 | 782 | 250 | 250 |
| Obs. in Bandwidth | 9,212 | 8,249 | 4,262 | 3,533 | 7,576 | 6,802 |
| Panel B | *Labor Investment$_{f,c,t}$ (%)* | | | | | |
| RD estimate | -4.08** | -0.72 | 17.63*** | 18.94*** | -4.69* | -0.68 |
| | (-1.99) | (-0.34) | (3.11) | (3.11) | (-1.94) | (-0.27) |
| Orig. Obs. | 77,041 | 67,023 | 4,511 | 3,974 | 72,530 | 63,049 |
| Bandwidth | 428 | 428 | 1,034 | 1,034 | 334 | 334 |
| Obs. in Bandwidth | 13,837 | 12,383 | 2,304 | 1,870 | 9,542 | 8,504 |
| Polynomials | Linear | Linear | Linear | Linear | Linear | Linear |
| Controls | No | Sub., Firm, Firm Ctry | No | Sub., Firm, Firm Ctry | No | Sub., Firm, Firm Ctry |
| Fixed Effects | No | Yr | No | Yr | No | Yr |

*Notes:* This table presents results of estimating the effect of CbCR on firm-country-level economic activity based on unconsolidated financial information of subsidiaries located in Europe using a regression discontinuity design. We aggregate data of a multinational firm's subsidiaries in the same country in a given year. The outcome variables are a firm's subsidiaries' change in fixed tangible fixed assets since 2015 scaled by total assets in 2015 (Panel A) and a firm's subsidiaries' change in labor expenses since 2015 scaled by labor expenses in 2015 (Panel B). In Columns (1) and (2), we use the full sample of observations in all European countries. In Columns (3) and (4), we only include observations from European countries with preferential tax regmies (Switzerland, Cyprus, Ireland, Luxembourg, Malta, and the Netherlands). In Columns (5) and (6), we only include observations from all other European countries. Columns (1), (3), and (5) use nonparametric local linear regressions with mean-squared-error optimal bandwidths following Calonico et al. (2014) without controls. Columns (2), (4), and (6) add year fixed effects and pre-treatment controls (i.e., as of 2015). Subsidiary controls include the natural logarithms of firm-country lagged total assets and employees, and firm-country lagged leverage. Firm controls include the number of employees, cash, return on assets, leverage, intangible ratio, the effective tax rate, an indicator variable equal to one if a firm is publicly listed, and an indicator variable equal to one if a firm appears in the Global 500 list of valuable consumer brands. Firm country controls include the natural logarithm of GDP as well as the corporate income tax rate in the multinational firm's country of incorporation. We calculate bias-corrected standard errors following Calonico et al. (2014). T-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Table 7: CbCR and MNEs' Consolidated Investment and Investment Efficiency

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Firm-level Investment (RDD) | | | |
| Panel A | *Cons. Capital Investment$_{f,t}$ (%)* | | | |
| RD estimate | 0.41 | 0.84 | 1.26 | 1.44 |
| | (0.69) | (1.28) | (1.21) | (1.16) |
| Orig. Obs. | 46,405 | 36,922 | 46,405 | 36,922 |
| Bandwidth | 242 | 242 | 374 | 374 |
| Obs. in Bandwidth | 3,736 | 3,154 | 6,155 | 5,234 |
| Panel B | *Cons. Labor Investment$_{f,t}$ (%)* | | | |
| RD estimate | -0.82 | 6.03** | -9.40 | -4.57 |
| | (-0.24) | (2.25) | (-1.53) | (-0.98) |
| Orig. Obs. | 37,200 | 30,107 | 37,200 | 30,107 |
| Bandwidth | 358 | 358 | 416 | 416 |
| Obs. in Bandwidth | 4,476 | 3,889 | 5,395 | 4,662 |
| Polynomials | Linear | Linear | 3rd-order | 3rd-order |
| Controls | No | Firm, Firm Ctry | No | Firm, Firm Ctry |
| Fixed Effects | No | Yr | No | Yr |
| | Firm-level Investment Efficiency (DiD) | | | |
| Panel C | *Cons. Capital Investment$_{f,t}$ (%)* | | *Cons. Labor Investment$_{f,t}$ (%)* | |
| *Treated$_f$ * Post$_t$ * Q(median)$_{f,t}$* | -0.13** | -0.12* | -0.10 | 0.12 |
| | (-2.06) | (-1.74) | (-0.39) | (0.80) |
| *Q(median)$_{f,t}$* | 0.13*** | 0.16 | 0.23 | 0.62* |
| | (2.71) | (1.33) | (1.24) | (1.83) |
| *Treated$_f$ * Post$_t$* | 0.17 | 0.10 | 0.85 | 1.06 |
| | (1.07) | (0.33) | (0.93) | (0.59) |
| Obs. | 49,441 | 10,364 | 41,819 | 8,372 |
| Bandwidth | Full Sample | 500 | Full Sample | 500 |
| Adj. R2 | 0.158 | 0.186 | 0.137 | 0.093 |
| Controls | Firm, Ctry | Firm, Ctry | Firm, Ctry | Firm, Ctry |
| Fixed Effects | Firm, Yr | Firm, Yr | Firm, Yr | Firm, Yr |

*Notes:* see next page.

Table 7: CbCR and MNEs' Consolidated Investment and Investment Efficiency (*continued*)

*Notes:* This table presents results of estimating the effect of CbCR on multinational firms' consolidated investment outcomes using a regression discontinuity design (Panels A and B) and a differences-in-differences OLS design (Panel C). The sample includes multinational firms outside the financial services sector from around the world but falling under the EU's CbCR regime based on their operations in Europe. The outcome variables are the change in multinational firm parent entity consolidated fixed tangible assets since 2015 scaled by lagged consolidated total assets in 2015 in Panel (A) and the change in multinational firm parent entity consolidated labor expenses since 2015 scaled by lagged consolidated labor expenses in 2015 in Panel (B). In Panel C, the investment measures are defined as annual changes. In Panels A and B, Columns (1) and (2) use nonparametric local linear regressions while Columns (3) and (4) use nonparametric local third-order polynomial regressions with mean-squared-error optimal bandwidths following Calonico et al. (2014). Columns (2) and (4) add year fixed effects and pre-treatment controls (i.e., as of 2015). Firm controls include the number of employees, cash, return on assets, leverage, intangible ratio, the effective tax rate, an indicator variable equal to one if a firm is publicly listed, and an indicator variable equal to one if a firm appears in the Global 500 list of valuable consumer brands. Firm country controls include the natural logarithm of the GDP as well as the corporate income tax rate in the multinational firm's country of incorporation. We calculate bias-corrected standard errors following Calonico et al. (2014). T-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively. In Panel C, the difference-in-differences indicator variable is interacted with a Tobin's Q measure defined as the median country-industry-year ratio of market value of equity plus consolidated assets less the book value of equity to consolidated total assets. Results reported in odd-numbered columns are based on the full sample of MNEs. Results reported in even-numbered columns are based on MNEs reporting €250 to €1,250 million in 2016 revenue. Firm controls include the number of employees, cash, return on assets, leverage, intangible ratio, the effective tax rate in a given year. Firm country controls include the natural logarithm of the GDP as well as the corporate income tax rate in the multinational firm's country of incorporation. Robust standard errors are clustered at the multinational firm level. T-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Table 8: CbCR and MNEs' Organizational Structures - Regression Discontinuity Analysis

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Panel A | *Log. Preferential Tax Regime Subs.*$_{f,t}$ | | | |
| RD estimate | -0.12** | -0.17*** | -0.16** | -0.22*** |
| | (-2.58) | (-3.91) | (-2.56) | (-3.42) |
| Orig. Obs. | 52,316 | 37,888 | 52,316 | 37,888 |
| Bandwidth | 222 | 222 | 387 | 387 |
| Obs. in Bandwidth | 3,709 | 2,957 | 7,053 | 5,640 |
| Panel B | *Log. Tax Haven Subs. (any list)*$_{f,t}$ | | | |
| RD estimate | -0.20*** | -0.24*** | -0.26*** | -0.30*** |
| | (-4.45) | (-5.19) | (-3.85) | (-4.44) |
| Orig. Obs. | 52,316 | 37,888 | 52,316 | 37,888 |
| Bandwidth | 210 | 210 | 302 | 302 |
| Obs. in Bandwidth | 3,471 | 2,757 | 5,133 | 4,128 |
| Panel C | *Log. Subs.*$_{f,t}$ | | | |
| RD estimate | -0.13 | -0.38*** | -0.31** | -0.44*** |
| | (-1.47) | (-4.43) | (-2.08) | (-2.73) |
| Orig. Obs. | 52,316 | 37,888 | 52,316 | 37,888 |
| Bandwidth | 221 | 221 | 284 | 284 |
| Obs. in Bandwidth | 3,685 | 2,936 | 4,770 | 3,814 |
| Panel D | *Log. Org. Complexity*$_{f,t}$ | | | |
| RD estimate | -0.13 | -0.58*** | -0.47** | -0.80*** |
| | (-1.09) | (-4.61) | (-2.08) | (-3.24) |
| Orig. Obs. | 52,316 | 37,888 | 52,316 | 37,888 |
| Bandwidth | 239 | 239 | 284 | 284 |
| Obs. in Bandwidth | 3,977 | 3,173 | 4,761 | 3,805 |
| Polynomials | Linear | Linear | 3rd-order | 3rd-order |
| Controls | No | Firm, Firm Ctry | No | Firm, Firm Ctry |
| Fixed Effects | No | Yr | No | Yr |

*Notes:* This table presents results of estimating the effect of CbCR on multinational firms' organizational structures using a regression discontinuity design. The sample includes multinational firms outside the financial services sector from around the world but falling under the EU's CbCR regime based on their operations in Europe. The outcome variables are the natural logarithm of the number of subsidiaries a firm has in a preferential tax regime country in Europe (Panel A) and the natural logarithm of a firm's number of subsidiaries in tax havens according to any classification used in Bennedsen and Zeume (2018) (Panel B). In Panel C, the outcome variable is the natural logarithm of a firm's total number of majority-owned subsidiaries. In Panel D, the outcome variable is the natural logarithm of the sum of hierarchical subsidiary levels of majority-owned subsidiaries. Columns (1) and (2) use nonparametric local linear regressions while Columns (3) and (4) use nonparametric local third-order polynomial regressions with mean-squared-error optimal bandwidths following Calonico et al. (2014). Columns (2) and (4) add year fixed effects and pre-treatment controls (i.e., as of 2015). Firm controls include the number of employees, cash, return on assets, leverage, intangible ratio, the effective tax rate, an indicator variable equal to one if a firm is publicly listed, and an indicator variable equal to one if a firm appears in the Global 500 list of valuable consumer brands. Firm country controls include the natural logarithm of the GDP as well as the corporate income tax rate in the multinational firm's country of incorporation. We calculate bias-corrected standard errors following Calonico et al. (2014). T-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Table 9: Robustness: Difference-in-Differences Analysis of Subsidiary-Level Economic Activity

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | All Europe | | Preferential Tax Regimes | | Rest of Europe | |
| | *Capital Investment$_{i,t}$ (%)* | *Labor Investment$_{i,t}$ (%)* | *Capital Investment$_{i,t}$ (%)* | *Labor Investment$_{i,t}$ (%)* | *Capital Investment$_{i,t}$ (%)* | *Labor Investment$_{i,t}$ (%)* |
| Panel A | *Placebo event year: 2013* | | | | | |
| *Treated$_f$ * Post$_t$* | 0.05 | 0.82 | -0.07 | -2.78 | 0.05 | 0.92 |
| | (0.40) | (1.42) | (-0.18) | (-0.84) | (0.42) | (1.57) |
| Obs. | 378,265 | 229,649 | 30,561 | 6,394 | 347,193 | 222,840 |
| Adj. R2 | 0.033 | 0.043 | 0.031 | 0.091 | 0.034 | 0.041 |
| Panel B | *Placebo threshold: EUR 375 million* | | | | | |
| *Treated$_f$ * Post$_t$* | 0.42*** | 0.91 | 0.57 | 2.74 | 0.41*** | 0.80 |
| | (2.67) | (1.16) | (0.68) | (0.59) | (2.64) | (1.00) |
| Obs. | 288,339 | 168,780 | 18,015 | 3,786 | 269,956 | 164,804 |
| Adj. R2 | 0.029 | 0.047 | 0.045 | 0.156 | 0.029 | 0.045 |
| Panel C | *Anticipation test (first-time treated in 2018)* | | | | | |
| *Treated$_f$ * Post$_t$* | -0.01 | -0.91 | -0.97 | -7.72 | 0.04 | -0.73 |
| | (-0.06) | (-0.86) | (-0.79) | (-1.61) | (0.20) | (-0.68) |
| Obs. | 586,159 | 353,592 | 49,513 | 10,092 | 536,159 | 343,139 |
| Adj. R2 | 0.028 | 0.037 | 0.062 | 0.082 | 0.029 | 0.036 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* This table presents the results of falsification tests using the difference-in-differences model based on Table 4. The sample includes European subsidiaries of multinational firms outside the financial services sector from parent countries around the world. The outcome variables are the change in subsidiary unconsolidated fixed tangible assets scaled by lagged total assets in odd-numbered columns and the change in subsidiary unconsolidated labor expenses scaled by lagged labor expenses in even-numbered columns. In Columns (1) and (2), we use the full sample of subsidiary observations in all European countries. In Columns (3) and (4), we only include observations from European countries with preferential tax regmies (Switzerland, Cyprus, Ireland, Luxembourg, Malta, and the Netherlands). In Columns (5) and (6), we only include observations from all other European countries. Panel (A) uses 2013 as the falsified reform year and only uses observations on outcome variables before 2016. Panel (B) uses a falisified size threshold of €375 million (50 percent of the actual threshold) to determine the treatment indicator and only uses observations from untreated firms. In Panel (C), the treatment indicator takes on the value of one if a firm reports above €750 in revenue for the first time in 2018. Panel (C) only uses observations on outcome variables before 2018. Robust standard errors are clustered at the multinational firm level. T-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Table 10: Robustness: Regression Discontinuity Analysis of MNEs' Organizational Structures

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Falsification Tests | | | Manipulation Test | Bandwidth Sensitivity | |
| | RV in 2013 | RV 375 | Outcomes in 2015 | No Firms RV [-10;0] | 75% of Baseline | 150% of Baseline |
| Panel A | *Log. Preferential Tax Regime Subs.*$_{f,t}$ | | | | | |
| RD estimate | -0.05 | 0.14*** | 0.03 | -0.23*** | -0.18*** | -0.11*** |
| | (-0.96) | (3.46) | (0.35) | (-4.07) | (-3.52) | (-3.03) |
| Orig. Obs. | 40,236 | 52,316 | 17,281 | 52,206 | 52,316 | 52,316 |
| Bandwidth | 333 | 123 | 287 | 175 | 167 | 334 |
| Obs. in Bandwidth | 4,397 | 5,041 | 1,609 | 2,714 | 2,710 | 5,822 |
| Panel B | *Log. Tax Haven Subs. (any list)*$_{f,t}$ | | | | | |
| RD estimate | -0.08 | 0.02 | -0.15* | -0.25*** | -0.28*** | -0.20*** |
| | (-1.55) | (0.53) | (-1.66) | (-5.51) | (-5.66) | (-5.12) |
| Orig. Obs. | 40,236 | 52,316 | 17,281 | 52,206 | 52,316 | 52,316 |
| Bandwidth | 303 | 108 | 240 | 253 | 158 | 315 |
| Obs. in Bandwidth | 3,955 | 4,364 | 1,332 | 4,103 | 2,535 | 5,437 |
| Panel C | *Log. Subs.*$_{f,t}$ | | | | | |
| RD estimate | 0.04 | -0.01 | 0.01 | -0.20*** | -0.30*** | -0.13* |
| | (0.38) | (-0.25) | (0.04) | (-2.61) | (-2.97) | (-1.79) |
| Orig. Obs. | 40,236 | 52,316 | 17,281 | 52,206 | 52,316 | 52,316 |
| Bandwidth | 218 | 171 | 262 | 308 | 166 | 331 |
| Obs. in Bandwidth | 2,745 | 7,538 | 1,448 | 5,168 | 2,677 | 5,764 |
| Panel D | *Log. Org. Complexity*$_{f,t}$ | | | | | |
| RD estimate | 0.08 | 0.00 | 0.05 | -0.28** | -0.40*** | -0.14 |
| | (0.50) | (0.03) | (0.20) | (-2.57) | (-2.76) | (-1.33) |
| Orig. Obs. | 40,236 | 52,316 | 17,281 | 52,206 | 52,316 | 52,316 |
| Bandwidth | 222 | 180 | 278 | 347 | 179 | 358 |
| Obs. in Bandwidth | 2,802 | 7,961 | 1,547 | 6,018 | 2,903 | 6,380 |
| Polynomials | Linear | Linear | Linear | Linear | Linear | Linear |
| Controls | No | No | No | No | No | No |
| Fixed Effects | No | No | No | No | No | No |

*Notes:* This table presents the results of different falsification and robustness tests using the regression discontinuity design. Columns (1)-(3) report falsification tests obtained by replicating the baseline regression discontinuity design specifications presented in Column (1) of Table 8. The main results from the models based on local linear nonparametric regressions are reestimated after defining the year 2013 as the placebo event date for the €750 million threshold (Column (1)), using a placebo threshold of €375 million in 2016 consolidated revenue as the threshold for treatment (Column (2)), and using placebo outcome variables as of fiscal year 2015 (Column (3)). Column (4) presents the results of excluding firms reporting €740-750 million in 2016 revenue (i.e., with values of the running variable between -10 and 0). Columns (5) and (6) present results when using alternative bandwidths that are 25% smaller or 50% larger than the mean-squared-error optimal bandwidths used in the baseline results in Column (1) of Table 8. We calculate bias-corrected standard errors following Calonico et al. (2014). T-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.