Income Shifting, Digitalization, and Tax Policy

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Mannheim, July 2022 *Daniel Klein*

This dissertation is dedicated to my parents

- Silke and Jens-Uwe Klein -

for their unconditional support and love.

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Chapter 1

Introduction

Over the past decade, the international corporate tax system has been considerably criticized for not being equipped to deal with the challenges of a globalized and digitalized world. Many critics argue that the current system enables multinational firms to reduce large parts of their tax liability by engaging in base erosion and tax-motivated income shifting, leading to the loss of governments' tax revenues (OECD, 2013, 2015a, 2019). This development has led to a number of reform proposals aimed at fundamentally changing the current corporate tax framework (European Commission, 2018e; OECD, 2021). At the same time, however, governments use the domestic corporate tax system to attract businesses and corporate investment by providing tax incentives or competitive tax rates, leading to an intentional reduction of multinational firms' tax liability (e.g., HM Treasury, 2010; U.S. Government, 2017). Given these different motivations and developments in corporate taxation, it is important to understand and distinguish between the current challenges for the corporate tax system, the potential impact of corporate tax reforms, and the effect of domestic corporate tax policy. This dissertation contributes to each of these topics by revolving around four independent research questions:

1. What is the extent of base erosion and tax-motivated income shifting and how has it evolved over time?

2. Is the digital transformation a key enabler of tax-motivated income shifting of multinational firms?

3. What is the shareholder reaction to corporate tax reforms specifically targeted at digital firms?

4. What is the effect of corporate tax policy in developed countries on the economic activity in developing countries?

This dissertation consists of four distinct chapters that are based on four individual, empirical research papers. The research papers have been or aim to be published in academic journals and are the work of multiple co-authors. Table 1.1 lists each research paper and the respective co-authors and describes the current publication status as well as my own key contributions.

Table 1.1: Overview of Papers, Co-authors, and Contribution

Chapter	Paper	Co-authors	Publication status	Own key contributions
2	Quantifying the OECD BEPS Indicators - An Update to BEPS Action 11	Christopher Ludwig Katharina Nicolay Christoph Spengel	ZEW Discussion Paper No. 21-013	Research question Introduction and positioning of paper Literature review Data collection and preparation Econometric analyses (exc. PSM) Interpretation of results Critical evaluation of indicators Summary and policy recommendations
3	Internal Digitalization and Tax-efficient Decision Making	Christopher Ludwig Katharina Nicolay	Working Paper (prepared for submission to Top 3 Accounting Journal)	Research question Introduction and positioning of paper Literature review Development of hypotheses Econometric analyses Interpretation of results Conclusion
4	Taxing the Digital Economy: Investor Reactions to the European Commission's Digital Tax Proposals	Christopher Ludwig Christoph Spengel	National Tax Journal Volume 75(1), pp. 61-92.	Research question Introduction and positioning of paper Literature review Development of hypotheses Data collection Econometric analyses Interpretation of results Conclusion
5	Foreign Aid through Domestic Tax Cuts? Evidence from Multinational Firm Presence in Developing Countries	Jeffrey Hoopes Rebecca Lester Marcel Olbert	Working Paper (prepared for submission to Top 5 Economics Journal)	Research question, positioning of paper Data collection and preparation Development of hypotheses Econometric analyses Interpretation of results

Chapter 2 is based on the research paper "Quantifying the OECD BEPS Indicators – An update to BEPS Action 11", co-authored with Christopher Ludwig, Katharina Nicolay, and Christoph Spengel. This chapter focuses on quantifying the extent of base erosion and profit shifting (BEPS) over time. For this reason, we revisit three selected indicators that the OECD introduced in 2015 to quantify and evaluate BEPS activity. First, we transparently replicate Indicator 1, which intends to assess the disconnect between financial and real economic activities and show a moderately decreasing trend of the indicator estimates. Second, replicating Indicator 4, we find that multinational firms have, on average, lower effective tax rates than domestic firms. We confirm this result using a state-of-the-art propensity score matching approach. Third, the replication of Indicator 5, which intends to capture profit shifting through intangibles, shows a stable trend of the annual indicator estimates that extends beyond the OECD's sample period. Overall, we conclude that the proposed indicators in the Final Report on BEPS Action 11 provide only limited information on the extent of BEPS and we point out potential pitfalls when applying these indicators.

Chapter 3 is based on the paper "Internal Digitalization and Tax-efficient Decision Making", co-authored with Christopher Ludwig and Katharina Nicolay. This chapter focuses on the second research question of this dissertation and examines the impact of internal digitalization on corporate tax planning decisions. Specifically, this study investigates the effect of firms' digitalization on tax-motivated income shifting. We exploit affiliate-level survey data to construct a novel digitalization index that provides a holistic picture of firms' digitalization. We find a significant association between the level of digitalization and tax-motivated income shifting. An instrumental variables approach and additional tests corroborate this finding and mitigate endogeneity concerns. Furthermore, we disentangle the index into its individual software components and find that communication technology is a key enabler of efficient tax planning. Overall, we provide two forms of evidence. First, digitalization is an important firm-specific mechanism of tax-motivated income shifting. Second, digitalization has positive performance effects in support business functions.

Chapter 4 is based on the paper "Taxing the Digital Economy: Investor Reaction to the European Commission's Digital Tax Proposals", co-authored with Christopher Ludwig and Christoph Spengel. This chapter analyzes the effect of tax policy that is specifically targeted on a single industry. As setting, the study analyzes the investor reaction to the European Commission's proposals on the taxation of digital firms. Examining the stock returns of potentially affected firms surrounding the proposals' release, we find a significant abnormal capital market reaction of -0.692 percent. This corresponds to an absolute market value reduction of more than 52 billion euros, 40 percent of which is attributable to US firms. Investor reaction is stronger for firms that engage more in tax avoidance and for those with higher European Union exposure. Overall, investors perceive the event as a threat to digital firms' future profitability and react in line with the proposals' intentions to secure tax revenues and to extract location-specific rent.

Chapter 5 is based on the paper "Foreign Aid through Domestic Tax Cuts? Evidence from Multinational Firm Presence in Developing Countries", co-authored with Jeffrey Hoopes, Rebecca Lester, and Marcel Olbert. This chapter addresses the fourth research question of this dissertation and studies whether corporate tax cuts in developed countries affect economies in the developing world. We focus on one of the most prominent fiscal policies – the corporate income tax regime – and study a major U.K. tax cut as an exogenous shock to foreign investment in Africa. Difference-in-differences estimates show that multinational U.K. firms increase their subsidiary presence in sub-Saharan Africa by 17-24 percent following the 2010 announcement of U.K. tax rate reductions. Exploiting location-specific nighttime luminosity data as well as local data from the African Demographic and Health Surveys, we also document increased economic activity and higher employment rates of African citizens within close proximity (10 kilometers) of local U.K.-owned subsidiaries. Our findings imply that, beyond the goal of motivating home country investment, developed countries' corporate tax cuts have economic impact in developing nations.

Chapter 6 concludes with a summary of the main findings of this dissertation.

Chapter 2

Quantifying the OECD BEPS Indicators – An Update to BEPS Action 11

Co-Authors: Christopher Ludwig, Katharina Nicolay, and Christoph Spengel

Abstract: In 2015, the OECD introduced six indicators to quantify and evaluate base erosion and profit shifting (BEPS) activity over time. In this study, we revisit three selected indicators and point out potential pitfalls when interpreting the indicator results. First, we transparently replicate Indicator 1, which intends to assess the disconnect between financial and real economic activities, and show a moderately decreasing trend of the indicator estimates. Second, replicating Indicator 4, we find that multinational firms have, on average, lower effective tax rates than domestic firms. We confirm this result using a state-of-the-art propensity score matching approach. Third, the replication of Indicator 5, which intends to capture profit shifting through intangibles, shows a stable trend of the annual indicator estimates that extends beyond the OECD's sample period. Overall, we conclude that the proposed indicators in the Final Report on BEPS Action 11 provide only limited information on the extent of BEPS.

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2.1 Introduction

> *"The use of any indicators to identify the scale and economic impact of BEPS can only provide 'general indications' and the interpretation of any such indicators must be heavily qualified by numerous caveats."* (OECD, 2015b, p. 41)

Profit shifting of multinational corporations is a pressing topic in the public debate, academic research and on the political agenda. The debate on legal tax avoidance is fuelled by anecdotal evidence on extremely low effective tax rates (ETRs) by multinational enterprises (MNEs). In particular, US companies with valuable intellectual property (IP), such as Google, Apple, and Amazon, are in the public focus for being tax 'aggressive'.[1]

The issue of 'aggressive' tax planning and cross-border income relocation is, of course, not new to policymakers. The release of the well-known Action Plan on Base Erosion and Profit Shifting (BEPS) by the Organization for Economic Cooperation and Development (OECD) in 2013 has lifted the issue to one of the top priorities in international politics. Since then, many nations have implemented far-reaching reforms to prevent 'aggressive' income shifting, to strengthen anti-tax avoidance legislation, and to conserve corporate tax revenues. While some reforms are part of coordinated supranational actions, e.g., the EU Anti-Tax Avoidance Directive, others are purely unilateral legislations to protect national tax revenues, e.g., the French Digital Services Tax. Perhaps most prominently, the OECD has recently proposed a far-reaching two-pillar reform to adjust the worldwide corporate tax system, which is supported by 137 member countries (OECD, 2021).[2]

Despite the proposed actions to prevent BEPS and the heightened public awareness against 'aggressive' tax planning, it is still a major challenge to credibly measure the

[1]The effective tax rate of big tech companies is regularly discussed in the public media and Margarethe Vestager, European Commissioner for Competition, has become publicly known for her focus on illegal state aid cases and tax affair investigations. See, for example, Financial Times (2018a), The Guardian (2018), and Bloomberg (2019).

[2]Pillar One proposes a "Unified Approach" that is designed to allocate taxing rights to market jurisdictions (Beer et al., 2020). Pillar Two, the "Global Anti-Base Erosion" (GloBE) proposal, intends to counteract all remaining profit shifting risks by introducing a coordinated global minimum tax and a deduction disallowance that should, in general, apply to all transactions (Devereux et al., 2020).

extent of profit shifting and to assess its economic relevance (Blouin and Robinson, 2021; Bradbury et al., 2018; Tørsløv et al., 2021). In the 2015 published Final Report on "Measuring and Monitoring BEPS, Action 11", the OECD introduced six indicators to measure and evaluate BEPS activity over time and on different levels of aggregation (OECD, 2015b). The six OECD BEPS indicators intend to identify the scale and economic impact of BEPS, track changes in BEPS over time and monitor the effectiveness of measures implemented to reduce BEPS (OECD, 2015b). In conjunction with the introduction, the OECD provides numbers for each indicator for the period from 2005 to 2012. These values are interpreted to provide strong signals on the existence and exacerbation of BEPS (Bradbury and O'Reilly, 2018). However, ever since these indicators have not been revised or quantitatively updated by the OECD.

In this paper, we transparently replicate a selection of the six OECD indicators to measure and monitor BEPS. We provide an update to the numbers on which the ongoing political debate to reform the global corporate income tax system is based. Our work builds on the theoretical evaluation of the indicators by Heckemeyer et al. (2021). The authors argue that the main objective of the OECD BEPS indicators, to provide understandable and easy to replicate measures of BEPS, comes at the price of too simplistic measures that prevent a reliable tracing of profit shifting. It is beyond the scope of this paper to conceptually re-assess whether the indicators are well suited to capture profit shifting. Rather, we, first, aim to provide a quantitative update for the indicator values and, second, to highlight potential pitfalls when interpreting the results. Our motivation is twofold. First, we agree to the notion that a broad range of estimates on the existence and extent of profit shifting is necessary to provide policymakers with a solid foundation for decision making. Second, we want to increase awareness that particularly easy-to-understand indicators may contain severe shortcomings.

We categorize the six OECD BEPS indicators in three different groups based on their underlying data and measurement rationale. The first indicator group uses macro data to highlight a potential disconnect between financial and real economic activities. This category comprises "Indicator 1: Concentration of foreign direct investment relative to the Gross Domestic Product (GDP)". The second group uses micro data to identify surprisingly low profits or suspicious tax activity. This category comprises "Indicator 2: Differential profit rates compared to effective tax rates"; "Indicator 3: Differential profit rates between low-tax locations and worldwide MNE operations"; and "Indicator 4: Effective tax rates of large MNE affiliates relative to non-MNE entities with similar characteristics". The third indicator group uses micro and macro data to measure the use of potential profit shifting channels. This category comprises "Indicator 5: Concentration of high levels of royalty receipts relative to R&D spending"; and "Indicator 6: Interest expense to income ratios of MNE affiliates in high-tax locations". We revive one indicator from each category to shed light on its development over time. Our choice relies on the assessment in Heckemeyer et al. (2021) and we are confident to focus on the most convincing indicator in each category, namely Indicator 1, Indicator 4 and Indicator 5.

Indicator 1 relies on macro-level data and intends to indirectly measure BEPS that takes place through the use of offshore tax havens. By replicating this indicator, we transparently show that countries that are often expected to serve as conduits or final destinations for BEPS have a disproportionately high amount of gross or net FDI in relation to economic activity measured by GDP. Extrapolating the indicator values to recent years reveals a downward (stable) trend for the relation of average net (gross) FDI to GDP ratios between countries with very high and lower concentrations of FDI relative to their economic activity. However, since the indicator is unable to distinguish between real economic activity and BEPS, the estimates and their variation over time may be driven by factors unrelated to BEPS, such as trade openness or business cycles.

Indicator 4 employs firm-level micro data to evaluate cross-border profit shifting of multinational corporations and addresses the well-known drawbacks of highly aggregated macro data. Due to the usage of advanced statistical methods and counterfactuals, Indicator 4 has been identified in previous work to represent the most promising approach (Heckemeyer et al., 2021). Replicating the OECD's regression, we show that the ETR differential between MNE affiliates and domestic firms is negative and statistically significant for almost all years in our sample. Yet, the difference diminishes over time. In line with Bilicka (2019), we extend the OECD's fourth indicator by applying a propensity score matching approach. The qualitative insight holds. MNE affiliates tend to have lower effective tax rates than comparable domestic firms. Despite the promising approach to compare MNEs with similar domestic firms, the unconsolidated ETR is by construction not suitable to capture profit shifting. If at all, the recommended measure indicates certain forms of special tax incentives, loss-offsets, hybrid mismatch arrangements, tax negotiations or other non-profit shifting related methods to reduce a firm's tax burden.

Indicator 5 relies again on macro-data rather than firm-level data and is concerned with profit shifting through intangibles. We show that countries with high ratios of royalty receipts to research and development spending are countries with low corporate income tax rates or IP box regimes, e.g., Ireland, the Netherlands and Luxembourg. Moreover, the indicator remains constant over time and does not seem to react to recent policy actions to curb BEPS. However, the simplistic design of Indicator 5, which lacks any link to countries' tax rates, is to some extent arbitrary and neglects potential real economic activities of MNEs.

Our analysis shows that despite the OECD's intention to provide a dashboard of indicators to evaluate the existence and scale of BEPS and to measure and monitor how BEPS evolves over time, the indicators presented in the Final Report on BEPS Action 11 are unlikely to achieve this goal convincingly. Their simplistic design comes at the price of making them vulnerable to a number of confounding factors and economic effects that go beyond profit shifting. The OECD (2015b) acknowledges several shortcomings of their indicators itself and our selected replication of three indicators confirms these issues. Overall, the indicators provide only limited information on the extent of profit shifting and lack the ability to precisely identify any changes to BEPS that result from recent tax reforms and enactments of BEPS countermeasures.

With this transparent replication and update of simple indicator values that are taken as a rationale for global tax reforms, we contribute to the public and political debate on profit shifting of MNEs. However, we recommend to base policy decisions on the numerous empirical studies that in general exploit well-specified identification strategies and granular data to show the existence of BEPS and to develop convincing estimates of the level of income shifting and the effects of BEPS countermeasures (Hanlon and Heitzman, 2010; Heckemeyer and Overesch, 2017; Riedel, 2018; Wilde and Wilson, 2018). The academic debate on the extent of BEPS and its fiscal effects has recently gained momentum (Blouin and Robinson, 2021; Bradbury et al., 2018; Tørsløv et al., 2021). Blouin and Robinson (2021) critically discuss the broad range of profit shifting estimates and assess different data sources. Especially, the careful evaluation of new data such as public country-by-country reporting can shed light on the extent of profit shifting (Clausing, 2020; Dutt et al., 2021). Moreover, analysing different profit shifting channels separately allows to apply targeted measures and data sources and provides a promising approach to evaluate the effectiveness of specific BEPS countermeasures (e.g., Beer and Loeprick, 2015; Clausing, 2003; Dischinger and Riedel, 2011; Lohse and Riedel, 2013; Overesch, 2016; Saunders-Scott, 2015).

This paper is organized as follows. Section 2.2 covers the replication of Indicator 1, the concentration of foreign direct investment relative to GDP. The subsequent section 2.3 covers the replication of Indicator 4, the comparison of effective tax rates of large MNE affiliates with non-MNE entities. Section 2.4 covers the replication of Indicator 5, profit shifting through intangibles. Each section has three major subsections. First, we describe the methodology and data necessary to estimate each indicator. Second, we show the results and third, we critically assess the rationale and shortcomings of the indicators. Finally, section 2.5 concludes.

2.2 Concentration of FDI relative to GDP

> *"This macro-economic indicator is the ratio of the stock of FDI to a country's GDP, measure of real economic activity. The indicator compares the FDI ratio in countries with relatively high values of FDI to GDP ratios to the same ratio in the rest of the included countries."* (OECD, 2015b, p. 49)

2.2.1 Methodology and Data

OECD Indicator 1 relies on macro-level data and intends to indirectly measure BEPS that takes place through the use of offshore tax havens, which is the strategy of MNEs to channel funds to affiliates in low-tax countries for tax purposes. In order to measure the movement of funds, the OECD focuses on foreign direct investments (FDI). FDI measures the amount of cross-border investments of related affiliates and includes not only investments related to BEPS but also to real economic activity. As FDI patterns can generally be expected to be proportional to the economic size of the involved countries (Head and Ries, 2008), significantly high concentrations of FDI to GDP may signal BEPS. Following these considerations, Indicator 1 is based on the ratio of FDI stock in a country owned by foreign investors to the GDP of that country in a given year. Based on the magnitude of this ratio in a pre-determined base year, countries are assigned to two different groups – high-ratio countries and low-ratio countries – and remain in this group in all years. For each group, the average of the ratio of FDI stock to GDP is determined and the indicator expands as follows:

$$Indicator\ 1_t = \frac{\frac{\sum_{i=1}^{I} FDI_{it}}{\sum_{i=1}^{I} GDP_{it}}}{\frac{\sum_{j=1}^{J} FDI_{jt}}{\sum_{j=1}^{J} GDP_{jt}}}, \tag{2.1}$$

where subscript i refers to countries in the high-ratio group and subscript j to countries in the low-ratio group.

The OECD distinguishes between two different measures of FDI. The net FDI of a country is calculated as the inward FDI stock in that country owned by foreign investors from OECD countries less the outward FDI stock from domestic investors that is held in OECD countries. Hence, this measure is supposed to identify those countries that are the ultimate destination of foreign direct investments for the purpose of BEPS. The OECD determines a threshold of 50 percent of the net FDI to GDP ratio for assigning countries into the two groups.

The second measure of FDI relies on the gross inward FDI stock in a country owned by foreign investors from OECD countries. In addition to countries that are the ultimate destinations for FDI, this measure is intended to also capture conduit countries with a high proportion of FDI stock relative to GDP. The OECD defines a threshold of 200 percent of gross FDI to GDP for assigning countries into the two groups.

We replicate both measures using 2012 and 2018 as the base years for group allocation. We also conduct the analyses by recalculating the two groups continuously on a yearly basis. For our calculations, we employ two different sets of data from the OECD Foreign Direct Investment Statistics. We firstly use FDI position data of the 3rd edition of the Benchmark Definition of FDI (BMD3). The data includes inward and outward FDI positions from and to OECD countries for the time period from 2005 to 2013. Secondly, we use FDI position data of the 4th edition of the Benchmark Definition of FDI (BMD4) for the time periods from 2013 to 2018.[3] Additionally, we obtain GDP data in current US dollar for the years 2005 until 2018 from the World Bank.[4] The dispersion of countries with available data is shown in Table 2.1.

Moreover, the BMD4 data allow us to observe FDI inflows and outflows of special purpose entities (SPEs). As defined by the OECD, SPEs are established in economies other than those in which the parent firm is resident and engage primarily in international transactions but in few or no local operations. Therefore, FDI in SPEs might be considered especially BEPS-motivated.

2.2.2 Results

First, we replicate the OECD's estimation of Indicator 1, taking net FDI to GDP as the measure and 2012 as the base year. For 2012, we have data on 202 countries, of which 14 are assigned to the high-ratio group. Unsurprisingly, members of the high-ratio group are countries with low or no CIT rate or preferential tax systems, e.g., the Bahamas, Cayman Islands or Ireland. The structure of the high-ratio group is depicted in Table

[3]Since September 2014, the OECD has been collecting FDI statistics from member countries according to the updated benchmark definition BMD4. The methodology of the FDI statistics published between 1990 and end-2013 relates to the 3rd edition of the benchmark definition.

[4]World Bank indicator code: NY.GDP.MKTP.CD

Table 2.1: Indicator 1 – Country-Year Distribution

	Net FDI	Gross FDI
Year	No of Countries	No of Countries
2005	189	197
2006	190	200
2007	190	202
2008	193	201
2009	192	200
2010	195	200
2011	195	202
2012	196	202
2013	198	203
2014	196	199
2015	196	199
2016	195	197
2017	193	196
2018	187	191

Notes: The table shows the number of countries with available data per year for the construction of Indicator 1. Net FDI refers to the net FDI to GDP measure. Gross FDI refers to the gross FDI to GDP measure. The years 2005 until 2012 rely on the BMD3 definition of Foreign Direct Investment, while the years 2013 until 2018 rely on BMD4.

2.2. Using the BMD3 data from 2005 to 2012, we can closely replicate the results of the OECD, which are shown in Table 2.3 Panel (A) and graphically plotted in Figure 2.1 Panel (A). In 2011, the indicator shows that the average ratio of net FDI to GDP of the high-ratio countries was about 43 times higher than the average ratio of low-ratio countries. The indicator values are depicted in Figure 2.1 Panel (B). However, we do not find the OECD's sharp increase in the indicator value in the year 2012, in which our estimate increases to 54.8 in contrast to 99.2 estimated by the OECD. Employing the BMD4 data from 2013 onwards, we see a drop in the indicator value to 23.9, which then steadily decreases to 11.7 in 2018.

When taking 2018 as the base year for group allocation, we identify 13 countries in the high-ratio group, which is depicted in column (3) of Table 2.2. In 2005 and 2006, the average ratio of net FDI to GDP of the high-ratio group was only marginally higher. After this time, we find a steady increase to an indicator value of around 14, which remains stable until 2018.

Second, we replicate the OECD's indicator using the gross FDI to GDP ratio. The OECD claims that by using gross FDI values, the indicator also captures those countries

Table 2.2: Indicator 1 – Countries in High-Ratio Group Distribution

	Net FDI		Gross FDI	
Number	Base Year 2012	Base Year 2018	Base Year 2012	Base Year 2018
1	Bahamas	Barbados	Bahamas	Bahamas
2	Barbados	Cyprus	Barbados	Barbados
3	Bermuda	Dominica	Bermuda	Curacao
4	Cayman Islands	Ireland	Cayman Islands	Cyprus
5	Hong Kong	Marshall Islands	Curacao	Ireland
6	Hungary	Mauritius	Ireland	Luxembourg
7	Ireland	Mongolia	Luxembourg	Malta
8	Liberia	Netherlands	Malta	Marshall Islands
9	Malta	Panama	Marshall Islands	Mauritius
10	Marshall Islands	Papua New Guinea	Netherlands	Netherlands
11	Mauritius	St. Kitts and Nevis		Switzerland
12	Singapore	Singapore		
13	St. Kitts and Nevis	Turks and Caicos Islands		
14	Trinidad and Tobago			

Notes: This table shows countries belonging to the high FDI to GDP ratio group. Countries with a FDI to GDP ratio above 50 percent for net FDI or above 200 percent for gross FDI are assigned to the group of high-ratio countries while the other countries form the group of low-ratio countries. In columns 2 and 3, the group structure is shown for the net FDI to GDP measure using base years 2012 and 2018, respectively. In columns 4 and 5, the group structure is shown for the gross FDI to GDP measure using base years 2012 and 2018, respectively.

that function as conduits for BEPS. When using 2012 as the base year, ten out of 202 countries are assigned to the high-ratio group and, indeed, countries such as Ireland, the Netherlands and Luxembourg are part of the high-ratio group which are considered as members of the top ten conduit countries (Van 't Riet and Lejour, 2018). Table 2.2 columns (4) and (5) display the list of all countries in the high-ratio group. From 2005 to 2012, we are able to closely replicate the OECD's estimates. Table 2.3 Panel (B) columns (3) to (5) depict the indicator values. In 2005, the gross FDI to GDP ratio of the high-ratio countries is about 12 times higher than the ratio of the low-ratio countries and doubles to 24 until 2012. In the following years, the indicator value remains at a level of about 23. Figure 2.2 Panel (A) shows that in 2018, the amount of gross FDI per euro of GDP in the high-ratio group of countries was, on average, 19 times higher than the average ratio for the remaining countries. When taking 2018 as the base year, eleven countries belong to the high-ratio group and Figure 2.2 Panel (B) shows that the indicator trend over time is steadily increasing and doubles between 2005 and 2018.

In addition, we repeat both analyses allocating countries to into high and low-ratio

Table 2.3: Indicator 1 – Results

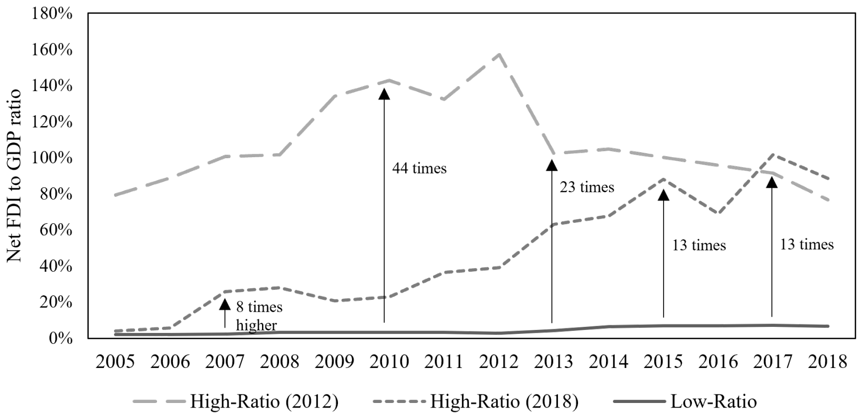
Panel (A)	Net FDI						
	OECD	Base Year 2012			Base Year 2018		
Year	Indicator	High	Low	Indicator	High	Low	Indicator
2005	37.6	79%	2%	37.5	4%	3%	1.3
2006	36.3	89%	2%	43.1	6%	3%	1.8
2007	37.4	101%	2%	44.6	26%	3%	8.0
2008	31.9	102%	3%	31.3	28%	4%	6.8
2009	41.9	134%	3%	42.3	21%	5%	4.5
2010	44.9	143%	3%	44.3	23%	5%	4.8
2011	43.1	132%	3%	41.4	36%	4%	8.5
2012	99.2	157%	3%	54.8	39%	4%	9.2
2013		102%	4%	23.9	63%	4%	14.3
2014		105%	6%	16.3	68%	7%	10.4
2015		100%	7%	14.6	88%	7%	13.3
2016		96%	7%	13.9	69%	7%	9.9
2017		91%	7%	12.9	102%	6%	15.8
2018		77%	7%	11.7	88%	6%	14.9

Panel (B)	Gross FDI						
	OECD	Base Year 2012			Base Year 2018		
Year	Indicator	High	Low	Indicator	High	Low	Indicator
2005	13.0	175%	14%	12.4	125%	14%	8.9
2006	13.9	202%	15%	13.1	145%	15%	9.4
2007	15.9	247%	16%	15.0	167%	17%	9.9
2008	17.4	262%	16%	16.5	176%	16%	10.9
2009	18.9	323%	18%	17.7	207%	19%	11.0
2010	21.1	349%	18%	19.9	215%	18%	11.9
2011	23.4	359%	16%	22.1	215%	17%	12.8
2012	26.7	406%	17%	24.4	240%	17%	14.0
2013		504%	21%	23.5	332%	21%	15.6
2014		518%	23%	22.5	363%	22%	16.3
2015		602%	25%	24.0	417%	24%	17.2
2016		612%	26%	23.5	434%	25%	17.4
2017		660%	27%	24.4	482%	26%	18.7
2018		524%	25%	20.8	422%	23%	18.0

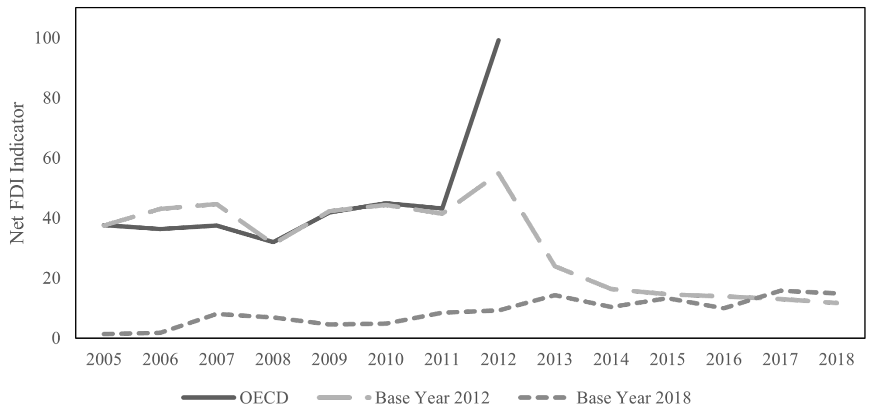
Notes: The table presents the estimated values of Indicator 1. In Panel (A) net FDI is used to estimate the indicator. In Panel B gross FDI is used. Column (2) shows indicator values estimated by the OECD (OECD, 2015b). For each country, the ratio of FDI to GDP is calculated. Based on this ratio in a pre-defined base year, countries are assigned to high-ratio groups or low-ratio groups. The threshold values amounts to 50 percent in Panel (A) and 200 percent in Panel (B). In columns (3) to (5), the year 2012 is the base year. Columns (3) and (4) show the ratio of the countries in the high-ratio group and low-ratio group, respectively. Column (5) displays the estimated indicator value. In columns (6) to (8), the year 2018 is the base year. Columns (6) and (7) show the ratio of the countries in the high-ratio group and low-ratio group, respectively. Column (8) displays the estimated indicator value. The years 2005 until 2012 rely on the BMD3 definition of Foreign Direct Investment as data source while the years 2013 until 2018 rely on BMD4.

Figure 2.1: Indicator 1 - Net FDI Results

(A) Net FDI to GDP by groups



(B) Net FDI Trend



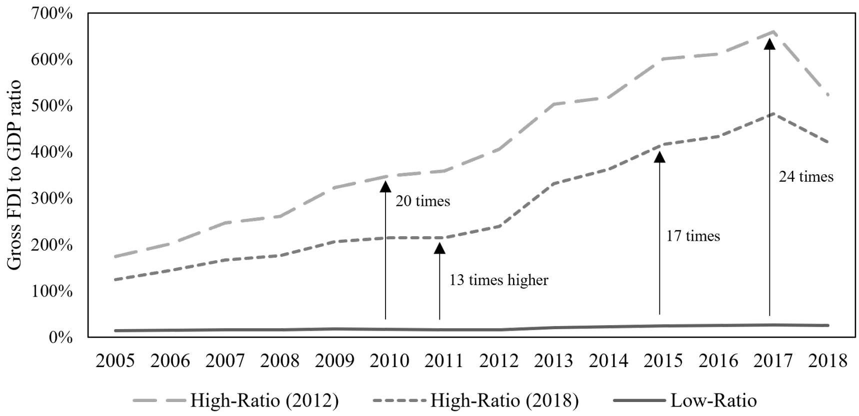
Notes: The figure presents estimates of Indicator 1 over time using net FDI to GDP. In Panel (A), the dashed line depicts the trend of the average net FDI to GDP ratio for the group of countries that have a ratio above 50 percent in 2012. The dotted line depicts the trend of the average net FDI to GDP ratio for the group of countries that have a ratio above 50 percent in 2018. The solid line displays the average ratio of the remaining (low-ratio) countries. In Panel (B), the dashed line shows the trend of Indicator 1 using net FDI to GDP as measure and 2012 as base year. The dotted line uses 2018 as base year for group allocation. The solid black line shows the indicator's trend estimated by the OECD (OECD, 2015b).

groups on a continuous basis every year. We show results in Figure 2.3. For the net FDI analysis, the indicator value follows closely that of taking 2012 as base year. The gross FDI indicator ranges between 60 and 100 for the years 2005 to 2010. This is about five times the value of that when taking 2012 as base year. In year 2011, the indicator value drops to 24 and remains in this magnitude for the rest of the sample period.

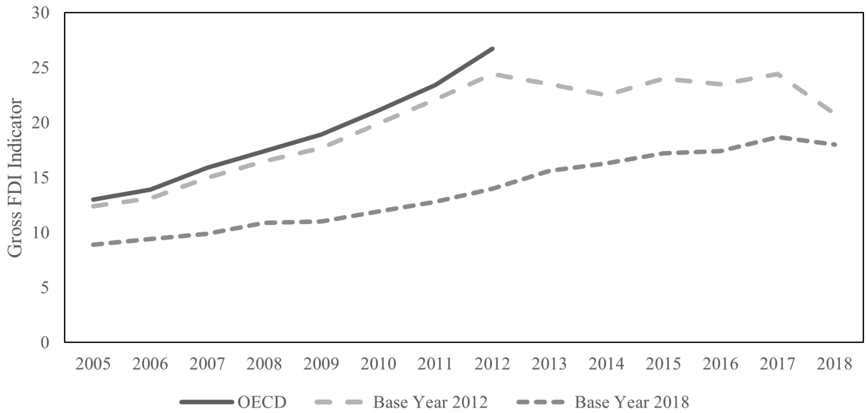
As robustness test, we replicate our analysis keeping only those countries for which we have data available over the whole period from 2005 to 2018 and find very similar

Figure 2.2: Indicator 1 - Gross FDI Results

(A) Gross FDI to GDP by groups



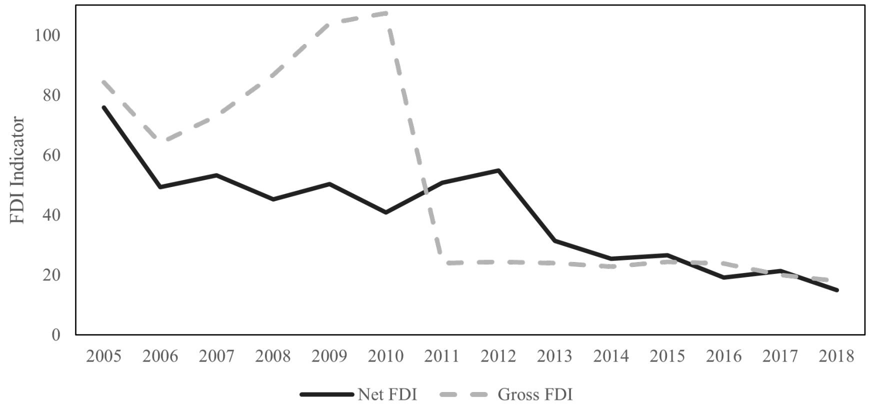
(B) Gross FDI Trend



Notes: The figure presents estimates of Indicator 1 over time using gross FDI to GDP. In Panel (A), the dashed line depicts the trend of the average gross FDI to GDP ratio for the group of countries that have a ratio above 200 percent in 2012. The dotted line depicts the trend of the average gross FDI to GDP ratio for the group of countries that have a ratio above 200 percent in 2018. The solid line displays the average ratio of the remaining (low-ratio) countries. In Panel (B), the dashed line shows the trend of Indicator 1 using net FDI to GDP as measure and 2012 as base year. The dotted line uses 2018 as base year for group allocation. The solid black line shows the indicator's trend estimated by the OECD (OECD, 2015b).

results. Furthermore, we exploit FDI positions of SPEs in the time period from 2013 to 2018. Due to the variation in data availability, we do not find consistent results. Nevertheless, the prior identified countries in the high-ratio groups are again those countries with the highest ratios.

Figure 2.3: Indicator 1 – Net and Gross FDI with Continuous Base Year



Notes: The figure presents estimates of Indicator 1 over time using gross FDI to GDP or net FDI to GDP. The allocation of countries into high-ratio and low-ratio countries is rebuilt on a continous basis every year. The solid black line shows the trend of Indicator 1 using net FDI to GDP as a measure, recalculating the group composition every year. The dashed line shows the trend of Indicator 1 using gross FDI to GDP as a measure, recalculating the group composition every year.

2.2.3 Rationale and Shortcomings

Indicator 1 relies on the assumption that a country's magnitude of (inward) FDI stock to GDP provides an indication of BEPS. Specifically, MNEs are supposed to channel funds to affiliates in low-tax countries for tax reasons and not for reasons of real economic activity. Indeed, prior literature provides evidence on the adverse relationship of taxes and FDI (Buettner et al., 2018; Desai et al., 2004; Janeba, 1995). BEPS related FDI is expected to create a disproportion between the FDI in a country and the economic activity of this country, measured by GDP. The indicator intends to capture this disproportion.

However, the definition of the indicator has many drawbacks. First, FDI includes both investments related to BEPS and investments related to real economic activity. Since the indicator is unable to distinguish between these types of investment, the estimates and their variation over time may be driven by factors unrelated to BEPS, such as trade openness or business cycles. Second, the indicator does not provide any direct linkage to countries' tax rates which is the key driver for BEPS related FDI. Third, the indicator values highly depend on the specific threshold and base year to assign countries to the two different groups. This is highlighted by the high indicator value dispersion when using continuous base years. Overall, we explicitly point out that the results have to be treated with caution and conclude that Indicator 1 does not provide convincing (indirect) evidence of BEPS.

2.3 Effective Tax Rates of Large MNE Affiliates

> *"Indicator 4 compares the ETRs of large MNE affiliates with non-MNE entities with similar characteristics in the same country. The indicator measures the extent to which large MNE affiliates have lower ETRs than comparable non-MNE entities."* (OECD, 2015b, p. 58)

2.3.1 Methodology and Data

This OECD indicator relies on firm-level micro data to evaluate cross-border profit shifting of MNEs and addresses the drawbacks of highly aggregated macro data that is used to estimate the first OECD Indicator. In contrast to purely domestic firms, which operate only in one country, MNEs have incentives to relocate income to affiliates located in countries with lower corporate tax rates. The fourth OECD indicator exploits this difference between domestic and multinational corporations. Domestic firms serve as a counterfactual benchmark group to assess the extent of income shifting by comparable multinationals. The indicator uses financial data of multinational affiliates and domestic firms to compare the effective tax rate (ETR) between the two groups. The OECD expects that the ETR of MNEs is, on average, lower than that of comparable domestic firms (OECD, 2015b).

The presumption of lower ETRs for large MNEs is tested using the following regression framework following OECD (2015b) Annex 3.A1:

$$\begin{aligned} ETR_{fcit} = \beta_1 Large_{fcit} + \beta_2 Large_{fcit} \times MNE_{fci} + \\ \beta_3 Small_{fcit} \times MNE_{fci} + \alpha_j X_{fcit} + \gamma_i + \delta_{ct} + \epsilon_{fcit}, \end{aligned} \quad (2.2)$$

where ETR_{fcit} is the effective tax rate of firm f in country c, industry i and year t. ETR_{fcit} is the ratio of tax payments to earnings before income and taxes (EBIT) in percent. Multinational firms are identified using the dummy variable MNE_{fci}. We define a multinational firm as a firm that has at least one cross-border affiliate relationship. An affiliate belongs to a multinational firm when it is majority owned. The MNE group structure is based on the ORBIS ownership information at the end of year 2016 and is assumed to be constant in our panel.[5] $Large_{fcit}$ is a dummy variable that takes the value of 1 for firms with more than 250 employees. $Small_{fcit}$ is the counterpart of $Large_{fcit}$. The OECD excludes the baseline effect of MNE_{fci} on the ETR_{fcit} to obtain direct estimates of the differences in ETRs between multinational firms and domestic firms along different size classes. The estimate of the interaction between large and multinational firms, β_2, is the coefficient of interest and the estimated value of Indicator 4. X_{fcit} is a vector of firm-specific control variables. It includes the size of a firm, measured as the logarithm of total assets, the profitability ratio of a firm and an estimate for the degree of firms' innovation activities. We use the ratio of intangible to total assets as a proxy for firms' innovativeness in contrast to the number of patents that is used by the OECD. Furthermore, a dummy variable that indicates if a firm is the global ultimate owner controls for a firm's position in the group. γ_i are industry fixed effects at the two-digit NACE classification and δ_{ct} are country-year fixed effects.

In line with the OECD, we use unconsolidated financial data from the Bureau van Dijk ORBIS database to replicate the estimates of Indicator 4. Our panel starts in 2000 and has data up to 2016. Similar to other studies on profit shifting, we exclude observations with implausible financial data such as total assets below 0 and exclude all observations that have a negative effective tax rate or one above 100 percent (Beer and Loeprick, 2015; Dischinger and Riedel, 2011; Huizinga and Laeven, 2008). Furthermore, we restrict the sample to firms with a profitability ratio, which is determined as EBIT to total assets, between 0 and 100 percent, i.e., we exclude loss-making firms and overly

[5]This assumption is commonly used in the literature on profit shifting. Many changes in the ownership structure result from data improvements by the data provider.

profitable corporations. Finally, we exclude all firms with less than three years of basic accounting data available in our panel (Beer and Loeprick, 2015).

2.3.2 Results

Our panel from 2000 to 2016 consists of more than 1,000,000 unique firms and 5,048,716 firm-years, thereof about 22 percent belong to multinational firms. Descriptive statistics are depicted in Table 2.4. The mean (median) firm has an *ETR* of 31.25 percent (28.07 percent) and employs 136 (15) people. In 7.5 percent of firm-year observations, firms employ more than 250 people and are considered as *Large*. Furthermore, the mean (median) firm has €12.33 million (€1.58 million) in total assets and a profitability rate of 11.6 percent (7.7 percent).

Table 2.4: Indicator 4 – Descriptive Statistics

Variable	N	Mean	SD	Min	p25	Median	p75	Max
ETR	5,048,716	31.248	20.075	0.000	17.975	28.073	38.462	100.000
Employees	5,048,716	136	2,477	1	5	15	51	1,477,200
Large	5,048,716	0.075	0.263	0.000	0.000	0.000	0.000	1.000
MNE	5,048,716	0.220	0.414	0.000	0.000	0.000	0.000	1.000
Profitability	5,048,716	0.116	0.122	0.000	0.040	0.077	0.146	1.000
Total Assets	5,048,716	12,332.85	31,017.53	0.25	445.53	1,579.24	7,005.73	158,697.24
Innovation	5,048,716	0.054	0.117	0.000	0.001	0.007	0.041	0.785
Group Position	5,048,716	0.684	0.465	0.000	0.000	1.000	1.000	1.000

Notes: The table depicts the descriptive statistics for measuring Indicator 4. *ETR* is the ratio of tax payments to profit and loss before tax. *Employees* is the number of employees. *Large* is a dummy variable that is equal to one for all firms with more than 250 employees. *MNE* is a dummy variable that is equal to one for all firms that belong to a group with at least one cross-border affiliate relationship. *Profitability* is the ratio of earnings before income and taxes (EBIT) to total assets (TOAS). *Innovation* is the ratio of intangible fixed assets (IFAS) to total assets. *Group Position* is a dummy variable that is equal to one for all headquarters. *ETR*, *Profitability*, and *Innovation* are measured in percent. *Employees* is displayed in absolute numbers and *Total Assets* in thousand euro.

In our baseline regression, depicted in Table 2.5, we estimate large MNEs to have a 1.02 percentage points lower ETR than large domestic firms, which is significant at the one percent level. This estimate decreases to about -1.56 percentage points if we only consider the period from 2000 until 2010, which corresponds to the OECD sample period. Yet, our estimates lie consideraby below the OECD's estimate of 3.3 percentage points. (OECD, 2015b).

Table 2.5: Indicator 4 – Baseline Result

	(1)	(2)
	ETR	
Variable	2000-2016	2000-2010
Large	0.518***	0.602***
	(0.09)	(0.11)
Small × MNE	-0.204***	-0.321***
	(0.04)	(0.05)
Large × MNE	-1.018***	-1.556***
	(0.10)	(0.12)
Profitability	-23.386***	-19.048***
	(0.09)	(0.11)
log Total Assets	-0.220***	-0.095***
	(0.01)	(0.01)
Innovation	-2.384***	-3.548***
	(0.10)	(0.11)
Group Position	-0.820***	-1.007***
	(0.03)	(0.04)
Industry Fixed Effects	Yes	Yes
Country-Year Fixed Effects	Yes	Yes
Observations	5,048,716	2,796,459
Number of Firms	1,001,429	751,148
Adj. R2	0.362	0.363

Notes: This table presents the regression results for estimating Indicator 4 using Equation 2.2. The dependent variable is the effective tax rate (*ETR*). *Large* is a dummy variable that is equal to one for all firms with more than 250 employees. *MNE* is a dummy variable that is equal to one for all firms that belong to a group with at least one cross-border affiliate relationship. *Profitability* is the ratio of earnings before income and taxes (EBIT) to total assets (TOAS), innovation is the ratio of intangible fixed assets (IFAS) to total assets and position in group is a dummy variable that is equal to one for all headquarters. In column (1), we use all observations in our sample in the period 2000 to 2016. In column (2), we restrict the sample to the period 2000 to 2010. All continuous variables are winsorized at the 1 and 99 percentile. We report standard errors clustered by firm in parentheses. ***, **, * denote statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively.

To further compare trends over time, we replicate the yearly estimates of the ETR differential, which refer to the yearly interaction coefficients of large and multinational corporations (Indicator 4).[6] We compare the estimates graphically to the OECD's estimates in Figure 2.4. Our estimates on the ETR differential between large MNE affiliates and non-MNE entities are considerably below the annual estimates of the OECD. Yet, we also see a negative ETR differential for almost all years. In the second half of our sample period, which extends beyond the OECD's period, the ETR differential

[6]To do so, we modify Equation 2.2 by taking out the *Small* dummy and including *Time* dummies into the specification, which changes the interpretation slightly. Large MNEs are now compared to all domestic firms.

Figure 2.4: Indicator 4 – Trends over Time

Notes: This figure presents the estimated value of Indicator 4 over time by different estimation approaches. The grey dotted line depicts the annual regression estimates for Indicator 4 in Section 2.3.2. The grey dashed line depicts the annual regression estimates for the ATE, based on the two-step propensity score matching method in Section 2.3.3. The solid black line shows the indicator's trend estimated by the OECD (OECD, 2015b).

follows an upward trend and converges towards zero. The formal regression results are presented in Appendix Table A.1 columns (1) and (2).

We conduct several additional tests. First, we restrict the sample to specific regions. In column (3) of Appendix Table A.1, we only consider firms located in an OECD country, and in column (4), we only consider firms located in EU countries. The results do not change materially. Second, we change the outcome variable to tax payments over total assets to account for the critique on the chosen outcome variable in the OECD regression approach (Heckemeyer et al., 2021). The regression is depicted in Appendix Table A.1 column (5). The relative tax payments to total assets are, on average, only significantly lower for large MNEs than for domestic firms in the early sample period.

2.3.3 Additional Analysis – Propensity Score Matching

The OECD's regression approach to compare domestic and multinational corporations is an intuitive empirical methodology and the differences to alternative, more sophisticated, matching estimates are presumably of minor empirical importance (Angrist and Pischke, 2008). Yet, the quality of the comparison between multinational and domestic firms crucially hinges on the matching quality, i.e., the similarity and comparability of the two groups is essential for any inferences. A well-established method of creating a control group that is as similar as possible to the treated group in a non-experimental setting is the so-called propensity score matching (Abadie and Imbens, 2006, 2016; Rosenbaum and Rubin, 1983; Rubin, 1974). Bilicka (2019) applies propensity score matching for a sample of UK MNEs and domestic firms to evaluate BEPS. The matching process follows a two-step procedure. First, the likelihood of firms being domestic or multinational is estimated based on observable characteristics. Second, domestic and multinational firms are matched based on the estimated propensity scores. The method excludes firms that are very unlikely to serve as a comparable benchmark group. The benefits of the propensity score matching approach go beyond the OECD's regression framework. First, a key advantage is the possibility to assess the similarity of the two groups of MNE affiliates and non-MNE entities at a glance after the first matching step. Second, the propensity score matching allows to fine tune the proximity of the two groups along the observable matching dimensions. Third, a successful matching allows to directly compare the variable of interest, here the average ETR, between the two groups.

Table 2.6: Indicator 4 – Propensity Score Matching Evaluation

	Standardized Differences			Variance Ratio	
Variable	Raw	Matched	Bias reduction	Raw	Matched
log Total Assets	1.2628	-0.0134	98.94%	1.3302	0.9296
log Productivity	0.1151	-0.0351	69.51%	1.0248	0.9369
D/E Ratio	-0.2246	0.0267	88.14%	0.4634	0.9854
Innovation	-0.0967	-0.0091	90.57%	0.8897	0.9408

Notes: This table presents the evaluation of the matching procedure on the natural logarithm of total assets (*log Total Assets*), the natural logarithm of productivity (*log Productivity*), which is the ratio of sales to total wages, the debt to equity ratio (*D/E Ratio*), and *Innovation*, which is the ratio of intangible fixed assets to total assets. The column *Raw* depicts the standardized differences and variance ratios in the unmatched sample. The column *Matched* depicts the standardized differences and variance ratios in the matched sample. The column *Bias Reduction* is the percentage reduction in the standardized differences between the unmatched and matched sample. The values depict the averages of all years. Standardized differences close to 0 and variance ratios close to 1 are indicators of a good matching quality.

Hence, we extend the OECD's fourth indicator by applying a propensity score matching approach to estimate the average treatment effect (ATE), i.e., the differential between the average ETR of MNE affiliates and non-MNE entities or the effect of being a multinational firm, on the effective tax rates. We borrow from Bilicka (2019) and match MNEs to domestic firms. We match firms based on the logarithm of total assets, the logarithm of firm's productivity, the debt to equity ratio and the ratio of intangible to total assets within year, industry and country. All observable characteristics, which we use in the matching process, should be similar in the matched sample. Table 2.6 shows how the similarity of the two groups improves in the matched sample. A solid sign of matching quality is a standardized difference between the samples of close to zero and a variance ratio of about one. On average, our matched sample approaches this standard for all observable matching characteristics within each country industry and year matching cluster.

Table 2.7: Indicator 4 – Propensity Score Matching Estimates on ETR difference

Year	ATE	Year	ATE	Year	ATE
2000	-1.0154***	2006	-0.1611	2012	-0.5112***
	(0.3164)		(0.1900)		(0.1591)
2001	-0.7162**	2007	-0.5774***	2013	-0.5548***
	(0.2794)		(0.1758)		(0.1576)
2002	-0.2295	2008	-0.8239***	2014	-0.3559**
	(0.2754)		(0.1835)		(0.1528)
2003	-0.4179	2009	-0.1339	2015	-0.5237***
	(0.2605)		(0.1780)		(0.1555)
2004	-0.5825**	2010	-0.1824	2016	-0.9351***
	(0.2290)		(0.1688)		(0.1572)
2005	-0.7053***	2011	-0.4686***		
	(0.2086)		(0.1627)		
Observations			3,669,138		

Notes: The table depicts the annual average treatment effects (*ATE*) of being a multinational corporation on the *ETR*. The *ATE* estimates are based on a propensity score estimation procedure. The groups of multinational and domestic firms are matched on the logarithm of total assets (*log Total Assets*), the logarithm of productivity (*log Productivity*), which is the ratio of sales to total wages, the debt to equity ratio (*D/E Ratio*), and *Innovation*, which is the ratio of intangible fixed assets to total assets. Standard errors are rely on the adjustment by Abadie and Imbens (2006) and take into account that the propensity scores to match the groups are estimated. ***, **, * denote statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively.

We then estimate the average treatment effect for each year in our sample. Table 2.7 depicts the yearly estimated treatment effect of being a multinational firm on the ETR. We have plotted the estimated average treatment effects in Figure 2.4. The estimated ETR differences range between -0.13 and -1.02 percentage points and do not follow a specific trend in our sample period. In comparison to the regression estimates, the differentials are slightly smaller in the first half of our sample period. Moreover, in several years the estimates are statistically indistinguishable from zero. The estimated effective tax rate differentials depend – as the choice of control variables in the OECD regression approach – on the specific observable characteristics used for the propensity score matching. Hence, alternative controls or matching characteristics could lead to differences in the magnitude of the estimated ETR differentials.

2.3.4 Rationale and Shortcomings

Indicator 4 is the only indicator that includes the usage of counterfactuals as control group. This is the key advantage of Indicator 4 in contrast to all other suggested indicators to measure and monitor BEPS. However, the specifications of this indicator as defined by the OECD include other shortcomings that go beyond the matching quality that we have addressed in the preceding subsection.

It is highly questionable if the dependent variable ETR is a suitable measure to capture profit shifting. The unconsolidated ETR, which relates to tax expenditures over reported pre-tax profits, does not capture any of the known profit shifting channels such as transfer pricing, debt shifting nor royalty allocation (Heckemeyer et al., 2021). Specifically, the unconsoldated ETR's nominator and denominator are affected by profit shifting. By construction it can – if at all – indicate certain forms of special tax incentives, loss-offsets, hybrid mismatch arrangements, tax negotiations or other non-profit shifting related methods to reduce a firm's tax burden. Moreover, the unconsolidated ETR is rarely a key performance indicator of multinational corporations. Managers, and stakeholders rather focus on a group's overall tax burden, i.e., the consolidated ETR. In its current design, the indicator also neglects any differences within the group of multinational firms. While groups with affiliates in tax haven locations can be presumed to engage more actively in profit shifting, groups without links to low-tax jurisdictions might not have a strong incentive to relocate income.

2.4 Profit Shifting through Intangibles

> *"The indicator compares the average ratio of royalties received to R&D expenditures for a group of high-ratio countries to the average ratio for the other countries in the sample."* (OECD, 2015b, p. 60)

2.4.1 Methodology and Data

OECD Indicator 5 relies on macro-data rather than firm-level data and is concerned with profit shifting through intangibles. Profit shifting through intangibles is commonly defined as the strategy of transferring IP from high-tax to low-tax countries for tax purposes after it has been developed in high-tax countries. Using this structure, affiliates in high-tax countries pay (potentially high amounts of) royalties for the use of the IP to affiliates in a low-tax country. The indicator shall indirectly capture the extent of BEPS through IP transfer. Following the logic of transferring IP to low-tax countries for tax purposes, IP receiving countries should have a higher ratio of royalty receipts to research and development (R&D) spending compared to those countries where the IP was developed. For this reason, in a first step, the ratio of royalty receipts relative to R&D spending is measured for each country. Next, countries are assigned into two groups based on their concentration in a given year. Countries with a ratio above 50 percent are assigned to the group of high-ratio countries while the other countries form the group of low-ratio countries. By dividing the average ratio of the high-ratio group with the average ratio of the low-ratio group, Indicator 5 is formed for year t as:

$$Indicator\ 5_t = \frac{\frac{\sum_{i=1}^{I} Royalty\ receipts_{it}}{\sum_{i=1}^{I} R\&D\ spending_{it}}}{\frac{\sum_{j=1}^{J} Royalty\ receipts_{jt}}{\sum_{j=1}^{J} R\&D\ spending_{jt}}}, \tag{2.3}$$

where the subscript i refers to members of the high-ratio group and subscript j to members of the low-ratio group in year t.

In its 2015 report, the OECD uses the year 2011 as the base year to identify the composition of the high-ratio and low-ratio group, which is held constant in the other years. We replicate the indicator using 2011 and 2017 as the base year.[7] We also replicate the indicator by recalculating the two groups continuously on a yearly basis. Furthermore, we check the robustness of our results through different tests.

We obtain country-level data on receipts for the use of IP as balance of payments in current US dollar for the years 2005-18 from the World Bank.[8] Moreover, we use data on the gross domestic expenditure on R&D from the UNESCO Institute for Statistics (UIS.Stat).[9] The data availability is depicted in Table 2.8.

Table 2.8: Indicator 5 – Country-Year Distribution

Year	No of Countries
2005	64
2006	60
2007	72
2008	70
2009	70
2010	70
2011	69
2012	68
2013	74
2014	68
2015	74
2016	72
2017	76
2018	56

Notes: The table shows the number of countries per year used for the construction of Indicator 5. These countries have available data on receipts for the use of IP and available data on the gross domestic expenditure on R&D.

2.4.2 Results

First, we replicate the OECD's estimation of royalty receipts to R&D spending and take 2011 as the base year for allocating countries into high-ratio and low-ratio groups. In 2011, data is available for 69 countries, of which eight countries are assigned to the high-ratio group. The structure of the high-ratio group is shown in Table 2.9.

[7]We take 2017 instead of 2018 as the base year for data availability reasons.

[8]World Bank indicator code: BX.GSR.ROYL.CD

[9]The OECD names the World Development Indicators as its data source on R&D expenditures. However, we could only find data on R&D expenditures as a percentage of GDP. Using this data would have added even larger measurement error to our calculations. We verify our results using R&D spending data from the OECD, where we obtain similar results.

In fact, members of the high-ratio group are European countries with low corporate income tax rates or preferential tax systems. For example, Ireland, the Netherlands and Luxembourg are part of this group.

Table 2.9: Indicator 5 – Countries in High-Ratio Group

No	Base Year 2011	Base Year 2017
1	Guatemala	El Salvador
2	Hungary	Hungary
3	Ireland	Luxembourg
4	Lesotho	Madagascar
5	Luxembourg	Malta
6	Madagascar	Netherlands
7	Malta	Singapore
8	Netherlands	Switzerland
9		United Kingdom

Notes: The table shows the countries belonging to the high-ratio group. High-ratio countries are those countries that have a royalty receipts to R&D spending ratio of above 0.5 in a pre-defined base year. Column 1 and 2 refer to base years 2011 and 2017, respectively.

In the year 2011, the high-ratio countries received €1.53 of royalty for every €1 invested in R&D while the low-ratio countries received only €0.18. Thus, the ratio for the high-ratio countries is almost nine times larger than that of the low-ratio countries, leading to an indicator value of 8.7. Table 2.10 provides annual estimates of Indicator 5. Over the years, the indicator does not vary significantly. In 2005, the indicator takes a value of 7.7, which increases to 9.1 until 2010. After being stable for about three years, the indicator increases to 11.9 in 2015 but decreases again to 9.8 in the year 2017. Figure 2.5 Panel (A) plots the development of Indicator 5 graphically. In contrast to the estimates of the OECD, our estimated Indicator 5 value is higher but we do not observe a strong increase over time. Our estimates confirm that some countries receive comparably very high shares of royalties to R&D spending. In 2011, the eight countries in the high-ratio group received about 13.4 percent of the overall royalties of the 69 countries examined.

Second, we take the year 2017 as a base year for group allocation to replicate the OECD's results. The group of high-ratio countries consists of nine countries, which are named in Table 2.9. Table 2.10 and Figure 2.5 Panel (B) depict the estimates. From 2005 to 2018, the indicator ranges between 3.5 and 5.7, taking its peak in 2017. Again,

Table 2.10: Indicator 5 – Results

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	OECD	Base Year: 2011			Base Year: 2017			Base Year: Continuous		
Year	Indicator	High	Low	Indicator	High	Low	Indicator	High	Low	Indicator
2005	2.8	131%	17%	7.7	64%	16%	4.0	148%	17%	8.7
2006	2.5	130%	17%	7.6	61%	16%	3.8	157%	17%	9.2
2007	2.6	140%	18%	7.7	62%	18%	3.5	168%	18%	9.1
2008	2.5	150%	18%	8.4	66%	17%	3.9	126%	17%	7.2
2009	2.7	153%	17%	9.1	74%	16%	4.6	178%	17%	10.6
2010	4.3	156%	17%	9.1	72%	17%	4.3	152%	17%	8.9
2011	5.8	153%	18%	8.7	70%	17%	4.0	153%	18%	8.7
2012	5.8	169%	19%	9.0	75%	18%	4.3	127%	18%	7.2
2013		168%	18%	9.5	78%	17%	4.5	146%	18%	8.3
2014		201%	18%	10.9	86%	18%	4.8	127%	18%	7.1
2015		235%	20%	11.9	92%	18%	5.1	126%	18%	7.0
2016		186%	19%	9.9	84%	18%	4.6	168%	18%	9.2
2017		194%	20%	9.8	98%	17%	5.7	98%	17%	5.7
2018		195%	18%	10.7	86%	18%	4.7	195%	18%	10.7

Notes: Notes: The table depicts the values of Indicator 5 by year. Column (2) shows indicator values estimated by the OECD (OECD, 2015b). For each country, the ratio of royalty receipts to R&D spending is calculated. Based on this ratio in a pre-defined base year, countries are assigned to high-ratio groups or low-ratio groups. The threshold values amounts to 50 percent. Column (3) and (4) show the ratio of the countries in the high-ratio group and low-ratio group, respectively. Column (5) shows the estimated indicator value. Vertical lines define the different base years for group allocation. The Worldbank and UNESCO are used as data source.

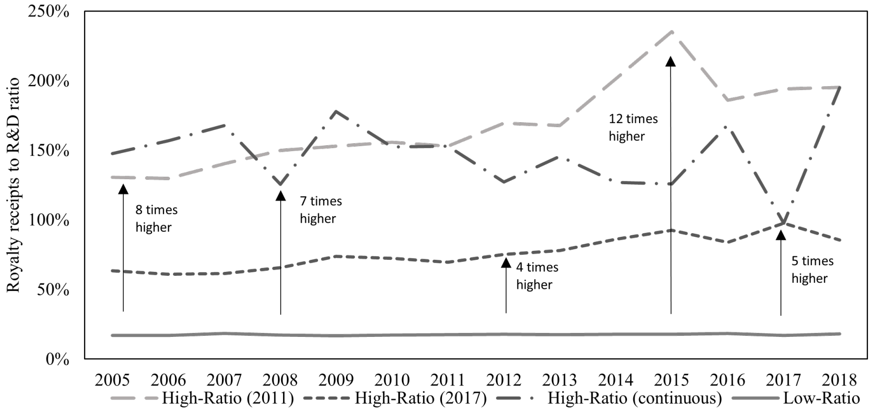
the indicator values seem to be stable over time and have about the same size as the OECD's estimates. The high-ratio countries received €0.98 of royalties for every €1 invested in R&D in 2017, while the low-ratio countries received only €0.17.

Third, we refrain from pre-determined group allocation but re-estimate the allocation of the high-ratio and low-ratio group every year. As shown in Figure 2.5 Panel (A), the indicator values range between 5.7 and 10.7 without a clear pattern over the years. The greater dispersion can be explained by the annual re-calculation of the sample for the indicator estimation. Nevertheless, the values do not exceed or fall below those of the samples with base years.

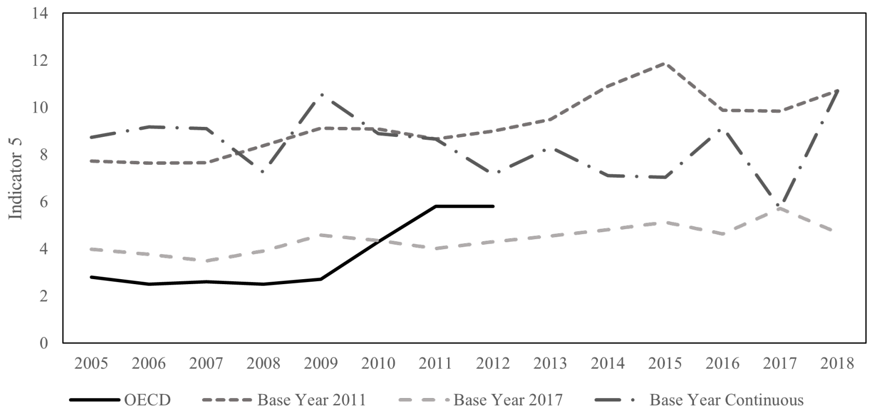
Finally, we test the robustness of our analysis. First, we use OECD data on R&D spending as alternative data source for R&D spending. Even though the R&D data are only available for, on average, 37 countries, the results verify our previous findings. Second, we replicate our analysis using only countries for which we have at least 10 years or 14 years of data available. Again, the values of the indicator do not change

Figure 2.5: Indicator 5 - Graphical Results

(A) Royalty Receipts to R&D Spending by group



(B) Indicator Trend over time



Notes: This figure plots the trend of Indicator 5 over time. In Panel (A), the dashed and dotted line depict the trend of the average royalty receipts to R&D spending ratio for the group of countries that have a ratio above 0.5 in 2011 and 2017, respectively. The dash-dotted line depicts the trend of for the group of countries that have a ratio above 0.5 in the particular year. The solid black line displays the average ratio of the remaining (low-ratio) countries. In Panel (B), the dashed and dotted line shows the trend of Indicator 5 using 2012 or 2017 as base year for group allocation. The dash-dotted line redefines the group allocation every year. The solid black line shows the indicator's trend estimated by the OECD (OECD, 2015b).

significantly. Third, we also obtain similar indicator results if we use the lag values of R&D spending for estimation. We do this to adjust for the possibility that between the time of receipts from royalty and the time of R&D spending a time gap exists.

2.4.3 Rationale and Shortcomings

Previous research has shown that MNEs transfer intellectual property to affiliates located in countries with relatively lower corporate tax rates for BEPS reasons (e.g., Dischinger and Riedel, 2011; Karkinsky and Riedel, 2012). Indicator 5 is based on this rationale and aims to measure BEPS as the income stream generated by IP relative to the R&D expenditures in a country. Specifically, this assumes that MNEs use the resources of industrial countries, which often levy higher corporate tax rates, for complex R&D tasks and, subsequently, transfer the developed IP to countries with lower tax rates, causing a deviation between royalty payments and R&D expenditures.

However, the simplicity of the indicator leads to various shortcomings that need to be considered when interpreting the results. First, Indicator 5 is an indirect measure of BEPS and no assertion can be made about the scale of BEPS. Second, royalty payments may not only be linked to R&D spending but also to the use of trademarks, copyrights or franchises (Heckemeyer et al., 2021). Third, it is assumed that MNEs shift IP for tax reasons. However, the definition of the indicator neither provides a direct link to taxes nor does it capture the movement of IP. Thus, this indicator could, for example, also proxy R&D productivity by capturing the difference between countries with highly valuable R&D and less valuable R&D. Fourth, the proposed tax planning strategy of transferring IP from R&D countries to low-tax countries may be limited and undesirable since exit-taxation could eliminate potential tax benefits (Ernst and Spengel, 2011). Fifth, even though we try to account for time lags between R&D and IP output in robustness tests, the true time period is unobservable and potentially very diverse. Hence, the indicator variation over time might be misleading. Fifth, this indicator is on the aggregated country level and does not account for country size. Thus, small countries might be overrepresented. Lastly, the group assignment of the indicator depends on an arbitrarily chosen threshold without taking other factors into account.

2.5 Conclusion

Profit shifting of multinational firms is a pressing topic in the public debate, academic research and on the political agenda. Yet, measuring the extent of profit shifting and assessing the economic relevance of it is a major challenge. In its 2015 published Final Report on "Measuring and Monitoring BEPS, Action 11", the OECD has introduced six indicators to measure and evaluate BEPS activity over time and on different levels of aggregation. We replicate one indicator from each of the three subordinate categories and update the numbers underlying the ongoing political debate to reform the global corporate tax system. We build on the conceptual evaluation of the indicators by Heckemeyer et al. (2021) and focus our analysis on the most convincing indicators: Indicator 1 (Disconnect between financial and real economic activities), Indicator 4 (MNE vs. "comparable" non-MNE effective tax rate differentials) and Indicator 5 (Profit shifting through intangibles).

Following the OECD's specification, we closely replicate the estimates of Indicator 1, which intends to indirectly measure BEPS through the use of offshore tax havens. We transparently show that countries with low or no corporate income tax (CIT) rates or preferential tax systems, e.g., the Bahamas, Cayman Islands or Ireland, have very high concentrations of FDI relative to their GDP. Extrapolating the indicator to recent years, the net FDI to GDP ratio shows a moderately decreasing trend and the gross FDI to GDP ratio remains at a stable level. The replicated regression estimates of Indicator 4 show that multinational firms have lower effective tax rates than domestic firms. This difference diminishes over time. Our annual estimated ETR differential is lower even in the years that overlap with the OECD sample period. We repeat the analysis using a propensity score matching approach, finding similar results. The replication of Indicator 5, which is concerned with profit shifting through intangibles, shows a stable trend of the annual indicator estimates that extends beyond the OECD's sample period. Similar to the first indicator, we transparently show that countries with high ratios of royalty receipts to research and development (R&D) spending are countries with low corporate income tax rates or IP box regimes, e.g., Ireland, the Netherlands and Luxembourg.

Overall, the OECD's intend to provide a convincing and simple dashboard of indicators that allows to evaluate the existence and scale of BEPS and to measure and monitor how BEPS evolves over time comes with a number of shortcomings. The indicators highly depend on the underlying assumptions, the availability of data and may be influenced by various confounding factors beyond BEPS. Hence, the informative value of the indicators for policymakers is limited. Yet, transparent updates on the existence and extent of BEPS are important for the ongoing public and academic debate on the necessity to reform the corporate income tax system. We endorse the ongoing empirical research that exploits well-specified identification strategies and granular data to measure the existence and extent of BEPS and propose to tackle the issue from different angles. Only multidimensional approaches allow to develop a holistic view of BEPS and to evaluate ongoing proposals to reform the global corporate income tax system.

Chapter 3

Internal Digitalization and Tax-efficient Decision Making

Co-Authors: Christopher Ludwig and Katharina Nicolay

Abstract: This study examines the impact of internal digitalization on corporate tax planning decisions. Specifically, this study investigates the effect of firms' digitalization on tax-motivated income shifting. We exploit affiliate-level survey data to construct a novel digitalization index that provides a holistic picture of firms' digitalization. We find a significant association between the level of digitalization and tax-motivated income shifting. An instrumental variables approach and additional tests corroborate this finding and mitigate endogeneity concerns. Furthermore, we disentangle the index into its individual software components and find that communication technology is a key enabler of efficient tax planning. Overall, we provide two forms of evidence. First, digitalization is an important firm-specific mechanism of tax-motivated income shifting. Second, digitalization has positive performance effects in support business functions.

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3.1 Introduction

This paper studies how firm's internal digitalization – that is the availability, accessibility, and usage of sophisticated software – affects corporate tax planning. Prior work in computer science and economics shows that digitalization increases efficiencies within the firm and leads to better performance (e.g., Brynjolfsson et al., 2011; Bloom et al., 2012). However, there is very little evidence about the direct role of digitalization within the accounting literature, despite work using coarse proxies to measure and study internal information quality (Gallemore and Labro, 2015). Only recently has accounting research started to study specific investments in software to investigate the discrete mechanism by which firms invest in digitalization (Charoenwong et al., 2022). We add to this nascent literature by examining whether and to what extent digitalization affects tax department's performance.

Digitalization should be particularly impactful to the corporate tax department, given the wide range of taxes the department manages and the multiple jurisdictions in which firms operate. For example, firms must navigate complexities of income and non-income taxes (such a payroll, property, sales, value-added, customs, and other non-income taxes), as well as complexities of tax law at the federal, state, local, and international level. These issues are even more pronounced in recent years, given an increasing number of multilateral requirements (such as country-by-country reporting) that require a firm to aggregate, analyze, and construct new reports about activities across countries.

We use a novel measure of within-firm digitalization to study tax-motivated income shifting, a strategy particularly sensitive to the amount and quality of information. Digitalization may specifically help tax departments in this endeavor for two reasons. First, digitalization should improve data quality and facilitate collaboration across a dispersed firm, thereby aiding in cross-country planning strategies such as tax-motivated income shifting. Second, digitalization should also provide operational efficiencies for routine tasks and tax compliance, thereby freeing up resources and human capital to pursue more value-added tax planning. However, we may not observe any effect of digitalization if anectodal evidence remains true that firms are either extremely hesitant to introduce digitalization in the tax department or the introduction evokes significant implementation costs in the form of inefficiencies (KPMG, 2019).

While information quality is a central construct within the broader accounting and tax literature, prior work contains extremely little empirical evidence documenting the specific digitalization mechanism by which firms improve their internal reporting. One reason for this relatively lack of evidence is the difficulty of observing and measuring software use. In line with concurrent work in accounting (Charoenwong et al., 2022), we use international survey data that allows us to directly observe digitalization at the affiliate level of multinational firms. We create a novel micro-level digitalization index that captures affiliates' access to three software solutions, namely enterprise resource planning (ERP) software, database management systems (DBMS), and communication software. For example, ERP software supports real-time management of tax processes, such as defining transfer prices in complex value chains. DBMS enables the storage and processing of large amounts of tax information and is used by multinationals such as Volkswagen and Unilever. Communication software provides real-time exchange and collaboration between tax managers within the firm. We use these components to reflect both information technologies and communication technologies.

We begin our analysis by investigating the association between digitalization and the tax sensitivity of reported profits using the Huizinga and Laeven (2008) cross-border income shifting estimation approach. Exploiting 131,642 European affiliate-year observations of 11,957 multinational firms in the period from 2006 to 2016, we find reported profits of digitalized affiliates to be significantly more responsive to group wide tax rate differentials than reported profits of non-digitalized affiliates. The positive association between digitalization and tax-motivated income shifting holds if we control for a number of observable affiliate characteristics, such as *Capital*, *Labor*, and *Productivity*, macro controls, such as *GDPperCapita*, *Unemployment*, and *Inflation*, and if we include different fixed effects, such as year fixed effects, country-by-year fixed effects, industry fixed effects, and affiliate fixed effects. This finding suggests that digitalization is an important firm-specific mechanism for efficient tax-motivated income shifting. The result is also economically significant. The coefficient estimate of our baseline result indicates that, at the margin, a one standard deviation increase in the level of digitalization increases the tax sensitivity of reported profits by 35 percent.

We acknowledge that unobservable affiliate-specific characteristics, such as affiliates' openness to new technological developments or willingness to enter new geographical markets, could be associated with both investment in digitalization and tax-motivated income shifting. To mitigate these endogeneity concerns, we undertake two additional analyses. First, we rerun our analysis but interact all control variables, including the fixed effects structure, with our variable of interest. The complete interaction controls for differences in *observable* time-varying affiliate characteristics that may be correlated with digitalization. Next, we employ an instrumental variables regression to address concerns that potential time-varying *unobservable* characteristics affect our results. We use affiliates' distance to SAP, the leading European business software provider, and, following e.g., Bloom et al. (2016), affiliates' one-year lagged level of digitalization as instruments for digitalization. Our inferences remain unchanged.

Having established that overall digitalization is associated with tax-motivated income shifting, we examine whether the distinct components of the digitalization index vary in their effect. For example, Bloom et al. (2014) show that information technologies and communication technologies have different effects on decision making and control in firms. We disentangle the digitalization index into the underlying software components. Using our baseline approach, we find significant evidence that communication software is associated with the tax sensitivity of reported profits. This result suggests that software improving the communication and collaboration between departments and managers is a key driver of multinational firms' efficient tax planning. This is consistent with prior evidence on the importance of communication software for corporate decisions in multinational teams (Artail, 2006; Andriole, 2010; de Vreede et al., 2016). Furthermore, we find some weak evidence that ERP software is positively associated with tax-motivated income shifting but find no effect for DBMS.

To further investigate the association between ERP software and tax-motivated income shifting, we leverage the introduction of a new ERP software by the European market leader SAP in 2009 as an alternative identification strategy. Using a triple difference-in-differences design, we find that affiliates which implement an ERP software engage significantly more in tax-motivated income shifting relative to affiliates that do not implement an ERP software. We further show that the parallel trend assumption is met and that the results are robust to controlling for the availability of other software solutions (Olden and Men, 2022). Given the previous finding on communication software, this result indicates that data and information provided by ERP software serve as an important prerequisite for the implementation of efficient income shifting strategies.

In additional analyses, we investigate heterogeneity in multinational firm characteristics. First, we test the hypothesis that digitalization is more helpful in settings with higher complexity. To do so, we examine the geographic dispersion of multinational firms because efficient tax-motivated income shifting will be more complex if affiliates are located in various countries across the globe. Indeed, we find that the results are more pronounced for affiliates that are part of an international dispersed multinational firm. Second, we analyze whether the association of digitalization and tax-motivated income shifting varies across industries. We find a significant association between digitalization and the sensitivity of reported profits for affiliates in the manufacturing industry which is in line with the increased complexity of this industry relative to the service industry or retail industry. However, the differences in coefficients between the industries are not significant in statistical terms.

Finally, we conduct a battery of robustness tests. The association between digitalization and tax-motivated income shifting remains robust when controlling for affiliates' usage of intellectual property, replacing the income shifting incentive measure by the statutory corporate tax rate, varying the dependent variable, or changing the functional form of the digitalization index.

This paper contributes to the existing literature by demonstrating that digitalization serves as a firm-specific mechanism of tax-motivated income shifting. While prior research has shown that firms strategically optimize their tax planning (e.g., Kim et al., 2019; Hopland et al., 2018), we demonstrate that digitalization is a key enabler and mechanism for these tax planning optimizations. In this sense, the study is also related to the broader body of literature that investigates general determinants of tax-motivated income shifting, such as foreign employment (Drake et al., 2022), losses (De Simone et al., 2017), tax systems (Markle, 2016), or accounting standards (De Simone, 2016).

Moreover, this study provides new evidence to the strand of literature examining the association between internal information quality and tax planning. Gallemore and Labro (2015) show that higher internal information quality is associated with lower effective tax rates. They proxy for internal information quality using the time gap between the end of fiscal year and earnings announcement date, management's earnings forecast accuracy, material weaknesses in internal controls, or the absence of restatements. Building on this finding, McGuire et al. (2018) find a positive association between internal information quality and tax-motivated income shifting. Finally, Laplante et al. (2021) examine the association between internal information quality and state level tax planning. While these studies rely on broad proxies for internal information quality on the consolidated level derived from external information, we exploit granular data on software availability at the affiliate level. This allows us to precisely identify one of the specific channels of internal information quality. Distinguishing between software types that increase the quality of data and those that increase the efficiency of collaboration, we further highlight the important role of communication software for efficient tax planning.

Lastly, our results indicate that digitalization has positive performance effects in tax departments. Hence, we contribute to the open question of how digitalization in support business functions changes decision making and management practices (Brynjolfsson et al., 2021). Several studies show that investments in digital technology and data-driven decision making positively impact firm performance (e.g., Brynjolfsson et al., 2011; Melville et al., 2004; Hitt et al., 2002), while other studies demonstrate that information systems do not necessarily affect firm performance (Li and Sandino, 2018; Fredrich and Bouncken, 2021). Analyzing the effect of digitalization in multiple business functions can help to uncover the holistic effect of digitalization on firm performance.

3.2 Theoretical Background

3.2.1 Decision Making in Tax Departments

Historically, tax departments spent significant resources on manually collecting, manipulating, and validating data in spreadsheets to fulfill internal and external compliance demands (Deloitte, 2015; PWC, 2015). At the same time, the tax function is one of the "largest consumers of data in an organization" with virtually every transaction having a potential tax impact (PWC, 2015). Hence, it comes as little surprise that tax consultancies urge corporations on tapping the full potential of digitalization for the tax function. On the one hand digitalization could improve efficiencies in recurring tasks and tax compliance, on the other hand free up resources and increase data quality to engage in proactive tax planning activity.[1] While tax compliance benefits are difficult to measure empirically, the impact of digitalization is presumably best observable in decisions derived under tax departments' objective function of after-tax profit maximization (Robinson et al., 2010; Scholes et al., 2016). Since multinational firms operate in various countries that differ in their tax rates, maximizing after-tax profits is achieved by minimizing the tax burden through efficient income allocation from high-tax countries to low-tax countries. Hence, to analyze whether digitalization leads to better decision making in tax departments, we measure the effect of digitalization on firms' ability to exploit income shifting incentives.

Following Scholes et al. (2016), the strategy of tax minimization "requires the planner

[1] For example, readily available accounting information, stored in fast database management systems, facilitates the preparation of annual income tax declarations and ensures readily available documentation. Seamless invoice management, with highly interconnected enterprise resource planning systems, ensures tax compliance with regard to transaction taxes.

to consider the tax implications of a proposed transaction for all parties to the transaction." In multinational firms with global operations, this endeavor may be highly complex, opaque, and costly (Hines and Rice, 1994; Huizinga and Laeven, 2008). Digitalization might help to reduce this complexity leading to improved management decisions on internal transfer prices and financing decisions. Conceptually, we expect better and timelier information on intra-group transactions to reduce the marginal costs of tax-motivated income shifting. This implies that for given marginal benefits of tax-motivated income shifting, we expect an increase in the share of shifted income caused by digitalization. Hence, we hypothesize that affiliates with higher levels of digitalization engage more actively in tax-motivated income shifting than affiliates with lower levels of digitalization.

3.2.2 Operational Dimensions of Digitalization

Digitalization does not refer to a single, comprehensive tool. Rather, it is the umbrella term for the usage of various software solutions that improve business processes along specific operational dimensions. In this paper, we focus on three software solutions that are key enabler of firms' digitalization and most relevant for efficient decision making in tax departments.

First, we focus on enterprise resource planning (ERP) software. An ERP software is a package that provides detailed information on a firm's resources and activities. In general, ERP software is adapted to the specific needs of a firm's operations and designed to integrate, optimize, and control different stages of the value creation process. Core features of the software usually help multinational firms to plan and monitor procurement, production, invoicing, human resources, and financial reporting. ERP software has become increasingly important for all kinds of business models and are essential for corporations' digitalization process (Haddara and Elragal, 2015; Hitt et al., 2002). In the last decade, ERP providers, such as SAP or Oracle, have developed applications that allow real-time analysis of processes and offer flexible solutions for small and large businesses. With respect to the tax department, ERP software has an influence on compliance with direct taxes, indirect taxes (e.g., Value Added Tax, Goods and Services Tax), and customs. Integrating taxes into the affiliate's ERP software saves time and money on recurring tasks while providing an opportunity for tax departments to focus on value-add tasks, such as tax planning. The systematic and real-time documentation of internal transactions and affiliates' financing situation enables tax managers to efficiently monitor and adjust transfer-prices and financing structures.

Second, we focus on database management software (DBMS). DBMS enables the systematic storage of information and data, its maintenance, and interaction with information and data (Connolly and Begg, 2014). A rigorous information and data management is essential for internal process evaluation and a critical infrastructure element to enable big data analytics (Grover et al., 2018). According to Grover et al. (2018), DBMS generates the principal value for big data analytics – that allows real-time business insights and the basis for well-reasoned decision making – by combining different existing and new data sources. Well-maintained database management systems ensure the availability and accessibility of data in case of any documentation requests by fiscal authorities. Hence, DBMS is the foundation for the tax department to access real-time information on firms, transactions, costs, products, and accounts across the entire multinational firm. Readily accessible, real-time data enables tax departments to increase regulatory compliance, to adjust transfer prices to changing circumstances, and to reliably forecast an affiliate's tax burden.

Third, we focus on communication software. Communication software enables close interaction and information exchange within a multinational firm. While some early studies provide evidence on the reduced efficiency of indirect communication via digital channels compared to face-to-face interaction (Hightower and Sayeed, 1995; McGrath and Hollingshead, 1994; Shim et al., 2002), interactive groupware software, with communication tools such as videoconferencing, can create effective virtual teams that swiftly process information and efficiently collaborate in decision making processes. In this regard, communication software, such as Microsoft Teams or Zoom, has been a major facilitator of collaboration between dispersed team members during the COVID-

19 pandemic. From a tax perspective, communication software improves group-wide cooperation and maintains the awareness and communication of global tax planning strategies. The close interaction and exchange between members of the tax department and managers in cross-border affiliates facilitate the management of transfer-pricing contracts and ensures a better awareness of tax planning strategies.

Overall, each of the software solutions should increase the information quality within the tax department, improve processes between affiliated tax departments, and contribute to more successful decision making. Thereby, we combine software solutions that are related to information technologies and software solutions that are associated to communication technologies (Bloom et al., 2014). Hence, we assume that using these software solutions will provide us with an holistic picture on affiliates' level of digitalization.

3.2.3 Prior Literature

We draw on the insights of Brynjolfsson et al. (2011) who unfold how more digitalization translates to better information and decision making. The authors demonstrate that digitalization allows for a more granular knowledge on the potential outcomes of decisions by reducing the noise between the possible results. In addition, firms with sophisticated information processing techniques provided through digitalization convert information into value at lower costs and with greater efficiency (Brynjolfsson et al., 2011; Galbraith, 1974). While several studies evaluate the effect of digitalization on the performance of core business operations (Li and Sandino, 2018; Müller et al., 2018; Aral et al., 2012; McAfee, 2002), it is still an open question how the advantages of more digitalization materialize in better decision making and management practices in non-central business functions (Brynjolfsson et al., 2021).

By examining this question with respect to the tax department, we contribute to the strand of literature that aims at decoding the "black box" of tax planning decisions (Shackelford and Shevlin, 2001; Hanlon and Heitzman, 2010). While many studies focus on the outcome (e.g., Huizinga and Laeven, 2008; Klassen and Laplante, 2012;

Dharmapala and Riedel, 2013; De Simone et al., 2019; Tørsløv et al., 2021) or determinants (e.g., Drake et al., 2022; De Simone et al., 2017; De Simone, 2016; Markle, 2016) of unobserved tax-motivated income shifting decisions, evidence on the firm-specific mechanisms for developing tax strategies is comparably scarce. It is, however, of ample importance to learn how firms form their tax planning decisions in order to anticipate how policy changes or technological progress affect these strategies.

In addition, our study relates to the insights of Gallemore and Labro (2015), McGuire et al. (2018), and Laplante et al. (2021). Gallemore and Labro (2015) uncover the association between proxies for internal information quality and tax avoidance, measured by variations in cash effective tax rates. McGuire et al. (2018) build on their findings and link proxies for internal information quality, such as earnings announcement speed and lack of accounting restatements, to cross-border income shifting. Laplante et al. (2021) examine the association between internal information quality and state tax planning.[2] Internal information quality is by definition private to the firm and unobservable to the researcher. Hence, the studies derive internal information quality from external and coarse proxies such as the time gap between the end of fiscal year and earnings announcement date, management's earnings forecast accuracy, material weaknesses in internal controls, or the absence of restatements. On the contrary, our study relies on granular information on software usage observed at the affiliate level. This allows us to precisely uncover one of the specific channels of internal information quality. Distinguishing between software types that increase the quality of data and those that increase the efficiency of collaboration, we identify the exact mechanisms contributing to efficient tax planning.

[2]Moreover, Hamilton and Stekelberg (2016) show that multinational firms listed on a magazine's list of 500 digital firms tend to avoid taxes more than firms not listed. Yet, it remains questionable which exact channels contribute to the reduced effective tax rates since firms apply to be named on the magazine's list.

3.3 Data and Empirical Strategy

3.3.1 Measuring Digitalization

We exploit the European Aberdeen Computer Intelligence Technology Database (CiTDB) to identify firms' level of digitalization. The database comprises detailed and high-quality survey data on the use of digital technology and covers firms across twenty European countries (Bloom et al., 2016). The Aberdeen Group, which maintains the CiTDB mainly to support sales and marketing decisions of information technology (IT) devices and services distributors, contacts more than 200.000 firms per year and questions high-level IT employees on the status of a firm's hardware and software usage. Our European Aberdeen CiTDB survey panel covers the years 2006 through 2016 and includes firms with at least 100 employees. Even though this potentially excludes newly founded and small firms, firms with at least 100 employees will be the most relevant firms for our cross-country empirical analysis. The US version of the database has been used in several empirical studies in the economics literature to measure different dimensions of digitalization at the micro-level (Bloom et al., 2012, 2014, 2016; Bresnahan et al., 2002; Brynjolfsson and Hitt, 2003; Candel Haug et al., 2016; Forman et al., 2014; Mahr, 2010; De Stefano et al., 2017). Yet, most of the prior studies focus on core digital technology equipment such as computers or IT staff. We are expanding previous literature by creating a novel digitalization index that incorporates key software solutions.

As outlined in section 3.2.2, the digitalization index incorporates software solutions along three operational business process dimensions that are most relevant for efficient decision making in tax departments. The included software solutions comprise enterprise resource planning (ERP) software, database management software (DBMS) and communication software. We combine all survey responses on the availability of one of the three software categories to create an additive index that ranges from zero, no software is available, to three, the firm uses all software categories. A firm with no access to any of the software categories (indicator equals zero) is considered a non-digitalized firm. Firms with an indicator value of three, i.e., using all software types, are classified as the most digitalized firms in our sample.

The composition of the digitalization index over time is shown in Figure 3.1 Panel (A). As expected given the overall advancement in digital technologies, the number of affiliates with zero digitalization decreases over time, while the number of affiliates with digitalization index values of two or three increases. At the end of our sample period in 2016, most affiliates have a digitalization level of two. In Panel (B) of Figure 3.1, we graphically display the digitalization index composition split by industries. The within-industry distribution of the digitalization index seems to be heterogenous but generally comparable across industries. Across industries, around 35 percent of affiliates have a digitalization index value of two. Affiliates in the manufacturing industry and retail industry seem to have relatively higher levels of full digitalization. This finding is unsurprising because business processes in these industries tend to be more complex as they include, for example, the production and distribution of products. Hence, software solutions will be most valuable in these industries.

3.3.2 Sample Composition and the Income Shifting Incentive

To evaluate the relation of firms' level of digitalization and tax-motivated income shifting, we enrich the Aberdeen dataset with detailed financial information. We use unconsolidated financial data and ownership information from the Bureau van Dijk's ORBIS database because the unit of observation in the Aberdeen survey panel is affiliate level.[3] All unconsolidated affiliate level financial data for our sample from 2006 to 2016 is subject to a basic cleaning procedure following Kalemli-Ozcan et al. (2015). We merge the Aberdeen CiTDB to the ORBIS database, based on unique affiliate names.[4]

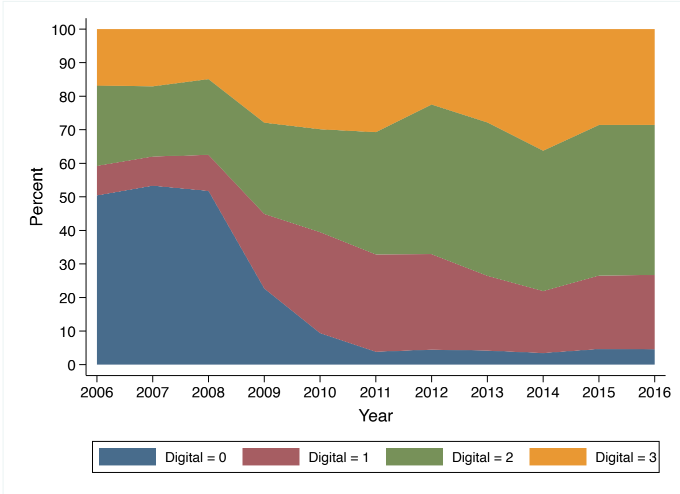
In a first step, we keep all affiliates of multinational firms for which we find at least one affiliate within the business group with a CiTDB to ORBIS concordance. As we

[3]This means that the Aberdeen group does not survey headquarters of multinational groups to obtain information on the overall group's digitalization but rather is interested in the affiliate's individual level of digitalization. For some firms, Aberdeen collects information even at the branch level. Our sample is based on information on affiliates, i.e., separate legal entities, affiliated to multinational firms.

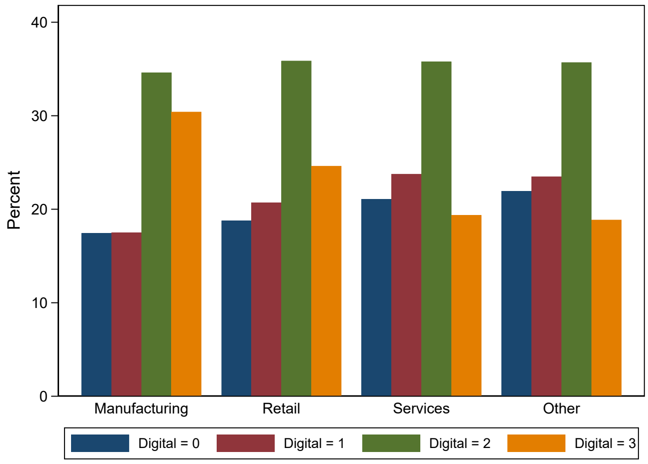
[4]A simple name matching procedure is the most appropriate method to link the CiTDB affiliates – due to a lack of a globally applicable identifier – to the ORBIS database.

Figure 3.1: Digitalization over Time and across Industries

(A) Digitalization over time



(B) Digitalization across industries



Notes: Panel (A) shows the distribution of the digitalization index (*Digital*) in the baseline sample over time. Panel (B) shows the distribution of *Digital* by industry (one digit NACE).

want to investigate the cross-border activities, we keep only affiliates in our sample that belong to multinational firms.[5] To measure the income shifting activity on the affiliate level, we rely on the income shifting incentive measure (C) of Huizinga and Laeven (2008). C is an intra-group incentive measure defined as the operating revenue (*OPRE*)-weighted average tax rate differential of each affiliate relative to all other affiliates within

[5]We define multinational firms as a group of affiliates with more than 50 percent ownership chains and at least one cross-border relation.

the group per year.

$$C_{it} = \frac{\sum_{k \neq i}^{n} OPRE_{kt} \cdot (CIT_{it} - CIT_{kt})}{\sum_{k=1}^{n} OPRE_{kt}}, \tag{3.1}$$

where i indicates the affiliate, k indicates the related affiliates within in the group, and n indicates the total number of affiliates per group. CIT refers to the statutory corporate income tax rate. In essence, the measure can be interpreted such that multinational firms have an incentive to relocate income for tax purposes from affiliates with positive C values to affiliates with negative C values. We calculate the intra-group C for each affiliate of a multinational firm.

In a second step, we only keep affiliates for which we observe a direct CiTDB survey response.[6] We do so because anecdotal evidence suggests that the digitalization can differ greatly between affiliates that belong to the same multinational firm.[7]

Following prior literature, we exclude loss-making affiliates and affiliates with insufficient data on our independent variables. Our final sample consists of 131,642 affiliate-years, with 24,025 unique affiliates that belong to 11,957 multinational firms. Table 3.1 shows the geographic dispersion of the sample. The sample covers 20 European countries and seems to be well-balanced in relation to the size of the national economies. Most observations come from Germany (15 percent), followed by France (13 percent) and the U.K. (12 percent). Furthermore, the relation of affiliate-years to unique affiliates reveals that we observe an affiliate, on average, for 5.5 years in our sample.

[6]If an affiliate is not part of the survey wave in a specific year, but the database provides information for preceding and subsequent year, we interpolate the available information.

[7]Anecdotal evidence from consultation with SAP sales persons suggests that the deployment of digital technologies differs greatly between affiliates that belong to the same multinational firm. Additionally, practitioners in an IT department of a global multinational firm have reassured us that the roll-out of digital technologies across affiliates is generally subject to local adjustments and that speed of adoption varies significantly across affiliates, leading to different levels of digitalization within a multinational firm.

Table 3.1: Geographical Dispersion of Sample

Country	Affiliate-Years	Percent	Affiliates	Percent
Austria	9,426	7.20	1,479	6.20
Belgium	9,774	7.40	1,454	6.10
Czech Republic	5,841	4.40	1,054	4.40
Denmark	4,088	3.10	691	2.90
Finland	3,853	2.90	631	2.60
France	17,106	13.00	3,399	14.10
Germany	19,220	14.60	3,648	15.20
Hungary	3,007	2.30	414	1.70
Ireland	1,537	1.20	324	1.30
Italy	14,406	10.90	2,399	10.00
Luxembourg	849	0.60	162	0.70
Netherlands	2,224	1.70	639	2.70
Norway	2,446	1.90	482	2.00
Poland	2,588	2.00	668	2.80
Portugal	3,305	2.50	577	2.40
Slovak Republic	1,813	1.40	351	1.50
Spain	12,516	9.50	2,142	8.90
Sweden	1,813	1.40	380	1.60
Switzerland	85	0.10	13	0.10
United Kingdom	15,745	12.00	3,118	13.00
Total	131,642	100	24,025	100

Notes: This table presents the country dispersion of the sample. Column (1) shows the affiliate-year observations per country and column (3) the unique affiliate observations per country.

3.3.3 Empirical Approach

To measure the impact of digitalization on multinational firms' income shifting activities, we employ the methodology of Hines and Rice (1994), later extended by Huizinga and Laeven (2008). The model assumes that the total income of an affiliate is the sum of true profits, approximated by the Cobb-Douglas production function, and shifted profits. Extending the production function with an income shifting incentive measure allows estimating the responsiveness of the total income to shifting activities. Exploiting this setting allows us to draw first insights on whether digitalized affiliates shift income more efficiently.

The model is commonly applied in the income shifting literature and has been extended by many authors to capture different income shifting determinants (e.g., Beer and Loeprick, 2015; Markle, 2016; De Simone, 2016; De Simone et al., 2017). We enhance the model with a measure for affiliates' level of digitalization:

$$
\begin{aligned}
log\ Profit_{it} = \beta_1\, log\ Capital_{it} + \beta_2\, log\ Labor_{it} + \beta_3\, Productivity_{it} + \\
\beta_4\, C_{it} + \beta_5\, Digital_{it} + \beta_6\, C_{it} \times Digital_{it} + \\
\alpha_j\, X_{it} + \gamma_t + \delta_{ind} + \epsilon_{it},
\end{aligned} \tag{3.2}
$$

where i and t are indicators for the affiliate and year, respectively. The dependent variable is the natural logarithm of profit and loss before tax (*Profit*) from unconsolidated financial accounts. In line with prior literature, we use the natural logarithm of tangible fixed assets (*Capital*) as a proxy for capital, the natural logarithm of employee compensation (*Labor*) as a proxy for labor and the median return on assets within industry, country and year (*Productivity*) as a proxy for productivity (De Simone et al., 2017; Markle, 2016). C is the income shifting incentive measure as described in section 3.3.2.

Digital refers to the digitalization index. This modification of the standard Huizinga and Laeven (2008) model allows us to evaluate the heterogeneity of income shifting between affiliates with different degrees of digitalization. X_{it} is a vector of j control variables. In line with the literature on income shifting, we control for the natural logarithm of GDP per capita, the unemployment rate, and the inflation rate in the affiliate's host country (Beer and Loeprick, 2015). Further, we follow Gallemore and Labro (2015) and McGuire et al. (2018) and include year fixed effects, γ_t, and industry fixed effects, δ_{ind}. Year fixed effects control for time-variant shocks that are common to all affiliates, while industry fixed effects capture time-invariant industry characteristics, such as openess to digitalization. In other specifications, we include affiliate fixed effects to control for time-invariant factors at the affiliate, θ_i, or interact all control variables and fixed effects with *Digital* to control for differences between affiliates that are correlated with digitalization.[8] Finally, ϵ_{it} is an error term. All variables are defined in Appendix Table B.1.

[8] Affiliates' ownership information are static and do not change over time. Hence, when including affiliate fixed effects, we also control for time invariant effects at the multinational group level.

3.3.4 Descriptive Statistics

We present descriptive statistics of our sample in Table 3.2. The full sample of 131,642 profitable affiliate-years reports a mean (median) pre-tax *Profit* of €5.8 million (€1.7 million). The components of the production function *Capital*, *Labor*, and *Productivity* have mean (median) values of €11.8 million (€2.5 million), €10.7 million (€5.5 million), and 5.34 (5.23), respectively. The affiliates demonstrate an average *C* of -0.001, representing a weak income shifting incentive to increase profits in the jurisdiction.[9] The median *C* is zero. Furthermore, the average affiliate has an digitalization index value of 1.67.

Table 3.2: Descriptive Statistics

Variables	Observations	Mean	P10	P25	Median	P75	P90	SD
Affiliate-level								
Profit	131,642	5,773.91	187.00	555.35	1,656.06	4,717.24	12,659.50	13,722.00
C	131,642	-0.00	-0.06	-0.01	0.00	0.02	0.05	0.05
Digital	131,642	1.67	0.00	1.00	2.00	3.00	3.00	1.05
ERP	131,642	0.45	0.00	0.00	0.00	1.00	1.00	0.50
DBMS	131,642	0.60	0.00	0.00	1.00	1.00	1.00	0.49
Communication	131,642	0.63	0.00	0.00	1.00	1.00	1.00	0.48
Capital	131,642	11,831.80	123.62	544.08	2,506.10	8,800.00	24,523.97	32,460.49
Labor	131,642	10,663.01	1,416.79	2,755.31	5,546.68	11,535.74	23,967.70	15,540.58
Productivity	131,642	5.34	2.47	3.72	5.23	6.82	8.36	2.45
RoA	131,588	10.96	1.91	3.99	7.97	14.53	23.62	10.17
Intangible Assets	99,122	2,054.78	4.83	26.15	142.77	742.00	3,539.00	7,450.59
EBIT	131,588	5,308.65	232.61	618.85	1,697.77	4,606.67	11,828.30	11,675.88
Distance SAP	87,264	0.20	0.02	0.09	0.18	0.27	0.38	0.14
log Profit	131,642	7.37	5.24	6.32	7.41	8.46	9.45	1.68
log Capital	131,642	7.63	4.83	6.30	7.83	9.08	10.11	2.08
log Labor	131,642	8.64	7.26	7.92	8.62	9.35	10.08	1.13
log Intangible Assets	99,122	5.00	1.76	3.30	4.97	6.61	8.17	2.40
log EBIT	131,588	7.42	5.45	6.43	7.44	8.44	9.38	1.56
Country-level								
GDPperCapita	131,642	30,302.85	16,144.15	25,655.33	31,301.51	33,926.98	36,975.59	9,664.36
Unemployment	131,642	8.52	4.82	5.72	7.66	9.60	12.15	4.25
Inflation	131,642	1.65	0.04	0.51	1.58	2.49	3.40	1.25
CIT	131,642	29.34	20.00	25.00	30.95	34.43	37.25	6.18
log GDPperCapita	131,642	10.26	9.69	10.15	10.35	10.43	10.52	0.35

Notes: This table presents descriptive statistics of the variables used in empirical tests. All absolute affiliate-level financial values are stated in thousand euro. *log* refers to the natural logarithm of the respective variable. *RoA*, *Unemployment*, *CIT* and *Inflation* are stated in percent. The income shifting incentive measure (C) is stated in decimals. *Distance SAP* is stated in thousand kilometers. All affiliate-level financial variables and country-level variables are winsorized at the 1 and 99 percentile. All variables are defined in Appendix Table B.1.

The lower level of Table 3.2 reports the statistics of macroeconomic control vari-

[9] By construction, the average *C* should equal 0. The slight deviation is due to the fact that we drop all affiliates for which we have no information on their level of digitalization after constructing *C*.

ables. Information on effective corporate income tax rates (*CIT*) are taken from the Taxation and Customs Union Directorate-General (TAXUD) database, the Oxford Center for Business Taxation tax database and the EY's Worldwide Corporate Tax Guides. Country-level control data on *GDPperCapita*, *Unemployment*, and *Inflation* are obtained from the World Bank's World Development Indicators and OECD Stats. We winsorize all affiliate-level financial variables and country-level variables at the 1 and 99 percentile to account for outliers.

Table 3.3: Descriptive Statistics by Digitalization

	Digital = 0	Digital = 1	Digital = 2	Digital = 3	Total
Observations	25,048	26,522	46,397	33,675	131,642
Panel (A): Mean					
Profit	5,414.66	5,292.96	6,018.97	6,082.29	5,773.91
Capital	9,850.83	10,017.58	12,523.29	13,781.40	11,831.80
Labor	9,650.78	9,779.74	10,792.61	11,933.03	10,663.01
Productivity	5.87	5.05	5.25	5.32	5.34
RoA	11.69	11.11	10.90	10.39	10.96
Panel (B): Median					
Profit	1,502.50	1,405.69	1,666.40	1,987.53	1,656.06
Capital	2,126.72	1,778.16	2,507.00	3,744.46	2,506.10
Labor	4,966.35	4,693.99	5,538.05	6,869.05	5,546.68
Productivity	5.83	4.94	5.09	5.20	5.23
RoA	8.49	8.06	7.91	7.62	7.97

Notes: The table presents descriptive statistics of relevant affiliate characteristics by level of digitalization (*Digital*). Panel (A) presents the mean value of each variable while Panel (B) presents the median value. All variables are defined in Appendix Table B.1.

In Table 3.3, we provide summary statistics by the level of digitalization. The median affiliate has access to two software solutions. This group consists of 46,397 affiliate-year observations and represents 35 percent of the total sample. *Digital* is zero for 19 percent of the sample, and in 25 percent of the affiliate-years, the index has the highest value of three. The remaining 21 percent of observations belong to affiliates employing one software solution. While more digitalized affiliates seem to have slightly higher *Profit* and report higher levels of *Capital* and *Labor*, there is no systematic difference in *Productivity* or profitability (*RoA*) between the groups. This provides us with some confidence that there are no systematic differences between the groups that could bias our results. Nevertheless, throughout section 3.4 and 3.6, we will provide additional tests to alleviate potential endogeneity concerns.

3.4 Digitalization and Tax-motivated Income Shifting

3.4.1 Graphical Evidence

We start our analysis by providing descriptive graphical evidence of the relationship between affiliates' reported profitability and the income shifting incentive conditioned on the level of digitalization. Figure 3.2 depicts a binned scatterplot of the average return on assets (*RoA*) and *C*. We use return on assets, defined as pre-tax profits scaled by total assets, rather than absolute pre-tax profits to take size effects into account and increase comparability. For each digitalization index group, the binned scatterplot clusters the affiliate-year observations along the x-axis into twenty bins and reports the average *RoA* for each bin.

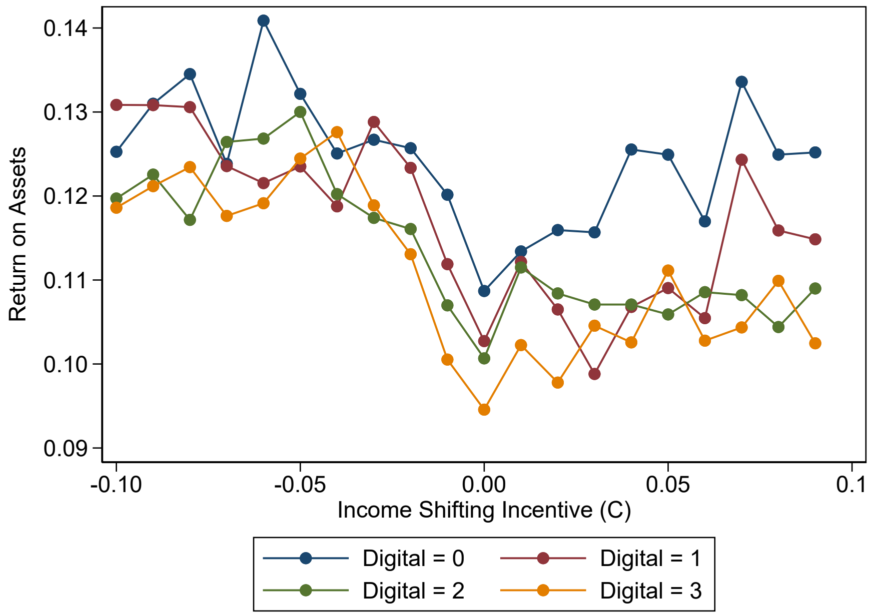
Prior findings suggest that we should observe a clear negative association between *RoA* and *C*, meaning that affiliates increase reported profits in jurisdictions with a negative income shifting incentive and decrease reported profits in jurisdictions with positive *C* values (Huizinga and Laeven, 2008; Markle, 2016; De Simone et al., 2017).

Figure 3.2 shows that this negative association only exists for digitalized affiliates. While all affiliates report higher profits when *C* is negative, only more digitalized affiliates effectively adjust their profitability to changes in *C*. In fact, non-digitalized affiliates report on average the same profitability for positive and negative *C* values. Figure 3.2 further shows that affiliates with the highest level of digitalization are most effective in transitioning to lower profits when *C* increases. This descriptive evidence provides an first indication that digitalized affiliates relocate income more actively than non-digitalized affiliates, suggesting that digitalization might be a key factor for affiliates to observe the most profitable tax planning measures.

3.4.2 Baseline Result

We begin the empirical analysis by replicating the basic Huizinga and Laeven (2008) regression to verify that our sample of multinational firm affiliates is comparable to

Figure 3.2: Relation of Profit and Income Shifting Incentive by Level of Digitalization



Notes: This figure shows a binned scatterplot of the average return on assets for different levels of the income shifting incentive measure (C). Affiliates at each digitalization index level are grouped into 20 equally distanced bins along the range of C. Each bin covers a range of 0.01, starting at the first percentile of the distribution of C, i.e., -0.1, and ending at the 99 percentile, i.e., 0.1. The colored dots depict the average return on assets (in decimals) within each bin. Each color represents a different degree of digitalization.

previous studies. We estimate a negative and statistically significant coefficient for the income shifting incentive measure in column (1) of Table 3.4. This indicates that affiliates, on average, are sensitive to relocating income to tax-favorable jurisdictions. In other words, affiliates decrease their profits in high-tax countries and report higher profits in low-tax countries. In terms of magnitude, our estimate of -0.683 is slightly below the estimate of -0.85 of Huizinga and Laeven (2008), but in line with estimates using samples of more recent time periods (Dharmapala, 2014; Heckemeyer and Overesch, 2017). Additionally, we show that our model is well-behaved. The estimates of the Cobb-Douglas components - *Capital*, *Labor*, *Productivity* - are in line with the predicted positive direction.

In column (2) of Table 3.4, we estimate the empirical model outlined in Equation 3.1, which provides the baseline result for the association between digitalization and tax-motivated income shifting. We estimate an interaction coefficient between C and

Table 3.4: Affiliates' Digitalization and Tax-Motivated Income Shifting

	log Profit						
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
C × Digital		-0.229**	-0.244**	-0.166*	-0.405***	-0.351**	-0.195**
		(0.12)	(0.12)	(0.09)	(0.13)	(0.16)	(0.10)
C	-0.683***	-0.304	0.777***	0.439*	-0.010	0.944***	0.507**
	(0.17)	(0.25)	(0.29)	(0.25)	(0.27)	(0.33)	(0.25)
Digital		-0.010	-0.008	0.003	.	.	-0.139
		(0.01)	(0.01)	(0.01)			(0.16)
log Capital	0.142***	0.142***	0.142***	0.027***	0.158***	0.157***	0.021**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
log Labor	0.748***	0.749***	0.753***	0.548***	0.711***	0.707***	0.559***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Productivity	0.046***	0.046***	0.049***	0.088***	0.040***	0.048***	0.082***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
log GDPperCapita	0.063**	0.063**	.	-0.501***	0.027	.	-0.523***
	(0.03)	(0.03)		(0.18)	(0.04)		(0.18)
Unemployment	-0.002	-0.002	.	-0.015***	-0.011**	.	-0.020***
	(0.00)	(0.00)		(0.00)	(0.00)		(0.00)
Inflation	0.025***	0.025***	.	-0.016***	0.035***	.	-0.012*
	(0.01)	(0.01)		(0.01)	(0.01)		(0.01)
Digital × log. Capital					-0.010**	-0.010**	0.003
					(0.00)	(0.00)	(0.00)
Digital × log. Labor					0.024***	0.028***	-0.007
					(0.01)	(0.01)	(0.01)
Digital × Productivity					0.003	-0.001	0.004**
					(0.00)	(0.00)	(0.00)
Digital × log GDPperCapita					0.017	.	0.013
					(0.02)		(0.02)
Digital × Unemployment					0.004**	.	0.003**
					(0.00)		(0.00)
Digital × Inflation					-0.006	.	-0.002
					(0.01)		(0.00)
Year Fixed Effects	Yes	Yes	.	Yes	.	.	Yes
Industry Fixed Effects	Yes	Yes	Yes	.	.	.	.
Country-Year Fixed Effects	No	No	Yes	No	.	.	No
Affiliate Fixed Effects	No	No	No	Yes	No	No	Yes
Year-Digital Fixed Effects	No	No	No	No	Yes	Yes	No
Industry-Digital Fixed Effects	No	No	No	No	Yes	Yes	No
Country-Year-Digital Fixed Effects	No	No	No	No	No	Yes	No
Observations	131,642	131,642	131,642	128,653	131,639	131,633	128,653
Number of Affiliates	24,025	24,025	24,025	21,036	24,024	24,024	21,036
Adj. R2	0.424	0.424	0.429	0.772	0.427	0.432	0.772

Notes: This table presents regression estimates for the baseline approach outlined in Equation 3.2, testing the effect of digitalization on tax-motivated income shifting. *C* refers to the income shifting incentive measure as defined by Huizinga and Laeven (2008). The dependent variable is the natural logarithm of profits before tax (*log Profit*). In column (1), we measure the tax sensitivity of reported profits. Columns (2) to (7) include a measure for affiliates' level of digitalization and provide estimates of how the level of digitalization affects the tax-sensitivity of reported profits. The digitalization index (*Digital*) is determined as an additive index that captures affiliates' access to ERP software, DBMS, or communication software. It is based on a yearly affiliate-level survey over the period 2006 to 2016. In columns (5) to (7), we interact all control variables with *Digital*. In columns (5) and (6), we include two-way fixed effects with *Digital*. All continuous variables are winsorized at the 1 and 99 percentile. We report heteroskedasticity-robust standard errors clustered by affiliate standard errors in parentheses. ***, **, * denote statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively. All variables are defined in Appendix Table B.1.

Digital of -0.229, which is statistically significant at the 5 percent level. This result suggests that higher levels of digitalization enable affiliates to relocate income for tax purposes more efficiently. The effect of digitalization is also economically meaningful. From Table 3.4 column (2), we can infer that an one standard deviation change in C (0.05) for non-digitalized affiliates (*Digital* = 0) is associated with an 1.52 percent tax-motivated change in reported profits (0.05×0.304). Using the mean *Profit* of non-digitalized affiliates of €5.4 million, this translates to an absolute value change of €82,303. For a fully digital affiliate (*Digital* = 3), an one standard deviation change in C translates to a 4.96 percent or €301,377 tax-motivated change in reported profits ($0.05 \times [0.304 + 0.229 \times 3]$). Put differently, given a C of 0.05, we observe an incremental tax-motivated change in reported profits of 3.44 percentage points if a non-digitalized affiliate fully digitalizes. Considering the average level of digitalization (1.67) and a conceivable change in digitalization of one standard deviation (1.05), this translates to a tax-motivated change in reported profits of 1.20 percentage points (from 3.43 percent to 4.63 percent) or of 34.99 percent ($[4.63 / 3.43] - 1$).

In columns (3) and (4), we vary the fixed effect structure. First, we add country-by-year fixed effects which control for macroeconomic shocks that affect all affiliates within a given country. This means that these fixed effects also control for statutory corporate tax rate changes in the affiliate's country. Hence, recalling Equation 3.1, the variation in C in this specification comes either from changes in the affiliate's operating revenue relative to the other affiliates in the multinational firm or from changes in affiliates' statutory tax rate in other countries belonging to the same multinational firm. Second, we add affiliate fixed effects to control for unobserved affiliate-specific effects that are constant over time. Our inferences remain unchanged.

One concern is that digitalized affiliates are structurally different from non-digital affiliates, meaning that the affiliates also differ in other characteristics than digitalization. To address this concern, we modify the specifications in columns (2), (3), and (4) and fully interact all control variables with digitalization. The interaction of control variables and *Digital* captures differences between the groups in observable covariates.

For example, we control for potential differences in productivity between affiliates depending on the level of digitalization. In addition, we also interact the fixed effects with *Digital* to control for unobservable characteristics that are correlated with digitalization. Using this approach, we control for country-year shocks or time-invariant industry characteristics that are specific to affiliates with certain levels of digitalization.[10] The magnitude and significance of the interaction coefficients between C and *Digital* in columns (5) to (7) increase to -0.405, -0.351, and -0.195, respectively. Thus, our results remain unchanged.

In columns (5) and (6), the interaction coefficients of *Digital* and other affiliate-specific control variables indicate that significant differences in the effect on profitability between the groups exist in *Labor* and *Capital*. However, these coefficient reverse and become insignificant once affiliate fixed effects are included in column (7). Hence, the level of digitalization does not seem to be systematically associated with other structural differences between affiliates. Overall, our results indicate that digitalization is a key enabler of tax-motivated income shifting.[11]

3.4.3 Instrumental Variables Approach

The results of our baseline estimation approach provide evidence on the association between digitalization and tax-motivated income shifting. We find our regression results to be robust to the inclusion of affiliate fixed effects and the full interaction of all variables with our digitalization index. However, we acknowledge that unobserved and time-varying affiliate-specific characteristics could be associated with both investment in digital infrastructure and cross-border income shifting opportunities. We use an instrumental variables regression approach to directly address this endogeneity concern.

As a first instrument, we use the distance between an affiliate and a regional SAP

[10] We cannot apply the fixed effect interaction to the specification in column (4) of Table 3.4 because there is not enough variation in C at the affiliate level to include Affiliate × Digital fixed effects.

[11] In robustness tests, we include the ratio of intangible to total assets as an additional covariate to control for this very specific channel of cross-border tax planning (Dischinger and Riedel, 2011). In addition, we exchange the dependent variable and vary the functional form of *Digital*. See section 3.6.2.

office. It is reasonable to assume that the distance to a regional SAP office is inversely correlated to the implementation of digital technologies (satisfying the inclusion criteria). SAP is the largest European developer and distributor of digital technologies for firms and has at least one regional office in each European country. The close proximity of SAP sales people to regional affiliates creates opportunities, e.g., at informal events or in local sports clubs, to convince decision makers at local affiliates to invest in digitalization. However, the distance between SAP regional offices and affiliates is unlikely to have a direct effect on reported profits (satisfying the exclusion criteria). Importantly, SAP regional offices are not exclusively located in large and economically striving regions.

In line with prior studies on the effect of digitalization on firm performance, the second instrument exploits the panel structure of our data and uses lagged values of the digitalization index as a valid instrument (Cardona et al., 2013; Tambe and Hitt, 2012; Bloom et al., 2012; Han and Mithas, 2013). While the lagged variables of the digitalization index in year $t-1$ are closely related to the digitalization index in year t (satisfying the inclusion criteria), they should not have a direct effect on the reported profitability in year t (satisfying the exclusion criteria).

We report the results of the instrumental variables regression in Table 3.5. In column (1), the first stage regression reveals that there is a strong negative association between the distance to a regional SAP office and the level of digitalization. Further, we find a positive relation between the degree of digitalization in the preceding year and *Digital*. In line with Shevlin et al. (2019), we conduct a number of tests to confirm that our choice of instruments is valid and that our model is fully identified. The test for whether the model is under-identified (Kleibergen-Paap LM test), which is rejected at the one percent level, implies that our instruments are strongly correlated with the digitalization index. The reported F-statistic of 11,189.68 which stand in contrast to the 10 percent Stock-Yogo (2005) critical value of 16.87, rejects the null hypothesis that our instrumental variables are only weakly correlated with the digitalization index (Kleibergen-Paap Wald F test).

Table 3.5: Instrumental Variable Regression

	First Stage	Second Stage	First Stage	Second Stage
Dependent Variable	Digital	log Profit	Digital	log Profit
Variable	(1)	(2)	(3)	(4)
C × Digital		-0.358*		-0.645**
		(0.201)		(0.285)
Digital		-0.016		-0.006
		(0.011)		(0.015)
Distance SAP	-0.077***		-0.079***	
	(0.017)		(0.026)	
Digital($t-1$)	0.777***		0.792***	
	(0.003)		(0.004)	
C × Distance SAP	0.218***		0.160**	
	(0.055)		(0.076)	
C × Digital($t-1$)	-0.761***		-0.451**	
	(0.141)		(0.196)	
C	0.007***	0.151***	0.010***	0.122***
	(0.001)	(0.007)	(0.002)	(0.010)
log Labor	0.017***	0.728***	0.004	0.757***
	(0.002)	(0.013)	(0.003)	(0.020)
Productivity	0.004***	0.004	0.001	-0.013*
	(0.001)	(0.004)	(0.002)	(0.007)
log GDPperCapita	-0.029***	0.098***	-0.310***	0.754***
	(0.007)	(0.034)	(0.052)	(0.256)
Unemployment	-0.003***	-0.015***	-0.016***	0.005
	(0.001)	(0.004)	(0.002)	(0.008)
Inflation	0.016***	0.015	-0.034***	0.119***
	(0.003)	(0.010)	(0.007)	(0.022)
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	69,237	69,237	37,159	37,159
Number of Affiliates	13,789	13,789	7,714	7,714
Adj.-R2	0.645	0.410	0.669	0.381
Kleibergen-Paap LM Statistic	3,630.07		3,031.13	
(p-value)	0.000		0.000	
Kleibergen-Paap F Statistic	11,189.68		12,434.94	

Notes: This table presents results of an instrumental variable regression, examining the effect of digitalization on tax-motivated income shifting. In the second stage regressions, *Digital* is instrumented by *Distance SAP* and *Digital(t - 1)*. *Distance SAP* referts to the distance between the affiliate and the nearest local SAP local retailer in 1000 kilometers. *Digital(t - 1)* is the digitalization index in year *t - 1*. *C* refers to the income shifting incentive measure as defined by Huizinga and Laeven (2008). The dependent variable in the second stage is the natural logarithm of profits before tax (*log Profit*). In columns (1) and (2), we use all affiliates in our sample. Columns (3) and (4) include only affiliates in countries with the highest number of SAP regional offices. These countries are Germany, U.K., France, Italy, and Ireland. All continuous variables are winsorized at the 1 and 99 percentile. We report heteroskedasticity-robust standard errors clustered by affiliate standard errors in parentheses. ***, **, * denote statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively. All variables are defined in Appendix Table B.1.

We find a negative and statistically significant interaction coefficient of our instrumented digitalization index and the income shifting incentive measure (*C*) in our second stage regression in column (2). The magnitude of the estimated interaction coefficient (-0.358) is in line with our baseline regression results reported in Table 3.4. The instrumental variables regression approach indicates that the association between digitalization and tax-motivated income shifting is robust to concerns that our findings are partly driven by omitted variables and endogeneity.

To further confirm this finding, we rerun the analysis keeping only affiliates in countries with the highest number of SAP regional offices. In these countries, the distance to a regional SAP office is, on average, smaller. Thus, the exposure to SAP sales persons should be relatively higher and the instrument even better suited to measure the effect of digitalization. The countries included are Germany, U.K., France, Italy, and Ireland. While the associations in the first stage regression in column (3) of Table 3.5 remains similar to column (1), we find a stronger association between digitalization and tax-motivated income shifting in the second stage shown in column (4).

Overall, the combined results of our baseline estimation approach and our additional instrumental variables analysis complement and validate our prior understanding on the relationship between digitalization and tax-motivated income shifting.

3.5 Effect of Individual Software Solutions

3.5.1 Disentangling the Digitalization Index

Due to data constraints, prior studies relied on aggregated proxies to measure the degree of firms' internal information quality and digitalization (Gallemore and Labro, 2015; McGuire et al., 2018). One of the advantages of our granular data is that we observe digitalization at the affliliate-product level. This allows us to further investigate which software component drives the association between digitalization and tax-motivated income shifting. Prior research shows that there can be differences in the effect of information technologies and communication technologies with respect to control and decision making (Bloom et al., 2014).

We re-estimate our baseline estimation from Equation 3.2 but replace the explanatory variable *Digital* by three separate software variables. These dummy variables indicate the availability of either *ERP* software, *DBMS* systems or *Communication* software at the affiliate level. We rerun the specification varying the interaction of the dummy variable with *C* while controlling for the availability of the alternative software solutions. Furthermore, we interact the software variable of interest with the control variables and fixed effects. Otherwise, the estimation remains equivalent to our baseline approach.

The results in Table 3.6 indicate that each software solution is associated with increased tax-motivated income shifting. When using *Communication* as dummy variable, we estimate a statistically significant interaction coefficient of -0.737 in column (1). This estimate is robust for including affiliate fixed effects in column (2). The importance of communication software for tax-motivated income shifting is plausible for two reasons. First, the growing relevance of intangible transactions has made transfer pricing arrangements increasingly complex in multinational firms and requires intensified intragroup coordination (Greil et al., 2019). Second, multinational firms become significantly larger and internationally dispersed requiring an increased collaboration and coordination among international teams (Tan et al., 2020). Our finding is in line with prior research highliting the relevance of communication software for corporate decisions in multinational teams (Artail, 2006; Andriole, 2010; de Vreede et al., 2016). In this vein, our results indicate that among the information channels discussed by Gallemore and Labro (2015), coordination of tax planning across different parts of a firm seems to be a key enabler of tax planning strategies.

In addition, we document a weak positive association of ERP software and the tax sensitivity of reported profits in column (3), however, this result attenuates when including affiliate fixed effects. In columns (5) and (6), we further show that DBMS is positively associated, albeit not significant in statistical terms. Our results suggest that communication software which supports the coordination, collaboration, and exchange across different parts of a multinational firm are a key enabler of tax-motivated income shifting.

Table 3.6: Effect of each Software Component on Tax-Motivated Income Shifting

	log Profit					
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Software:	Communication		ERP		DBMS	
C × Software	-0.737***	-0.445**	-0.451*	-0.031	-0.386	-0.152
	(0.28)	(0.21)	(0.26)	(0.19)	(0.27)	(0.19)
C	-0.202	0.456*	-0.473**	0.169	-0.423*	0.254
	(0.22)	(0.24)	(0.21)	(0.21)	(0.24)	(0.23)
Communication	.	0.017	-0.034*	0.017	-0.088***	0.017
		(0.01)	(0.02)	(0.01)	(0.02)	(0.01)
ERP	0.050*	-0.001	.	-0.001	0.029	-0.001
	(0.03)	(0.01)		(0.01)	(0.03)	(0.01)
DBMS	-0.017	-0.005	0.048**	-0.005	.	-0.005
	(0.03)	(0.01)	(0.02)	(0.01)		(0.01)
log Capital	0.161***	0.027***	0.145***	0.027***	0.146***	0.027***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
log Labor	0.712***	0.548***	0.738***	0.548***	0.733***	0.548***
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
Productivity	0.038***	0.088***	0.047***	0.088***	0.044***	0.088***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
log GDPperCapita	0.043	-0.496***	0.050	-0.487***	0.058	-0.492***
	(0.03)	(0.18)	(0.04)	(0.18)	(0.04)	(0.18)
Unemployment	-0.011***	-0.015***	-0.000	-0.015***	-0.007**	-0.015***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Inflation	0.014	-0.016***	0.043***	-0.016***	0.038***	-0.016***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Interaction: Software × Controls	Yes	No	Yes	No	Yes	No
Year Fixed Effects	.	Yes	.	Yes	.	Yes
Industry Fixed Effects	.	.	.	.	.	.
Affiliate Fixed Effects	No	Yes	No	Yes	No	Yes
Year-Software Fixed Effects	Yes	No	Yes	No	Yes	No
Industry-Software Fixed Effects	Yes	No	Yes	No	Yes	No
Observations	131,642	128,653	131,642	128,653	131,642	128,653
Number of Affiliates	24,025	21,036	24,025	21,036	24,025	21,036
Adj. R2	0.426	0.772	0.426	0.772	0.426	0.772

Notes: This table presents regression estimates for the baseline approach outlined in Equation 3.2 but includes each software category of *Digital* individually, testing the effect of each software on tax-motivated income shifting. *C* refers to the income shifting incentive measure as defined by Huizinga and Laeven (2008). The dependent variable is the natural logarithm of profits before tax (*log Profit*). *Software* refers to ERP software in column (1) and (2), to DBMS in columns (3) and (4), or to communication software in columns (5) and (6). In odd columns, we interact all control variables with the respective *Software* of interest and include two-way fixed effects with *Software*. All continuous variables are winsorized at the 1 and 99 percentile. We report heteroskedasticity-robust standard errors clustered by affiliate standard errors in parentheses. ***, **, * denote statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively. All variables are defined in Appendix Table B.1.

3.5.2 Release of SAP ERP Software

We employ an additional identification strategy to further examine the role of information technology in tax-motivated income shifting. In particular, we exploit the first release of a comprehensive business software solution bundle by the European market leader SAP in 2009.[12] One product of this bundle is a new ERP software. We conduct a triple difference-in-differences analysis to measure whether affiliates that implement an ERP software solution for the first time after the market release make more efficient income shifting decisions than affiliates which do not have access to an ERP system.[13] We estimate the following specification for the period 2006 to 2012:

$$\begin{aligned} log\ Profit_{it} = \beta_1\, log\ Capital_{it} + \beta_2\, log\ Labor_{it} + \beta_3\, Productivity_{it} + \beta_4\, C_{it} + \\ \beta_5\, SAP\ ERP_{it} + \beta_6\, C_{it} \times SAP\ ERP_{it} + \beta_7\, C_{it} \times Post_t + \\ \beta_8\, SAP\ ERP_{it} \times Post_t + \beta_9\, C_{it} \times SAP\ ERP_{it} \times Post_t + \\ \alpha_j\, X_{it} + \gamma_t + \delta_{ind} + \epsilon_{it}. \end{aligned} \tag{3.3}$$

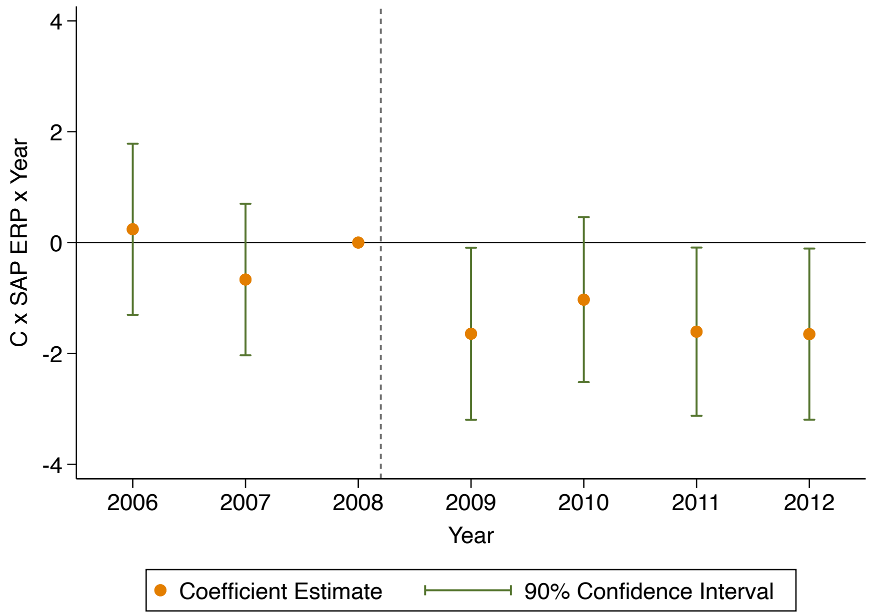
SAP ERP is a dummy variable that takes the value of one for affiliates that implement an ERP system for the first time in 2009 or 2010 and zero for affiliates that never introduce an ERP software. *Post* is a dummy variable that takes the value of one for years 2009 to 2012. In addition to the control variables used in the baseline estimation from Equation 3.1, we control for the availability of the other software solutions, DBMS and communication software. We use 2009 and 2010 as event years because the roll-out of a new software is a staggered process between SAP and its customer. The sample consists of 38,135 affiliate-years, with 7,062 unique affiliates. Of these 7,062 affiliates, 2,589 affiliates introduce an ERP system. In line with our hypothesis, we expect that the coefficient of the triple interaction, β_9, is negative, i.e., affiliates that implement a new ERP software engage more actively in tax-motivated income shifting after the introduction.

Figure 3.3 graphically depicts an event study reporting the estimated coefficients of the triple interaction, in which the *Post* variable is substituted by a year dummy. The base year is 2008. The graph highlights that prior to the software release, there is no

[12] For more information on the business software solution, see https://news.sap.com/uk/2009/05/sap-business-suite-7-now-available-to-customers-worldwide/ (accessed: 02/25/2022).

[13] We acknowledge that we do not specifically observe whether the implemented ERP system is an SAP ERP software. However, since we only consider European affiliates and SAP is the European market leader that just offered a new product, we assume that all affiliates that implement a new ERP solution after 2009 implement the new SAP version. See https://news.sap.com/2012/05/sap-named-worldwide-market-share-leader-forenterprise-resource-planning/ (accessed: 02/25/2022).

Figure 3.3: Tax Sensitivity after Introduction of new ERP Software



Notes: This figure presents results of a difference-in-differences event study, testing the effect of affiliates' introduction of a new ERP software on the tax sensitivity of reported profits. The specification is based on Equation 3.3 subsituting the *Post* variable with yearly dummies. 2008 is the base year. The vertical line marks years after the SAP software release. Heteroskedasticity-robust standard errors are clustered at the affiliate level.

significant difference in tax-motivated income shifting behavior between the groups, providing us with confidence that the parallel trends assumption holds. Figure 3.3 provides evidence that affiliates which implement an ERP software engage significantly more in income shifting for tax purposes relative to the control group in the post period.

This graphical evidence is corroborated by our formal regression results in Table 3.7. Column (1) shows that tax-motivated income shifting is significantly stronger for treated affiliates after the release of the new SAP ERP software relative to the control group. The result remains robust for including country-year fixed effects and affiliate fixed effects in columns (2) and (3). These results shed light on the effect of the introduction of a new digital technology on tax-motivated income shifting decisions of multinational firms and support our main finding that affiliates with higher levels of digitalization engage more actively in tax-motivated income shifting than affiliates with lower levels of digitalization. Furthermore, given the previous finding on communication software in Section 3.5.1, this result suggests that data and information provided by ERP software serve as an important prerequisite for the implementation of efficient income shifting strategies.

Table 3.7: Implementation of new ERP Software and Tax-Motivated Income Shifting

	log Profit		
Variable	(1)	(2)	(3)
C × SAP ERP × Post	-1.335**	-1.480**	-1.051*
	(0.61)	(0.61)	(0.54)
C × SAP ERP	-0.281	-0.213	-0.098
	(0.65)	(0.65)	(0.70)
C × Post	0.170	0.829*	-0.175
	(0.40)	(0.49)	(0.35)
SAP ERP × Post	-0.039	-0.033	0.002
	(0.02)	(0.03)	(0.02)
C	-0.595	-0.340	0.286
	(0.43)	(0.50)	(0.45)
SAP ERP	-0.086***	-0.088***	.
	(0.03)	(0.03)	
log Capital	0.135***	0.137***	0.028**
	(0.01)	(0.01)	(0.01)
log Labor	0.750***	0.752***	0.532***
	(0.02)	(0.02)	(0.04)
Productivity	0.050***	0.050***	0.085***
	(0.01)	(0.01)	(0.00)
DBMS	0.044*	0.040*	-0.025
	(0.02)	(0.02)	(0.02)
Communication	-0.033	-0.031	0.013
	(0.02)	(0.03)	(0.02)
log GDPperCapita	0.040	.	0.145
	(0.05)		(0.35)
Unemployment	-0.005	.	-0.016***
	(0.00)		(0.00)
Inflation	0.049***	.	-0.005
	(0.01)		(0.01)
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	No	.
Country-Year Fixed Effects	No	Yes	No
Affiliate Fixed Effects	No	No	Yes
Observations	38,135	38,135	38,022
Number of Affiliates	7,062	7,062	6,949
Adj. R2	0.429	0.432	0.785

Notes: The table presents the results of triple difference-in-differences specification as oulined in Equation 3.3, estimating the effect of the implementation of a new ERP software on the tax sensitivity of reported profits. In 2009, the European market leader SAP introduced a new ERP software. *SAP ERP* is dummy variable taking the value of one if an affiliate has implemented a new ERP software in 2009 or 2010 and the value of zero if an affiliate never introduces an ERP software. *Post* is one for years after 2008. The sample period is 2006 to 2012. *C* refers to the income shifting incentive measure as defined by Huizinga and Laeven (2008). The dependent variable is the natural logarithm of profits before tax (*log Profit*). We report heteroskedasticity-robust standard errors clustered by affiliate in parentheses. ***, **, * denote statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively. All variables are defined in Appendix Table B.1.

3.6 Additional Analyses

3.6.1 Cross-sectional Analyses

In this section, we exploit differences in the characteristics of affiliates and affiliates' multinational firms to shed further light on the association between digitalization and tax-motivated income shifting. First, we look at the complexity of the corporate structure. The advantages of digitalization should be particularly effective in complex multinational firm structures because digitalization allows to reduce the information asymmetries between different departments or affiliates, provides real-time and structured data across national borders, and helps to increase the collaboration and communication within the business group. We proxy for the complexity of a multinational firm by its international dispersion (*Country Dispersion*), which we measure as the ratio of the number of countries in which the affiliate's multinational firm operates relative to the multinational firm's total number of affiliates. Table 3.8 Panel (A) depicts the results. We provide evidence that the association between income shifting incentive and digitalization is significantly stronger for internationally dispersed firms. In other words, the higher the international dispersion and the higher the degree of digitalization, the more negative is the association between reported income and C. The result remains stable if we change the fixed effects structures and control variables.

Second, we analyze whether the association of digitalization and tax-motivated income shifting varies across industries. The impact of digitalization may be very different given the different business models and structures across industries. An ERP system, for example, might be especially effective for setting optimal transfer prices in industries with complex value chains. In Panel (B) of Table 3.8, we show that the association between digitalization and the sensitivity of reported profits for affiliates is strongest in the manufacturing industry, which is in line with the increased complexity of business models in this industry relative to the service industry or retail industry. However, the differences in coefficients between the industries are not significant in statistical terms.

Table 3.8: Cross-Sectional Analyses based on Affiliates' Characterisitics

Panel (A): Geographic Dispersion	log Profit			
Variable	(1)	(2)	(3)	(4)
C × Digital × Country Dispersion	-0.452*	-0.446*	-0.460*	-0.427*
	(0.24)	(0.23)	(0.24)	(0.26)
C × Digital	-0.024	-0.132	-0.026	-0.191
	(0.15)	(0.15)	(0.16)	(0.17)
C × Country Dispersion	0.427	0.189	0.174	0.237
	(0.50)	(0.50)	(0.50)	(0.54)
Digital × Country Dispersion	0.005	0.006	0.007	.
	(0.01)	(0.01)	(0.01)	
C	-0.455	0.838**	0.772**	-0.106
	(0.32)	(0.35)	(0.36)	(0.36)
Digital	-0.008	-0.007	-0.008	.
	(0.01)	(0.01)	(0.01)	
Country Dispersion	-0.239***	-0.247***	-0.249***	.
	(0.02)	(0.02)	(0.02)	
Controls	Yes	Yes	Yes	Yes
Interaction: Digital × Country Dispersion × Controls	No	No	No	Yes
Year Fixed Effects	Yes	Yes	.	.
Industry Fixed Effects	Yes	Yes	Yes	.
Country Fixed Effects	No	Yes	.	No
Country-Year Fixed Effects	No	No	Yes	No
Year-Digital-Country Dispersion Fixed Effects	No	No	No	Yes
Industry-Digital-Country Dispersion Fixed Effects	No	No	No	Yes
Observations	131,642	131,642	131,642	131,635
Number of Affiliates	24,025	24,025	24,025	24,023
Adj. R2	0.429	0.433	0.434	0.436

Panel (B): Industry	log Profit			
Variable	Manufacturing	Retail	Services	Other
C × Digital	-0.446***	-0.281	-0.113	0.089
	(0.17)	(0.24)	(0.32)	(0.38)
C	0.947**	0.784	1.164*	-0.154
	(0.40)	(0.59)	(0.67)	(0.87)
Digital	-0.017*	0.001	0.018	0.001
	(0.01)	(0.01)	(0.02)	(0.02)
Difference to (1) in C × Digital		0.165	0.332	0.535
		(0.29)	(0.36)	(0.42)
Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	Yes
Observations	61,774	27,681	20,537	21,650
Number of Affiliates	10,235	5,085	4,288	4,417
Adj. R2	0.431	0.414	0.366	0.367

Notes: This table presents regression estimates of cross-sectional analyses based on the baseline approach outlined in Equation 3.2. *C* refers to the income shifting incentive measure as defined by Huizinga and Laeven (2008). The dependent variable is the natural logarithm of profits before tax (*log Profit*). The digitalization index (*Digital*) is determined as an additive index that captures affiliates' access to ERP software, DBMS, or communication software. In Panel (A), *Country Dispersion* is included, defined as the ratio of the number of countries in which the affiliate's multinational firm operates relative to the multinational firm's total number of affiliates, to measure how geographic complexity affects the assocation between digitalization and tax-motivated income shifting. In Panel (B), the sample is partitioned based on affiliates' one digit NACE Industry code. Controls are the same control variables as in Table 3.4. In column (4) of Panel (A), all control variables are interacted with *Digital* and *Country Dispersion* and three-way fixed effects are included. All continuous variables are winsorized at the 1 and 99 percentile. We report heteroskedasticity-robust standard errors clustered by affiliate in parentheses. ***, **, * denote statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively.

3.6.2 Robustness Tests

In robustness tests, we include additional control variables, change our measures of the income shifting incentive and the dependent variable, use a non-interpolated digitalization index, and relax the functional form assumption of our index.

First, in Appendix Table B.2, we include the logarithm of intangible assets as an additional control variable in our regression. Several studies show that intangible assets, patents or research and development activities provide an opportunity to relocate income (e.g., Dischinger and Riedel, 2011). Intangible assets are, in general, difficult to value for tax purposes and their relocation or extensive license payments provide a channel to shift profits. Columns (1) and (2) show that we still find a significant negative coefficient for the interaction between *C* and digitalization when holding the level of intangibles constant. This confirms our evidence that digitalized affiliates – independent of their use of intangible assets – relocate more income for tax purposes than non-digitalized affiliates.

Second, we replace the income shifting incentive measure. *C* as a weighted tax rate differential is difficult to interpret because it can be affected by many different factors, e.g., tax rate changes or changes in affiliates' turnover (De Simone et al., 2017). Instead, we use the corporate income tax rate as an easy to interpret measure. In general, higher corporate income taxes should be associated with lower reported profits if the income shifting hypothesis holds. Our estimates in columns (3) and (4) of Appendix Table B.2 indicate that this hypothesis holds. The interaction coefficient of *CIT* and *Digital* is negative and significant. Given a fully digitalized affiliate, an increase of the corporate tax rate of 10 percentage points is associated with a reduction in the affiliate's reported profit by 15.22 percent (-0.0571 + 3 $\times$ -0.0317).

Third, we replace the dependent variable, *log Profit*, with the natural logarithm of earnings before interest and taxes, *log EBIT*. This measure neglects debt shifting as income shifting channel because interest receipts or payments are not included in the earnings calculation. Hence, the results in columns (5) and (6) of Appendix Table B.2 focus mainly on transfer pricing or royalty payments as income shifting channels. As in our main analysis, we find a negative association between the interaction term and reported income. However, our income shifting estimate is slightly smaller, implying that affiliates use all income-shifting channels available to them.

Fourth, we replicate our main table with a non-interpolated digitalization index to control for any potential bias by our interpolation. The results are depicted in Appendix Table B.3 columns (1) and (2). Even though we lose some observations, lowering the statistical power, the inferences remain the same.

Finally, we replace the digitalization measure with a dummy variable that takes the value of one if the digitalization level is larger than zero. This variation of our digitalization index measure allows us to provide a clear-cut comparison between non-digitalized affiliates and affiliates that invested in digitalization. Columns (3) and (4) of Appendix Table B.3 show that digitalized affiliates shift significantly more income. Further, we disentangle the different levels of our digitalization index more formally. In columns (5) and (6), we include *Digital* as categorical instead of a continuous variable and interact each index level separately with the income shifting incentive measure. The inclusion of a categorical variable relaxes the functional form assumption and allows us to estimate the tax sensitivity of reported profits for each index level separately. We find that the estimated tax sensitivity of reported profits increases for digitalized affiliates relative to affiliates with no digitalization.

3.7 Conclusion

This study provides evidence that digitalization is an important firm-specific mechanism of tax-motivated income shifting. Digitalization refers to the availability, accessibility, and usage of software solutions that improve business processes, operations, and communication within the multinational firm. We create a novel micro-level digitalization index that captures affiliates' access to information technology and communication technology.

We show that affiliates' level of overall digitalization is positively associated with the tax sensitivity of reported profits. Importantly, our inferences remain unaffected if we address potential endogeneity concerns by using an instrumental variables approach. Moreover, we investigate the individual components of digitalization and find that communication and collaboration within a multinational firm are key enabler of efficient tax planning. By exploiting changes in the supply of ERP software, we further show that ERP software serves as an important prerequisite.

Our study adds to the understanding of the effect of digitalization on performance of support functions such as the tax department. Existing studies provide first evidence on the association between better internal information quality and corporate tax avoidance. We extend their findings by using affiliate-level digitalization data that goes beyond consolidated proxies of firms' internal information quality. This allows us to uncover the underlying firm-specific channels of internal information quality. Overall, our results provide new evidence on the association between digitalization and tax-motivated income shifting, which can also be helpful to the discussion of the overall effect of digitalization on firm performance.

Chapter 4

Taxing the Digital Economy: Investor Reaction to the European Commission's Digital Tax Proposals

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Abstract: This study analyzes investor reaction to the European Commission's proposals on the taxation of digital firms. Examining the stock returns of potentially affected firms surrounding the proposals' release, we find a significant abnormal capital market reaction of -0.692 percent. This corresponds to an absolute market value reduction of more than 52 billion euros, 40 percent of which is attributable to US firms. Investor reaction is stronger for firms that engage more in tax avoidance and for those with higher European Union exposure. Overall, investors perceive the event as a threat to digital firms' future profitability and react in line with the proposals' intentions to secure tax revenues and to extract location-specific rent.

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4.1 Introduction

To curb tax avoidance of digital firms and to increase tax revenues within the European Union (EU), on March 21, 2018, the European Commission published a "digital tax package" containing two proposals for tax measures directly targeted at a single industry: the digital economy (European Commission, 2018b). The first proposal suggests the immediate introduction of an interim digital services tax (DST) of 3 percent on gross revenues from certain digital services of large digital firms, deviating from the current system of taxing corporate profits. The second proposal lays down the rules for taxing corporate profits that are attributable to a significant digital presence in the long run.

In this study, we analyze whether investors perceive the introduction of digital tax measures as a threat to future profitability. We also analyze heterogeneous effects depending on the specific characteristics of digital firms. In doing so, we provide evidence regarding whether investors understand and react to legislative drafts' underlying intentions.

Because firm-specific costs and benefits will ultimately be reflected in a change in firm value, we focus on the proposals' effect on firm value. The observable change in firm value is a combination of investors' expectations of the effects of the proposed measures on a firm's future profitability and the ex-ante probability of enactment (Wagner et al., 2018a). At the time of the proposals' release, it was seen as very likely that a new measure, such as the DST, would become effective.[1] Pierre Moscovici, commissioner of taxation, stated on March 21, 2018, "Digital taxation is no longer a question of 'if' — this ship has sailed" (European Commission, 2018c).

We employ a short-term event study design to measure digital firms' investor reaction and find a significant cumulative average abnormal return of −0.692 percent in response to the release of the proposals. This finding suggests that investors, on

[1]The finance ministers of the EU member states have expressed a large interest in a temporary digital tax measure and the EU Commission explicitly points out that "this proposal answers these calls for action, and addresses in an interim way the problem that the current corporate tax rules are inadequate for the digital economy" (European Commission, 2018b). Hence, from the political context in March 2018, investors and corporate managers could expect with some certainty that a DST will be introduced.

average, perceive the increased likelihood of the introduction of digital tax measures as negative news for digital firms' future profitability. Moreover, it suggests that investors perceive the demand for digital services to be not perfectly inelastic, so the capital market expects that digital firms will be unable to pass through all of the additional tax expenses.

To evaluate whether investors react in line with the proposals' intentions, we analyze the variation of abnormal returns across firm characteristics. The proposed tax measures are designed to reach two specific goals: first, to safeguard national tax revenues from large digital firms in the EU that are perceived to avoid taxation (European Commission, 2018e; Fuest et al., 2018), and second, to extract part of the location-specific rent of digital firms, which is expected to emerge through high user involvement in market countries, that is, countries with many consumers (European Commission, 2018e; Cui, 2019; Cui and Hashimzade, 2019). These objectives are particularly reflected in the conception of the DST proposal, including arbitrarily chosen size thresholds and the taxation of revenues in market countries.

In line with the first objective, we find that the negative abnormal return is significantly stronger for firms that engage more in tax avoidance and for firms that have higher profit-shifting potential. This is attributable to the fact that the mechanisms to avoid corporate taxation or to relocate net income are not applicable to the proposed gross revenues DST.[2] Our finding suggests that firms receive a market premium for tax avoidance and that the premium diminishes when the European Commission releases the "digital tax package." In line with the second objective, we find that the stock market reaction is more severe for firms with a higher proportion of revenues generated in the EU. Overall, the investor reaction reflects the intention of the European Commission's proposals to secure tax revenues and to extract location-specific rent, suggesting that the capital market expects that the proposals' objectives are achievable.

Furthermore, we examine the magnitude of the observed market reaction. We

[2]Note that the DST is deductible from the corporate income tax base. Hence, if firms are unable to avoid the DST on gross revenues, the effective tax burden of firms that currently avoid more corporate income taxes will increase relatively more.

estimate the total abnormal market value change to be at least -52 billion euros over the two-day event window. Approximately 40 percent of the economically meaningful reduction is attributable to firms located in the United States, supporting the argument that a DST will mainly affect large US firms. It remains questionable whether additional tax revenues, which are estimated to be approximately 3.9–5 billion euros per annum, can outweigh the effect on shareholders' wealth (European Commission, 2018a; Fuest et al., 2018). Based on a theoretical present-value evaluation, we estimate that it will take at least six years for the additional tax revenues to compensate for the initial drop in shareholder wealth. The magnitude of the abnormal market value reduction indicates that investors do not expect that the DST will quickly be repealed.

Our analysis adds to the recent call in the literature for empirical research on the proposed measures of taxing the digital economy and the adaptation of the international tax framework to the digital era (Devereux and Vella, 2018; Olbert and Spengel, 2019). While prior studies mostly focus on a technical evaluation of the DST and a virtual permanent establishment concept (Becker and Englisch, 2018; Nieminen, 2018; Cui, 2019; Russo, 2019; Vella, 2019), the literature is largely silent about the economic effects of such measures on firms. However, such an evaluation is especially critical against the background of ongoing tax discussions at the level of the Organisation for Economic Co-operation and Development (OECD) and unilateral actions of several jurisdictions to introduce a DST (Pellefigue, 2019; Vella, 2019). Our results indicate that policymakers should be aware that investors perceive digital taxes as a threat to firms' profitability. The economic effects of reduced profitability and growth disincentives of digital companies may outweigh potential tax revenue benefits.

Furthermore, this paper complements the literature that examines the effect of anti-tax avoidance policies to safeguard tax revenues. Prior research shows that the introduction of anti-tax avoidance policies, such as thin capitalization rules or controlled foreign company rules, have positive tax revenue effects for governments and lead to real effects at the level of the firm in the form of altered capital structures and investment behavior (Blouin et al., 2014; Egger and Wamser, 2015; Clifford, 2019; De Mooij and Liu, 2021). Our results indicate that firms receive a market price premium for higher tax avoidance activities, which the proposed digital tax measures effectively diminish.

Moreover, we contribute to the mixed evidence on the elasticity of demand in the digital economy. On the one hand, Einav et al. (2014) and Baugh et al. (2018) found a relatively high elasticity of demand for online sales over platforms such as eBay or Amazon. On the other hand, Cohen et al. (2016) and Bibler et al. (2021) show that demand is relatively inelastic on sharing economy platforms. Our capital market analysis reveals that investors expect to bear some of the incidence of the digital tax package and perceive the elasticity of demand for digital services to be relatively high.

Finally, our study contributes to the literature concerned with the effect of tax reforms on shareholder value. Doidge and Dyck (2015), among others, show that additional corporate taxes imply a negative effect on firm value. Several studies analyze the stock market reaction in response to the recent US tax reform and find heterogeneous stock price reactions across firms and countries (Wagner et al., 2018b; Gaertner et al., 2020; Overesch and Pflitsch, 2021). Hoopes et al. (2016) analyze the events around the US sales tax reform for online retail companies. Their study provides evidence of negative abnormal returns for targeted online retailers. Different studies find inconclusive results on investor reaction to the introduction of mandatory tax disclosure rules in Europe and Australia (Johannesen and Larsen, 2016; Chen, 2017; Hoopes et al., 2018; Dutt et al., 2019). To the best of our knowledge, we are the first to examine the stock market reaction in response to reforms on taxing digital corporations.

4.2 Institutional Background and Hypotheses Development

4.2.1 The Digital Tax Initiatives in the European Union

Despite the innovative character of most digital business models and their positive contribution to economic growth, digital firms are repeatedly subject to intensive public and political debate on their tax avoidance activities.[3] The dependence on a physical presence for the establishment of a taxable nexus, which is a central feature of the existing international tax framework, poses a significant challenge for the taxation of cross-border transactions of digital businesses. In response, the European Commission published a "digital tax package" on March 21, 2018, containing two proposals that are concerned with the taxation of digital activities and services (European Commission, 2018b,d,e). The first proposal aims to introduce a new EU-wide DST on revenues from certain digital services as an interim solution. The second proposal focuses on a long-term solution, presenting rules and provisions for the corporate taxation of a significant digital presence (European Commission, 2018d).

The DST constitutes a gross revenue tax of 3 percent.[4] Those revenues that result from the provision of three types of digital services are taxable: first, the placement of advertising on digital interfaces targeted on users of that interface; second, the provision of digital interfaces to users, which allow users to find each other, to interact, and to exchange goods and services; and third, the transmission of user data generated from users' activities on digital interfaces (European Commission, 2018e). The DST paid is deductible from the corporate income tax base.

The proposal suggests limiting the DST to firms that exceed two size thresholds. First, the consolidated amount of worldwide company turnover must exceed 750 million euros within a financial year. Second, the total amount of taxable revenues within the EU — those revenues that are taxable under the scope of the DST — must exceed 50 million euros in the same financial year (European Commission, 2018e).[5]

The second proposal of the European Commission aims for a comprehensive solution for the long run. It intends to establish a new taxable nexus for firms that maintain a nonphysical but significant digital presence in one or more EU member states. Using

[3]The effective tax rate of big tech companies is regularly discussed in the public media and Margrethe Vestager, European commissioner for competition, has become publicly known for her focus on illegal state aid cases and tax affair investigations (see, e.g., Financial Times, 2018a; The Guardian, 2018; Bloomberg, 2019).

[4]In contrast to net income, the management of the gross revenue figure on the income statement is to a lesser extent at the discretion of firms.

[5]The explanatory memorandum in the proposal limits the scope of the DST to corporations.

a significant digital presence as a taxable nexus extends the existing physical permanent establishment concept by the concept of a "virtual permanent establishment." According to the proposal, a significant digital presence exists in a member state if a firm supplies digital services through a digital interface and meets one or more of the following thresholds of digital activity in a member state in the tax period: first, revenues from supplying digital services to users exceed 7 million euros; second, the number of users of digital services exceeds 100,000; or third, the number of business contracts concluded for the supply of digital services exceeds 3,000 (European Commission, 2018d).

Overall, the finance ministers of EU member states have expressed a large interest in a temporary digital tax measure (European Council, 2017). The EU Commission points out that the DST proposal "answers these calls for action, and addresses in an interim way the problem that the current corporate tax rules are inadequate for the digital economy" (European Commission, 2018e). Consequently, the DST proposal contains detailed provisions on the tax subject, the tax base, and the tax rate. In contrast, the European Commission explicitly states that the proposal concerning the corporate taxation of a significant digital presence is thought of as a long-term solution and subordinate to a multilateral agreement at the level of the OECD. As a result, the proposal's conceptual framework is not as developed as that of the DST proposal.

Despite the European Commission's effort to gain political agreement on the DST proposal as a "quick fix" for the international tax framework, member states could not reach a collective understanding.[6] The two concepts remain formal proposals, and the European Commission indicated that it may revive the proposals if no consensus at the level of the OECD is reached.[7] The European Commission's vice president recommended that member states use the DST proposal as a framework for legislative actions at the national level.[8] Several countries followed this recommendation and started to

[6]For the main results of the ECOFIN meetings on December 04, 2018, and March 12, 2019, see European Council (2018, 2019).

[7]As of 2021, the OECD member states are proceeding with an initiative to reframe the international corporate tax system. The OECD proposes a corporate tax reform that intends to shift taxing rights to the market jurisdiction and to introduce a global minimum tax and deduction disallowance (OECD, 2019).

[8]See debate in the European Parliament on April 15, 2019 (European Parliament, 2019).

introduce a DST unilaterally.[9] As of the beginning of 2021, the European Commission restarted the formal process to introduce a DST.[10] The political and academic debate on digital tax measures is ongoing, and empirical insights into the economic effects of such methods are highly valuable.

4.2.2 Implications of the Digital Tax Package and Hypotheses

It is widely accepted that tax policy changes may have significant effects on stock prices and that it is crucial to be aware of the potential effects (Downs and Tehranian, 1988; Doidge and Dyck, 2015). In general, stock prices are related to the cash flow distributions expected to be generated by the firm and incorporate all available information of the market (McWilliams and Siegel, 1997). Therefore, ceteris paribus and without perfectly inelastic demand, additional corporate taxes intuitively and negatively affect a firm's stock price as they reduce the after-tax cash flow (DeAngelo and Masulis, 1980; Doidge and Dyck, 2015; Wagner et al., 2018a).[11]

With regard to the digital tax package, stock prices might be affected both by the interim DST proposal and by the proposed long-term tax reform for digital companies. From the proposals' different levels of conceptual detail and the political context in March 2018, investors and corporate managers could expect with some certainty that the DST will be introduced while the adoption of the significant digital presence proposal was always doubtful (Cui, 2019). Thus, we assume that investors mainly evaluate and react to the proposed DST. However, throughout the paper, we will reflect on this assumption. Academics and practitioners immediately and heavily criticized both proposals for being populist and shortsighted (e.g., Fuest et al., 2018; Næss-Schmidt et al., 2018; Spengel, 2018). With regard to the detailed proposal of a DST, prior literature points out that a gross revenue tax deviates from the conceptual fundamentals of the existing tax framework of corporate profit taxation and that this addition to

[9]For an overview of the countries, see Tax Foundation (2021).

[10]In January 2021, the European Commission started a public consultation process to introduce a digital tax to address the issue of fair taxation of the digital economy (European Commission, 2021).

[11]While the "asset price" models of shareholder incidence take general equilibrium effects from the taxation of existing and new assets into account, we lean on the "cash flow" model of incidence, which leaves relative price effects of tax reforms aside (Cutler, 1988).

the existing system is likely to create a complex and discriminatory tax system that distorts competition and harms the position of EU member states in terms of international tax competitiveness (CFE Fiscal Committee, 2018; Petruzzi and Koukoulioti, 2018; Sheppard, 2018; van Horzen and van Esdonk, 2018).

In contrast to the corporate income tax, which is a net profit tax, the DST is, in essence, an additional *ad valorem* excise tax. The statutory incidence of the proposed DST lies on the producer side and is not levied per customer transaction but on an aggregate level on the overall revenues from digital services. However, the economic incidence of an excise tax is not clear upfront. Prior literature shows that the demand for digital services on sharing economy platforms is relatively inelastic but that additional sales taxes on e-tailers can lead to a quite elastic change in demand (Cohen et al., 2016; Baugh et al., 2018; Bibler et al., 2021). Research also shows that comparable excise taxes on products with inelastic demand functions, such as gasoline or alcohol, can be fully passed through (Marion and Muehlegger, 2011; Hindriks and Serse, 2019). To the extent that the incidence of the additional tax burden is not on customers or labor, owners will bear the burden of the newly proposed DST.

Due to the inverse relation between corporate profitability and the effective tax burden, the effect of a gross revenue tax on the after-tax cash flow may well exceed the burden of an income tax.[12] This may cause severe consequences for firms with relatively low profit margins in terms of competitiveness, forcing them to either raise prices or go out of business.

Furthermore, the fixed thresholds lead to the undesirable effect that around the limit value, additional gross income reduces the net income of a firm. In the same vein, distortion of competition is conceivable, as one competitor, slightly above a threshold, would have to pay the tax, while another competitor, slightly below the relevant threshold, would be tax-exempt. As a consequence, large digital firms are subject to an additional tax, even though several researchers show the impracticability and distortive effect of such practices (Schön, 2018; Olbert and Spengel, 2019). Simultaneously, the

[12] A 3 percent gross revenue tax translates to a 30 percent income tax for firms with a profit margin of 10 percent and to a 60 percent income tax for firms with a profit margin of 5 percent.

broadly defined digital service revenue categories increase the risk that the scope of firms affected by the proposed digital tax measures is overshooting.[13] In addition, the newly proposed measures introduce considerable tax uncertainty for affected firms, and prior literature has shown that this increasing uncertainty is positively associated with costly cash holdings (Hanlon et al., 2017).

Based on the findings in prior literature and our assessment of the European Commission's proposals, we expect a mean negative investor reaction in response to the European Commission's proposals and extensive media attention on March 21, 2018.

Hypothesis 1 *The abnormal stock price reaction in response to the European Commission's digital tax proposals is negative for affected firms.*

In addition, the digital tax proposals are motivated by the widespread political perception that digital firms pay fewer taxes (OECD, 2015a; European Commission, 2018d). The European Commission promotes the newly proposed measures to compensate tax revenue losses from aggressive profit shifting. The design of both proposed measures intends to safeguard tax revenues and allocate taxing rights to market jurisdictions (European Commission, 2018b). The interim DST is designed as a nonavoidable gross revenue tax, and the virtual permanent establishment proposal is designed as a countermeasure to base erosion in market jurisdictions. Hence, we expect that the proposals will have larger effects on firms that engage more in tax avoidance and firms with more profit-shifting potential.

Hypothesis 2 *The negative stock market reaction is more pronounced for digital firms that avoid taxes more or have more profit-shifting potential than others.*

Moreover, the proposals' objective is to extract part of the location-specific rent of digital businesses (Cui, 2019; Cui and Hashimzade, 2019). The European Commission considers digital firms' business models to rely heavily on users and assumes, in line with Evans and Schmalensee (2010), that they play a vital role in the value-creation

[13] Nondigital corporations such as the *New York Times* or the German publishing company *Springer*, which have a growing online business model, would be subject to the proposed digital taxes.

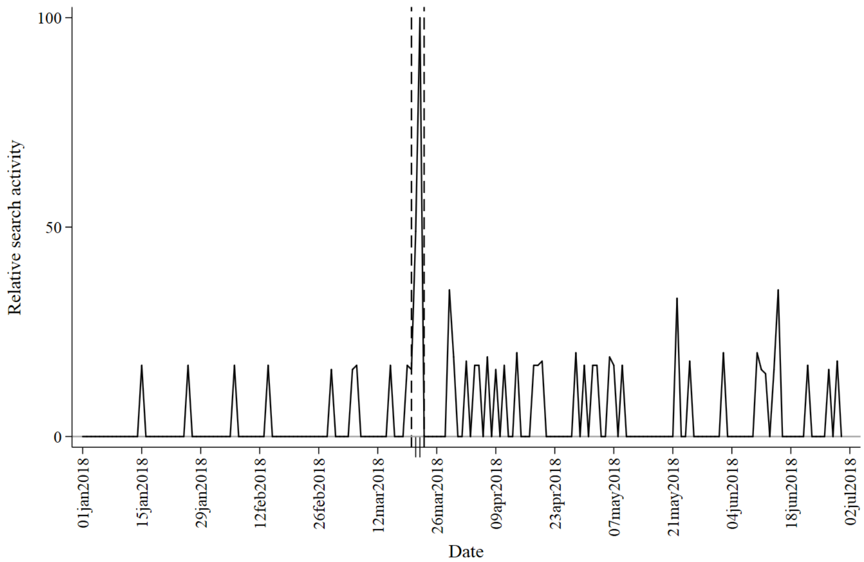
process by creating network effects. Given that these users are located in the EU, a fair share of taxation should be allocated there (European Commission, 2018b). In this regard, the DST is designed to explicitly apply to the location-specific digital revenues generated within the EU single market. As the precise amount of such taxable revenues is hardly observable, investors may consider the overall engagement in the European market as a proxy to evaluate whether a firm is affected. We expect that the stock market reaction is more negative for firms with a greater share of revenue attributable to the European market. Because the tax burden of the DST is proportional to revenues rather than profits, we also expect that the capital market reaction is in absolute terms larger for firms with higher revenues. Furthermore, investors might perceive the proposals as a threat to firm growth and expect that loss-making firms might not have the necessary funds to finance the additional taxes on gross revenues. Hence, we expect investors to differentiate their response depending on a firm's characteristics.

Hypothesis 3 *The negative stock market reaction is more pronounced for digital firms with a greater share of revenue in the European market, larger digital firms, digital firms in a state of loss, or digital firms with higher growth potential.*

4.3 Data and Research Design

We conduct an event study to estimate the effect of the proposed "digital tax package" on the stock returns of affected firms. The event study methodology, which measures the magnitude of the effect an event has on the expected profitability, is based on three assumptions. First, we assume markets are efficient. Hence, we interpret the change between the preevent and after-event price of a stock, adjusted by general market movements, as the market's unbiased estimate of the effect of that event on the value of a firm and the wealth of investors (Fama et al., 1969; Kothari and Warner, 2007). Second, we assume that market participants were not aware of — and did not anticipate — the digital tax package's detailed content before its release by the European Commission and only subsequently started to process and incorporate the relevant information into stock prices. Third, we rely on the assumption that no confounding event systematically affects the stock market reaction of treated and control firms around the event date.

Figure 4.1: Google Trends Analysis



Notes: Google Trends Index for "EU Digital Tax" over the first half of 2018. We plot the Google Trends Index for "EU Digital Tax" over the first six months of 2018 when EU policymakers were actively working on the digital tax proposals. The index (y-axis, relative search activity) varies from 0 to 100, where 100 represents the highest search activity for a specific time period. All other search activities are displayed relative to the highest search activity. The local peaks correspond to periods of relatively high search activity regarding "EU Digital Tax" and comprise our events of interest. The dates enclosed by the light grey scattered lines are our event window. The crossing ticks on the x-axis represent March 21 and 22, 2018, respectively.

To support the adequacy of our assumptions, we undertake several analyses. In line with prior studies, we first conduct a Google Trends analysis to capture the event date that is most likely to be relevant for the stock price effect (Gaertner et al., 2019, 2020). Google Trends provides the frequency of search requests on a specified topic of interest over a time horizon as an index value. Figure 4.1 depicts the Google Trends analysis for the term *EU digital tax*.[14] We can see a considerable spike on March 21, 2018, which corresponds to the date the European Commission released the proposals accompanied

[14] We search for several terms that could relate to the EU digital tax proposals, such as: *digital tax*, *EU commission proposal*, *digital services tax*, *digital permanent establishment*, and *significant digital presence*. All terms lead to similar patterns around the release of the directive proposals. Our main specification relies on the most commonly used term to describe both proposals: *EU digital tax*. Furthermore, for the term *EU digital tax*, most searches stem from the UK, followed by the United States. We find the same spike using the key words in other languages, for example, German (*EU Digitalsteuer*). The top country searches in our event window for the term *digital tax* originate from Ireland, UK, Sweden, and the United States.

by a major press release. The interest in the EU digital tax proposals reached an even higher level on March 22, 2018. Second, we analyze the media attention toward the EU digital tax proposals, which follows a similar pattern. We use the global news database Factiva to systematically search newspapers and media reports for the coverage of the digital tax proposals over time. Appendix Figure C.1 shows the number of articles on that topic per day. Most articles on the EU digital tax were published on March 21, 2018, and the day after. Especially, major US newspapers reported on March 21, 2018 (Wall Street Journal, 2018; New York Times, 2018). Ultimately, we include March 21, 2018, and March 22, 2018, the days with the highest online search activities and media attention, in our event window.

In the next step, we check that no decisive information regarding the detailed content of the digital tax proposal has entered the market before our event window. First, considering the importance of major accounting firms for analyst and shareholder information, we search the websites of the Big Four accounting firms to see when they first report about the tax proposals. While KPMG, Deloitte, and PwC publish their first statements on our event date March 21, 2018, EY does not report until March 22, 2018. Second, we use the Edgar advanced full-text search to systematically search for different keywords regarding digital taxation in all available 10-K reports of the last 10 years.[15] Overall, we find 98 10-K reports speaking about digital taxation. However, none of them mentions digital taxation before March 2018. We provide the results in Appendix Table C.1. In addition, for every US firm in our treatment group, we hand-search the respective 10-K statements regarding digital taxation. We find that none of the treated US firms mentions digital taxation in their annual report before March 2018. We further find that 14 of the 88 US firms in the treatment group actively report digital taxation as a risk factor, often explicitly mentioning the EU Commission's proposals. We outline the statements in Appendix Table C.2. This analysis suggests that our event window in March 2018 measures the indicated effect and that digital taxation is seen as a threat by

[15]We search for the terms *digital services tax, digital service tax, digital services taxes, digital service taxes, digital tax, taxation of the digital economy, taxation of specified digital services*, and *taxation of digital services* in the Edgar database: `https://www.sec.gov/edgar/search/#`.

digital firms. It also suggests that no detailed information has been incorporated into stock prices beforehand. However, if this were the case, this should rather attenuate potential stock market reactions.

Finally, we again use the global news database Factiva to search for topics that could alternatively and systematically affect digital firms' stock price movement in our event window. We search all newspaper articles in the *Wall Street Journal*, *Washington Post*, *New York Times* and *Guardian* on March 21, 2018, and March 22, 2018, and create clusters by counting the number of articles referring to the same topic. We provide the results of our search in Appendix Table C.3. In addition to the release of digital tax proposals, we identify two other clusters with heightened media attention: first, the apology of Mark Zuckerberg, CEO of Facebook, after Facebook collaborated with a third-party company that improperly kept and used Facebook's user data; second, US president Donald Trump's announcement of potential tariffs against Chinese origin goods as well as steel and aluminum imports. However, we are confident that none of the identified clusters confound our results since the Facebook data scandal had already become public on March 17, 2018. International trade conflicts should lead to general market movements rather than to systematic reactions against digital firms. In particular, by using the market model or a portfolio of all nondigital firms for estimating abnormal returns, the results should be robust against the identified clusters. Hence, the release of the proposed directives is, to the best of our knowledge, the only event that could affect all digital firms targeted by the proposals' specifications. Moreover, in contrast to other European Commission directives, the proposed measures were not part of a broader policy package that could confound the analysis.[16]

We select treated firms based on the characteristics outlined in the proposals. We base our sample selection procedure on two studies that estimate the expected additional tax revenue from the proposed DST (European Commission, 2018a; Fuest et al., 2018). We use data from the Bureau van Dijk ORBIS database to identify all publicly listed corporations with consolidated worldwide turnover above 750 million euros in

[16]See, e.g., the introduction of country-by-country reporting for banks, which was part of the major Capital Requirements Directive IV (Dutt et al., 2019).

the last financial year known at the time of the proposal. In line with the study of Fuest et al. (2018), we restrict the sample to firms active in industries that are likely to fall in the scope of the "digital tax package".[17] There are 192 corporations that satisfy the size and industry criteria. Furthermore, accompanying the proposals, the European Commission released an impact assessment. It explicitly refers to 112 top digital corporations that are assumed to be affected by the measures (UNCTAD, 2017; European Commission, 2018a). We add 58 listed firms to our sample that are named in the impact assessment and exceed the size threshold but are not captured by our initial classification.

We obtain one year of daily stock market data from the Thomson Reuters EIKON database ending 10 trading days after our event date. We use the return index (RI), which shows the theoretical value of a shareholding, assuming that dividends are reinvested to purchase additional shares at the closing price applicable on the ex-dividend date as a base for our daily return calculations.[18] In line with Frischmann et al. (2008) and Dutt et al. (2019), we drop 22 firms without sufficient stock market information and trading activity. Finally, we exclude six corporations that had an earnings announcement immediately before, on, or after the event date to eliminate all stock market reactions not directly linked to the proposals. Overall, our final sample consists of 222 corporations, which are listed in Appendix Table C.4.[19] We show descriptive statistics for the sample in Table 4.1. The average daily stock return of treated firms is 0.08 percent, with a standard deviation of 1.65 percent.

For our main analysis, we follow the event study design of Eckbo et al. (2007) and Frischmann et al. (2008). Our event window covers the day of the release of the proposals, March 21, 2018, and the subsequent day (0 through +1). We set our

[17]The relevant NACE Rev. 2 codes are: 6201, 6209, 6311, 6312, 4791, and 5811–5819.

[18]With P_{it} as share price of firm i on day t, $RI_{it} = RI_{it-1} \times P_{it}/P_{it-1}$. Except when t equals the ex-dividend-date, then: $RI_t = RI_{t-1} \times P_t + D_t/P_t - 1$ with D_t being the dividend payment associated with the ex-date. Based on this price information, daily (total) returns (R_{it}) are calculated. Daily returns are winsorized at the 1 and 99 percent levels, which amount to -5.136 and 5.618 percent, respectively. We acknowledge the view that winsorizing return data may distort the "true" market movement. Untabulated tests reveal that our inferences remain unaffected if we use nonwinsorized returns.

[19]The DST has been accused of directly targeting US digital firms (e.g., Hufbauer and Lu, 2018). In our sample, approximately 40 percent of the firms are headquartered in the United States and 24 percent in the EU at the time of the proposal.

Table 4.1: Descriptive Statistics

Variable	N	Mean	SD	P25	Median	P75	Min	Max
Stock return	53,724	0.08	1.65	-0.72	0	0.87	-5.14	5.62
Market return (S&P 1200)	53,724	0.05	0.57	-0.15	0.07	0.33	-4.07	1.61
ETR	42,350	25.63	12.29	18.37	25.62	31.66	0.06	85.71
Intangible to total assets	53,482	31.67	23.97	9.05	29	49.96	0	89.46
EU revenue/total revenue	50,820	46.25	39.05	1.54	46.71	85.15	0	100
Revenues in billion euro	53,724	6.15	14.6	1.32	2.35	5.1	0.66	148.31
Loss-making (last year)	53,724	0.09	0.29	0	0	0	0	1
Revenue growth (last year)	52,514	0.17	1.11	-0.07	0.01	0.12	-0.54	12.26
Asset growth (last year)	52,514	0.1	0.64	-0.06	0.01	0.11	-0.39	8.59

Notes: Treated firms are listed firms with consolidated annual turnover above 750 million euros that are classified to be affected by the digital tax proposals. All values, except for the number of firms N, *Loss-making (last year)* and *Revenues in billion euro*, are stated in percent.

estimation window to contain trading days -11 through -250 relative to the event day. We estimate the following conditional market model:

$$R_{it} = \alpha + \beta\, R_{mt} + \gamma\, D_t + \epsilon_{it}. \tag{4.1}$$

R_{it} is the return of firm i on day t that is likely to fall under the scope of the digital tax proposal (group of treated firms). R_{mt} is the return of the market index m (S&P Global 1200) on day t. D_t is a dummy set equal to one in the two-day event window, and ϵ_{it} is an error term. α provides an estimate for the alpha of an equally weighted portfolio of all 222 treated firms, and β is the estimate of the portfolio's market beta.[20] The coefficient γ provides an estimate for the average abnormal return during the event window. This coefficient has to be multiplied by the number of days in the event window to obtain an estimate for the cumulative average abnormal return (CAAR) (Eckbo et al., 2007; Doidge and Dyck, 2015).[21]

For our cross-sectional analyses (Hypotheses 2 and 3), we include a parameter to account for a firm's level of tax avoidance, profit-shifting potential, or other firm-specific characteristics, which we obtain from the ORBIS database. The conditional

[20] We would obtain similar results, if we use the return of an equally weighted portfolio of all affected firms as the dependent variable ($R_{pt} = \alpha_p + \beta_p\, R_{mt} + \gamma\, D_t + \epsilon_{it}$, where the subscript p stands for the portfolio) (Frischmann et al., 2008). However, our setting allows us to extend our model by including firm-specific characteristics, as depicted in Equation 4.2.

[21] Equivalently, computing a regression for each individual firm ($R_{pt} = \alpha_i + \beta_i\, R_{mt} + \gamma_i\, D_t + \epsilon_{it}$) and taking the coefficients' averages would lead to similar results (Kothari and Warner, 2007). We describe this analysis in Appendix Table C.5.

market model expands as follows:

$$R_{it} = \alpha + \beta R_{mt} + \gamma D_t + \rho I_i + \delta I_i D_t + \epsilon_{it}. \tag{4.2}$$

The variables are defined as before, and I_i is an indicator for firm-specific characteristics. The estimate of the interaction coefficient, δ, becomes the coefficient of interest.

4.4 Results

4.4.1 Main Results

The baseline results of the event study are presented in Table 4.2. In the event period of interest, we find a mean negative CAAR of -0.692 percent, which is significant at the 1 percent level. The analysis provides significant statistical evidence of a mean negative stock price reaction of affected firms to EU digital tax proposals relative to the market (S&P Global 1200).[22] This result confirms our first hypothesis. Assuming efficiency of capital markets, this mean negative change in firm values around the event date represents both the expected costs and profits of the event as well as the ex-ante probability that the event occurs, that is, the net present value that is associated with the proposals (Johannesen and Larsen, 2016; Wagner et al., 2018a). Our result is consistent with investors expecting an increased likelihood of the introduction of digital taxes, which constitute negative news for digital firms' future profitability. Moreover, the result indicates that the capital market expects some part of the tax incidence to lie with shareholders. This implies that investors believe that the additional expenses cannot be fully passed through to consumers or labor and that the demand for digital services

[22]We replicate our analysis employing the Fama-French three-factor model and the model used by (Kothari and Warner, 2007) and obtain similar results. See Appendix Tables C.5 and C.6. Furthermore, our results remain similar if we use a value-weighted portfolio instead of an equally weighted portfolio (Appendix Table C.7). Finally, to mitigate concerns with the statistical significance of the results, we employ additional parametric and nonparametric significance tests (Appendix Table C.8).

is not perfectly inelastic.[23]

Table 4.2: Cumulative Average Abnormal Return – Baseline Result

	(1)
	Stock return
Alpha	0.044**
	(0.019)
Market return (S&P 1200)	0.715***
	(0.048)
21-22 Mar. 2018	-0.692***
	(0.070)
Observations	53,724
Firms	222
Adj.-R2	0.063

Notes: The table presents the results of the conditional market model $R_{it} = \alpha + \beta R_{mt} + \gamma D_t + \epsilon_{it}$. R_{it} is the return of firm i on day t that is likely to fall under the scope of the digital tax proposal, R_{mt} is the return of the market index m (S&P Global 1200) on day t, D_t is a dummy set equal to 1 in the two-day event window, and ϵ_{it} is an error term. α provides an estimate for the alpha of an equally weighted portfolio of all 222 treated firms and β is the estimate of the portfolio's market beta. The coefficient estimate of γ (and the corresponding standard error) is multiplied by 2 to account for the length of the two-day event window (Eckbo et al., 2007). γ can thus be interpreted as an estimate for the cumulative average abnormal return (CAAR) over the two-day event window. The model is estimated using returns of 250 trading days before the event date, excluding the 10 trading days immediately prior to the event date. Clustered standard errors by firm and trading days are in parentheses. Asterisks denote significance at the 1% (***) and 5% (**) levels.

To further understand the investor reaction, we test our second hypothesis by analyzing the market reaction with regard to firms' tax avoidance activities. We interact our event date dummy with different measures of tax avoidance. We define the variable *Tax avoidance* as the negative of the effective tax rate (ETR). Based on financial statement information, we calculate a one-year short-term and a five-year long-run ETR measure for all potentially affected firms (Dyreng et al., 2008). The one-year short-term ETR measure is based on the most recent financial statement information that is at hand for investors on the event date. The five-year measure is based on the annual statements from 2013 to 2017. We assume that firms with lower ETRs engage more actively in tax planning and tax avoidance. In addition, we define the variable *Profit-shifting potential*

[23] We acknowledge that a clear-cut distinction between the effects of the two directives is unfeasible as they were released at the same time. However, in contrast to the proposal on a significant digital presence, the precise and detailed proposal on the DST allows investors to perceive a direct analysis of the effect of the potential new legislation on profits.

as the ratio of intangible assets to total assets. Various studies show that intangible assets, and implicitly the level of research and development activities, are positively associated with engagement in profit shifting (Griffith et al., 2014; Heckemeyer et al., 2014; De Simone et al., 2016).

Table 4.3 depicts the results. As expected, the regression results in Column (1) show that the capital market reaction is more pronounced for firms that avoid more taxes. A firm with an ETR of 25.63 percent (the average in our sample) has a negative stock market reaction in our event window of 0.679 percent, and a 1 percentage point decrease in the ETR is associated with a 0.021-percentage-point-lower two-day CAAR. We find similar results if we use the long-run ETR measure to proxy tax avoidance (Column (2)). Furthermore, stock prices seem to decrease more for firms with a higher profit-shifting potential, albeit not significantly in conventional terms (Column (3)). Overall, the results are in line with our second hypothesis. The findings indicate that investors pay a premium for the shares of tax-avoiding digital firms and that investors believe that the proposed measures will hamper tax avoidance, increasing affected firms' effective tax burdens to similar levels as those of less tax-avoiding firms.[24] Hence, the price premium for tax-avoiding firms diminishes upon the proposals' release, which is in line with the European Commission's intention to safeguard tax revenues from base erosion and profit shifting.

Next, we test our third hypothesis to evaluate whether investors perceive the digital tax as effective in extracting location-specific rent from digital firms. Because exact information about the amount of user value creation is not observable and the extent of firms' digital activity, digital revenues, or number of users in a country is not disclosed publicly, it is difficult for investors to assess precisely to what extent a firm is affected by digital tax proposals. For this reason, investors may instead evaluate a firm's engagement in the European market. We assume that the level of engagement in the European market is positively correlated with the level of revenues that are recognized

[24] Note that due to the deductibility of the DST from the corporate income tax base, those firms that currently pay more corporate income tax will be able to deduct more of the DST paid. Assuming that firms are unable to avoid the DST since it is a tax on revenues and, thus, pay taxes in proportion to their digital revenues in the EU, the effective tax burdens of affected firms will converge.

Table 4.3: Cross-Sectional Analysis – Tax Avoidance

	(1)	(2)	(3)
	Stock return	Stock return	Stock return
Alpha	0.047**	0.047**	0.044**
	(0.020)	(0.020)	(0.019)
Market return (S&P 1200)	0.676***	0.676***	0.714***
	(0.050)	(0.050)	(0.048)
21-22 Mar. 2018	-0.679***	-0.727***	-0.692***
	(0.166)	(0.154)	(0.078)
Tax avoidance	0.001		
	(0.001)		
Tax avoidance × 21-22 Mar. 2018	-0.021***		
	(0.006)		
Tax avoidance (5-year)		0.000	
		(0.001)	
Tax avoidance (5-year) × 21-22 Mar. 2018		-0.022**	
		(0.010)	
Profit shifting potential			-0.001
			(0.001)
Profit shifting potential × 21-22 Mar. 2018			-0.009
			(0.010)
Observations	42,350	42,350	53,482
Firms	175	175	221
Adj.-R2	0.06	0.06	0.062

Notes: The table presents the results of the conditional market model: $R_{it} = \alpha + \beta\, R_{mt} + \gamma\, D_t + \rho\, I_i + \delta\, I_i\, D_t + \epsilon_{it}$. R_{it} is the return of firm i on day t that is likely to fall under the scope of the digital tax proposal, R_{mt} is the return of the market index m (S&P Global 1200) on day t, D_t is a dummy set equal to 1 in the two-day event window, and ϵ_{it} is an error term. I_i is an estimate for the tax avoidance or the profit shifting potential of a firm. Column (1) measures *Tax avoidance* as the negative of a firm's effective tax rate (ETR). Column (2) uses the five-year long-run ETR measure. In both specifications, firms with negative ETRs are excluded from the sample. The negative conversion allows for an intuitive interpretation of the coefficient δ on the two-day CAAR. The Tax avoidance variable is centered on the mean. Column (3) measures *Profit shifting potential* as the ratio of intangible to total assets. Coefficients can be interpreted as in Table 4.2. In addition, ρ measures the effect of the firm-specific indicator on the stock return, respectively. δ is an estimate of the effect of the firm-specific indicator on the two-day CAAR. The model is estimated using returns of 250 trading days before the event date, excluding the ten trading days immediately prior to the event date. Clustered standard errors by firm and trading days are in parenthesis. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

in the financial statements of European affiliates of multinational groups. We define the variable *EU exposure* as the ratio of EU affiliates' revenues to the total revenue of the group's affiliates. The higher the ratio, the more a group is engaged in the European market. Table 4.4 depicts the results of the regressions that include firm-specific interaction variables. Column (1) highlights that higher *EU exposure* has a significant negative effect on the two-day CAAR. This result indicates that investor reaction is in line with the scope of the proposals that are limited to digital services provided in the EU. This analysis also corroborates our assumption that investors mainly reacted to the DST proposal. The DST is an additional tax in the European market, regardless of whether a taxable nexus already exists. In contrast, the virtual permanent establishment proposal is designed to create a new nexus for firms that thus far do not have a taxable nexus but engage in significant digital activities in the EU. Thus, if investors had reacted to the significant digital presence proposal rather than to the DST proposal, we should have observed no or a positive association between EU exposure and stock prices.

Column (2) indicates that, as intuitively expected, investor reaction is more negative for firms with higher revenues. Our data do not allow us to disentangle digital services revenues and nondigital revenues, but we assume that digital revenues are proportional to the overall revenues of digital firms. The capital market seems to have incorporated the effects of a flat gross revenue tax that increases the tax burden proportional to the level of turnover. The last column of Table 4.4 indicates that the reduction in stock prices is higher for corporations that have suffered a loss in the preceding financial year, although the interaction coefficient is not significant in traditional terms.[25]

Furthermore, we analyze whether investors perceive the proposals as a threat to future growth rates. Given that future growth perspectives are based on investors' expectations and are uncertain, we use the revenue growth and total asset growth of past years as a predictor for future growth. Table 4.5 depicts the results. The first (second) column shows that the two-day CAAR is more negative for firms that experienced larger (mean) revenue growth rates one year (over five years) before the release of the proposals. The effect on the two-day CAAR is similar for firms' total assets growth rate, as depicted in Columns (3) and (4). Investors seem to devalue firms with higher growth rates preceding the proposals' release more than firms with lower growth rates. This result indicates that investors perceive proposals to mitigate the future growth potential of digital firms.

[25]The small fraction of loss-making firms in our sample (only 20 firms with negative earnings before income and tax in 2017) limits the statistical power of this analysis.

Table 4.4: Cross-Sectional Analysis – Firm-Specific Characteristics

	(1)	(2)	(3)
	Stock return	Stock return	Stock return
Alpha	0.043**	0.043**	0.043**
	(0.019)	(0.020)	(0.019)
Market return (S&P 1200)	0.703***	0.715***	0.715***
	(0.048)	(0.048)	(0.048)
21-22 Mar. 2018	-0.621***	-0.668***	-0.619***
	(0.112)	(0.080)	(0.188)
EU exposure	0.000		
	(0.000)		
EU exposure × 21-22 Mar. 2018	-0.012**		
	(0.006)		
Revenues		0.000	
		(0.000)	
Revenues × 21-22 Mar. 2018		-0.012**	
		(0.005)	
Loss-making (2017)=1			0.015
			(0.039)
Loss-making (2017)=1 × 21-22 Mar. 2018			-0.77
			(1.348)
Observations	50,820	53,724	53,724
Firms	210	222	222
Adj.-R2	0.063	0.063	0.063

Notes: The table presents the results of the conditional market model: $R_{it} = \alpha + \beta\, R_{mt} + \gamma\, D_t + \rho\, I_i + \delta\, I_i\, D_t + \epsilon_{it}$. R_{it} is the return of firm i on day t that is likely to fall under the scope of the digital tax proposal, R_{mt} is the return of the market index m (S&P Global 1200) on day t, D_t is a dummy set equal to 1 in the two-day event window, and ϵ_{it} is an error term. I_i is an for firm-specific characteristics. Column (1) includes *EU exposure* as the ratio of revenues by subsidiaries located in the EU to the overall revenue of all the firm's subsidiaries. Column (2) includes *Revenues* as a firm's consolidated revenues. The variable is centered on the mean. Column (3) includes *Loss-making* as a dummy variable indicating firms with losses in the financial year 2017. Coefficients can be interpreted as in Table 4.2. In addition, ρ measures the effect of the firm-specific indicator on the stock return, respectively. δ is an estimate of the effect of the firm-specific indicator on the two-day CAAR. The model is estimated using returns of 250 trading days before the event date, excluding the 10 trading days immediately prior to the event date. Clustered standard errors by firm and trading days are in parentheses. Asterisks denote significance at the 1% (***) and 5% (**) levels.

Overall, the findings imply that the market differentiates its response depending on firm characteristics when evaluating the effect of the "digital tax package." The cross-sectional results suggest that investors incorporate the intention of the European Commission's proposals to secure tax revenues and extract location-specific rent in their reaction.

Table 4.5: Cross-Sectional Analysis – Growth Ratios

	(1)	(2)	(3)	(4)
	Stock return	Stock return	Stock return	Stock return
Alpha	0.045**	0.045**	0.045**	0.045**
	(0.020)	(0.020)	(0.020)	(0.020)
Market return (S&P 1200)	0.720***	0.720***	0.720***	0.720***
	(0.049)	(0.049)	(0.049)	(0.049)
21-22 Mar. 2018	-0.720***	-0.718***	-0.720***	-0.718***
	(0.073)	(0.082)	(0.075)	(0.083)
Revenue growth (last year)	0.000**			
	(0.000)			
Revenue growth (last year) × 21-22 Mar. 2018	-0.004***			
	(0.001)			
Revenue growth (5-year)		0.000**		
		(0.000)		
Revenue growth (5-year) × 21-22 Mar. 2018		-0.009***		
		(0.003)		
Asset growth (last year)			0.000**	
			(0.000)	
Asset growth (last year) × 21-22 Mar. 2018			-0.010***	
			(0.002)	
Asset growth (5-year)				0.000
				(0.000)
Asset growth (5-year) × 21-22 Mar. 2018				-0.005**
				(0.002)
Observations	52,514	52,514	52,514	52,514
Firms	217	217	217	217
Adj.-R2	0.063	0.063	0.063	0.063

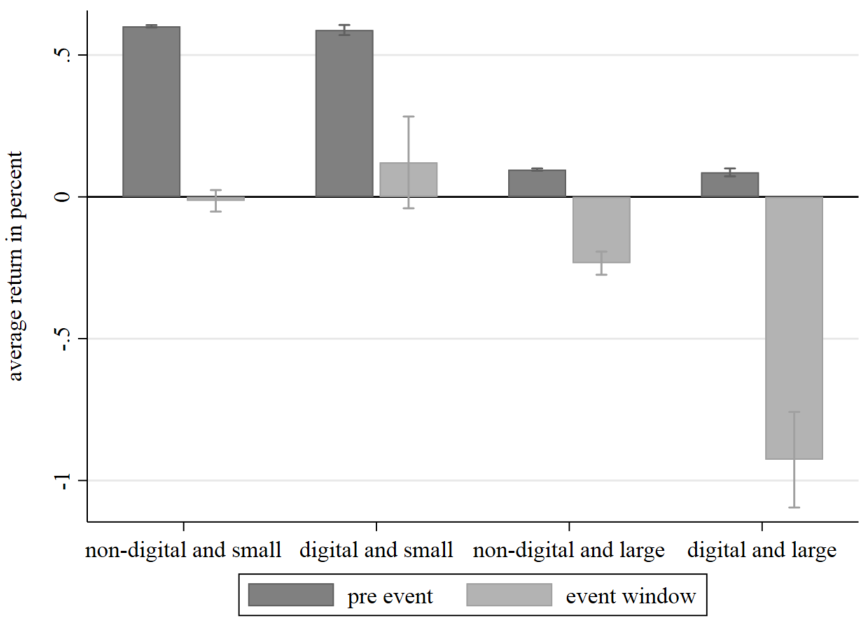
Notes: The table presents the results of the conditional market model: $R_{it} = \alpha + \beta\, R_{mt} + \gamma\, D_t + \rho\, I_i + \delta\, I_i\, D_t + \epsilon_{it}$. R_{it} is the return of firm i on day t that is likely to fall under the scope of the digital tax proposal, R_{mt} is the return of the market index m (S&P Global 1200) on day t, D_t is a dummy set equal to 1 in the two-day event window, and ϵ_{it} is an error term. I_i is an for firm-specific growth ratios. Column (1) includes the revenue growth rate of 2016–2017, that is, the year preceding the release of the proposals. Column (2) includes the five-year average revenue growth rate for the years 2013–2017. Column (3) includes the total assets growth rate of 2016–2017. Column (4) includes the five-year average total assets growth rate for the years 2013–2017. The growth rates are centered on the mean. Coefficients can be interpreted as in Table 4.2 and 4.4. Clustered standard errors by firm and trading days are in parentheses. Asterisks denote significance at the 1% (***) and 5% (**) levels.

4.4.2 Additional Analyses

In this section, we apply two additional analyses to corroborate our main result. First, we directly leverage all listed firms' returns — affected and not affected by the EU proposal. For this reason, we obtain stock market data on all actively traded firms in the countries of our initial sample, that is, all countries from which the treated digital firms are coming. Our extended sample consists of 17,370 firms, which can be grouped into four categories. The first category comprises 13,360 nondigital and small firms (revenue below 750 million euros). The second category comprises 767 digital and small firms. The third category consists of 3,021 nondigital and large firms. Finally, we have — as in our initial sample — 222 digital and large firms.

We begin by demonstrating the descriptive differences in average returns for each group before and within the event window. Figure 4.2 depicts the coefficients graphically. For each group, the figure shows that the average return in the event window is below the preevent period but that for digital and large firms, the average return is the most negative and is significantly different from zero in the event window. The strong negative investor reaction in contrast to the other groups validates that the reaction can be tied to the release of digital tax proposals.

Figure 4.2: Comparison of Affected and Unaffected Firms by Size and Industry



Notes: The graphic depicts the average returns of four different groups of firms over 250 trading days before the event date, excluding the ten trading days immediately prior to the event date and within the two-day event window from March 21 to March 22, 2018. 13,360 firms are non-digital and small (revenue below 750 million Euro), 767 firms are digital and small, 3,021 firms are non-digital and large and 222 firms are digital and large. The vertical lines represent the 95 percent confidence intervals.

Furthermore, we use this extended sample to apply an alternative empirical approach and reestimate the event study using the following empirical design:

$$\begin{aligned} R_{it} = \alpha + \beta_1\, Large_i + \beta_2\, Digital_i + \beta_3\, Event_t + \beta_4\, Large_i \times Digital_i + \\ \beta_5\, Large_i \times\; Event_t + \beta_6\, Digital_i \times Event_t + \\ \beta_7\, Large_i \times Digital_i \times Event_t + \epsilon_{it}. \end{aligned} \tag{4.3}$$

R_{it} is, as in our main specification, the return of firm i on day t. *Large*$_i$ is a dummy variable that identifies firms above the revenue threshold of 750 million euros. *Digital*$_i$ is a dummy variable that identifies all firms that are part of the digital economy.[26] *Event*$_t$ is a dummy variable that takes the value of 1 in the event window. The coefficient of the triple interaction, β_7, is our coefficient of interest and indicates whether the return of large and digital firms, those affected by the proposals, is different in the event window relative to nonaffected firms. While in the main analysis the abnormal return is estimated as the return's deviation from the expected return using a firm's alpha, beta, and the general market movement, in this analysis, we estimate the abnormal return of affected firms relative to nonaffected firms. We find a negative and statistically significant average abnormal return of -0.832 percent of large and digital corporations in the two-day event window relative to the groups of nonaffected firms.[27] Hence, we find a qualitatively similar result to our main specification.

Second, we exploit the fact that many European countries started introducing a DST unilaterally, as no consensus at the EU level had been reached. Among others, France passed a DST in July 2019, which applied retroactively as of January 1, 2019. We exploit this legislation to analyze how investors react to the actual passing of a DST policy. Due to the ongoing debate in the EU and France, investors knew well before the bill passed the French Senate on July 11, 2019, that digital firms might be subject to an additional tax. Hence, it is feasible that we do not observe any market reaction because

[26] As in the initial specification, the relevant NACE Rev. 2 codes are: 6201, 6209, 6311, 6312, 4791, and 5811–5819.

[27] The results are depicted in Appendix Table C.9. Inferring that the average abnormal return in the event window is attributable to the news about digital taxes rather than to general trends between groups is contingent on the assumption that affected and nonaffected firms share parallel trends in the preevent period. Appendix Figure C.2 confirms that the stock market movement is not systematically different between affected and nonaffected firms in the preevent period.

the effect was already incorporated into market prices. However, the French DST introduction was subject to significant public attention and political pressure by the US government (New York Times, 2019; Wall Street Journal, 2019; US Trade Representative, 2019). Hence, investors could also believe that an introduction is unlikely due to the threat of a costly US intervention. Notwithstanding the US government's pressure, the French Senate voted in favor of a DST, which is widely based on the European Commission's proposal. An impact assessment before the introduction identifies 23 listed digital MNEs to be affected (Pellefigue, 2019). We find a significant negative CAAR for these firms of -0.28 percent and depict the analysis in Table 4.6.[28] Exploiting this setting is particularly valuable because it shows investors' reaction to the actual enactment of a DST. Finding a negative reaction at the actual policy passing, and in addition to the reaction to the European Commission's proposal release, supports our suggestion that investors perceive the effect of digital taxes to be highly negative and extends our previous findings.

4.4.3 Economic Magnitude

Based on our findings of a negative capital market reaction, we estimate the market value reduction in absolute terms. Market values are obtained from the EIKON database and converted into euros using the applicable exchange rate on our event date. The total market value of all 222 affected firms is more than four trillion euros. We estimate the firm-specific change in abnormal market value as the product of a firm's market value and the firm-specific abnormal return in our two-day event window (Malatesta, 1983; Peterson, 1989; Cline et al., 2018).[29] The overall abnormal market value change is the sum of all affected firms' abnormal market value changes. We find that the market value of firms that are likely to be affected by EU digital tax proposals dropped by

[28] We limit the estimation period in this analysis to the months between the final rejection of the DST on an EU-wide level to avoid any confounding events during our preevent period.

[29] We estimate $\Delta MV = \sum_{i=1}^{222} \sum_{t=0}^{1} MV_{it} \times AR_{it+1}$, where MV_{it} refers to the closing market value of firm i at trading day t. AR denotes the abnormal return. Variable $t = 0$ refers to March 20, 2018. The firm-specific AR is estimated using the method by Kothari and Warner (2007); see Appendix Table C.5 for an explanation. We do this because multiplying our result of the CAAR from the regression analysis with the market value of the treated firms would lead to slightly different results, as the CAAR in our baseline regression is drawn from an equally weighted portfolio.

Table 4.6: Analysis of French Enactment of the Digital Services Tax

	(1)
	Stock return
Alpha	0.044
	(0.044)
Market return (S&P 1200)	1.105***
	(0.129)
11-12 Jul. 2019	-0.282***
	(0.069)
Observations	1,679
Firms	23
Adj.-R2	0.164

Notes: The table presents the results of the conditional market model $R_{it} = \alpha + \beta\, R_{mt} + \gamma\, D_t + \epsilon_{it}$. R_{it} is the return of firm i on day t that is likely to fall under the scope of the French digital tax proposal, R_{mt} is the return of the market index m (S&P Global 1200) on day t, D_t is a dummy set equal to 1 in the two-day event window, and ϵ_{it} is an error term. α provides an estimate for the alpha of an equally weighted portfolio of all 23 treated firms and β is the estimate of the portfolio's market beta. The coefficient estimate of γ (and the corresponding standard error) is multiplied by 2 to account for the length of the two-day event window (Eckbo et al., 2007). γ can thus be interpreted as an estimate for the cumulative average abnormal return (CAAR) over the two-day event window. The model is estimated using returns of 82 trading days before the event date, excluding the 10 trading days immediately prior to the event date. Clustered standard errors by firm and trading days are in parentheses. Asterisks denote significance at the 1% (***) level.

52.854 billion euros in excess of the normal market movement. A considerable share of the abnormal market value change is born by US-based firms, which constitute the largest group of treated firms. In numbers, approximately 40 percent of the market value reduction is attributable to firms headquartered in the United States.

Intuitively, the economically significant abnormal change in market value stands in contrast to the annual tax revenue estimates generated from the DST of 3.9–5 billion euros (European Commission, 2018a; Fuest et al., 2018). We translate the annual tax revenue estimates in present-value figures to compare them with the market value reduction. Unfortunately, we cannot directly observe a firm's digital revenue generated in the EU to directly compare market value changes with tax payments at the firm level. In a back-of-the-envelope calculation, we proxy the aggregated present value (PV) of the estimated tax revenues to find the breakeven point of the reduction in shareholder wealth and the increase in social wealth.[30] We graphically depict the PV of the estimated annual tax revenues in Appendix Figure C.3. For example, if we assume five billion euros of annual tax revenues, which the European Commission expects to increase by 20 percent per annum and the current zero-interest rate environment as a discount rate for the PV calculation, it will take approximately six years to recover the initial drop in market value with tax revenue. Altering the assumptions, it will take seven or 11 years. We acknowledge that both figures are not precisely comparable because the deadweight loss and the economic incidence of the newly proposed tax are unclear and tax revenues might develop differently and certainly nonlinearly over time.

4.4.4 Additional Robustness Tests

We conduct additional tests to verify the robustness of our main results. In Appendix Table C.10, we replicate our main analysis for four alternative event dates to mitigate concerns that the event has materialized at different points in time.[31] We test the market reaction on, first, dates before the release of the proposals, on which some rumors about a new European DST spread publicly; and second, dates after the release of the proposals on which it became more certain that an EU-wide political agreement on the DST would not be reached. All results are indistinguishable from zero. Except on March 12, 2019, the abnormal return estimates are significantly negative. This finding is counterintuitive, as the date marks the time when it became more certain that the EU would not enact a common DST in the near future. However, several economy-wide shocks regarding the ongoing debate about the exit of Great Britain from the EU hit the market on the same date.

Next, we address concerns that news regarding a trade war could have triggered the market reaction. If the firms affected by the digital tax proposals had reacted to the

[30] We estimate a model of the following form: $PV_0 = TaxRevenue_0 \times \sum_{t=1}^{T} (1+g)^t/(1+r)^t$, where g refers to the expected annual growth rate of tax revenue per year t and r to the discount rate.

[31] On February 26, 2018, the first rumors on a potential digital tax initiative by the European Commission were spread. On March 15, 2018, occasional reports on the soon-to-be-released directive proposals can be found (Becker and Englisch, 2018; Bloomberg, 2019; Financial Times, 2018b). At the Economic and Financial Affairs Councils on December 4, 2018, a strong opposition against the proposals was formed and on March 12, 2019, the EU digital services tax proposal was finally taken off the agenda in an official debate.

increased probability of a trade war, investors would presumably also react similarly on other dates of the heightened probability of a trade war. Hence, we test the market reaction on dates with heightened media attention on a potential trade war. Conducting a Google Trends analysis, we find that on at least four dates in 2018, the term *trade war* received great attention. We replicate the event study analysis for these dates and depict the results in Appendix Table C.11. Overall, we cannot see a significant negative capital market reaction on one of the alternative dates that heightened the risk for a (tax-)trade war.

Finally, we analyze the dates surrounding our event window to mitigate concerns that other events close to our event window confound our findings. In Appendix Figure C.4, we show the abnormal buy-and-hold return for the portfolio of treated digital firms. That is, we display the abnormal value development of a portfolio that is bought one trading day before the event window and held until 12 days after the event window. We confirm that a significant negative abnormal return drop is observable only during our event window and that this drop does not revert over the subsequent days. Next, we quantitatively disentangle the dates surrounding the event. Appendix Table C.12 shows the results. The daily abnormal returns range between -0.380 and 0.167 percent.[32] The positive abnormal return on the date before our event window indicates no stock market anticipation of the proposals' release. In line with this result, we find a smaller CAAR if we extend our event window length to a three-day window. This confirms the event window choice based on Google Trends analysis and media search.

4.5 Conclusion

The era of digitalization has led to an intense political and academic debate on how to adapt the principles of corporate taxation to the digital economy. However, empirical evidence on the effects of proposed adjustments to corporate taxation is scarce. Our

[32]In an untabulated analysis, we also confirm that our results are not biased by a small number of sizeable negative abnormal returns. Of the 222 affected firms, 144 firms have negative abnormal returns in our event window.

study contributes to the recent call for further research on the proposed policies of taxing the digital economy and helps to evaluate the effects of digital tax measures.

Analyzing the capital market reaction in response to the European Commission's digital tax proposals, we find a significant reduction in the firm value of 222 digital firms, which are likely to be affected. The negative abnormal market reaction of -0.692 percent translates to a market value decrease of digital corporations by at least 52 billion euros, 40 percent of which is attributable to US-based corporations. Our main result has three central implications: first, it suggests that investors, on average, perceive the increased likelihood of the introduction of digital tax measures as negative news for firms' future profitability, and investors do not anticipate that firms are able to easily avoid the additional tax; second, our evidence implies that investors expect that firms will not be able to pass through all of the additional tax expenses to labor or customers; third, the economic magnitude of the reaction implies that the capital market does not expect these tax measures to be repealed in the short term.

Our cross-sectional analyses reveal that the market differentiates its response depending on firm characteristics. We find that the negative abnormal return is significantly stronger for firms that are more tax-avoiding and for firms that have higher profit-shifting potential. This result suggests that firms receive a market premium for tax avoidance and that the premium diminishes with the proposed tax measures.

Overall, the investor reaction reflects the intention of the European Commission's proposals to secure tax revenues and extract location-specific rent, suggesting that the capital market expects that the proposals' objectives are achievable. However, our results indicate that increasing the tax burden for a highly innovative industry contradicts political initiatives to promote an attractive investment climate and interferes with the EU's core objective to foster innovation and economic growth.

Chapter 5

Foreign Aid through Domestic Tax Cuts? Evidence from Multinational Firm Presence in Developing Countries

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Abstract: This paper studies whether corporate tax cuts in developed countries affect economies in the developing world. We focus on one of the most prominent fiscal policies – the corporate income tax regime – and study a major U.K. tax cut as an exogenous shock to foreign investment in Africa. Difference-in-differences estimates show that multinational U.K. firms increase their subsidiary presence in sub-Saharan Africa by 17-24 percent following the 2010 announcement of U.K. tax rate reductions. Exploiting location-specific nighttime luminosity data as well as local data from the African Demographic and Health Surveys, we also document increased economic activity and higher employment rates of African citizens within close proximity (10 kilometers) of local U.K.-owned subsidiaries. Our findings imply that, beyond the goal of motivating home country investment, developed countries' corporate tax cuts have economic impact in developing countries.

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5.1 Introduction

This paper studies whether corporate tax cuts in developed countries stimulate foreign investment in developing countries. A large literature demonstrates that business investment is influenced by tax policy (e.g., Devereux and Griffith, 1998; Desai et al., 2004; Djankov et al., 2010; Ohrn, 2018; Suárez Serrato and Zidar, 2018; Giroud and Rauh, 2019), but generally this work examines investment activity within the home jurisdiction. We study whether tax rate reductions in one country are associated with *foreign* investment. We focus on developing countries in particular to evaluate whether domestic corporate tax rate cuts serve as an indirect form of foreign aid. We show that major corporate tax cuts in the U.K. result in greater multinational firm investment in sub-Saharan Africa and drive increased economic activity and employment in local African markets.

Understanding the role of developed countries' investment in developing countries is important for a variety of reasons. First, while foreign direct investment ("FDI") contributes to economic growth, the extent to which this is effective in developing countries with weak institutions is unclear (Alfaro et al., 2004; Lensink and Morrissey, 2006). Thus, empirical evidence is essential to better evaluate the role of FDI in developing countries such as those in Africa (Barro, 1991; Englebert, 2000; Ghura and Hadjimichael, 1996; Savvides, 1995). Second, the potential consequences of multinational firm presence for developing countries is ambiguous. If FDI and domestic investment are complements (Desai et al., 2004, 2005), multinational firm investment could stimulate economic growth and generate large spillover effects in the local economy. However, multinational firms have been accused of exploiting local markets in developing countries, such as through natural resource extraction or the use of sweatshops, and thus may provide little overall benefit (e.g., Bond, 2006; Harrison and Scorse, 2010). Further, foreign firms may simply compete with local firms for a fixed market base, contributing little to the overall economic well-being of the country aside from the indirect effects of increased competition. Third, while direct transfers to developing countries are a critical source of aid, these direct policies are subject to local capture that limits the potential beneficial impact (Andersen et al., 2022). Quantifying the extent of other fiscal policies, as well as the role of private sector investment in disseminating capital (Cravino and Levchenko, 2017), informs the amount and type of spillover effects that can occur in these countries.

Our central prediction is that tax rate reductions in developed countries are associated with foreign investment in developing countries. Theory demonstrates that firms should invest in the highest after-tax net present value project, regardless of jurisdiction (Modigliani and Miller, 1958; Jorgenson, 1963; Hall and Jorgenson, 1967; Hayashi, 1982). Thus, if tax rate cuts in one country generate additional financial resources for a firm, the firm could increase domestic and/or foreign investment. Consistent with this theory, recent empirical work finds that multinational firms increase foreign investment and employment after domestic tax rate changes (Lester, 2019; Glaeser et al., 2021). However, this evidence primarily examines investment in developed countries that are characterized by strong institutions and that have relatively low (and investment-favorable) statutory tax burdens. Whether such increased investment occurs in developing countries with weaker institutions and relatively higher statutory tax burdens is unclear ex ante. In addition to examining whether investment flows to Africa, we also quantify, using a variety of outcomes and methods, how much tax-related investment occurs.

Our empirical tests focus on a major tax cut in the U.K. as a shock to firms' investment in developing African countries. We use this particular tax change for four reasons. First, the U.K. tax system changed substantially, with policymakers committing to reduce the corporate tax rate from 28% to 20% by 2015 (The Guardian, 2010a; Liu, 2020). These rate reductions were salient and large enough to have a significant impact on corporate investment. Second, the primary focus of the U.K. tax rate reduction was to encourage domestic investment in the U.K., and therefore, the rate change provides a powerful and exogenous setting in which to examine foreign investment activity. While domestic tax policy is often endogenous to corporate decisions and growth in domestic markets (Romer and Romer, 2010; Giroud and Rauh, 2019), our study does not face this challenge because tax policy is likely not endogenous to corporate decisions in markets outside the home jurisdiction. Third, the U.K. is home to hundreds of multinational firms and has close historical and colonial ties with Africa. Thus, the presence of multinational U.K. firms in Africa is a plausible and economically meaningful channel for investment in developing economies. Finally, our tests exploit variation across 46 different African countries, helping to rule out alternative explanations (such as low-taxed African regimes) and more confidently attribute our results to the U.K. tax rate change.

We construct a panel dataset of global firms from 2005 to 2018 from the Bureau van Dijk Orbis historical database. We focus on identifying majority-owned subsidiaries and track these subsidiaries across hierarchical layers in nearly all countries of the world.[1] We retain only those parent firms with a subsidiary in at least one of the 46 sub-Saharan African countries between 2005 and 2018. Our sample includes 22,000 distinct parent firms that own over 64,000 African subsidiaries. 22 percent (19 percent) of the subsidiaries are owned by parent firms in the same African country (other African countries); the remaining 59 percent of subsidiaries are owned by parents from over 90 other countries, including western countries such as France, Germany, the U.K., and the U.S. The sample consists of 250,000 subsidiary-years, which are aggregated to the parent level for a sample of 103,670 parent firm-country-year observations (for example, Unilever-Kenya-2008).

Our first tests study changes in U.K. subsidiary presence as a proxy for firm investment in Africa. Specifically, we use a difference-in-differences specification to compare the change in the number of U.K.-owned subsidiaries in Africa around the U.K. tax cut with the change in the number of African subsidiaries of other parent firms. We find that U.K. multinational firms increased the number of subsidiaries in African coun-

[1] While financial accounting data is typically only available for subsidiaries in developed countries that mandate the publication of financial reports, particularly in Europe, Orbis carefully collects data on legal entity ownership from business registers and various database providers worldwide. Further, the database providers use both a bottom-up and top-down approach to identify corporate ownership links. Therefore, we are able to track firm-level equity ownership of foreign subsidiaries in 46 African countries throughout the sample period. The Appendix documents several steps taken to validate the data and the ownership links; see also De Simone and Olbert (2021) and Olbert (2021).

tries by 17 percent after the tax cut relative to the control group. This effect increases to 24 percent in the sample conditioned on multinational firms having a pre-existing presence in a given African country prior to the U.K. tax rate reduction.[2] Limiting the control group to multinationals from OECD countries or multinationals from countries with former colonial ties to Africa yields consistent and even larger effects. We also find that results are robust to isolating the control group to French multinationals, who are most similar in terms of firm characteristics and historical investment patterns in Africa. Throughout all specifications, host country-by-year fixed effects control for the economic and political development in the specific African countries and thus hold key determinants of FDI constant (Asiedu, 2006). Further, results are robust to controlling for time-variant multinational firm and headquarter country characteristics. We also confirm that the parallel trends assumption appears to be met, as foreign-owned subsidiary presence was not statistically different between the U.K. and each of the control groups prior to the reform. The U.K. tax cut-induced investment effect is present in 2010 and 2011 and becomes even stronger in later years as the corporate tax rate further declines.

Additional analyses demonstrate predictably larger effects in several cross-sections, mitigating concerns that other U.K. policies or events drive the observed results. Those firms that should benefit the most from the U.K. tax cut, measured based on having relatively higher U.K. effective tax rates prior to the law change, exhibit the greatest increase in subsidiary presence post-reform. Moreover, former U.K. African colonies are especially likely to receive additional U.K. investment following the U.K. tax cut, with larger increases equivalent to 27.4 - 29.8 percent. We also observe effects in the labor-intensive industries of Manufacturing and, to a lesser extent, Construction, implying large potential employment spillovers in the local economy. Firms active in the natural resource extraction sector do not drive our results.

[2]We observe an increase in the number of foreign owned subsidiaries in our sample countries over time, reflecting both an increase in multinational investment and improved coverage in the database (a known issue with Orbis). Conditioning the empirical tests on an existing multinational presence prior to the U.K. tax rate reduction helps to mitigate concerns that the effects are driven by improved coverage in the dataset over time. Supplementary tests further validate the Orbis data; see the Appendix.

In addition to forming new African subsidiaries, U.K. firms may have responded to the tax rate change with increased capital investment and employment in their existing African subsidiaries. Thus, we next examine whether the U.K. tax change was broadly associated with increased economic activity surrounding any (existing or new) U.K. subsidiary after the U.K. tax rate change. We measure local economic activity in two ways. First, we use geo-coded nighttime luminosity obtained from the United States Air Force Defense Meteorological Satellite Program (DMSP) (Henderson et al., 2011, 2012; Chen and Nordhaus, 2011; Gennaioli et al., 2013; Michalopoulos and Papaioannou, 2013). Specifically, we construct 10 kilometer (km) grid-cell radii around each subsidiary location and measure changes in luminosity from the pre- to post-reform period. Estimation requires us to hand-collect and geo-code addresses of the U.K. subsidiaries in Africa, which necessarily reduces the sample. We find a 2.2 - 4.5 percent increase in luminosity following the U.K. tax rate in grid-cells containing a U.K. subsidiary as compared to both i) relatively near grid-cells that have similar luminosity values in the pre-period but lack a U.K. subsidiary, and ii) other grid-cells containing a French subsidiary.

Second, while luminosity data proxy for a variety of economic outcomes, we explicitly test whether employment increased in the local surrounding area. We use Demographic and Health Survey (DHS) survey data for African countries, which permits us to link the precise geographic location from each individual survey respondent to the closest U.K.-owned subsidiary in the same country.[3] We then test whether a surveyed individual living close to the U.K. subsidiary (within 10 km) was more likely to be employed after the U.K. tax cut as compared to individuals living in the same general area but outside of the 10 km radius. We find that employment increases by 2.7 to 3.6 percentage points relative to individuals living further away from U.K. subsidiaries. Evaluated at the sample mean, this effect represents a 5 percent increase in total local

[3]The DHS survey is funded by the United States Agency for International Development (USAID) and collects information from African citizens about their employment and health status. These data are used as a common measure of social outcomes in Africa (e.g., Kingdon and Knight, 2007; Hjort and Poulsen, 2019) because they do not suffer from reliability concerns that plague other available unemployment data (Jerven, 2013).

employment. After further refining the control sample to include only French firms, we continue to find positive effects ranging from 1.8-2.6 percentage points.

We contribute to two streams of literature. First, we build on work studying fiscal policy spillovers (Auerbach and Gorodnichenko, 2013; Faccini et al., 2016) and externalities of corporate tax policy in particular (e.g., Gaertner et al., 2020). Suárez Serrato (2019) shows that U.S. regulation aimed at reducing tax avoidance through foreign tax havens has the unintended effect of lower domestic investment due to higher overall tax burdens. Several recent studies provide evidence that international tax competition (i.e., lower corporate taxes in foreign countries) can impact investment and employment in domestic markets of developed countries (Donohoe et al., 2022; Glaeser et al., 2021; Kim et al., 2021; De Vito et al., 2021). However, few studies provide evidence on how domestic tax cuts affect foreign investment of multinational firms. Lester (2019) shows that U.S. firms increased foreign investment after 2004 when domestic tax burdens decreased. Glaeser et al. (2021) and De Vito et al. (2021) provide consistent evidence using large samples of European multinationals and exploiting several tax cuts. To the best of our knowledge, no studies exist on the potential externalities of corporate tax cuts originating in the developed world for developing economies.[4] The closest study to our paper is Liu (2020), who documents notable investment into low-tax countries following the U.K.'s change from a worldwide to a territorial system of taxing corporate profits. However, because most of the African countries in our sample have comparable or higher statutory tax rates as compared to the U.K., the results cannot be attributed to the same factors studied in Liu (2020). Thus, we offer novel evidence linking increased subsidiary ownership, luminosity, and African employment data with fiscal policies originating in the industrial world.

Second, we add to the work on foreign investment in developing countries, in particular on the integration of developing countries in the global economy (for reviews, see Goldberg, 2015; Paul and Singh, 2017). While host country characteristics, including

[4]Recent work finds spillover effects of OECD countries' corporate governance and disclosure regulations on the private sector natural resource extraction industry (Rauter, 2020; Christensen et al., 2021, 2022).

corporate tax rates, have been studied in the prior literature (e.g., Hartman, 1985; Alfaro et al., 2004)[5], the role of factors in investor countries are relatively unknown. We provide novel evidence on an unexplored determinant of FDI in the developing world, namely corporate tax cuts in developed countries, adding to the literature on the integration of these developing countries in the global economy.[6] This finding is of particular importance given recent concerns that development in these countries has stalled and that leaders may even abandon open trade and investment (The Economist, 2021). Our results imply instead that multinational firm presence is a likely channel through which FDI enters and alters the African economy.

5.2 Background

5.2.1 Taxes and Corporate Investment

The literature on the relation between taxes and investment generally examines increased investment within a firm's home jurisdiction (for example, U.K. investment in the U.K.). This is likely because the underlying policy motivation of investment tax incentives and tax rate reductions is often to motivate domestic spending. For example, Edgerton (2010) and Zwick and Mahon (2017) document U.S. corporate investment responses to changes in U.S. depreciation rules. Ohrn (2019) and Lester (2019) find corporate investment following U.S. manufacturing incentives. Studies have also examined U.S. state tax changes and international tax changes, finding semi elasticities of -0.5 for employment and business establishments at the state level (Giroud and Rauh, 2019) and notable foreign investment effects across borders (Devereux and Griffith, 1998; Feld and Heckemeyer, 2011). The implication of this literature is that corporate investment

[5]See Feld and Heckemeyer (2011) for a review and meta-study on taxation and FDI.

[6]For example, Hjort and Poulsen (2019) show that the arrival of fast internet has a positive impact on employment and average incomes, and Goldberg et al. (2010) shows that domestic firms in India benefit from product imports from richer countries.

in a home jurisdiction occurs following home jurisdiction tax rate reductions.[7]

However, corporate investment in *foreign* developing countries could also increase after *domestic* tax rate reductions. Corporate investment and tax rate changes are related through a variety of mechanisms. Subject to certain exceptions (for example, in the absence of full expensing, etc.) a tax rate cut is generally expected to increase investment by lowering the cost of capital (Jorgenson, 1963; Hall and Jorgenson, 1967). However, this result generally applies to domestic investment. In the case of foreign investment, three reasons may lead domestic tax rate reductions to increased foreign investment.

First, the tax cuts may provide increased cash flows, thereby aiding financially constrained firms to realize positive net present value projects abroad (Fazzari et al., 1988). As a result, we may observe financially constrained firms increasing investment in Africa following the U.K. tax cut. Second, even if firms were unconstrained, we may observe that increased cash flows attributable to the domestic tax rate reduction provide necessary funding if typical capital providers are unwilling to provide financing for projects in developing countries. For example, while U.K. financial institutions may typically finance a firm's investment project in the home country or in other developed countries, they may be less willing to do so in developing economies because the returns are subject to both more risk and information asymmetry. Furthermore, the returns may be subject to fewer government protections in less developed political systems. Consequently, external financing may be more difficult to obtain and firms that otherwise have easy or low-cost access to capital may need to rely on internal sources for developing country investment.

Third, in the case of integrated multinational firms, returns realized in Africa are at least to some extent taxed at a lower rate after the U.K. tax rate cuts. The reason is that multinational firms typically employ internal capital markets to facilitate foreign

[7]U.K. government analysis confirms that the U.K. tax rate reductions announced in 2010 intended to stimulate domestic investment. The U.K. report concludes their analysis is "broadly consistent with the wider academic literature, which finds that reductions in Corporate Tax boost investment leading to higher [domestic] GDP and partial revenue recovery" (HM Treasury and HM Revenue & Customs, 2013).

operations. For example, the U.K. headquarters entity or other U.K.-based functions engage with an African subsidiary by providing management services, delivering intermediary products, or providing access to the firm's intellectual capital. The African subsidiary will be charged for such internally provided products and services following the arm's length principle. As a result, the firm's U.K. entities realize higher profits, i.e., the firm's tax base in the U.K. increases due to more operations abroad. Thus, lower domestic tax rates partially decrease the cost of capital for investments in foreign countries, which can explain increased investment in Africa (Jorgenson, 1963).

During the sample period, firms were increasingly expanding their global footprint, suggesting a saturated domestic market, favorable foreign growth opportunities, or both. Whether and to what extent investment occurs in developing countries is unknown but is distinct from investment in developed countries due to the welfare benefits of the direct and spillover effects. We focus on African investment in particular because of the social importance of understanding the development factors in this continent.

5.2.2 U.K. Corporate Tax Changes

To study the effects of corporate tax rate reductions on multinational African presence, we focus on the U.K. tax rate reductions announced in 2010. In 2008, the year prior to the corporate tax rate change, the U.K. corporate income tax rate was 28%. While lower than other developed countries at the time (for example, the U.S. corporate income tax rate was 35%), the U.K. rate was widely viewed as uncompetitive on the basis that it was above the average OECD rate of 23.98% (OECD, 2020) and much higher than the low or 0% tax rates afforded by tax haven countries. Furthermore, the U.K. had a "worldwide" tax system wherein the profits of U.K. multinationals were taxed in the U.K. as well as in the country where they were earned.[8] By comparison, almost all other developed countries, except Japan and the U.S., had converted to a "territorial" system,

[8] To mitigate immediate double taxation of foreign-earned profits, the U.K. tax regime permitted the tax to be deferred until the foreign subsidiaries paid dividends to the U.K. parent. U.K. firms also could use foreign tax credits to reduce the U.K. tax liability. This system was similar to the U.S. tax system prior to 2017 (Egger et al., 2015).

in which earnings were taxed only where earned and were not subject to a second tax in the parent's home country. As a consequence of both the worldwide tax regime and the relatively high corporate tax rate, U.K. firms engaged in substantial tax planning activities to shift income, and in some cases, a firm's "real" geographic presence, to other lower-taxed jurisdictions (The Guardian, 2009; Trades Union Congress, 2010; The Guardian, 2010b; Independent, 2019). For example, several prominent U.K. firms reincorporated outside of the U.K. during this period, primarily to lower their tax burdens (Financial Times, 2008; The Guardian, 2008; Evening Standard, 2008).[9]

While other countries also had tax rate reductions during the sample period, we focus on the U.K. tax rate change for several reasons.[10] First, the rate change was substantial, providing an almost 30 percent decline in the corporate tax burden. Such a large corporate tax rate change likely had a meaningful effect on U.K. firms' user cost of capital, a necessary condition for stimulating investment spending (Hall and Jorgenson, 1967; Hassett and Hubbard, 2002; Hassett and Newmark, 2008).[11] Second, the U.K.'s pre-commitment to future corporate tax rates reduced regulatory uncertainty about future tax burdens. Prior work documents that policy uncertainty can affect a wide range of corporate decisions, including the decision to invest and expand (Ivanov et al., 2020; Gallemore et al., 2021). Because the U.K. rate reductions were legislated in advance, firms could more confidently make longer-term investments – such as foreign expansion – with these new, lower rates in mind. Third, U.K. multinational firms have a long history of investing in Africa, thereby making it a plausible investment pair to study. More than one dozen African countries were at one time colonies of the U.K., evidence of a close historical relationship shown to be positively correlated with FDI

[9]U.K.-based Regus created a new U.K.-listed, Jersey-incorporated holding company with head office and residency in Luxembourg. Other U.K. firms, including the Henderson Group, Charter, Shire, and United Business Media, switched their tax base or residency to Ireland.

[10]The Appendix presents results from testing responses to four other substantial tax changes during the sample period in Germany (2008), Canada (2008), Japan (2012), and Spain (2015). The analyses demonstrates that our results generalize to these other settings. We also acknowledge that several other countries had corporate tax rate changes during the sample period, but because many of these changes were relatively smaller in size, we expect that the amount of cross-border investment may be minimal.

[11]Consistent with the negative relation between the user cost of capital and investment, Sir Alan Budd, the Chair of the U.K. Office for Budget Responsibility, stated, "The measures to reform corporation tax, which are estimated to reduce the cost of capital faced by firms by about 3%, should have a positive effect on investment" (The Guardian, 2010a).

(Makino and Tsang, 2011; Glaister et al., 2020). Furthermore, European companies had a history of investing into Africa, thereby lowering the hurdle rate for businesses to further expand in Africa.

Finally, Liu (2020) shows that the switch to a territorial tax regime in 2009 motivated cross-border investment. However, Liu (2020) focuses on investment in low-tax, developed jurisdictions as destination countries. By studying African countries, we not only provide new evidence about investment into developing countries (which were explicitly excluded from earlier work), but we also exploit variation across African national tax rates, many of which are similar or higher than the U.K. rate. This variation in local country rates permits us to disentangle the effects of the change in regime (worldwide vs. territorial) from the change in tax rates (28% vs. 20%), a distinction that is particularly relevant for generalizing results to other countries that have concurrent changes in regimes and tax rates (such as the U.S. in 2017).

Figure 5.1 depicts multinational firm investment in Africa proxied by the number of multinational firms' subsidiaries in a given country. The color coding reflects the total number of foreign-owned MNE subsidiaries in a country in Panel (A); in Panel (B), the shading captures the total number of U.K.-owned subsidiaries. By far the most foreign investment overall, as well as the most U.K. investment in Africa, is in South Africa. Panel (B) shows that the U.K. firm presence is relatively strong and particularly pronounced in former U.K. colonial countries (e.g., Kenya), confirming a sufficient U.K. presence in the sample.

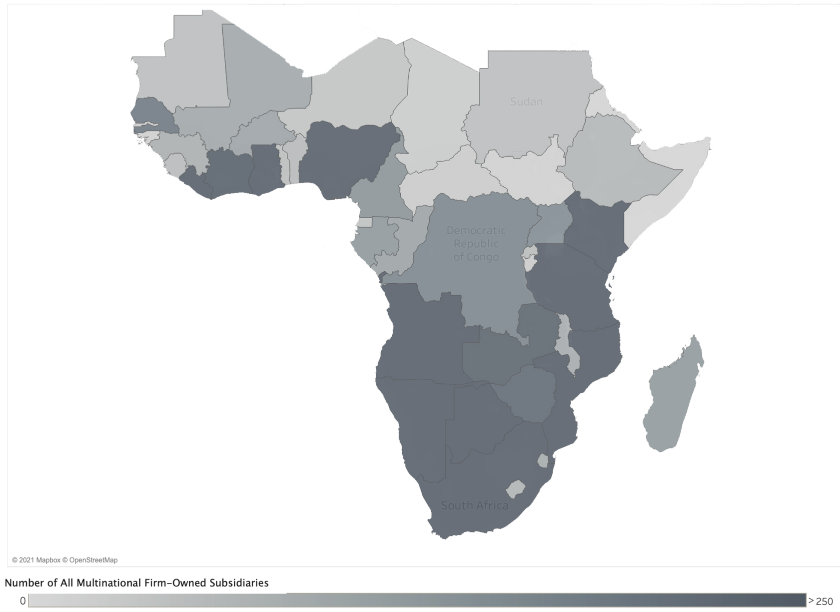
5.3 Data, Sample, and Descriptive Statistics

5.3.1 Firm data from Bureau van Dijk's Orbis

We obtain data on firms' subsidiary structures, as well as financial and industry characteristics, from Bureau van Dijk's Orbis, which includes detailed information on over 400 million companies in the world. While financial information is missing for many legal entities incorporated in countries without regulatory financial reporting mandates, Or-

Figure 5.1: Foreign-owned Multinational Firm Subsidiaries in Sub-Saharan Africa

(A) All Multinational Firm Subsidiaries



(B) U.K.-owned Subsidiaries

Notes: This figure shows the average number of all foreign-owned multinational firm subsidiaries (Panel (A)) and the number of U.K. multinational firm subsidiaries (Panel (B)) in sub-Saharan African countries across sample years. The color-coding is capped at 250 subsidiaries in a given year to improve readability. The following countries had more than 250 foreign-owned subsidiaries during the sample period: Angola (285), Botswana (275), Ghana (441), Kenya (964), Liberia (439), Mozambique (388), Namibia (403), Nigeria (626), South Africa (3,259), and Tanzania (295). In Panel (B), the only country with more than 250 U.K.-owned subsidiaries is South Africa (n=724).

bis typically provides ownership links for subsidiaries and shareholders for each entity in the database.[12] This ownership data allows researchers to recreate a fairly complete picture of firms' operations through legal entities across the world and throughout several layers of ownership linkages (Coppola et al., 2021). From this information, we construct a detailed ownership history for firms based on majority equity shareholdings among entities following De Simone and Olbert (2021) and Olbert (2021).

The data are at the legal entity level. The ownership links in the data permit observation of whether an entity is "standalone" (meaning that it is not owned by another entity) or if it is owned by another firm. We call a legal entity a subsidiary if it is owned by another entity.[13] Because we are focused on studying the destination countries of business investment, we retain all African subsidiaries between 2005 and 2018 and use the ownership links in the data to identify each subsidiary's parent company and home country of incorporation. We observe 64,549 unique subsidiaries across 46 sub-Saharan countries, with a total of 250,112 subsidiary-years for the period 2005 to 2018. These subsidiaries are owned by 22,830 firms from 149 home countries.

We present the number of subsidiaries by each sub-Saharan African country in Table D.1 in the Appendix. The greatest number of businesses are in Ghana, Kenya, Liberia, Nigeria, and South Africa. While the increase in the number of observed subsidiaries over time is consistent with the growth in globalization and FDI during the sample period, subsidiary coverage is also improving over time in the database, resulting in many more multinational firm subsidiaries in the post-period years as compared to the pre-period years. We address this issue through use of alternative samples in the empirical tests; see Section 5.4.3.

Table D.2 in the Appendix presents the sample based on parent firms' home coun-

[12] BvD collects this information for legal entities worldwide even if no publicly available financial reports are available. This information is obtained through alternative data sources, including commercial trade registers, legal documentation, M&A intelligence, and even telephone research. As the data provider also connects shareholder and subsidiary data through these various channels, the existence of certain legal entities can be measured through ownership links even if no information is directly provided by the underlying entity.

[13] The term "subsidiary" is typically used for a corporate entity as opposed to other legal entities such as partnerships. The data do not permit us to identify which entities are corporations, and therefore, we refer to any lower-tier entity as a subsidiary for simplicity. Our inability to observe entity type should not affect the inferences as the construct of interest, FDI in Africa, could occur through any of these entities.

tries. We first separately list the parent countries with the largest number of African subsidiaries and then report the count of remaining observations across three groups formed based on (i) parents in the same African country as the subsidiary ("Domestic African Groups"), (ii) parents in a different African country from the subsidiary ("African Multinationals"), and (iii) parents in the remaining countries in the sample ("Rest of World"). Approximately 20,000 subsidiary-years, or 8.0 percent of the total subsidiary-level observations, correspond to U.K. parent companies. The primary sample used in the empirical tests aggregates all subsidiaries of a parent firm to the country-year level, for a total of 103,670 parent firm-country-year observations; see Appendix Table D.3. This appendix also shows that 22 percent (20 percent) of the firm-country-year observations relate to parents of African subsidiaries in the same (different) African country, with the remaining observations spread across many other jurisdictions.

5.3.2 Nighttime Luminosity from the United States Air Force Defense Meteorological Satellite Program (DMSP)

The Orbis subsidiary data permit measurement of U.K. firm presence in Africa; however, they do not allow us to directly measure outcomes such as total investment and employment spending at the subsidiary level for both existing and new African subsidiaries. In the absence of firm-specific financial data for each African subsidiary, we indirectly measure African investment and employment activity using two additional datasets.

We first proxy for local investment activity using the density of nighttime light emissions within narrowly defined regions (grid-cells) around U.K. firms' subsidiary locations. The geo-coded nature of these data allows us to study spatial economic outcomes in very specific geographic areas where these firms operate. Prior work shows that light emissions are a more reliable, granular, and readily available proxy for economic development in the developing world than, for instance, GDP estimates based on national accounts (Henderson et al., 2012; Michalopoulos and Papaioannou, 2013, 2014). Furthermore, in addition to relatively high data quality, these data capture both the direct effects of increased investment at a subsidiary location, as well as indirect effects in the local area (for example, greater use of electricity by an increased number of residential housing units, increased vehicular traffic, etc.). Thus, these data permit estimation of the total local impact of U.K. firm presence in discrete African communities.

We obtain luminosity data from the United States Air Force Defense Meteorological Satellite Program (DMSP). DMSP collect satellite images of the planet's surface every night, permitting estimation based on a large number of data points. We download the annualized and processed data published by The National Geophysical Data Center (NGDC), which is purged of the effects of cloud coverage. The data are at the pixel-level; we construct 0.1×0.1 degree grid-cells from the pixel data to measure effects within a 10 km radius around each U.K. firm. Our tests use the annual cloud-free-observation-weighted average over all stable nightlight pixels. As with the Orbis data, we use data starting in 2005. However, we end the data used for these tests in 2013 due to the change in satellites in that year and the lack of concordance with the later imagery. Section 5.5.2 provides an example based on multinational presence in Kenya and includes a representative figure.

5.3.3 Employment data from the Demographic and Health Surveys (DHS)

We also study employment levels in the local area surrounding each U.K. African subsidiary, thereby capturing direct hiring by existing or new U.K. subsidiaries, as well as any local spillover effects resulting from increased economic activity in the area. We obtain data from the Demographic and Health Surveys (DHS), which are surveys of nationally representative, repeated cross sections of African citizens. The survey obtains information regarding respondents' demographic characteristics, health status, and employment. The data are collected in numbered areas drawn from census files (regions), where a random sample of individuals is selected from a census-provided list of households. Surveys occur in a random order of sampling clusters, which is an individual's neighborhood or village.

To construct a measure of employment, we follow Hjort and Poulsen (2019) and use responses to this specific question: "Aside from your own housework, have you done any work in the last seven days?" The data are reported for each individual respondent, which we aggregate to the sampling cluster level. As sampling clusters rarely appear in survey rounds both before and after the U.K. tax reform, we aggregate clusters into 10 × 10 km grid-cells using DHS-provided GPS coordinates so as to measure employment levels in the local area both pre- and post-change. Imposing the requirement that a grid-cell be observed at least once before and after the U.K. tax reform necessitates the use of some survey data from 1998.[14] We further restrict our sample of countries to those for which GPS coordinates are available so that we can link these data to the U.K. multinational subsidiary addresses. Table D.4 in the Appendix presents the survey years in each of the 22 countries with requisite DHS data. Section 5.5.3 provides an example of these data and a representative figure for an area in Kenya.

5.3.4 Merging Data on Firm Presence with Data on Local Economic Outcomes

We merge the DMSP satellite data and the DHS survey data to our sample of African companies by mapping the locations of DMSP satellite pixels and DHS survey respondents to U.K. firms' subsidiary addresses. First, we perform an internet search of the nearly 3,300 distinct U.K. African subsidiaries (excluding South Africa) to identify company addresses. We identify an address for 1,637 subsidiaries, or approximately

[14]For example, if a 10 km grid-cell in Kenya is surveyed in 2008, 2009, 2013, and 2014, we include all individual observations of this 10 km grid-cell into our analysis. However, if another 10 km grid-cell in Kenya is only surveyed in 2010 and 2014, we do not include observations of this 10 km grid-cell because there are no pre-period observations for that area.

50 percent of the (non-South African) U.K. subsidiaries.[15] This proportion appears reasonable given (i) the developing nature of the countries, (ii) the overall low level of internet presence by subsidiary companies of largely private firms, and (iii) the fact that some subsidiaries may not have a physical presence. Subsequent tests use French subsidiaries as a control sample, for which addresses were obtained in the same manner. That is, we search and then obtain addresses for the resulting 1,479 French subsidiaries (approximately 50 percent of the French sample). We then geo-code all addresses to obtain latitude and longitude coordinates.

Second, we retrieve GPS coordinates for all relevant pixels in the DMSP satellite data, as well as the latitude and longitude provided by DHS for each administered survey. Because the DHS surveys are administered in a central area of an individual's neighborhood or village, the GPS coordinates are in close proximity to a respondent's residence and, therefore, reflective of how close the survey respondent lives to the U.K. company.[16] Finally, we merge the location of the U.K. companies to both the satellite data and to survey respondents' addresses for our empirical tests.

5.3.5 Summary Statistics

Table 5.1 provides descriptive statistics about the sample. Recall that the 250,112 subsidiary-year observations are aggregated to the parent firm-African country-year level (e.g., Unilever-Kenya-2008), for a total of 103,670 firm-country-year observations. The median (average) parent firm has 1.00 (2.41) subsidiaries in an African country. Approximately 7.0 percent of this sample (7,100 observations) are U.K. parent firms. The median (average) parent firm has 13 (138.1) subsidiaries worldwide and operates in five (18.5) countries globally. Sample firms exhibit substantial heterogeneity with

[15]Three individual research assistants performed a manual internet search for each African subsidiary address using the name listed in the Orbis dataset. If a fuzzy name match is identified, the mailing address is then used to retrieve GPS coordinates. Two authors drew a random sample from the list of the 3,300 distinct U.K. subsidiaries and then verified the search results, thereby further ensuring that all available addresses have been correctly identified. We first conducted this search for all entities in countries other than South Africa to ensure completeness across the broadest set of jurisdictions; ongoing work is repeating this process for the large number of South African entities.

[16]The average village or neighborhood consists of 36.6 individuals, confirming the relatively small size of the area used in our tests. Across the 46 African countries in our sample, we observe 23,129 villages or neighborhoods.

respect to tax incentives that may affect the location of investment; the difference in tax rates between the highest and lowest taxed subsidiaries is 20.25 percent, and the average firm has 10.78 tax haven subsidiaries. The parents' home countries report relatively low inflation of 3.67 percent and have over $2.3 trillion in average GDP.

Table 5.1: Summary Statistics

Variable	Obs	Mean	P10	P25	Median	P75	P90	SD
Subsidiary Presence Variables (Firm-Country-Year)								
Log. Number Subsidiaries (in African-country)	103,670	0.41	0.00	0.00	0.00	0.69	1.39	0.70
Number Subsidiaries (in African-country)	103,670	2.41	1.00	1.00	1.00	2.00	4.00	7.33
UK Firm	103,670	0.07	0.00	0.00	0.00	0.00	0.00	0.25
Number Subsidiaries (worldwide)	103,670	138.13	2.00	2.00	13.00	94.00	398.00	472.20
Number Subsidiary Countries (worldwide)	103,670	18.45	1.00	2.00	5.00	25.00	59.00	27.04
Tax Differential within Firm (%)	103,670	20.25	0.00	0.00	15.50	35.00	55.00	19.03
Number Tax Haven Subsidiaries	103,670	10.78	0.00	0.00	0.00	5.00	25.00	56.68
UK Colonies	103,670	0.64	0.00	0.00	1.00	1.00	1.00	0.48
Consolidated Firm Variables (Firm-Country-Year)								
ETR	13,769	0.26	0.12	0.19	0.25	0.30	0.39	0.12
3y ETR	10,170	0.26	0.14	0.20	0.24	0.31	0.40	0.11
Home Country Controls (Firm-Country-Year)								
Population (in millions)	103,426	98.02	5.39	15.84	54.55	65.66	201.04	213.96
Inflation (%)	100,760	3.67	0.29	1.15	2.44	5.18	7.26	4.40
GDP (USD in trillions)	102,733	2.32	0.03	0.30	0.40	2.60	3.74	4.59
Nighttime Luminosity Analysis (Grid Cell-Year)								
Nighttime Luminosity (10km)	27,934	44.03	0.00	37.15	49.55	56.81	63.00	17.01
African Country Controls (Grid Cell-Year)								
GDP (USD in billions)	27,934	53.90	0.14	10.19	17.82	31.96	508.69	104.43
Population (in millions)	27,934	34.59	0.16	12.00	19.61	38.71	171.77	44.01
Local Employment (DHS) Analysis (Individual-Year)								
Employment (0/1)	610,064	0.64	0.00	0.00	1.00	1.00	1.00	0.48
U.K. Firm Presence (km)	610,064	117.05	2.43	9.11	60.05	156.09	329.69	149.76
Regional Employment	610,064	0.64	0.47	0.55	0.65	0.73	0.81	0.13
Male	610,064	0.28	0.00	0.00	0.00	1.00	1.00	0.45
Household Members	610,064	6.33	3.00	4.00	6.00	8.00	11.00	3.79
Age	610,064	28.99	17.00	20.00	27.00	36.00	44.00	10.26
Urban Region	610,064	0.54	0.00	0.00	1.00	1.00	1.00	0.50
Marital Status	602,870	0.96	0.00	0.00	1.00	1.00	2.00	1.09

Notes: This table presents descriptive statistics for the samples used in the empirical tests. We report the unit of observation for each group of variables in parentheses. For the luminosity analysis, grid-cells are defined as 0.1×0.1 decimal degrees, which is approximately 10×10 km. We construct equally-sized grid-cells for the nighttime luminosity analysis as well as the local DHS analysis.

We construct the luminosity sample by aggregating grid-cell data to the annual level for each of the U.K. and French subsidiaries for which address information was obtained (1,637 U.K. subsidiaries and 1,473 French subsidiaries). We obtain nine years of data (2005-2013) for the 3,110 locations with available luminosity data, resulting in a balanced panel of 27,934 subsidiary-year observations. The average 10 km radius grid-cell in which a multinational firm subsidiary is located exhibits a nighttime luminosity value of 44.03. This value, which is reported in the range between 0 (complete darkness) and 63 (maximum coding in the DMSP data), is higher, as expected, when compared to 7.7 around the relatively more remote extraction sites used in Christensen et al. (2022).

The sample used for the employment analysis includes 610,064 individual respondent-years, of which 157,587 relate to respondents within a 10 km radius of the U.K. subsidiary (untabulated). Approximately 64 percent of DHS survey respondents reported working outside the home (*Employment (0/1)*). The closest U.K. firm was on average 117 km from a DHS survey cluster. The average respondent lived in a household with 6.3 members and was 29 years old. Our empirical tests use varying radii around the survey cluster to compare employment status following an increased U.K. presence.

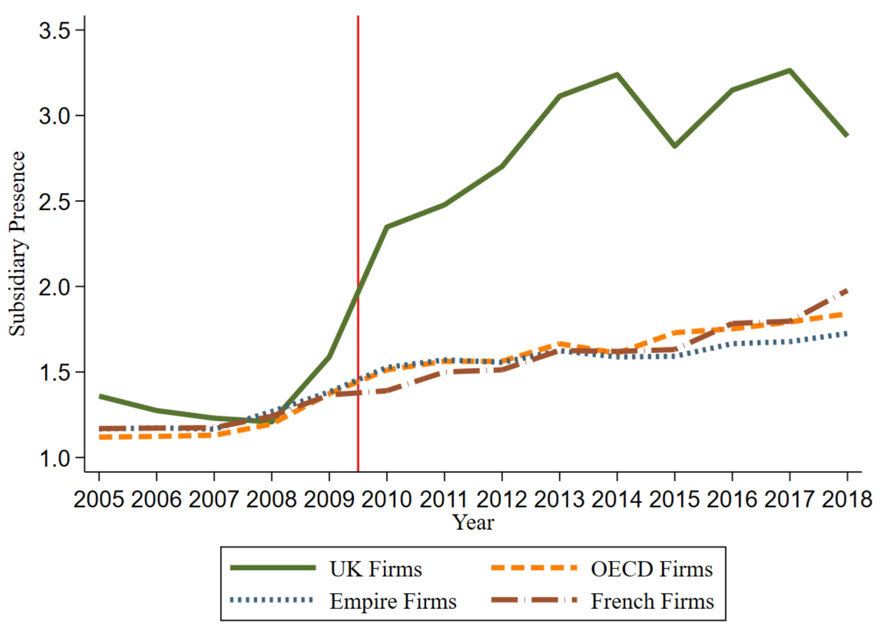
5.4 U.K. Tax Changes and Firm Presence in Africa

5.4.1 Graphical Evidence

Our first analyses examines the effect of the U.K. tax rate reduction on the total number of African subsidiaries owned by U.K. multinationals. Specifically, we analyze the relationship between the number of subsidiaries a firm owns in a given African country-year and whether the parent firm is incorporated in the U.K. Figure 5.2 graphically depicts the average number of U.K. African subsidiaries across the sample period (green solid line). The average number of subsidiaries in 2008 is 1.21, which increases to 2.35 in 2010, and further increases and peaks at 3.24 in 2014.

We compare U.K. multinational firm presence to that of three alternative groups of non-U.K. multinational firms that are likely to have similar incentives for sub-Saharan African investment. Figure 5.2 also presents three lines for each of these three groups. The orange dashed line represents multinational firms from other OECD countries. The tan dotted line captures multinationals from four other former colonial empires, including Belgium, France, Germany, Italy, Portugal, and Spain (Michalopoulos and Papaioannou, 2020). The maroon dash-dotted line captures French multinational firms only, who are most similar given their prominent and substantial African presence through the same period (Jeppesen and Smith, 2017; Fichter, 2019). From 2005 to 2009, the trends across the groups are relatively similar, but we observe that the OECD, Empire, and French firms report only a minor increase after 2009, consistent with firms growing their geographic presence over time and with relatively smaller tax cuts in some of these countries. The similarity in the lines prior to 2009 provides strong support for the parallel trends assumption key to the empirical strategy outlined below. The divergence in the green line after 2009 suggests an increased U.K. presence following the relatively large announced U.K. tax cuts.

Figure 5.2: U.K. Tax Cut and Multinational Firm Presence in Sub-Saharan Africa



Notes: This figure plots the mean number of subsidiaries owned by U.K. multinational firms (green solid line) in comparison to mean number of subsidiaries owned by multinational firms with parent entities incorporated in OECD countries (orange dashed line), multinational firms with parent entities incorporated in former colonial empire countries (blue dotted line), and French multinational firms (maroon dash-dotted line) in sub-Saharan African countries from 2005 to 2018. The mean number of subsidiaries is calculated on the multinational firm-African country-year level. The vertical line marks the major U.K. tax cut announcement.

5.4.2 Empirical Strategy

To estimate the effect of the U.K. tax rate reduction on U.K. multinationals' African presence, we estimate the following difference-in-differences model using OLS:

$$\begin{aligned} &Log.\,Number\ Subsidiaries_{f(i)c(i)t} = \\ &\qquad \alpha + \beta U.K.\,Firm_{f(i)} \times Post_t + \gamma Controls + \eta_{f(i)} + \zeta_{c(i)t} + \epsilon_{f(i)c(i)t} \end{aligned} \tag{5.1}$$

The dependent variable *Log. Number Subsidiaries* is the natural logarithm of the number of subsidiaries i a multinational firm f has in a given African nation c in year t. We estimate Equation 5.1 at the multinational firm-country-level ($n = 103,670$). *U.K. Firm* is an indicator variable equal to one if a firm f is incorporated in the U.K. and zero otherwise. *Post* is an indicator variable equal to one for years 2010 and later to capture the effects of the U.K. tax changes announced in the summer of that year.

We include two sets of control variables. The first set of variables controls for time-varying effects at the parent country level and includes inflation as well as the natural logarithms of population and GDP. The second set controls for time-varying differences in firms' multinational presence and includes the number of worldwide countries where the firm operates, as well as the tax differential and the natural logarithms of the total number of subsidiaries and subsidiaries in tax havens.[17] Including a measure of the tax differential controls for differences in tax rates within a multinational group and the corresponding investment incentives.

The nature of our data, where we have observations across many years, from many host countries, and from many industries, allows us to leverage fixed effect structures that help control for unobservable factors affecting investment in Africa. $\eta_{f(i)}$ denote firm fixed effects and control for time-invariant factors at the firm level, such as the general propensity to invest in an African country during the sample period. Country-year fixed effects, $\zeta_{c(i)t}$, control for macroeconomic shocks that affect all subsidiaries

[17]We cannot include subsidiary-level controls, as our dataset only provides the name, location, and (for a subset of observations) the date of incorporation of the subsidiary.

within a given African country across time. That is, these fixed effects control for regulatory and institutional changes in each African nation and allow for differential responses of African economies to shocks such as the global financial crisis in 2008. In some models, we also include industry-year fixed effects (defined at the firm level) that absorb average industry effects within a year (such as an oil price surge in 2008 affecting oil-reliant industries) and country-pair fixed effects that absorb non-time-varying characteristics between the country of the parent and the affiliate country (such as historical ties between the U.K. and former U.K. colonies).

5.4.3 Empirical Results - Firm Presence in Sub-Saharan African

5.4.3.1 Average Effects of the U.K. Tax Rate Change

Table 5.2 tabulates the results from estimating Equation 5.1. Based on the logarithmic transformation of the dependent variable, we interpret β as the percentage increase in the number of U.K. subsidiaries in Africa after U.K. tax reform. The comparison set of firm-country-year observations include those from all other non-U.K. firms in the sample; that is, it includes African affiliates of firms from 149 countries, including parents from the same or other African countries. In Table 5.2, the odd columns present results for the largest samples with requisite data. The even-numbered columns present results after imposing the additional sample restriction that the parent firm must be observed as owning at least one subsidiary within a country prior to 2010 (indicated by the label for "Balanced Presence"). Use of this sample mitigates concerns that the results are driven entirely by improving data coverage over time, as it requires a firm to be reporting in the African country prior to the U.K. tax change period. Thus, any new entity observed in those same countries post-reform can more confidently be attributed to the tax change as compared to coverage in the dataset.

The coefficient of 0.179 in Column (1) means that there is an approximately 17.9 percent increase in the number of U.K. multinational African subsidiaries after the announcement of the U.K. tax rate reduction, relative to the increase in the number of African subsidiaries owned by other firms. Estimation on alternative samples, with

Table 5.2: Domestic Tax Cuts and Foreign Subsidiary Presence in Sub-Saharan Africa

	(1)	(2)	(3)	(4)	(5)	(6)
Panel (A)	*Log. Number Subsidiaries*					
U.K. Firm	-0.117***	-0.117***				
	(0.03)	(0.03)				
U.K. Firm × *Post*	0.179***	0.226***	0.200***	0.208***	0.196***	0.240***
	(0.03)	(0.07)	(0.04)	(0.08)	(0.05)	(0.07)
Obs.	103,670	20,681	96,126	20,230	77,328	18,649
Adj. R2	0.013	0.098	0.546	0.690	0.660	0.795
Balanced Presence	No	Yes	No	Yes	No	Yes
Firm Controls	No	No	No	No	Yes	Yes
Firm Country Controls	No	No	No	No	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes	Yes
Country-Pair FE	No	No	No	No	Yes	Yes
Year FE	Yes	Yes	.	.	.	.
Industry-Year FE	No	No	No	No	Yes	Yes
Country-Year FE	No	No	Yes	Yes	Yes	Yes

Panel (B)	*Log. Number Subsidiaries*					
Control Group:	OECD Firms		Empire Firms		U.K. vs. FR Firms	
U.K. Firm × *Post*	0.217***	0.344***	0.198***	0.301***	0.171***	0.281***
	(0.04)	(0.07)	(0.04)	(0.07)	(0.05)	(0.08)
Obs.	49,980	12,644	27,059	7,826	14,633	5,335
Adj. R2	0.434	0.554	0.429	0.551	0.404	0.532
Balanced Presence	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports coefficient estimates of difference-in-differences specifications from Equation 5.1, which tests the effect of the U.K. tax cut on the natural logarithm of the number of a firm's subsidiaries in a given sub-Saharan country (*Log. Number Subsidiaries*) for the sample period 2005 to 2018. *U.K. Firm* is a binary variable equal to one if a multinational firm's parent entity is incorporated in the U.K. *Post* is a binary indicator equal to one for years after 2009. Panel (A) presents results using all firms in our sample, including African parent companies in the same country as the African subsidiary (African domestic groups), African parent companies in a different country from the African subsidiary (African multinationals), and all other multinational parent firms. Panel (B) presents results restricting the sample of control firms to multinational parent firms from OECD countries (Col. (1)-(2)), from former colonial empires (Belgium, France, Germany, Italy, Portugal, Spain in Col. (3)-(4)), and French multinational firms (Col. (5)-(6)). The models in odd-numbered columns include all firm-by-African country-year observations. The models in even-numbered columns only include firm-by-African country-year observations for firms that already had a subsidiary presence in a given African country in the pre-period (balanced presence) to mitigate concerns about improving data coverage over time. Firm data are from BvD Orbis. The list of control variables is displayed in Table 5.1. Standard errors are clustered at the firm level and presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

alternative fixed effects structures, and with differing control variables in Columns (2) through (6) produces results of a similar significance that provide a range of estimates. These coefficients suggest a 17.9 percent to 24.0 percent increase in the number of U.K. subsidiaries as compared to parent firms. Given that the average parent firm has 2.41 subsidiaries in an African country (from Table 5.1), this is equivalent to an increase of 0.43 - 0.58 subsidiaries per African country in which each U.K. firm operates.

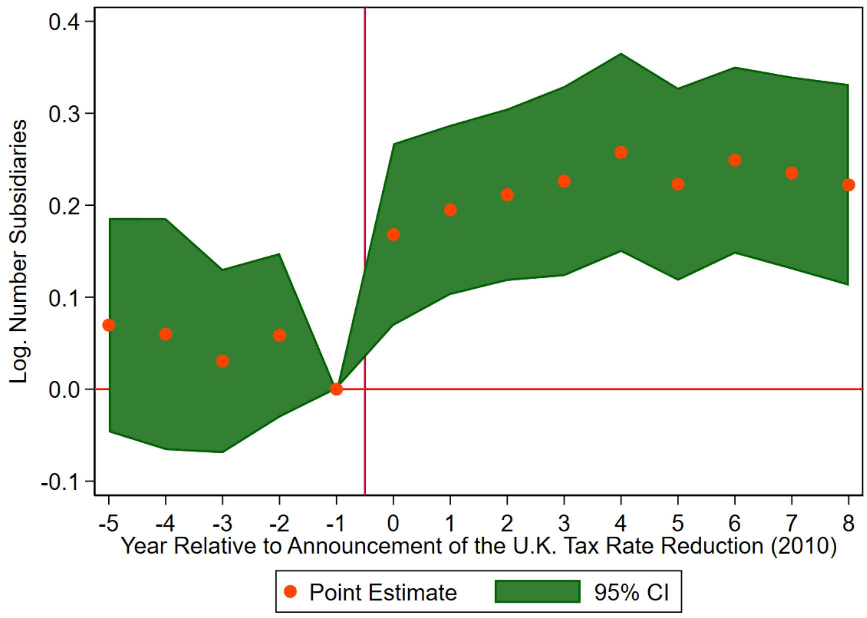
Figure 5.3 plots results of an event study test estimating annual treatment coefficients relative to 2009 as the base year before the reform ($t = -1$). The dots plot the point estimates that correspond to Table 5.2, Panel (A) Column 3, and the green shading provides the 95% confidence interval. The figure provides support for the validity of the common trends assumption given no statistically significant differences in years t-5 to t-1. The Figure also demonstrates an effect that begins one year following the announced U.K. tax rate reduction and persists throughout the sample period. The increasing effects reflect that the tax rate continued to decline later in the sample period.[18]

One concern is that the results in Panel (A) are due in part to comparing African subsidiaries of U.K. companies to subsidiaries of other multi-segment firms that may differ for unobservable reasons, thereby driving the increased U.K. presence after 2009. The inclusion of firm control variables, firm country control variables, and several fixed effects when estimating Equation 5.1 helps to mitigate this concern. However, we further address this by limiting the set of control observations to the three groups outlined above: i) multinationals from other OECD countries; ii) multinationals from other former colonial empires (Belgium, France, Germany, Italy, Portugal, and Spain); and iii) multinationals from France.

Table 5.2, Panel (B) presents robustness of results to using these three refined control

[18]Appendix Table D.5 presents results after including separate *Post* indicators for 2010-2011, 2012-2014, and after 2014. We also include an indicator for 2009, the last year preceding the announcement of the tax rate reduction, to evaluate any potential anticipatory effects. We find no anticipation effect in 2009, moderate and statistically significant effects in 2010-2011 and stronger effects in 2012-2014. After the full tax rate reduction is effective in 2015, the effect size remains stable at the highest level. These effect dynamics lend further support to our inference that the U.K. corporate tax rate reduction explains our findings of increased multinational firm investment in Africa.

Figure 5.3: Event Study Results for U.K. Presence in Sub-Saharan Africa



Notes: This figure displays the coefficient estimates and 95% confidence intervals for the event study regressions estimating the difference in the natural logarithm of the number of subsidiaries for U.K. firms as compared to non-U.K. multinational firms over time relative to the year preceding the U.K. tax cut announcement. The specifications are based on the model presented in Column (3) of Table 5.2 and include country-by-year and firm fixed effects. Standard errors are clustered at the firm level.

samples. We report results for two samples, one that includes all U.K. and control observations (corresponding to the sample from Panel (A), Column (3)), and one that includes all U.K. and control observations with an observed African presence prior to 2010 (corresponding to the sample from Panel (A), Column (4)). Across each of these tests, we continue to observe an increased U.K. affiliate presence, with the coefficients implying an increase of 17.1-21.7 percent in the odd columns and a larger 28.1-34.4 percent increase in the more restrictive samples presented in the even columns. These effects imply increases of 0.41-0.83 U.K. subsidiaries on average per U.K. firm-country relative to non-U.K. firms.[19]

[19]Table D.6, Panel (A) in the Appendix reports results that are robust to varying the fixed effects structure. In Appendix Table D.6 Panel (B), we use alternative control samples, including those with multinational firms from (i) all other foreign countries, (ii) the U.S., and (iii) Japan. Results remain qualitatively unchanged.

5.4.3.2 Heterogeneous Treatment Effects

Table 5.3 examines heterogeneous effects. We first study whether results vary based on a firm's geographic presence. Panel (A) presents results from partitioning the sample based on the colonial history of the African country in Columns (1) and (2), with the expectation that the results are likely to be concentrated in those countries that were previously British colonies.[20] We observe, as expected, that the results are strongest in those countries; furthermore, the 21.4 percentage point difference in coefficients across Columns (1) and (2) is statistically significant, confirming the greater increase in those jurisdictions. Columns (3) and (4) present results after refining the set of control observations to include only those of French multinationals. We continue to observe that the effects occur primarily in the U.K. colonies, with a 23.5 percentage point difference across the partitions. Untabulated tests show a similar, but slightly weaker, pattern of results when partitioning based on the extent of the pre-2010 U.K. presence.

We further study how the results vary based on industry in Panel (B). We first study the Manufacturing and Construction industries, given that they are generally more labor-intensive and have the greatest potential for job growth and spillover effects. We indeed find increased investment by manufacturing firms, with the coefficient implying a 23.4 percent increase in foreign manufacturing subsidiaries. We observe a weaker effect in the Construction industry (t-statistic of 1.56), which may be due in part to the relatively smaller sample. Finally, we study whether the effects occur in the Mining and Quarrying sectors. We examine this sector because resource extraction is a key industry studied extensively in the prior literature and therefore, we want to assess the extent to which this industry drives the documented effects. We find no evidence that the U.K. tax cut affected investment in this industry based on the coefficient estimate in Column (3) ($t = 0.38$). Thus, the phenomena documented in this manuscript extend

[20]Countries in our dataset with an U.K. colonial history are: Botswana, Eswatini, Gambia, Ghana, Kenya, Lesotho, Malawi, Nigeria, Sierra Leone, Somalia, South Africa, South Sudan, Sudan, Uganda, Zambia, and Zimbabwe. The sample is slightly smaller as compared to Table 5.2, Panel (A) due to the loss of singleton observations during estimation.

Table 5.3: Domestic Tax Cuts and Foreign Subsidiary Presence by African Country Characteristics and Industry

Panel (A)	(1)		(2)	(3)		(4)
	Log. Number Subsidiaries					
	Full Sample			U.K. vs. FR Firms		
	U.K. Colonies		Non-U.K. colonies	U.K. Colonies		Non-U.K. colonies
U.K. Firm × *Post*	0.274***		0.060	0.297***		0.063
	(0.06)		(0.06)	(0.07)		(0.06)
Difference		0.214***			0.235***	
		(0.08)			(0.08)	
Obs.	60,568		34,903	6,927		7,630
Adj. R2	0.604		0.499	0.477		0.378
Firm FE	Yes		Yes	Yes		Yes
Country-Year FE	Yes		Yes	Yes		Yes

Panel (B)	(1)	(2)	(3)
	Log. Number Subsidiaries		
	Manufacturing	Construction	Mining & Quarrying
U.K. Firm × *Post*	0.234***	0.250	0.057
	(0.07)	(0.16)	(0.15)
Difference vs. (1)		0.016	-0.176
		(0.17)	(0.16)
Obs.	20,204	3,005	4,932
Adj. R2	0.514	0.504	0.480
Firm FE	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes

Notes: This table reports coefficient estimates of difference-in-differences specifications from Equation 5.1, which tests the effect of the U.K. tax cut on the natural logarithm of the number of subsidiaries of a firm in a given sub-Saharan country (*Log. Number Subsidiaries*) for the sample period 2005 to 2018. *U.K. Firm* is a binary variable equal to one if a multinational firm's parent entity is incorporated in U.K. *Post* is a binary indicator equal to one for years after 2009. In Panel (A), the sample is partitioned at the sub-Saharan country level based on whether the subsidiary is located in a fomer U.K. sub-Saharan colony or not. Columns (1) and (2) present results using all parent firms in our sample. Columns (3) and (4) present results restricting the sample to French and U.K. multinational parent firms. Panel (B) presents estimates for partitioning the sample into different industry categories based on NACE Rev. 2 sections, where *Manufacturing* refers to section "C", *Construction* to section "F", and *Mining & Quarrying* to sections "B". Standard errors are clustered at the firm level and presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

beyond the resource extraction activity studied in prior work.

A key identifying assumption is that the tax policy changes were the primary economic event occurring in the U.K. during this sample period. To further validate that the effects can be attributed to the U.K. tax cut, we also examine heterogeneous effects related to the U.K. parent's tax position. Table 5.4 presents results after partitioning

Table 5.4: Domestic Tax Cuts and Foreign Subsidiary Presence by Firm Characteristics

	(1)	(2)	(3)	(4)
	Log. Number Subsidiaries			
	Pre-period Effective Tax Rate		Pre-three-year Effective Tax Rate	
	High	Low	High	Low
U.K. Firm × *Post*	0.218**	0.095	0.301**	0.132
	(0.09)	(0.09)	(0.12)	(0.12)
Difference	0.123		0.169	
	(0.13)		(0.17)	
Obs.	7,998	7,531	4,684	5,309
Adj. R2	0.556	0.528	0.570	0.556
Firm FE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes

Notes: This table reports coefficient estimates of difference-in-differences specifications from Equation 5.1, which tests the effect of the U.K. tax cut on the natural logarithm of the number of subsidiaries of a firm in a given sub-Saharan country (*Log. Number Subsidiaries*) for the sample period 2005 to 2018. *U.K. Firm* is a binary variable equal to one if a multinational firm's parent entity is incorporated in U.K. *Post* is a binary indicator equal to one for years after 2009. In columns (1) and (2), the sample is partitioned at the multinational firm level based on the median consolidated effective tax rates (ETRs) in 2009. In columns (3) and (4), the sample is partitioned based on the median of multinational firms' three-year (2007-2009) average consolidated effective tax rate. Standard errors are clustered at the firm level and presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

the sample based on firms' effective tax rates prior to the U.K. tax rate reduction, which reflects companies' effective tax position after taking into account both the U.K. statutory tax rate as well as certain tax incentives or planning strategies. U.K. parent firms with the highest effective tax rates prior to the tax rate reduction are presumably those that can benefit the most from the U.K. change and thus should have the greatest tax savings and the largest competitive gains for investment purposes. The sample size for this test is much smaller, given the requirement to observe consolidated financial statements, tax expense, and positive pre-tax income in the pre- and post-reform period. We observe that the effects appear strongest in the firms with relatively higher effective tax rates pre-reform; the coefficient of 0.218 in Column (1) means that those firms had a 21.8 percent increase in subsidiary presence after the reform. In contrast, those firms with lower effective tax rates had no statistically significant change (Column (2)). We find a similar pattern of results in Columns (3) and (4), which partition on the three year average effective tax rate that minimizes annual fluctuations introduced by accounting rules (Dyreng et al., 2008). This suggests that the results appear strongest in those firms that could benefit the most from the U.K. corporate tax change. However, while the coefficients for the high-ETR firms appear approximately twice the size of the coefficients for the lower- ETR firms, we note that the coefficients are not statistically different.

Collectively, the results in Tables 5.2 through 5.4 provide evidence that U.K. presence in Africa increased after 2010, that such increase is robust to controlling for the (potentially) endogenous decision by multinational firms to begin operating in a given country, and that it is also robust to use of varying control samples. We observe heterogeneity in predictable subsamples, providing further evidence to support the inferences about the effects of the U.K. tax rate reductions on African investment.

5.5 U.K. Tax Cuts, Firm Presence, and Net Economic Activity in Africa

5.5.1 Empirical Strategy

We next analyze the effects of the increased U.K. expansion on local economic activity in Africa. Using the nighttime luminosity and DHS survey employment data, we estimate the following model using OLS:

$$Local\ Economic\ Activity_{g(i)c(i)t} = \alpha + \beta U.K.FirmPresence_{g(i)} \times Post_t + \gamma Controls + \delta_{g(i)} \times U.K.FirmPresence_{g(i)} + \zeta_{c(i)t} + \epsilon_{g(i)c(i)t} \quad (5.2)$$

The dependent variable *Local Economic Activity* captures either the average annual nighttime luminosity or employment status of a surveyed African individual. When measuring nighttime luminosity, we use *Log. Luminosity*, the natural logarithm of average annual nighttime luminosity of grid-cell $g(i)$ with firm i in its center as the dependent variable. The model is estimated at the grid-cell level in a given country-year (i.e., $i = g$). The treatment indicator *U.K. Firm Presence* is equal to one if a grid-cell hosts an U.K. multinational firm subsidiary based on our manual search for addresses and GPS coordinates of U.K.-owned subsidiaries.[21] We use two distinct control groups, both of which include the same-sized grid-cell observations without any U.K. firm presence. The first control group consists of nearest-neighbor matched grid-cells within the same country and with similar average luminosity values (within ± 20% range of the treated cell in 2009), but we only permit matches that are at least 10 km away from the next U.K.-owned subsidiary. That is, we measure luminosity effects in the 10 km grid-cell around each U.K. subsidiary as compared to other grid-cells within the same country and with a similar preceding luminosity level. The second control group includes only those grid-cells in the same country that have French subsidiaries. *Post* is an indicator variable equal to one for years after 2009. Control variables include the natural logarithm of the African country's population and GDP (*Log. Population* and *Log. GDP*) as measures of the country's economic activity. All models include grid-cell fixed effects; some models also include country-year fixed effects, $\zeta_{c(i)t}$, which absorb the time-varying country-level control variables and capture other changes in economic activity over time for each country. We cluster standard errors at the grid-cell level. We expect that, if the U.K. corporate tax cut motivates foreign direct investment into Africa, we should observe greater light emission attributable to both direct and spillover effects. That is, not only does this test capture any new affiliates over the sample period, but it also captures increased investment activity at pre-existing affiliates. We predict that β will be positive. However, we may observe little benefit if the subsidiary produces little real substantive economic activity that drives increased luminosity.

When measuring employment with the DHS survey data, the dependent variable is *Employment (0/1)*, which is an indicator equal to one for each survey respondent i in grid-cell $g(i)$ that is employed, and zero otherwise, in year t. We estimate the model at the

[21]The data introduce two empirical challenges. First, because we manually search for addresses and match those to luminosity data, we are not able to identify addresses or change in addresses over time and thus must rely on the current address returned. Second, because of the necessity of having a U.K. address, the sample used for this test is substantially diminished.

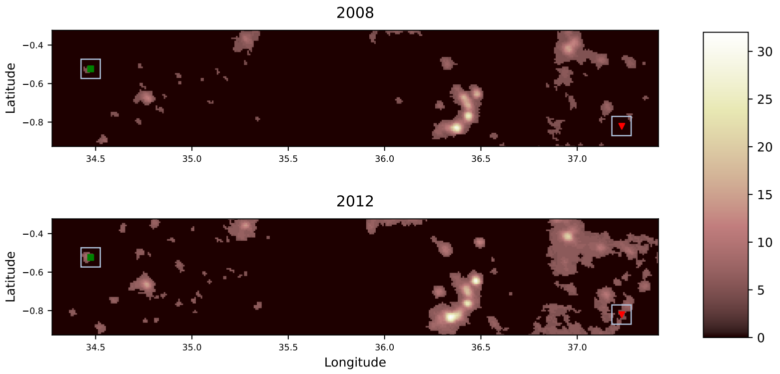
surveyed individual level in year t, where grid-cell g can include multiple respondents. The treatment indicator *U.K. Firm Presence* is equal to one if the residence of the survey respondent is equal or less than 10 km away from the closest U.K. subsidiary in the same country. This design assumes that individuals who live closer to the multinational subsidiary will be more likely to be employed by that subsidiary, or by other companies in the area that benefit from the U.K. multinational presence. *Post* is as defined above. To further ensure a high comparability of treated and control individuals when studying employment levels, we control for the average employment rate in a given region (i.e., survey cluster) as last measured before the U.K. tax reform. Some specifications also include additional demographic control variables obtained from the DHS data, such as *Male*, *Household Members*, *Marital Status*, *Age*, and whether the geographic area is in an *Urban Region*. Consistent with the approach in Hjort and Poulsen (2019), we include 10 km grid-cell fixed effects interacted with the treatment indicator, $\delta_{g(i)} \times U.K.FirmPresence_{g(i)}$, which control for any time-invariant differences at the local level that may be correlated with U.K. multinational firm subsidiary presence. We again use two distinct control groups, both of which are grid-cell observations with *U.K. Firm Presence* equal to zero. The first control group consists of individual respondents living in the same country but relatively further away from the U.K. subsidiary, using distances of 10 and 50 km. The second control group consists of individuals living at least 10 (or 50) km away from the next U.K.-owned subsidiary but equal or less than 25 (or 50) km away from a next French multinational firm subsidiary. We cluster standard errors at the enumeration area. As above, we predict that β will be positive if the U.K. corporate tax rate reduction motivates direct or spillover employment effects.

5.5.2 Empirical Results - Economic Activity Measured with Night-time Luminosity

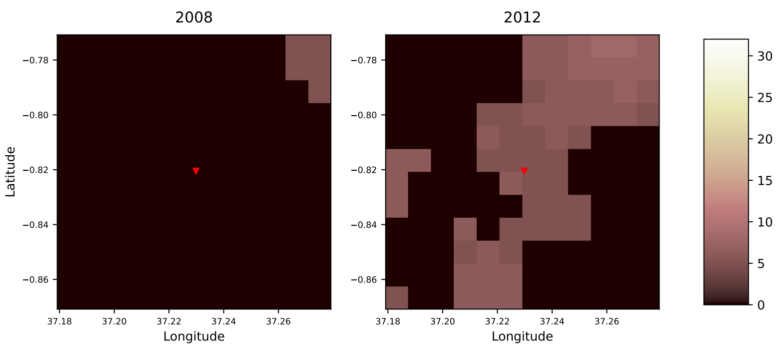
Figure 5.4 provides an example of the geo-coded luminosity data that depicts the effects we study formally when estimating Equation 5.2. The example presents the luminosity data in an area in Kenya with both U.K. and French multinational presence. Panel (A)

Figure 5.4: Example of Nighttime Luminosity around Multinational Firm Presence in Africa

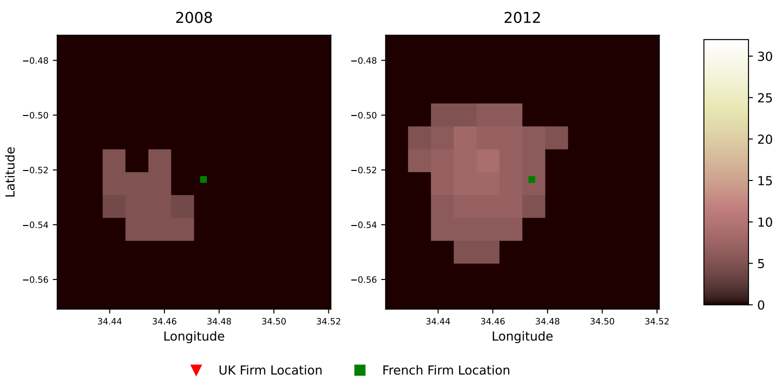
(A) Area in Kenya with U.K. and French Multinational Firm Presence



(B) Pixel-level Luminosity within 10km Grid-cell of U.K. Firm Location



(C) Pixel-level Luminosity within 10km Grid-cell of French Firm Location



Notes: This figure shows nighttime luminosity around the locations of a U.K. multinational firm subsidiary (red triangles) and French multinational firm subsidiary (green squares) in Kenya. Panel (A) presents the geographic distribution of pixel-level luminosity in 2008 and 2012. Panel (B) presents the pixel-level luminosity in the 10 km grid-cell around the U.K. firm in 2008 and 2012. Panel (C) presents the pixel-level nighttime luminosity in the 10 km grid-cell around the French firm in 2008 and 2012.

compares the location of the U.K. and French subsidiaries in 2008 to the same location in 2012. The red triangle (green dot) denotes the U.K. (French) subsidiary. A comparison of the two planes in Panel (A) demonstrates a marked increase in the luminosity pixels around the U.K. affiliate, with little change around the French subsidiary. Panels (B) and (C) show this effect in more detail by isolating the 10 km grid-cell around the U.K. and French locations, respectively. While there is some increased luminosity around the French affiliate, the change near the U.K. subsidiary appears much more substantial.

Table 5.5 reports formal regression results from estimating the effects of U.K. tax cuts on nighttime luminosity. We use two samples to do so. The first sample comparing grid-cells around U.K. subsidiaries to grid-cells further away - but in the same country - that lack a U.K. presence. Specifically, we match the grid-cells containing the 1,637 U.K. subsidiaries (with requisite address information) to other grid cells in the same country that had a similar level of luminosity (within 20 percent) as of the year preceding the tax rate change announcement. Because we require that the matched cells have similar levels of luminosity and be in the same country (but do not have a U.K. subsidiary), we retain 545 matched pairs. We then obtain luminosity data for these matched pairs, resulting in 9,810 subsidiary-year observations ($545 \times 2 \times 9$). Column (1) of Panel (A) reports results for this sample. We observe a 4.5 percent increase in luminosity in the local area surrounding U.K. subsidiaries in the years following the U.K. tax reform, controlling for the African country's population and overall level of economic activity. In Column (2), we replace the year fixed effects with country-year fixed effects to further absorb time-varying country characteristics and find that the point estimate is unchanged. Finally, Column (3) shows results with country-year fixed effects and grid-cell-by-U.K. firm presence fixed effects, explicitly controlling for any time-invariant differences at the local level that may be correlated with U.K. firm presence. The size and significance of the coefficient is again unchanged.

One concern with using a control sample of grid-cells that lack a U.K. subsidiary is that the tests effectively compare grid-cells in more industrialized areas that may naturally attract foreign presence to grid-cells in less industrialized areas, thereby

Table 5.5: Multinational Firm Presence and Local Economic Activity

	(1)	(2)	(3)
Panel (A)	*Log. Nighttime Luminosity (10km)*		
	U.K. Firm Presence within 10km		
U.K. Firm × Post	0.045***	0.045***	0.045***
	(0.02)	(0.01)	(0.01)
Log. GDP	0.037		
	(0.04)		
Log. Population	-0.898***		
	(0.34)		
Obs.	9,810	9,810	9,810
Adj. R2	0.983	0.987	0.987
Grid-cell FE	Yes	Yes	.
Grid-cell FE × U.K. Firm Presence	No	No	Yes
Year FE	Yes	.	.
Country-Year FE	No	Yes	Yes

	(1)	(2)	(3)
Panel (B)	*Log. Nighttime Luminosity (10km)*		
	U.K. Firm Presence vs. French Firm Presence		
U.K. Firm × Post	0.022***	0.026***	0.023**
	(0.01)	(0.01)	(0.01)
Log. GDP	0.038***		
	(0.01)		
Log. Population	1.176***		
	(0.14)		
Obs.	27,934	23,220	27,927
Adj. R2	0.978	0.981	0.982
Grid-cell FE	Yes	Yes	.
Grid-cell FE × U.K. Firm Presence	No	No	Yes
Industry FE	No	Yes	No
Year FE	Yes	.	.
Country-Year FE	No	Yes	Yes

Notes: This table reports coefficient estimates of difference-in-differences specifications from Equation 5.2, which tests the effect of the U.K. tax rate reduction on local economic activity measured with nighttime luminosity in sub-Saharan Africa. The unit of observation is a luminosity grid-cell with a 10 km radius in a given country-year. *U.K. Firm Presence* is equal to one if a grid-cell contains a U.K. subsidiary. *Post* is a binary indicator equal to one for years after 2009. In Panel (A), we match 10 km radius grid-cells centered around U.K. multinational firm presence to control grid-cells within the same country that are at least 10 km away from the next U.K.-owned subsidiary and that exhibit an average luminosity value within a ±20% range of the treated observation in the pre-period (2009). In Panel (B), control observations are 10 km radius grid-cells centered around a French multinational subsidiary. Luminosity data are obtained from the United States Air Force Defense Meteorological Satellite Program (DMSP), and we use the annualized, processed (cloud-free-observation-weighted) average over all stable nightlight pixels from The National Geophysical Data Center (NGDC) from 2005 to 2013. The dependent variable *Log. Luminosity* is the natural logarithm of the annualized mean luminosity value. Robust standard errors are clustered at the grid-cell level and presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

biasing the results. We address this concern in Panel (B), which presents results for measuring luminosity using a control sample of grid-cells with a centered French subsidiary. Specifically, we include grid-cells containing all 1,637 U.K. subsidiaries, as well as 1,473 grid-cells containing French subsidiaries, resulting in a total sample of 27,934 subsidiary-year observations after merging with the available luminosity data. We continue to observe that areas surrounding U.K. subsidiaries exhibit greater luminosity following U.K. tax cuts when using this sample in Panel (B). Although the size of the coefficients declines, the results continue to demonstrate a notable effect. Specifically, the coefficients imply a 2.2-2.6 percent increase in luminosity for U.K. subsidiaries as compared to any increase in French presence over the time period.

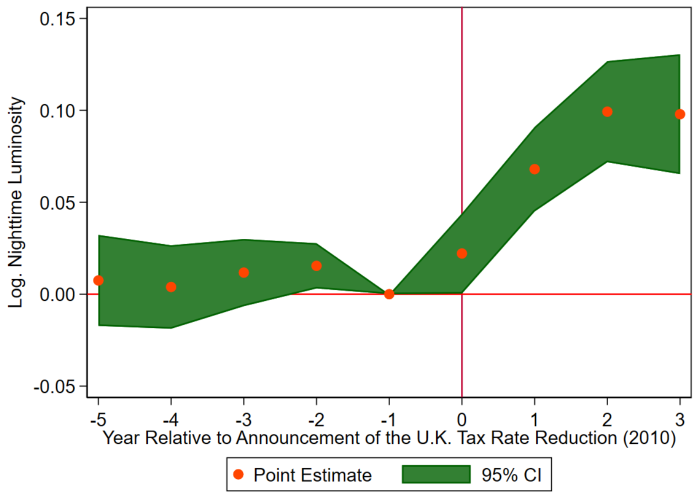
Figures 5.5 and 5.6 provide further evidence of the documented effects. Figure 5.5 plots results of an event study test estimating annual treatment coefficients with 2009 as the base year. Point estimates for each year with luminosity around U.K. subsidiaries as the outcome are presented in dots; 95% confidence intervals are shaded in green. Panel (A) plots results that correspond to Table 5.5 Panel A, which use other within-country grid-cells with similar pre-reform luminosity values as control observations; Panel (B) plots effects relative to grid-cells with a French multinational presence. Both figures demonstrate a clear change in luminosity in the years following the U.K. tax change, which captures both the increased number of U.K. affiliates (captured in our original tests using the Orbis data) as well as additional investments in pre-existing U.K. companies. Figure 5.6 further shows how effects vary based on differing radii around each location (1 km – 100 km). As expected, the effect of U.K. firm presence on local economic activity is robust across small radii and dissipates with increasing radii values.

5.5.3 Empirical Results - Local Employment

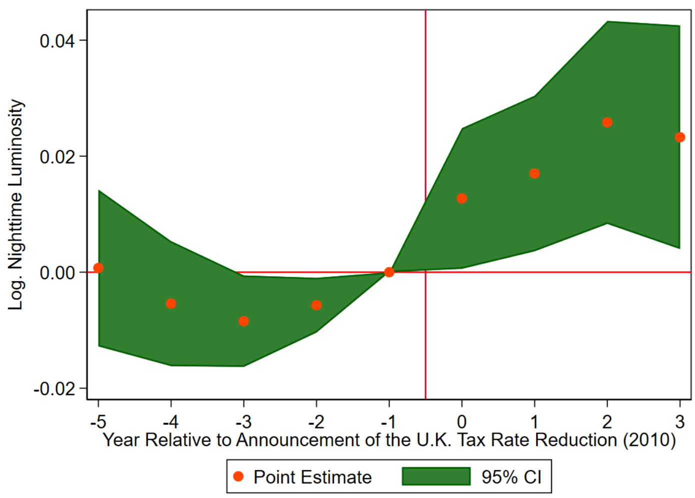
We next test whether employment increased in the local area surrounding U.K. subsidiaries after 2009. Figure 5.7 graphically depicts the DHS data used in this test. Panel (A) plots the DHS survey clusters in blue across sub-Saharan Africa. The map also

Figure 5.5: Event Study Results for the Effect of U.K. Presence in Sub-Saharan Africa on Local Economic Activity

(A) U.K. Presence within 10 km Radius Grid-cells vs. Placebo Regions

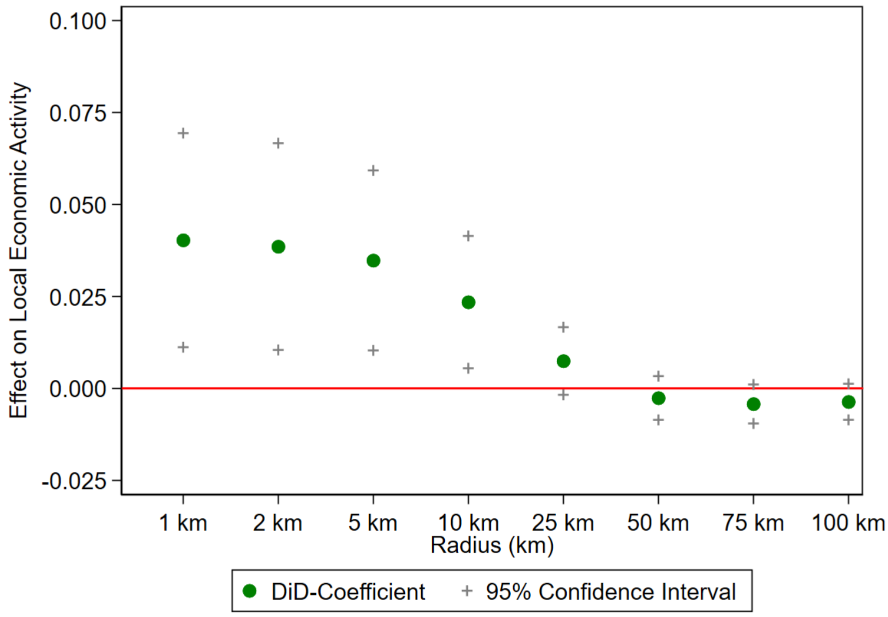


(B) U.K. Presence within 10 km Radius Grid-cells vs. French Presence within 10 km Radius Grid-cells



Notes: This figure displays the coefficient estimates and 95% confidence intervals for the event study regressions estimating the difference in the natural logarithm of average nighttime luminosity between 10 km radius grid-cells with centered U.K. multinational firm presence as compared to those without U.K. multinational firm presence over time. Effects are plotted relative to 2009, which is the year preceding the announcement of the U.K. tax rate reduction. In Panel (A), we match 10 km radius grid-cells with centered U.K. multinational firm presence to control grid-cells within the same country that are at least 10 km away from the next U.K.-owned subsidiary and that exhibited average luminosity values within a ±20% range of the treated observation in 2009. In Panel (B), control observations are 10 km radius grid-cells centered around French multinational firm presence. The specifications include country-by-year and grid-cell fixed effects, and standard errors are clustered at the grid-cell level.

Figure 5.6: Treatment Effects of Multinational Firm Presence on Local Economic Activity by Luminosity Grid-cell Size

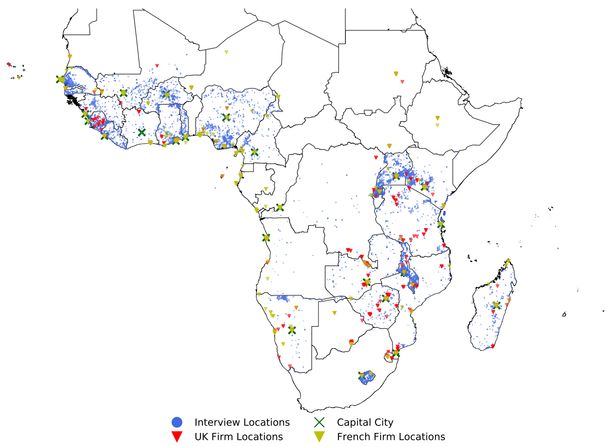


Notes: This figure plots coefficient estimates of *U.K. Firm* × *Post* from Equation 5.2 and 95% confidence intervals that correspond to results presented Table 5.5, Column (2). The dependent variable *Log. Luminosity* reflects varying grid-cell radii of 1 km, 2 km, 5 km, 10 km, 25 km, 50 km, 75 km, and 100 km. Treated observations refer to grid-cells with centered U.K. multinational firm presence, while control observations are grid-cells with centered French multinational firm presence. The specifications include country-by-year and grid-cell fixed effects. Standard errors are clustered at the grid-cell level.

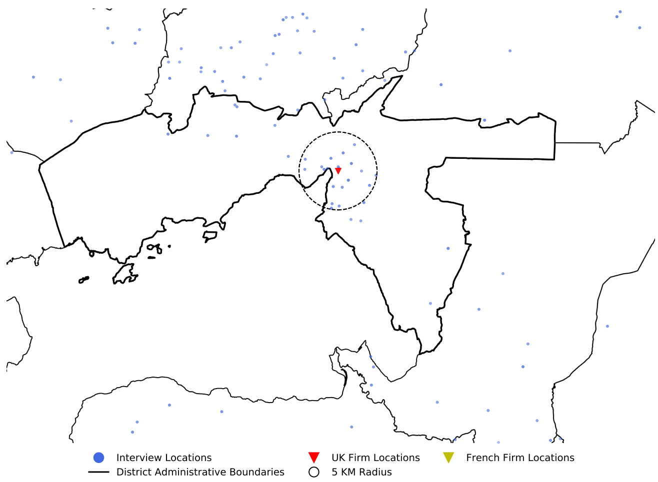
plots the location of U.K. and French multinational firm subsidiaries in red and green, respectively. We observe the most U.K.-owned subsidiaries in the west African countries of Nigeria, Ghana, and Sierra Leone, as well as in the east African countries of Kenya, Tanzania, Mozambique, and Zimbabwe. The figure shows that there is sufficient overlap between the countries with a U.K. presence and the countries where DHS surveys are conducted. Panel (B) presents a more detailed depiction of one particular region, the Kisumu Area in Kenya. The U.K. subsidiary is marked with the red triangle, and the dashed circle shows the 5 km radius for the treated area around this U.K. subsidiary. Each of the blue dots denotes a DHS interview location from which employment status is measured. Our empirical tests compare individuals surveyed within areas with a U.K. subsidiary to those individuals surveyed outside of the area, and to similar respondents in the same area prior to the tax rate reduction. As with the luminosity data, these tests capture both the effects in the pre-existing affiliates, as well as any new affiliates post-U.K. change (such as those tested using the Orbis data).

Figure 5.7: Map of DHS Survey Locations and U.K. and French Subsidiaries in Sub-Saharan Africa

(A) Locations throughout Sub-Saharan Africa



(B) Example of DHS Survey Clusters and U.K. and French Subsidiaries in the Kisumu Area (Kenya)



Notes: This figure shows the locations of DHS interviews (blue dots) and U.K. or French multinational firm subsidiaries (red or green triangles), respectively. Panel (A) presents the geographic distributions across all countries in sub-Saharan African. Panel (B) presents the geographic distributions in Kenya. Panel (B) also presents a 5 km radius with a dotted line around the U.K. multinational firm subsidiary located in Kisumu.

Table 5.6 reports results from estimating Equation 5.2 for employment outcomes. As the employment outcome at the surveyed individual level is binary, the coefficient estimates can be interpreted as percentage point changes in the probability of being employed. Column (1) estimates that individuals living close to a U.K. multinational subsidiary after the tax rate cut (*U.K. Firm Presence* × *Post*) were 3.2 percentage points more likely to be employed as compared to individuals living in grid-cells without a U.K. firm presence. Compared to a baseline employment rate of 64 percent, this is a substantial change, implying a 5 percent increase in the likelihood of having worked outside the home. We find similar effects in Column (2) after including control variables and country-year fixed effects, as well as in Columns (3) and (4) when altering the control group to include respondents within a 50 km radius. Untabulated analyses further confirms that the effects appear to be incremental employment, as we observe no decline in employment in the control regions that would otherwise be suggestive of employees switching from one firm to the next.[22]

Table 5.6, Panel (B) reports results using respondents living close to French-owned subsidiary locations as the control group. Use of this control group again ensures we are not mechanically comparing changes in employment in more versus less industrialized areas. Across all specifications, we document a 1.8 to 2.6 percentage point increase in employment for individuals living close to a U.K.-owned subsidiary as compared to those living close to a French-owned subsidiary. Again using the 64 percent baseline employment rate, this implies a change of 2.8-4.1 percent.

Figure 5.8 presents these effects graphically using five different radii (1 km, 2 km, 5 km, 10 km, and 25 km). For each control group, we continue to observe a positive but decreasing effect as the radii increases and the average respondent's distance to the subsidiary increases. In Panel (A), even at 25 km, Figure 5.8 still shows a positive employment effect that is statistically different from zero.

[22]One concern is that large metropolitan areas are the most likely destination for a U.K. multinational firm subsidiary and that, over this sample period, individuals living in large metropolitan areas were more likely to become employed. Explicitly controlling for *Urban Region* and including grid-cell fixed effects helps to mitigate these concerns.

Table 5.6: Multinational Firm Presence in Sub-Saharan Africa and Local Employment

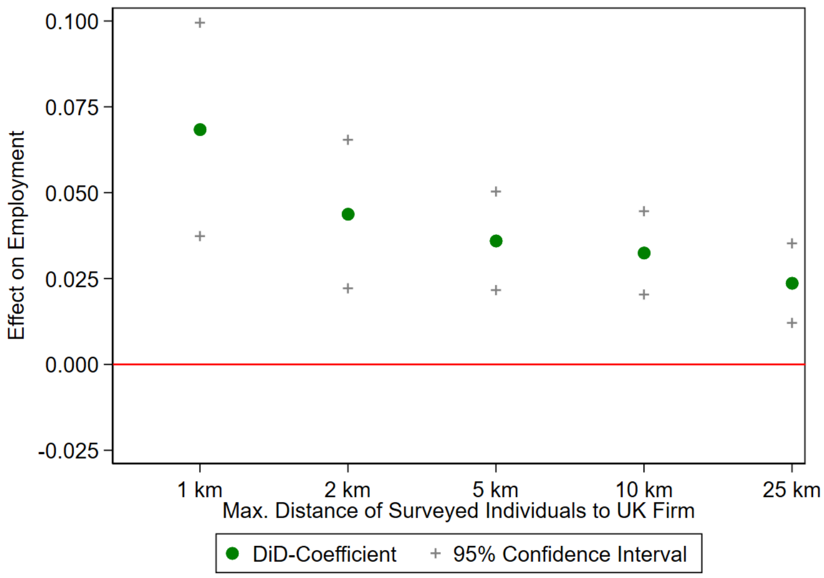
	(1)	(2)	(3)	(4)
Panel (A)	*Employment (0/1)*			
	U.K. Firm Presence vs. Non-U.K. Presence			
	Control Group > 10km		Control Group > 50km	
U.K. Firm Presence × Post	0.032***	0.027***	0.036***	0.030***
	(0.006)	(0.006)	(0.007)	(0.006)
Regional Employment (Pre-period)	0.193***	0.167***	0.195***	0.180***
	(0.046)	(0.047)	(0.053)	(0.054)
Male		0.134***		0.140***
		(0.004)		(0.005)
Household Members		-0.006***		-0.006***
		(0.000)		(0.000)
Marital Status		0.039***		0.039***
		(0.001)		(0.001)
Age		0.014***		0.014***
		(0.000)		(0.000)
Urban Region		-0.018***		-0.009*
		(0.005)		(0.006)
Obs.	610,064	602,870	485,878	480,592
Adj. R2	0.079	0.211	0.078	0.212
Grid-cell FE × U.K. Firm Presence	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes

	(1)	(2)	(3)	(4)
Panel (B)	*Employment (0/1)*			
	U.K. Firm Presence vs. French Firm Presence			
	French Firm ≤ 25km		French Firm ≤ 50km	
U.K. Firm Presence × Post	0.026**	0.023**	0.021**	0.018**
	(0.012)	(0.011)	(0.009)	(0.009)
Regional Employment (Pre-period)	0.253***	0.194**	0.232***	0.165**
	(0.072)	(0.078)	(0.064)	(0.069)
Male		0.130***		0.126***
		(0.009)		(0.008)
Household Members		-0.006***		-0.007***
		(0.001)		(0.001)
Marital Status		0.035***		0.035***
		(0.001)		(0.001)
Age		0.016***		0.015***
		(0.000)		(0.000)
Urban Region		-0.028***		-0.032***
		(0.010)		(0.008)
Obs.	198,776	197,021	245,152	243,147
Adj. R2	0.033	0.194	0.045	0.199
Grid-cell FE × U.K. Firm Presence	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes

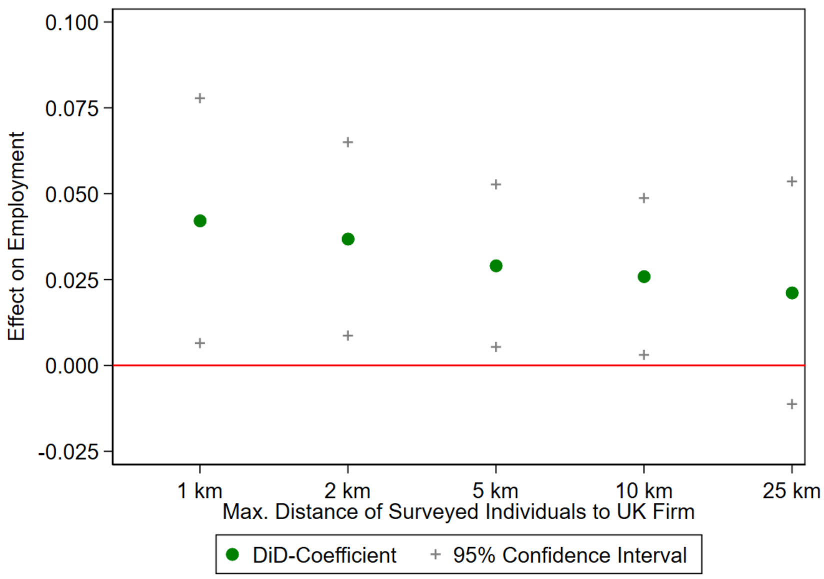
Notes: This table reports coefficient estimates of difference-in-differences specifications from Equation 5.2, which tests the effect of the U.K. tax rate reduction on local employment in sub-Saharan Africa from 1998-2019. The unit of analysis is the surveyed individual. *U.K. Firm Presence* is equal to one if a U.K. firm's subsidiary is located within a 10 km radius of a survey respondent's residence. *Post* is a binary indicator equal to one for years after 2009. Individual-level data are obtained from Demographic and Health Surveys (DHS). We use DHS data for regions that had survey rounds both before and after the U.K. tax rate reduction. The dependent variable *Employment (0/1)* is an indicator equal to one if an individual indicates that he/she is employed following Hjort and Poulsen (2019). In Panel (A), the control group consists of individuals living more than 10 or 50 km away from the nearest U.K.-owned subsidiary. In Panel (B), the control group consists of individuals living within a 25 km or 50 km radius of a French-owned subsidiary and not living within a 10 km radius of a U.K.-owned subsidiary. Robust standard errors are clustered at the grid-cell level and presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Figure 5.8: Treatment Effects of Multinational Firm Presence on Local Employment by Grid-cell Size

(A) U.K. Firm Presence vs. Non-U.K. Firm Presence



(B) U.K. Firm Presence vs. French Firm Presence



Notes: This figure plots coefficient estimates of *U.K. Firm* × *Post* from Equation 5.2 and 95% confidence intervals that correspond to results presented in Table 5.6, Column (6). We estimate the effect on the outcome variable *Employment* for varying distances of 1 km, 2 km, 5 km, 10 km, and 25 km between the surveyed individual and the closest U.K.-owned subsidiary. In Panel (A), control observations are surveyed individuals at least 10 km or 25 km away from the next U.K.-owned subsidiary. In Panel (B), control observations are surveyed individuals living within a 25 km distance to a French-owned subsidiary but at least 10 km or 25 km away from the closest U.K.-owned subsidiary. The specifications include country-by-year and grid-cell by U.K. firm presence fixed effects. Standard errors are clustered at the grid-cell level.

5.6 Supplementary Analyses and Robustness Tests

5.6.1 Tax Changes in Other Countries

Although we focus on the U.K. tax reform, we also expect that other major tax changes in developed countries may result in increased outbound FDI to less developed countries (conditional on those countries having positive NPV projects). In this section, we examine the effect on subsidiary presence in Africa following four other major corporate tax reforms over the last 20 years: Germany (2008), Canada (2008), Japan (2012), and Spain (2015).[23]

Figure D.1 in the Appendix graphs, in event time, the average number of total African subsidiaries of multinational firms headquartered in these four countries around the tax law change. These raw data show a stark increase in the number of African subsidiaries after the tax rate change (green line). In contrast, we find that parent firms from other countries without substantial tax rate reductions have a relatively consistent number of subsidiaries (orange dotted line).

While Appendix Figure D.1 provides descriptive evidence that corporate tax cuts in these other jurisdictions are associated with FDI in Africa, we acknowledge that other factors not controlled for in this analysis could drive an increased African presence. For example, the tax rate changes may be accompanied by other tax and policy changes in the home country that could alter firms' foreign investment. Thus, Appendix Table D.7 presents results from a staggered difference-in-differences test, where the variable *Tax Reform* is an indicator equal to one for the years following a tax rate change in each of the four countries. We document a positive and statistically significant coefficient that is robust to the inclusion of different controls and fixed effects, confirming that the documented effects from the U.K. setting also occur when examining these other four tax rate changes.

[23]We are unable to test the U.S. tax reform due to only one post-period year of available data (2018).

5.6.2 Database Sample Expansion

Another concern is that the effects we document are attributable to improving subsidiary coverage over time in the dataset and not to increased foreign investment by U.K. firms. While use of the samples with a pre-existing presence in a country helps to mitigate this concern, we also conduct three additional tests to further validate the data. Table D.8 in the Appendix first validates the data by testing the correlation between our dependent measure of U.K presence, *Log. Number Subsidiaries*, and the pairwise amount of foreign direct investment between the U.K. and the corresponding African country based on external data from the OECD. For example, we study whether the number of U.K. subsidiaries in Kenya in 2007 as observed in Orbis is correlated with the amount of FDI from U.K. to Kenya based on OECD Statistics on Globalisation data. We confirm a strong and positive correlation when using both the level and stock of FDI, thereby confirming that the data we use is consistent with macroeconomic data that measures a similar construct.

Second, we plot the ratio of a firm's consolidated total assets to its number of subsidiaries over the sample period, observing negatively-sloped lines across several different samples in Appendix Figure D.2. One possible explanation is that new subsidiaries have fewer assets over time, or that the newer subsidiaries hold different (more intangible) assets that are less likely to be recorded on firm financial statements. A second possible explanation is that the declining line is driven by a growing denominator attributable to improved coverage in the Orbis database, which would be problematic for our empirical tests. While it is not possible to distinguish between these explanations given the lack of subsidiary-level financial data, we note that this trend does not appear to differ for U.K. firms as compared to firms more generally, including those from other OECD countries, former colonial empire countries, or France.[24] This suggests that even if the increased number of subsidiaries is due to disclosure issues, it is not systematically different for U.K. firms as compared to other firms, thereby further

[24]The relatively higher ratio for U.K. firms in 2010 could suggest an general increase in U.K. firms' total assets due to additional investment and raised capital following the tax changes.

mitigating concerns that the Orbis data coverage is driving the observed results.

Third, in untabulated analyses, we further limit the balanced sample used in the empirical tests to only those multinational firms with a presence in the country from t-2 to t+2. We then re-estimate Equation 5.1. This restriction results in a substantial reduction in the sample to 6,000 firm-country-year observations, of which approximately 10 percent relate to U.K. firms. Despite this small sample, we find that the coefficient remains the same size and continues to be statistically significant when testing the effects relative to OECD groups, empire countries, and France.

5.6.3 The Effects of Territorial Tax Regime Change

As discussed previously, the U.K. had other changes to its corporate tax policy during our sample period. Specifically, the U.K. converted to a territorial system in 2009. Two factors suggest that the results we document relate to changes in the U.K. tax rate, not the change to the territorial regime. First, prior work examined FDI in response to the U.K.'s switch to the territorial regime, finding a negative change Matheson et al., 2013.[25] Second, the event study result presented in Figure 3 shows that the increased subsidiary presence occurs later in the sample period, in years that correspond with additional tax rate changes. While this evidence implies that tax rate changes drive the observed results, we conduct two additional analyses to further assess driver of these effects.

First, we verify that our observed results seem unlikely due to conversion to a territorial regime by examining the counterfactual: the Japanese corporate tax reform. Similar to the U.K., Japan had two substantial changes in tax policy during the sample period: a switch to a territorial regime and a tax rate reduction. However, unlike the U.K., where these events occurred close in event time, these were separated by three years in Japan. Specifically, Japan converted to a territorial system in 2009, but retained its existing corporate tax rate until 2012. In Panel (B) of Appendix Table D.6, we use

[25]This result occurs because, prior to the change, firms were penalized for repatriating income earned abroad and therefore would reinvest income in host countries. In a territorial system, there is no penalty for repatriation; thus, FDI decreases, especially in low-tax countries.

only Japanese firms in our control sample, finding a positive and statistically significant effect for the U.K. companies. This result permits us to attribute the results to the U.K. corporate income tax rate cuts, not to the transition from the worldwide to a territorial system of taxation. This finding is of importance when considering how the results may apply to U.S. observations following the decrease in the U.S. tax rate and the switch to a territorial-like system following the 2017 U.S. Tax Cuts and Jobs Act.

Second, we examine whether our results vary with the tax rate of the host African country. If the switch to territorial taxation drives the observed results, then we would expect to observe the results in countries with the lowest tax rate (and thus, the greatest difference between the U.K. and the statutory home country African rate). To examine this, we retrieve African statutory tax rates from the Tax Foundation, finding that only the countries of Comoros and Somalia had statutory tax rates lower than the U.K. rate after 2014. Observations from these countries account for less than 3 percent of all observations, thereby mitigating the concern that the switch to the territorial system drives our results. In Appendix Table D.9, we estimate our main model after partitioning the sample based on the tax rate differential between the U.K. and the African countries. We find a statistically significant effect in both subsamples, with no significant difference across these groups. Thus, the results do not appear driven by the change in tax regime.

5.6.4 Robustness Analyses

We conduct two additional robustness tests. First, our main tests focus on the number of subsidiaries a firm has in a country, refining the sample to require a presence in the pre-2009 period. In an additional test, we show that the effect of the U.K. tax reform on foreign subsidiary presence obtains at the extensive margin as well. Specifically, we estimate a linear probability model in which the outcome variable is an indicator equal to one once a subsidiary is established during our sample period (i.e., once it becomes observable in the database). Results are reported in Table D.10 in the Appendix. Estimates suggest that a newly formed subsidiary in Africa is approximately

1.79 percentage points more likely to be formed by a U.K. multinational firm after the U.K. tax rate change than by firms from other countries. As the baseline probability that an African subsidiary was U.K.-owned had a mean value of 6.88 percent (untabulated), this result suggests a 26 percent increase in the likelihood that an African subsidiary belongs to a U.K. firm. These effects occur when using a number of other control samples, as seen in Panel (B).

A related question is whether U.K. firms invest in countries other than in the developing countries we focus on in this study. In Table D.11 in the Appendix, we present results from testing whether the U.K. tax rate change is associated with investment into other *developed* countries. We find positive effects for U.K. firms' presence in Germany and in Ireland, as well as the broader group of OECD countries. However, the coefficients appear smaller as compared to the African analysis, with effects ranging from 4.2-7.5 percent. This relatively smaller effect may be explained by U.K. firms already being widely invested in these markets. We note that we observe a *negative* effect of the U.K. tax reform on the number of U.K. firms' domestic subsidiaries. One possible explanation for this result is that the U.K. motivated foreign investment into the country, whereas existing U.K. firms responded with either no new domestic subsidiaries but instead increased investment in existing entities.

5.7 Conclusion

The drivers and consequences of FDI in developing countries is a central policy question. A large literature examines the role of home country taxes on domestic investment and the attraction of foreign direct investment. We examine whether multinational firms respond to major tax rate cuts in their home countries by investing in foreign developing countries, thereby causing important fiscal policy spillovers originating in the developed world and affecting developing economies. We specifically focus on the substantial U.K. corporate tax rate reduction and study U.K. multinational firms' presence in sub-Saharan Africa.

We find that the corporate tax rate reduction is associated with an increased scope and an increased likelihood of U.K. multinational firms' presence in Africa. This result holds after using alternative comparison groups of other multinational firms, such as French and U.S. companies that should have similar investment interests and experience similar patterns in global demand and investment opportunities. Furthermore, we find that the effects are concentrated in those African countries with prior colonial ties to the U.K., as well as countries with a relatively higher existing U.K. multinational firm presence. Our evidence that links local residents to the local address of the U.K. facility, validated using luminosity data, points to a positive employment effect. These results are confirmed when studying employment in African countries affected the most by increases in U.K. multinational firm presence.

The results extend a literature that has typically focused on home country effects of corporate tax rate reductions. Thus, we extend the public economics literature on the relation between taxes and investment, and do so by studying activity in countries where multinational presence has the greatest potential to improve local economies – but also where such presence has been met with the most skepticism. We thereby also contribute to the literature in development economics and add to the scant evidence on the drivers and consequences of multinational firm investment in the developing world.

Chapter 6

Summary

This dissertation provides evidence along four central topics of corporate taxation. First, it contributes to the understanding of the measurement and extent of base erosion and tax-motivated income shifting in a globalized and digitalized world. Second, it adds to the question of determinants of tax-motivated income shifting and demonstrates that the digital transformation is a key enabler. Third, it provides evidence on capital market effects of tax reforms aimed at adjusting the international corporate tax system to safeguard tax revenues. Fourth, it documents spillover effects of domestic corporate tax policy on the developing world.

Credibly measuring the extent of BEPS over time and assessing its economic relevance is a necessary prerequisite to evaluate proposals for reforming the global corporate tax system. Using three of the indicators that the OECD introduced in its Final Report on "Measuring and Monitoring BEPS, Action 11", my co-authors and I show in Chapter 2 of this dissertation that simplified and highly aggregated indicators provide only limited evidence on BEPS and come with a number of shortcomings. These indicators highly dependent on the underlying assumptions, the availability of data, and may be influenced by various confounding factors beyond BEPS. Yet, given the ongoing political and academic debate on how to adjust the international corporate tax system, transparent updates on the existence and extent of BEPS are of highest importance. We show that identification strategies based on micro-data are a better suited tool for providing clear and holistic evidence on BEPS to policymakers.

Beyond documenting the mere existence of tax-motivated income shifting, one of the central challenges in empirical tax research is to identify the determinants of it. Chapter 3 of this dissertation provides evidence that internal digitalization is an important enabler of tax-motivated income shifting. Creating a novel micro-level digitalization index that captures affiliates' access to information technology and communication technology, my co-authors and I show that affiliates' level of overall digitalization is positively associated with the tax sensitivity of reported profits. In particular, we show that communication and collaboration within a multinational firm are key enabler of efficient tax planning. This study extends prior literature by uncovering the underlying firm-specific channels of internal information quality. Overall, the results imply that digitalization is a crucial foundation for timely, data-driven decision making that extends beyond core business functions to support functions such as the tax department.

Above all, it is the era of digitalization and globalization that has led to an intense political and academic debate on how to adapt the principles of corporate taxation to the digital economy. However, empirical evidence on the effects of proposed adjustments to corporate taxation is scarce. The study in Chapter 4 of this dissertation analyzes the capital market reaction in response to the European Commission's digital tax proposals released in March 2018. The digital tax proposals consist of two draft directives on the taxation of the digital economy. The first draft directive suggests the introduction of an interim tax of three percent on gross revenues from certain digital services. The second draft directive lays down the rules for taxing corporate profits attributable to a significant digital presence. My co-authors and I find a significant abnormal reduction in firm value of digital firms. Cross-sectional analyses reveal that the market differentiates its response depending on firms' tax-avoidance behavior and profit-shifting potential. Overall, the investor reaction reflects the intention of the European Commission's proposals to secure tax revenues and extract location-specific rent. Nevertheless, the study indicates that increasing the tax burden for a highly innovative industry contradicts political initiatives to promote an attractive investment climate and interferes with the EU's core objective to foster innovation and economic growth.

While the discussion on reforming the international corporate tax system to prevent income shifting and safeguard tax revenues is ongoing, policymakers often use the domestic corporate tax systems to attract business investment by offering tax incentives

or lower tax rates. A large literature examines the role of domestic country taxes on domestic investment and the attraction of foreign direct investment. The study in Chapter 5 of this dissertation investigates whether multinational firms respond to tax rate reductions in their home countries by investing in foreign developing countries. My co-authors and I find that the corporate tax rate reduction in the U.K. is associated with an increased U.K. multinational firms' presence in Africa. We further link local residents to the local address of the U.K. subsidiaries and show positive local economic and employment effects. The results extend a literature that has typically focused on domestic effects of corporate tax rate reductions and add to the scant evidence on the drivers and consequences of multinational firm investment in the developing world.

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Appendix

A Appendix to Chapter 2

Table A.1: Indicator 4 - Result over time and alternative Dependent Variable

	(1)	(2)	(3)	(4)	(5)
		ETR			Tax / TOAS
Variable	Full Sample	Full Sample	OECD	EU	Full Sample
Large × MNE × 2000	-1.7451***	-1.5094***	-1.6024***	-1.3151**	-0.0151
	(0.4446)	(0.4435)	(0.4997)	(0.5462)	(0.0597)
Large × MNE × 2001	-2.0168***	-1.7351***	-1.6660***	-1.0466**	-0.1056**
	(0.3979)	(0.3978)	(0.4783)	(0.4984)	(0.0464)
Large × MNE × 2002	-2.1409***	-1.9829***	-1.1959***	-1.0171**	-0.1709***
	(0.3257)	(0.3253)	(0.3976)	(0.4454)	(0.0361)
Large × MNE × 2003	-1.2244***	-1.0260***	-1.0556***	-1.2429***	-0.0489
	(0.2988)	(0.2978)	(0.3723)	(0.4335)	(0.0328)
Large × MNE × 2004	-2.4361***	-2.2845***	-1.8052***	-1.6114***	-0.1017***
	(0.2469)	(0.2463)	(0.3470)	(0.3947)	(0.0275)
Large × MNE × 2005	-1.5695***	-1.3694***	-1.0705***	-0.9054**	-0.0553*
	(0.2423)	(0.2431)	(0.3185)	(0.3516)	(0.0297)
Large × MNE × 2006	-2.0878***	-1.8566***	-0.7579***	-0.7890**	-0.1501***
	(0.2100)	(0.2101)	(0.2895)	(0.3176)	(0.0244)
Large × MNE × 2007	-0.9689***	-0.6266***	-0.3101	-0.0279	-0.0983***
	(0.2208)	(0.2219)	(0.2812)	(0.2908)	(0.0258)
Large × MNE × 2008	-1.5318***	-1.2965***	-1.5250***	-0.8234***	-0.1209***
	(0.2275)	(0.2279)	(0.2937)	(0.2941)	(0.0255)
Large × MNE × 2009	-0.8038***	-0.5968***	-0.2437	-0.2402	-0.0399*
	(0.2269)	(0.2268)	(0.2890)	(0.3038)	(0.0221)
Large × MNE × 2010	-0.8344***	-0.5917***	-0.7783***	-0.7657***	-0.0697***
	(0.2069)	(0.2069)	(0.2680)	(0.2868)	(0.0200)
Large × MNE × 2011	-1.1547***	-0.8956***	-1.0909***	-1.6932***	-0.1158***
	(0.1966)	(0.1964)	(0.2576)	(0.2806)	(0.0198)
Large × MNE × 2012	-1.1871***	-0.9812***	-1.3222***	-1.6207***	-0.1221***
	(0.1950)	(0.1949)	(0.2575)	(0.2894)	(0.0188)
Large × MNE × 2013	-0.5882***	-0.4110**	-0.5016**	-0.4745*	-0.0537***
	(0.1872)	(0.1873)	(0.2463)	(0.2750)	(0.0179)
Large × MNE × 2014	-0.7135***	-0.5245***	-0.9343***	-0.6510**	-0.0632***
	(0.1825)	(0.1826)	(0.2418)	(0.2673)	(0.0178)
Large × MNE × 2015	-0.3270*	-0.2036	-0.5201**	-0.0968	-0.0483***
	(0.1705)	(0.1711)	(0.2225)	(0.2520)	(0.0173)
Large × MNE × 2016	-0.7032***	-0.5873***	-1.0718***	-0.6808***	-0.0883***
	(0.1720)	(0.1724)	(0.2241)	(0.2530)	(0.0177)
Controls	No	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Country-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	5,048,716	5,048,716	4,320,449	4,353,789	5,048,716
Number of Firms	1,001,429	1,001,429	843,911	854,141	1,001,429
Adj. R2	0.345	0.362	0.354	0.365	0.666

Notes: This table presents the regression results for OECD BEPS Indicator 4 over time. In columns (1) to (4), the dependent variable is the effective tax rate (*ETR*). In column (5), the dependent variable is the ratio of tax payments to total assets (*Tax/TOAS*). In columns (2) and (3), the sample is restricted to firms located in the OECD or European Union, respectively. *Large* is a dummy variable that is equal to one for all firms with more than 250 employees. *MNE* is a dummy variable that is equal to one for all firms that belong to a group with a least one cross-border affiliate relationship. In columns (2) to (5), the same controls are included as in Table 2.5. The interaction of interest *Large* × *MNE* is interacted with a yearly *Time* dummy to provide annual estimates. All continuous variables are winsorized at the 1 and 99 percentile. We report standard errors clustered by firm in parentheses. ***, **, * denote statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively.

B Appendix to Chapter 3

Table B.1: Variable Definitions

Variable	Definition
Profit	Pre-tax earnings (PLBT) reported in the unconsolidated financial statement of affiliate i in year t.
C	Intra-group income shifting incentive of affiliate i in year t, measured as operating revenue-weighted average tax rate differential of each affiliate relative to all other affiliates in the group per year. See Equation 3.1.
Digital	Additive digitalization index ranging from 0 to 3 based on the usage of enterprise resource planning (ERP) software, database management system (DBMS), and groupware software of affiliate i in year t.
ERP / DBMS / Communication	Dummy variable taking the value of one if an enterprise resource planning (ERP) software, a database management system (DBMS), or groupware software is available in affiliate i in year t and zero otherwise.
Capital	Tangible fixed assets (TFAS) reported in the unconsolidated financial statement of affiliate i in year t.
Labor	Employee compensation expense (STAF) reported in the unconsolidated financial statement of affiliate i in year t.
Productivity	The median return on assets measured on affiliates i's country-industry level in year t, where industry refers to the two-digit NACE classification.
RoA	Return on Assets defined as pre-tax earnings of affiliate i in year t scaled by total assets of affiliate i in year t.
Intangible Assets	Intangible assets (IFAS) reported in the unconsolidated financial statement of affiliate i in year t.
EBIT	Earnings before interest and tax reported in the unconsolidated financial statement of affiliate i in year t.
Distance SAP	Affiliates' distance to the closest local SAP office measured in 1000 kilometers.
Country Dispersion	Ratio of the number of countries in which the affiliate's multinational firm operates relative to the multinational firm's total number of affiliates.
GDPperCapita	GDPperCapita of affiliate i's host country in year t.
Unemployment	Unemployment rate of affiliate i's host country in year t.
Inflation	Inflation rate of affiliate i's host country in year t.
CIT	Statutory corporate tax rate of affiliate i's host country in year t.

Notes: If *log* is specified before a variable, this refers to the natural logarithm of the variable, otherwise the definition remains the same.

Table B.2: Robustness Tests I

	Intangibles as Control		CIT instead of C		EBIT as Dependent Var.	
	(1)	(2)	(3)	(4)	(5)	(6)
Variable	log Profit		log Profit		log EBIT	
C × Digital	-0.474***	-0.223**			-0.293**	-0.104
	(0.15)	(0.11)			(0.12)	(0.08)
C	0.622*	0.352			-0.135	0.022
	(0.32)	(0.29)			(0.25)	(0.23)
CIT × Digital			-0.317***	-0.111*		
			(0.11)	(0.07)		
CIT			-0.571**	0.165		
			(0.23)	(0.21)		
Digital	.	0.001	.	0.036*	.	0.003
		(0.01)		(0.02)		(0.00)
log Intangible Assets	0.059***	-0.015***				
	(0.01)	(0.00)				
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Interaction: Digital × Controls	Yes	No	Yes	No	Yes	No
Year Fixed Effects	.	Yes	.	Yes	.	Yes
Industry Fixed Effects	.	.	.	.	.	.
Affiliate Fixed Effects	No	Yes	No	Yes	No	Yes
Year-Digital Fixed Effects	Yes	No	Yes	No	Yes	No
Industry-Digital Fixed Effects	Yes	No	Yes	No	Yes	No
Observations	99,120	96,284	131,642	128,653	131,639	131,633
Number of Affiliates	19,283	16,447	24,025	21,036	24,024	24,024
Adj. R2	0.443	0.773	0.429	0.772	0.427	0.432

Notes: This table presents regression estimates for the baseline approach outlined in Equation 3.2, testing the effect of digitalization on tax-motivated income shifting. *C* refers to the income shifting incentive measure as defined by Huizinga and Laeven (2008). In columns (1) to (4),the dependent variable is the natural logarithm of profits before tax (*log Profit*). In column (1) and (2), we include *log Intangible Assets* as an additional control variable. Columns (3) and (4) include the statutory corporate tax rate (*CIT*) instead of *C* as a measure for affiliates' tax incentives. In columns (5) and (6), we change the dependent variable to the natural logarithm of earnings before interest and tax (*log EBIT*). The digitalization index (*Digital*) is determined as an additive index that captures affiliates' access to ERP software, DBMS, or communication software. It is based on a yearly affiliate-level survey over the period 2006 to 2016. Controls are the same control variables as in Table 3.4. In odd columns, all control variables are interacted with *Digital* and include two-way fixed effects. All continuous variables are winsorized at the 1 and 99 percentile. We report heteroskedasticity-robust standard errors clustered by affiliate in parentheses. ***, **, * denote statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively. All variables are defined in Appendix Table B.1.

Table B.3: Robustness Tests II

	Non-Interpolation		Digital Dummy		Categorical Variable	
	(1)	(2)	(3)	(4)	(5)	(6)
Variable	log Profit		log Profit		log Profit	
C × Digital	-0.316**	-0.183*				
	(0.13)	(0.10)				
C × Digital Dummy			-0.852***	-0.535**		
			(0.31)	(0.23)		
C × Digital = 1					-0.691*	-0.461*
					(0.37)	(0.27)
C × Digital = 2					-0.639*	-0.619**
					(0.35)	(0.26)
C × Digital = 3					-1.434***	-0.494*
					(0.40)	(0.28)
C	-0.279	0.447*	0.042	0.588**	0.042	0.600**
	(0.27)	(0.27)	(0.31)	(0.27)	(0.31)	(0.27)
Digital	.	0.000				
		(0.01)				
Digital Dummy			.	-0.010		
				(0.01)		
Digital = 1					.	-0.023*
						(0.01)
Digital = 2					.	0.003
						(0.01)
Digital = 3					.	0.000
						(0.02)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Interaction: Digital × Controls	Yes	No	Yes	No	Yes	No
Year Fixed Effects	.	Yes	.	Yes	.	Yes
Industry Fixed Effects	.	.	.	.	.	.
Affiliate Fixed Effects	No	Yes	No	Yes	No	Yes
Year-Digital Fixed Effects	Yes	No	Yes	No	Yes	No
Industry-Digital Fixed Effects	Yes	No	Yes	No	Yes	No
Observations	109,405	106,193	131,642	128,653	131,639	131,633
Number of Affiliates	23,769	20,554	24,025	21,036	24,024	24,024
Adj. R2	0.426	0.768	0.425	0.772	0.427	0.432

Notes: This table presents regression estimates for the baseline approach outlined in Equation 3.2, testing the effect of digitalization on tax-motivated income shifting. *C* refers to the income shifting incentive measure as defined by Huizinga and Laeven (2008). The dependent variable is the natural logarithm of profits before tax (*log Profit*). The digitalization index (*Digital*) is determined as an additive index that captures affiliates' access to ERP software, DBMS, or communication software. It is based on a yearly affiliate-level survey over the period 2006 to 2016. In columns (1) and (2), we do not interpolate *Digital* if we observe a value in the previous and upcoming year. In columns (3) and (4), we replace *Digital* by a dummy variable that takes the value of one if *Digital* is greater than zero. In columns (5) and (6), we treat *Digital* as an categorical variable rather than a continuous variable. Controls are the same control variables as in Table 3.4. In odd columns, all control variables are interacted with *Digital* or *Digital Dummy* and include two-way fixed effects. All continuous variables are winsorized at the 1 and 99 percentile. We report heteroskedasticity-robust standard errors clustered by affiliate in parentheses. ***, **, * denote statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively. All variables are defined in Appendix Table B.1.

C Appendix to Chapter 4

Table C.1: List of 10-K Annual Reports with Reference to the Digital Tax Package

Filed	Reporting for	Filing entity/person	Filed	Reporting for	Filing entity/person
Search Term: "digital services tax"			Search Term: "taxation of the digital economy"		
26-Feb-20	31-Dec-19	Booking Holdings Inc. (BKNG)	20-Mar-20	31. Jan 20	Zoom Video Communications, Inc. (ZM)
28-Feb-20	31-Dec-19	ANGI Homeservices Inc. (ANGI)	22-Feb-19	31-Dec-18	Liberty TripAdvisor Holdings, Inc. (LTRPA)*
20-Mar-20	31-Dec-19	TRAVELZOO (TZOO)	19-Feb-20	31-Dec-19	Liberty TripAdvisor Holdings, Inc. (LTRPA)
27-Feb-19	31-Dec-18	Booking Holdings Inc. (BKNG)*	28-Feb-20	31-Dec-19	YELP INC (YELP)
19-Feb-20	31-Dec-19	TripAdvisor, Inc. (TRIP)	19-Feb-20	31-Dec-19	TWITTER, INC. (TWTR)*
28-Feb-20	31-Dec-19	IAC/INTERACTIVECORP (MTCH)	2-Mar-20	31-Dec-19	Upland Software, Inc. (UPLD)
27-Feb-19	31-Dec-18	Gannett Co., Inc.	5-Mar-20	31-Jan-20	SALESFORCE.COM, INC. (CRM)
22-Feb-19	31-Dec-18	Liberty TripAdvisor Holdings, Inc. (LTRPA)*	4-Mar-20	31-Dec-19	Cloudflare, Inc. (NET)*
27-Feb-20	31-Dec-19	Match Group, Inc.	1-Apr-20	31-Dec-19	True Nature Holding, Inc. (MITI, TNTY)*
19-Feb-20	31-Dec-19	Liberty TripAdvisor Holdings, Inc. (LTRPA)	27-Feb-19	31-Dec-18	Booking Holdings Inc. (BKNG)*
4-Feb-21	31-Dec-20	EBAY INC (EBAY, EBAYL)	19-Feb-20	31-Dec-19	TripAdvisor, Inc. (TRIP)
2-Mar-20	31-Dec-19	Gannett Co., Inc. (GCI)	22-Feb-19	31-Dec-18	TripAdvisor, Inc. (TRIP)*
8-Feb-19	31-Dec-18	Expedia Group, Inc. (EXPE)*	18-Mar-20	31-Dec-19	Maiden Holdings, Ltd. (MHLD)
22-Feb-19	31-Dec-18	TripAdvisor, Inc. (TRIP)*	Search Term: "taxation of specified digital services"		
14-Feb-20	31-Dec-19	Expedia Group, Inc. (EXPE)	7-Feb-20	31-Dec-19	PINTEREST, INC. (PINS)*
8-Feb-19	31-Dec-18	Liberty Expedia Holdings, Inc.*	5-Feb-21	31-Dec-20	PINTEREST, INC. (PINS)*
10-Dec-19	30-Sep-19	LIQUIDITY SERVICES INC (LQDT)	4-Mar-20	31-Dec-19	Cloudflare, Inc. (NET)*
1-Mar-19	31-Dec-18	ANGI Homeservices Inc. (ANGI)	6-Feb-19	31-Dec-18	Snap Inc (SNAP)*
30-Jan-19	31-Dec-18	EBAY INC (EBAY, EBAYL)	2-Mar-20	31-Dec-19	Uber Technologies, Inc (UBER)*
31-Jan-20	31-Dec-19	EBAY INC (EBAY, EBAYL)	Search Term: "taxation of digital services"		
23-Jul-20	31-Mar-20	MiX Telematics Ltd (MIXT)	14-Feb-20	31-Dec-19	HONEYWELL INTERNATIONAL INC (HON)
17-Sep-20	31-Jul-20	Zscaler, Inc. (ZS)	12-Feb-21	31-Dec-20	HONEYWELL INTERNATIONAL INC (HON)
1-Mar-19	31-Dec-18	IAC/INTERACTIVECORP (MTCH)	22-Feb-19	31-Dec-18	Travelport Worldwide LTD
2-Mar-20	31-Dec-19	Clarivate Analytics PLC (CCC)	Search Term: "digital services taxes"		
Search Term: "digital tax"			26. Feb 20	31-Dec-19	Booking Holdings Inc. (BKNG)
Search Term: "digital service tax"			20-Mar-20	31-Dec-19	TRAVELZOO (TZOO)
19-Feb-20	31-Dec-19	TripAdvisor, Inc. (TRIP)	11-Aug-20	30-Jun-20	NEWS CORP (NWS, NWSA)
11-Feb-20	31-Dec-19	VARONIS SYSTEMS INC (VRNS)	2-Mar-20	31-Dec-19	ROKU, INC (ROKU)
9-Feb-21	31-Dec-20	VARONIS SYSTEMS INC (VRNS)	27-Feb-20	31-Dec-19	Activision Blizzard, Inc. (ATVI)
4-Feb-21	31-Dec-20	EBAY INC (EBAY, EBAYL)	20-May-20	31-Mar-20	ELECTRONIC ARTS INC. (EA)
19-Feb-20	31-Dec-19	Liberty TripAdvisor Holdings, Inc. (LTRPA)	04-Feb-20	31-Dec-19	Alphabet Inc. (GOOG, GOOGL)
21-Feb-20	31-Dec-19	DROPBOX, INC. (DBX)*	24-May-19	31-Mar-19	ELECTRONIC ARTS INC. (EA)
18-Feb-20	31-Dec-19	Groupon, Inc. (GRPN)	03-Feb-21	31-Dec-20	Alphabet Inc. (GOOG, GOOGL)
19-Feb-20	31-Dec-19	TWITTER, INC. (TWTR)*	06-Feb-20	31-Dec-19	PayPal Holdings, Inc. (PYPL)
31-Jan-20	31-Dec-19	EBAY INC (EBAY, EBAYL)	12-Feb-21	31-Dec-20	Expedia Group, Inc. (EXPE)
12-Feb-19	31-Dec-18	Groupon, Inc. (GRPN)	05-Feb-21	31-Dec-20	PayPal Holdings, Inc. (PYPL)
7-Mar-19	31-Dec-18	Upwork Inc. (UPWK)	26-Feb-20	31-Dec-19	Square, Inc. (SQ)
28-Feb-19	31-Dec-18	ETSY INC (ETSY)	11-Feb-21	31-Dec-20	Carlyle Group Inc. (CG)
14-Feb-20	31-Dec-19	Expedia Group, Inc. (EXPE)	08-Feb-19	31-Dec-18	Expedia Group, Inc. (EXPE)*
27-Feb-20	31-Dec-19	ETSY INC (ETSY)	14-Feb-20	31-Dec-19	Expedia Group, Inc. (EXPE)
2-Mar-20	31-Dec-19	Upwork Inc. (UPWK)	08-Feb-19	31-Dec-18	Liberty Expedia Holdings, Inc.*
6-Feb-20	31-Dec-19	Intercontinental Exchange, Inc. (ICE)	26-Feb-20	31-Dec-19	Sabre Corp (SABR)
7-Feb-19	31-Dec-18	Intercontinental Exchange, Inc. (ICE)	13-Aug-19	30-Jun-19	NEWS CORP (NWS, NWSA)
Search Term: "taxation of the digital economy"			2-Mar-20	31-Dec-19	Uber Technologies, Inc (UBER)*
14-Feb-20	31-Dec-19	VERISIGN INC/CA (VRSN)	27-Feb-20	31-Dec-19	Chubb Ltd (CB)
1-Apr-19	31-Dec-18	True Nature Holding, Inc. (MITI, TNTY)*	12-Feb-20	31-Dec-19	Carlyle Group Inc. (CG)
8-Feb-19	29-Dec-18	CERNER CORP /MO/ (CERN)*	13-Feb-19	31-Dec-18	Carlyle Group L.P. (CG)
7-Feb-20	31-Dec-19	PINTEREST, INC. (PINS)*	Search Term: "digital service taxes"		
5-Feb-21	31-Dec-20	PINTEREST, INC. (PINS)*	18-Feb-20	31-Dec-19	Groupon, Inc. (GRPN)
27-Feb-19	29-Dec-18	CADENCE DESIGN SYSTEMS INC (CDNS)	26-Feb-20	31-Dec-19	Booking Holdings Inc. (BKNG)
20-Feb-18	30-Dec-17	CADENCE DESIGN SYSTEMS INC (CDNS)	12-Feb-19	31-Dec-18	Groupon, Inc. (GRPN)
24-Feb-20	28-Dec-19	CADENCE DESIGN SYSTEMS INC (CDNS)	26-Feb-20	31-Dec-19	Square, Inc. (SQ)A62C100A68:C108
10-Feb-20	28-Dec-19	CERNER Corp (CERN)*	*Indicates mentioning of March 2018/ EU Comission 2018*		

Table C.2: Exemplary Risk Statements in Annual Reports

Corporation	10-K risk statement
Booking Holdings Inc.	"In March 2018, the European Commission, also working on determining a solution to the tax treatment of the digital economy, released two draft directives on the Taxation of the Digital Economy. Although these proposals were not approved, a number of E.U. member states have indicated they will unilaterally introduce a digital services tax." 10-K December 2018 p. 21
Cerner Corp	"Further, during 2018, the European Commission issued proposals and the OECD issued an interim report related to the taxation of the digital economy. As these and other tax laws and related regulations change, our financial results could be materially impacted." 10-K December 2018 p. 13
Ebay Inc.	"Similarly, in Europe, and elsewhere in the world, there are various tax reform efforts underway designed to ensure that corporate entities are taxed on a larger percentage of their earnings. Companies that operate over the Internet, such as eBay, are a target of some of these efforts. If more taxing authorities are successful in applying direct taxes to Internet companies that do not have a physical presence in their respective jurisdictions, this could increase our effective tax rate." 10-K December 2018 p. 23
Expedia Group, Inc.	"Following the OECD's announcement, the European Commission published proposals for European Union (EU) member states to introduce a new digital services tax on the revenue of companies that provide certain digital services." 10-K December 2018 p. 20
Facebook, Inc.	"Similarly, the European Commission and several countries have issued proposals that would change various aspects of the current tax framework under which we are taxed. These proposals include changes to the existing framework to calculate income tax, as well as proposals to change or impose new types of non-income taxes, including taxes based on a percentage of revenue. For example, the United Kingdom, Spain, Italy, and France have each proposed taxes applicable to digital services, which includes business activities on social media platforms and online marketplaces, and would likely apply to our business." 10-K December 2018 p. 26
Godaddy Inc.	"Due to the global nature of the Internet, it is possible that any U.S. or foreign federal, state or local taxing authority might attempt to regulate our transmissions or levy transaction, income or other taxes relating to our activities. Tax authorities at the international, federal, state and local levels are regularly reviewing the appropriate treatment of companies engaged in e-commerce." 10-K December 2018 p. 44
Groupon, Inc.	"taxation (including the European Union's voucher directive, digital service tax and similar regulations)" 10-K December 2018 p. 15
Liberty Expedia Holdings, Inc.	"In March 2018, the OECD proposed measures to address the application of corporate tax to companies operating in the digital economy. Following the OECD's announcement, the European Commission published proposals for European Union (EU) member states to introduce a new digital services tax on the revenues of companies that provide certain digital services." 10-K December 2018 p. 31
Liberty Tripadvisor Holdings, Inc.	"The second directive provides for an interim solution whereby EU States are to apply a 3% revenue based Digital Services Tax, which if enacted, would be effective beginning in 2020. In the interim, certain EU States (Austria, France, Italy, Spain, Belgium and the United Kingdom) have proposed legislation to implement a Digital Services Tax that, if enacted, would impose a tax on revenue earned by larger companies from users of digital services located in these respective EU States as early as 2019." 10-K December 2018 p. 31
Match Group, Inc.	"The European Commission and several European countries have issued proposals that would change various aspects of the current tax framework under which we are taxed, including proposals to change or impose new types of non-income taxes (including taxes based on a percentage of revenue)." 10-K December 2018 p. 27
Paypal Holdings, Inc.	"Various levels of government, such as U.S. federal and state legislatures, and international organizations, such as the Organization for Economic Co-operation and Development (OECD) and the EU, are increasingly focused on tax reform and other legislative or regulatory action to increase tax revenue. Any such tax reform or other legislative or regulatory actions could increase our effective tax rate." 10-K December 2018 p. 29
Red Hat Inc.	"Moreover, the European Commission and some foreign jurisdictions have introduced proposals to impose a separate tax on specified digital service activity. It is unclear how or if such proposals, if enacted, would impact us." 10-K February 2019 p.33
Twitter, Inc.	"In addition, many countries in Europe, as well as a number of other countries and organizations, have recently proposed changes to tax laws regarding digital services that could significantly increase our tax obligations in many countries where we do business or require us to change the manner in which we operate our business." 10-K December 2018 p. 38

Table C.3: Core Media Topics around the Event Window

Topic	Number of Articles in Newspapers			
	Wall Street Journal	Washington Post	New York Times	Guardian
European Commission's Digital Tax Proposals	3	1	1	1
Facebook Data Leak	4	1	1	2
International Trade / Tariffs Discussion	3	2	3	2

Notes: The table depicts the number of articles in the respective journal referring to a core media topic in the event window.

Table C.4: List of Affected Firms

58.Com Inc.	Digital China Holdings Limited	Line Corporation	Scientific Games Corp
Activision Blizzard, Inc.	Discovery, Inc.	Masmovil Ibercom, S.A.	Scsk Corporation
Akamai Technologies INC	DUN & Bradstreet Corp.	Match Group, Inc.	Senshukai CO LTD
Alibaba Group Holding Limited	DXC Technology Company	Maxar Technologies Inc.	Servicenow, Inc.
Alliance Data Systems Corp	Ebay INC	Mediaset S.P.A.	Seven West Media Limited
Allscripts Healthcare Solutions INC	Econocom Group SA	Meredith Corp	SG & G Coporation
Alphabet Inc.	Elanders AB	Micro Focus International PLC	Shanghai Ganglian E-Commerce
Altran Technologies SA	Electronic Arts INC	Mixi Inc.	SK Holdings Co., Ltd.
Amadeus IT Group, S.A.	Entertainment ONE Limited	Modern Times Group AB	SKY Limited
Amazon.Com, Inc.	EOH Holdings Limited	Moody's Corporation	Softbank Group Corp
AMC Networks Inc.	Epam Systems, Inc.	Mphasis Limited	Solocal Group S.A.
Amdocs Limited	Equifax INC	N Brown Group PLC	Sonda S.A.
Anhui Xinhua Media Company Limited	Equinix INC	Nasdaq, Inc.	Sopra Steria Group
Arnoldo Mondadori Editore SPA	Esprinet S.P.A.	Naspers Limited	Square Enix Holdings Co., Ltd.
Asos PLC	Expedia Group, Inc.	Naver Corporation	Super Micro Computer, Inc.
Asseco Poland S.A.	Experian PLC	NET ONE Systems CO LTD	Sykes Enterprises INC
Atos SE	Facebook, Inc.	Netapp, Inc.	Synaptics Incorporated
Autohome Inc.	Factset Research Systems INC	Netease, Inc.	Systemax INC
Automatic Data Processing INC	Fairfax Media Limited	Netflix, Inc.	T-Gaia Corp.
Axel Springer SE	First Data Corporation	Netscout Systems INC	Take-Two Interactive Software Inc.
Baidu Inc.	Fiserv INC	NEW Media Investment Group Inc.	Takkt AG
Bechtle AG	Formula Systems (1985) Limited	NEW York Times CO	Tata Consultancy Services Limited
Belluna CO LTD	Fuji Soft Inc.	News Corporation	Tech Mahindra Limited
Bitauto Holdings LTD	Gakken Holdings Co., Ltd.	Nexon CO LTD	Teradata Corporation
Booking Holdings Inc.	Gannett Co., Inc.	Next PLC	Thomson Reuters Corporation
Broadridge Financial Solutions, Inc.	Gartner INC	Nielsen Holdings PLC	Transcosmos INC
Caci International INC	Gemalto N.V.	Nomura Research Institute, Ltd.	Transunion
Cancom SE	Global Payments INC	NTT Data Corporation	Travelport Worldwide Limited
Capgemini SE	GMO Internet Inc.	Otsuka Corporation	Trend Micro Incorporated
CBS Corporation	Godaddy Inc.	Overstock.Com, Inc.	Trivago N.V.
CDW Corp	Graham Holdings Company	Paypal Holdings, Inc.	Twenty-First Century Fox, Inc.
Cerner Corp	Groupon, Inc.	PC Connection INC	Twitter, Inc.
Check Point Software Technologies Limited	Grupo Televisa S.A.B. de C.V.	Pcm, Inc.	Ubisoft Entertainment SA
China South Publishing & Media Group	GS Home Shopping Inc.	Pearson PLC	Verint Systems, Inc.
Chinasoft International Limited	HCL Technologies Limited	Pivot Technology Solutions, Inc.	Verisign INC
Cimpress N.V.	Henan Dayou Energy Co., Ltd.	Playtech PLC	Verisk Analytics, Inc.
CIR S.P.A.	Henry Jack & Associates INC	Presidio, Inc.	Viacom, Inc.
Citrix Systems INC	Houghton Mifflin Harcourt Company	Prosiebensat.1 Media SE	Vipshop Holdings LTD
CJ ENM CO. Ltd.	Iliad	Quebecor INC	Virtusa Corporation
Cofide - Gruppo de Benedetti S.P.A.	Indra Sistemas SA	Qurate Retail, Inc.	Vmware, Inc.
Cognizant Technology Solutions Corp	Informa PLC	Rakuten INC	Wayfair Inc.
Comcast Corporation	Infosys Limited	RED HAT INC	Weibo Corporation
Computacenter PLC	Insight Enterprises INC	Redington (India) Ltd.	Wipro Limited
Conexio Corporation	Internet Initiative Japan INC	Relx PLC	Wirecard AG
Constellation Software Inc.	Itochu Techno-Solutions Corporation	Reply S.P.A.	Wolters Kluwer NV
Convergys Corp	Jd.Com Incorporated	Rizap Group, Inc.	Workday, Inc.
Copart INC	Jiangsu Phoenix Publishing & Media	Rizzoli Corriere Della Sera	Worldline
CoreLogic Inc.	John Wiley & Sons, Inc.	RTL Group SA	Xinhua Winshare Publishing and Media
Criteo SA	Kadokawa Dwango Corporation	S&P Global Inc.	Yandex N.V.
Cyberagent INC	Konami Holdings Corporation	Sabre Corporation	Yirendai Ltd.
DAI Nippon Printing CO LTD	Lagardere SCA	Salesforce.Com, Inc.	Yonyou Network Technology Co., Ltd.
Daily Mail and General Trust PLC	Larsen & Toubro Infotech Limited	Samsung SDS Co.,Ltd.	YY Inc.
Daou Tech Inc.	Leidos Holdings, Inc.	Sanoma OYJ	Zalando SE
Dassault Systemes SE	Liberty Expedia Holdings, Inc.	Schibsted ASA	Zozo, Inc.
Datatec Limited	Liberty Global PLC	Scholastic Corp	
DHC Software Co., Ltd.	Liberty Tripadvisor Holdings, Inc.	Science Applications	

Notes: In total, 222 companies are classified to be affected by the EU Commission's proposals. The country dispersion is as follows: Australia 2; Belgium 1; Bermuda 1; Canada 5 Cayman Islands 12; Chile 1; China 8; Finland 1; France 11; Germany 7; India 8; Israel 2; Italy 7; Japan 28; South Korea 7; Luxembourg 1; Mexico 1; Netherlands 5; Norway 1; Poland 1; South Africa 3; Spain 3; Sweden 2; UK 15; U.S. 88.

Table C.5: Cumulative Average Abnormal Return – Alternative Event Study Method

	(1)
Expected return estimation	market model
21-22 Mar. 2018	-0.690*
	(0.417)

Notes: This model estimates the cumulative average abnormal return (CAAR) in line with Kothari and Warner (2007). $CAAR(t_0, t_1) = \sum_{t=t_0}^{t=t_1} (\frac{1}{N} \sum_{i=1}^{i=N} AR_{it})$. Daily abnormal returns AR_{it} are calculated as the difference between actual returns and expected returns $AR_{it} = R_{it} - R_{it}^{exp}$. We use parameters from the market model regression for each individual firm to estimate the expected return R_{it}^{exp}: $AR_{it} = R_{it} - (\hat{\alpha}_i + \hat{\beta}_i R_{mt})$. R_{mt} is the return of the market index m (S&P Global 1200) on day t. The ratio of the CAAR and its estimated standard deviation ($\hat{s}$) provides – in the absence of abnormal returns – a normally distributed test statistic. The 222 treated firms are stock-listed firms whose global consolidated revenue exceeds 750 million euros and the firms operate in an industry that is likely to be affected by the EU digital tax proposal. Standard errors are in parenthesis. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

Table C.6: Cumulative Average Abnormal Return – Fama-French adjusted Three Factor Model

	(1)
	Stock return (adjusted for the risk free rate of return)
Alpha	0.017
	(0.015)
Market return (S&P 1200)	0.883***
	(0.037)
FF-SMB Factor	0.166**
	(0.073)
FF-HML Factor	-0.312***
	(0.066)
21-22 Mar. 2018	-0.730***
	(0.109)
Observations	53,724
Firms	222
Adj.-R2	0.076

Notes: The model presents the results using the Fama-French three-factor model to estimate abnormal returns (Fama and French, 1993; Kothari and Warner, 2007): $R_{it} - R_{ft} = \alpha + \beta_1(R_{mt} - R_{ft}) + \beta_2 SMB_t + \beta_3 HML_t + \gamma D_t + \epsilon_{it}$. This model expands the conditional market model by adding the risk-free rate of return, R_{ft}, size risk, SMB_t, and value risk, HML_t, to the equation. We obtain daily data for the market excess return, the size and value factor returns, as well as the risk-free rate from Ken French's website (http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html#Research). The risk-free rate is virtually zero on almost all trading days. Equivalently to the market model regression, γ provides an estimate for the average abnormal return of our treated portfolio of digital firms during the event window. The average abnormal return has to be multiplied with the number of days in the event window to obtain the CAAR. The coefficients can be interpreted correspondingly. The model is estimated using returns of 250 trading days before the event date, excluding the ten trading days immediately prior to the event date. Clustered standard errors by firm and trading days are in parenthesis. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

Table C.7: Value-Weighted Portfolio

	(1)
	Stock return
Alpha	0.036**
	(0.016)
Market return (S&P 1200)	0.473***
	(0.125)
21-22 Mar. 2018	-0.590***
	(0.159)
Observations	53,724
Firms	222
Adj.-R2	0.016

Notes: This table presents the results of the conditional market model with a value-weighted portfolio. It reflects the sum of each firm's market capitalization in the sample on each day in the estimation and event window. The model is the following: $R_{it} = \alpha + \beta\, R_{mt} + \gamma\, D_t + \epsilon_{it}$. R_{it} is the return of firm i on day t that is likely to fall under the scope of the digital tax proposal, R_{mt} is the return of the market index m (S&P Global 1200) on day t. D_t is a dummy set equal to 1 in the two-day event window, and ϵ_{it} is an error term. α provides an estimate for the alpha of an equally-weighted portfolio of all 222 treated firms and β is the estimate of the portfolio's market beta. The coefficient estimate of γ_i (and the corresponding standard error) is multiplied by two to account for the length of the two-day event window (Eckbo et al., 2007). γ can thus be interpreted as an estimate for the cumulative average abnormal return CAAR over the two-day event window. The model is estimated using returns of 250 trading days before the event date, excluding the ten trading days immediately prior to the event date. Clustered standard errors by firm and trading days are in parenthesis. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

Table C.8: Alternative Test Statistics

	(1)
Expected return estimation	Market model
21-22 Mar 2018	-0.69
Parametric test alternative	(-1.809)*
Corrado rank-sum test	(-2.438)*

Notes: The table depicts additional parametric and non-parametric test statistics for the main results (Bernard, 1987). The parametric test alternative is based on Kothari and Warner (2007) in Appendix Table C.5 and is calculated as $t_{parametric} = \frac{CAAR(0,1)}{\sqrt{s^2(CAAR(d))}}$, with $s^2(CAAR(d))$ as the variance of cumulated average abnormal two-day returns in the estimation period. The Corrado rank-sum test (Corrado, 1989) is calculated as $z_{Rank} = \frac{\sum_{t=0}^{t=1} \frac{1}{242} \sum_{i=1}^{242}(k_{it}-E(k))}{\sqrt{d \times s^2(k)}}$, with K_{it} denoting the rank of the abnormal return of firm i at day t in the time series. The expected rank *E(k)* is one-half plus half the number of time-series days and d is the number of days. The test statistic is assumed to be distributed asymptotic standard normal. Test statistics are in parenthesis. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

Table C.9: Comparison of Affected and Unaffected Firms

	(1)
	Stock return
Digital × Large × Event (21-22 Mar. 2018)	-0.832***
	(0.055)
Digital × Large	0.003
	(0.088)
Digital × Event (21-22 Mar. 2018)	0.149***
	(0.045)
Large × Event (21-22 Mar. 2018)	0.286***
	(0.025)
Digital	-0.013
	(0.083)
Large	-0.505***
	(0.03)
Event (21-22 Mar. 2018)	-0.615***
	(0.037)
Constant	0.601***
	(0.041)
Observations	4,203,540
Firms	17,370
Adj.-R2	0.003

Notes: The table presents the results of the estimation model: $R_{it} = \alpha + \beta_1 Large_i + \beta_2 Digital_i + \beta_3 Event_t + \beta_4 Large_i \times Digital_i + \beta_5 Large_i \times Event_t + \beta_6 Digital_i \times Event_t + \beta_7 Large_i \times Digital_i \times Event_t + \epsilon_{it}$. R_{it} is the return of firm i on day t. *Large*$_i$ is a dummy variable that identifies firms above the revenue threshold of 750 million euros. *Digital* is a dummy variable that identifies all firms that can be classified as digital. The interaction term *Large × Digital* identifies firms that are likely to fall in the scope of the "digital tax package". *Event* is a dummy variable that takes the value of one in the event window and ϵ_{it} is an error term. The model is estimated using returns of 250 trading days before the event date, excluding the ten trading days immediately prior to the event date. Clustered standard errors by firm and trading windows are in parenthesis. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

Table C.10: Alternative Event Dates

	(1)	(2)	(3)	(4)
	Stock return	Stock return	Stock return	Stock return
Alpha	0.038*	0.045*	0.012	0.028
	(0.019)	(0.019)	(0.023)	(0.023)
Market return (S&P 1200)	0.732***	0.718***	0.787***	0.909***
	(0.054)	(0.05)	(0.047)	(0.044)
26-27 Feb. 2018	-0.148			
	(0.67)			
15-16 Mar. 2018		-0.300		
		(0.285)		
4-5 Dec. 2018			-0.017	
			(0.23)	
12-13 Mar. 2019				-1.275***
				(0.046)
Observations	53,692	53,716	52,734	52,320
Firms	222	222	222	222
Adj.-R2	0.058	0.057	0.102	0.12

Notes: The table presents the results of the conditional market model $R_{it} = \alpha + \beta R_{mt} + \gamma D_t + \epsilon_{it}$. R_{it} is the return of firm i on day t that is likely to fall under the scope of the digital tax proposal, R_{mt} is the return of the market index m (S&P Global 1200) on day t, D_t is a dummy set equal to 1 in the two-day event window, and ϵ_{it} is an error term. α provides an estimate for the alpha of an equally weighted portfolio of all 222 treated firms and β is the estimate of the portfolio's market beta. The coefficient estimate of γ (and the corresponding standard error) is multiplied by 2 to account for the length of the two-day event window (Eckbo et al., 2007). γ can thus be interpreted as an estimate for the cumulative average abnormal return (CAAR) over the two-day event window. The model is estimated using returns of 250 trading days before the event date, excluding the 10 trading days immediately prior to the event date. Clustered standard errors by firm and trading days are in parentheses. Asterisks denote significance at the 1% (***) and 5% (**) and 10% (*) levels.

Table C.11: Alternative Event Dates – Increased Probability of Trade-War

	(1)	(2)	(3)	(4)
	Stock return	Stock return	Stock return	Stock return
Alpha	0.039**	0.042**	0.051***	0.037*
	(0.019)	(0.018)	(0.018)	(0.020)
Market return (S&P 1200)	0.747***	0.710***	0.703***	0.708***
	(0.056)	(0.044)	(0.043)	(0.042)
5-6 Mar. 2018	0.41			
	(0.718)			
18-19 Jun. 2018		-0.28		
		(0.651)		
2-3 Jul. 2018			0.098*	
			(0.051)	
17-18 Sep. 2018				-0.414
				(0.717)
Observations	53,700	53,454	53,400	53,070
Firms	222	222	222	222
Adj.-R2	0.06	0.069	0.065	0.068

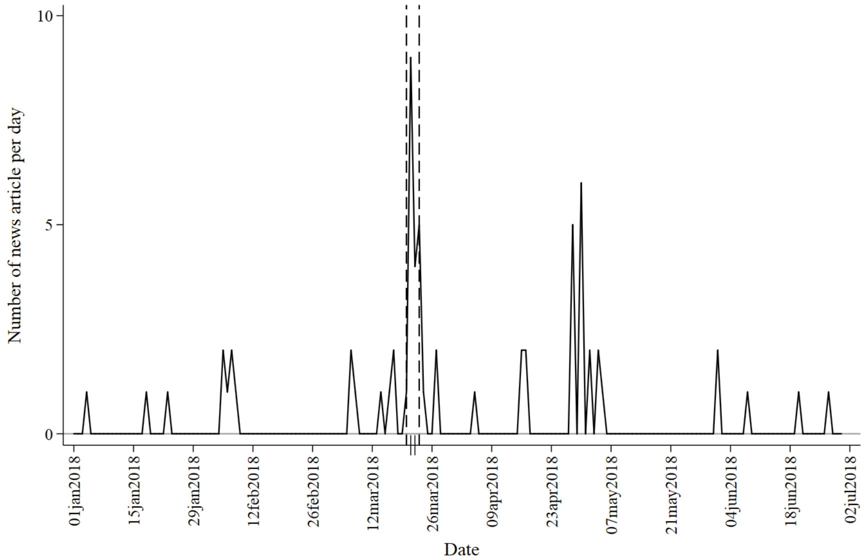
Notes: The table presents the results of the conditional market model $R_{it} = \alpha + \beta\, R_{mt} + \gamma\, D_t + \epsilon_{it}$. R_{it} is the return of firm i on day t that is likely to fall under the scope of the digital tax proposal, R_{mt} is the return of the market index m (S&P Global 1200) on day t, D_t is a dummy set equal to 1 in the two-day event window, and ϵ_{it} is an error term. α provides an estimate for the alpha of an equally weighted portfolio of all 222 treated firms and β is the estimate of the portfolio's market beta. The coefficient estimate of γ (and the corresponding standard error) is multiplied by 2 to account for the length of the two-day event window (Eckbo et al., 2007). γ can thus be interpreted as an estimate for the cumulative average abnormal return (CAAR) over the two-day event window. The model is estimated using returns of 250 trading days before the event date, excluding the 10 trading days immediately prior to the event date. The event dates mark dates with peaks in a Google Trends Analysis on the key phrase 'trade war'. They match with announcements of the U.S. government or retaliation responses by affected governments. We exclude any event that is too close to our main event date (i.e., all events ten trading days prior and post to 21 Mar. 2018). Clustered standard errors by firm and trading days are in parentheses. Asterisks denote significance at the 1% (***) and 5% (**) and 10% (*) levels.

Table C.12: Alternative Event Windows

	(1)	(2)
	Stock return	Stock return
Alpha	0.044**	0.044**
	(0.019)	(0.019)
Market return (S&P 1200)	0.716***	0.718***
	(0.049)	(0.049)
20 Mar. 2018	0.167***	
	(0.041)	
21 Mar. 2018	-0.380***	
	(0.043)	
22 Mar. 2018	-0.310***	
	(0.059)	
20-22 Mar. 2018		-0.517
		(0.418)
Observations	53,946	53,946
Firms	222	222
Adj.-R2	0.062	0.062

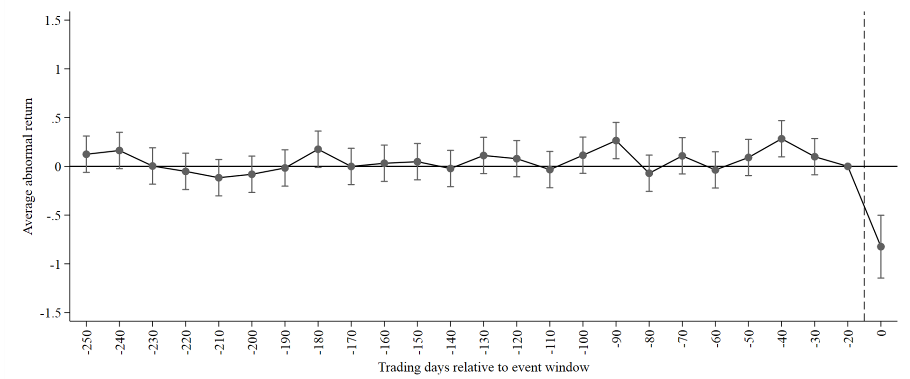
Notes: Column (1) presents the results of the conditional market model: $R_{it} = \alpha + \beta R_{mt} + \sum_{d=-2}^{d=1} \gamma_d D_{dt} + \epsilon_{it}$. Column (2) presents the results of the conditional market model: $R_{it} = \alpha + \beta R_{mt} + \gamma_3 D_{3t} + \epsilon_{it}$. R_{it} is the return of firm i on day t that is likely to fall under the scope of the digital tax proposal, R_{mt} is the return of the market index m (S&P Global 1200) on day t. In column (1), D_{dt} is a dummy set equal to 1 on the respective day. In column (2), D_{3t} is a dummy set equal to 1 in the three-day event window, and ϵ_{it} is an error term. α provides an estimate for the alpha of an equally weighted portfolio of all 222 treated firms and β is the estimate of the portfolio's market beta. In column(2), the coefficient estimate of γ_3 (and the corresponding standard error) is multiplied by three to account for the length of the three-day event window (Eckbo et al., 2007). γ_3 can thus be interpreted as an estimate for the cumulative average abnormal return (CAAR) over the two-day event window. The model is estimated using returns of 250 trading days before the event date, excluding the 10 trading days immediately prior to the event date. Clustered standard errors by firm and trading days are in parentheses. Asterisks denote significance at the 1% (***) and 5% (**) and 10% (*) levels.

Figure C.1: Factiva Search on Media Articles Covering "EU Digital Tax" Topics



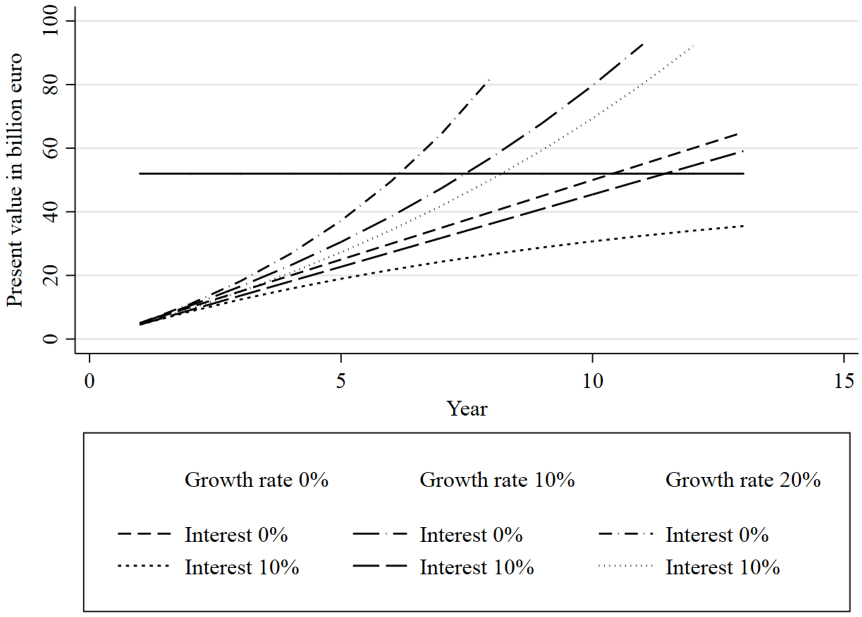
Notes: We plot the number of articles per day that cover the topic of "EU Digital Tax" based on a Factiva search over the first six months of 2018. Overall, we find 64 different articles on the topic. The dates enclosed by the light grey scattered lines are our event window. The crossing ticks on the x-axis represent March 21 and March 22, 2018, respectively.

Figure C.2: Comparison of Affected and non-Affected Firms in the pre-event Period



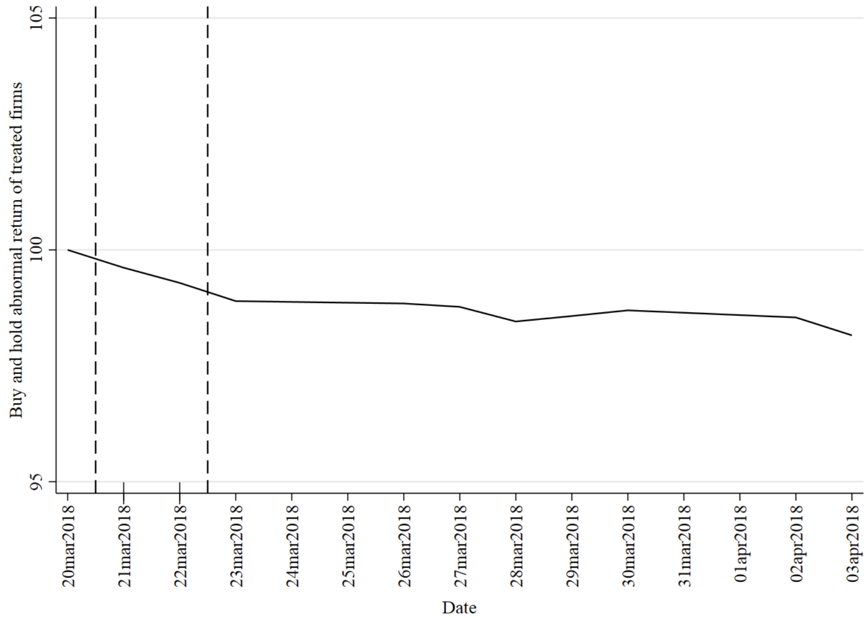
Notes: The graphic is based on the results of estimating the regression model from Equation 4.3: $R_{it} = \alpha + \beta_1 Large_i + \beta_2 Digital_i + \beta_3 Event_t + \beta_4 Large_i \times Digital_i + \beta_5 Large_i \times Event_t + \beta_6 Digital_i \times Event_t + \beta_7 Large_i \times Digital_i \times Event_t + \epsilon_{it}$. R_{it} is the return of firm i on day t. *Large*$_i$ is a dummy variable that identifies firms above the revenue threshold of 750 million euros. *Digital* is a dummy variable that identifies all firms that can be classified as digital. The interaction term *Large* × *Digital* identifies firms that are likely to fall in the scope of the "digital tax package". *Event* is a categorical variable that groups the firms' stock market returns into 26 bins relative to the event window. Each bin prior to the event window includes 10 trading days. The event window is from March 21 to March 22, 2018. The figure depicts the average abnormal return (β_7) of digital and large firms relative to all other firms over time. All coefficients are relative to the bin consisting of the eleven to 20 trading days before the event. We exclude the ten trading days immediately prior to the event date. The vertical line on each dot represents the 95 percent confidence interval.

Figure C.3: Comparison of Share Value Drop and Expected Revenue Gains



Notes: The graphic depicts the expected present value of future tax revenue gains from the DST in comparison to the estimated market value drop in the event window. The present value of future tax revenues is calculated as $PV_0 = TaxRevenue_0 \times \sum_{t=1}^{T} \frac{(1+g)^t}{(1+r)^t}$, where g refers to the expected annual growth rate of tax revenue per year t and r to the discount rate. The vertical line represents the market value drop of 52 billion euros. In line with the European Commission's impact assessment, we assume for this back of the envelope calculation a revenue of 5 billion euros in the first year and assume different growth rates. We depict two different interest rate scenarios in this graphic: 10 percent and 0 percent. The interaction of the black vertical line and the revenue estimates indicates after how many years the additional tax revenues offset the initial market value drop.

Figure C.4: Abnormal Buy and Hold Return



Notes: The figure displays the abnormal buy and hold return of an equally-weighted portfolio of all potentially by the proposals affected firms. The figure is indexed to 100 on March 20, 2018. The scattered lines enclose our event window March 21 and March 22, 2018.

D Appendix to Chapter 5

Table D.1: Subsidiaries by African Country and Year

Country	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	Total
Angola	9	35	46	59	69	143	227	283	389	599	672	785	820	757	4,893
Benin	4	4	7	7	31	48	55	62	74	78	96	113	134	129	842
Botswana	6	7	8	9	43	156	271	294	444	515	818	866	915	878	5,230
Burkina Faso	3	5	12	14	29	48	73	97	153	167	193	220	266	254	1,534
Burundi	3	3	2	2	7	14	17	29	29	30	44	50	60	61	351
Cameroon	14	20	23	28	96	133	153	166	199	210	222	244	294	286	2,088
Cape Verde	3	13	18	16	24	56	70	89	102	177	192	195	207	205	1,367
Central African Republic	0	0	1	2	11	13	19	27	35	26	35	36	42	38	285
Chad	1	2	3	6	17	19	23	27	36	41	42	48	59	61	385
Comoros	0	0	1	1	1	4	8	12	17	16	14	15	11	21	121
Congo	1	6	8	9	46	64	87	96	116	135	189	220	280	251	1,508
Cote d'Ivoire	22	38	42	50	149	191	230	253	320	358	471	525	643	666	3,958
Democratic Republic of Congo	4	10	17	15	47	92	126	161	216	243	306	348	464	467	2,516
Equatorial Guinea	0	2	1	3	15	29	32	38	51	59	75	82	99	91	577
Eritrea	0	0	1	2	2	3	3	8	13	15	12	15	14	16	104
Eswatini	3	3	3	3	12	51	78	97	114	140	209	219	258	261	1,451
Ethiopia	0	2	3	4	5	15	25	52	83	96	144	176	253	279	1,137
Gabon	6	13	22	23	73	92	123	128	188	208	235	271	312	287	1,981
Gambia	1	1	2	3	11	13	21	27	29	43	45	57	65	66	384
Ghana	25	39	44	43	87	190	258	308	446	508	651	1,897	1,959	1,997	8,452
Guinea	1	3	7	11	27	44	65	89	107	112	150	173	205	199	1,193
Guinea-Bissau	0	2	2	1	2	8	13	13	20	23	24	36	37	35	216
Kenya	18	71	67	76	105	261	381	479	587	791	1,005	1,137	1,390	1,515	7,883
Lesotho	3	3	3	3	5	36	48	57	69	72	111	380	381	577	1,748
Liberia	2	2	2	5	20	156	225	541	719	765	832	1,036	1,046	2,858	8,209
Madagascar	7	14	19	20	64	86	110	129	192	181	234	259	286	300	1,901
Malawi	8	10	11	12	21	69	89	117	159	167	224	248	298	297	1,730
Mali	1	2	4	6	29	50	85	109	153	154	179	195	238	244	1,449
Mauritania	0	3	3	5	16	42	52	60	67	82	109	127	143	134	843
Mozambique	4	25	39	44	48	140	211	287	357	596	827	991	1,123	1,132	5,824
Namibia	6	6	8	9	23	208	330	390	511	644	999	1,155	1,222	1,220	6,731
Niger	3	6	5	5	18	22	32	41	61	59	69	74	90	86	571
Nigeria	49	149	107	130	234	477	655	775	1,014	1,262	1,533	1,808	1,986	1,955	12,134
Rwanda	2	3	3	4	10	25	32	38	46	82	119	142	175	193	874
Sao Tome and Principe	1	1	1	1	3	5	7	9	14	36	44	57	61	50	290
Senegal	14	20	24	29	98	142	198	226	290	297	338	377	444	437	2,934
Sierra Leone	2	3	3	4	8	25	33	46	73	78	105	127	152	150	809
Somalia	0	0	0	0	4	4	3	7	8	9	9	10	11	15	80
South Africa	774	836	964	1,065	3,238	6,048	7,941	9,678	11,095	11,228	18,895	20,693	21,951	21,705	136,111
South Sudan	0	0	0	0	0	1	3	4	5	9	16	22	23	26	109
Sudan	2	3	10	13	19	58	76	97	181	182	198	222	231	215	1,507
Tanzania	12	14	20	22	37	156	212	277	416	490	652	729	843	902	4,782
Togo	3	4	4	5	19	32	43	59	76	81	89	103	123	119	760
Uganda	10	22	15	17	27	102	136	184	220	247	303	343	392	398	2,416
Zambia	15	25	34	39	43	122	178	235	331	373	550	610	688	692	3,935
Zimbabwe	25	28	30	35	51	196	239	331	457	526	883	978	1,050	1,080	5,909
Total	1,067	1,458	1,649	1,860	4,944	9,889	13,296	16,532	20,282	22,210	33,162	38,414	41,744	43,605	250,112

Notes: This table presents the number of subsidiary observations in the sample by African country and year.

Table D.2: Subsidiaries by African Country and Parent Home Country

Country	Belgium	France	Germany	Italy	Portugal	Spain	U.K.	U.S.	African Domestic Groups	African Multinationals	Rest of World	Total
Angola	41	254	86	135	2,064	172	214	186	297	780	664	4,893
Benin	14	264	50	13	0	12	25	15	60	154	235	842
Botswana	35	126	132	18	4	18	458	304	327	2,895	913	5,230
Burkina Faso	33	334	42	11	0	4	129	36	62	133	750	1,534
Burundi	27	47	7	5	0	0	8	9	21	113	114	351
Cameroon	32	779	43	74	4	42	146	105	110	229	524	2,088
Cape Verde	12	16	5	100	673	160	24	14	129	120	114	1,367
Central African Republic	3	94	22	9	0	7	24	6	8	41	71	285
Chad	8	157	8	0	0	3	11	11	14	80	93	385
Comoros	0	41	0	0	0	0	3	7	11	49	10	121
Congo	50	425	20	78	9	26	85	74	66	163	512	1,508
Cote d'Ivoire	112	1,284	85	200	13	89	275	147	346	456	951	3,958
Democratic Republic of Congo	337	239	52	105	6	12	240	77	219	320	909	2,516
Equatorial Guinea	13	124	4	7	18	172	30	83	26	29	71	577
Eritrea	10	11	0	13	0	0	14	0	20	3	33	104
Eswatini	11	30	34	25	9	0	151	64	171	844	112	1,451
Ethiopia	24	16	23	77	0	6	105	38	194	195	459	1,137
Gabon	13	834	33	47	14	46	112	81	96	207	498	1,981
Gambia	16	27	16	6	7	9	31	15	50	125	82	384
Ghana	114	322	102	101	17	101	366	357	3,886	1,126	1,960	8,452
Guinea	24	245	9	18	12	24	167	74	63	171	386	1,193
Guinea-Bissau	1	15	2	3	50	21	0	9	20	54	41	216
Kenya	62	279	260	120	30	81	1,034	599	1,416	2,103	1,899	7,883
Lesotho	5	45	32	4	0	0	116	50	800	599	97	1,748
Liberia	60	45	162	54	0	16	294	298	172	2,322	4,786	8,209
Madagascar	54	608	64	54	3	29	108	71	111	364	435	1,901
Malawi	5	61	39	19	43	0	233	134	250	598	348	1,730
Mali	12	299	28	28	8	11	93	41	78	209	642	1,449
Mauritania	31	126	23	10	0	61	30	19	156	113	274	843
Mozambique	27	182	93	229	1,776	123	538	139	347	1,086	1,284	5,824
Namibia	88	126	144	72	35	147	619	215	600	3,580	1,105	6,731
Niger	2	212	12	11	0	4	38	38	22	71	161	571
Nigeria	220	702	257	321	36	62	1,120	1,102	2,721	3,101	2,492	12,134
Rwanda	61	45	23	0	1	1	54	36	114	300	239	874
Sao Tome and Principe	2	9	0	0	98	12	1	1	8	97	62	290
Senegal	96	982	75	240	26	177	119	173	154	287	605	2,934
Sierra Leone	11	48	19	15	8	14	129	39	53	160	313	809
Somalia	0	23	1	0	0	0	3	0	11	17	25	80
South Africa	837	2,937	3,593	1,461	292	954	10,132	7,561	37,672	54,466	16,206	136,111
South Sudan	3	10	0	1	0	0	17	5	7	42	24	109
Sudan	3	41	7	10	0	0	72	9	637	351	377	1,507
Tanzania	44	138	127	97	9	18	650	263	518	1,028	1,890	4,782
Togo	22	234	55	14	1	7	44	22	31	153	177	760
Uganda	32	117	54	38	20	17	281	214	244	715	684	2,416
Zambia	39	111	72	49	0	6	535	225	473	1,342	1,083	3,935
Zimbabwe	9	90	111	87	8	15	1,110	289	1,168	2,514	508	5,909
Total	2,655	13,154	6,026	3,979	5,294	2,679	19,988	13,255	53,959	83,905	45,218	250,112

Notes: This table presents the number of subsidiary-year observations by African country and by the home country of the parent firm.

Table D.3: Parent Firm Presence by African Country and Parent Home Country

Country	Belgium	France	Germany	Italy	Portugal	Spain	U.K.	U.S.	African Domestic Groups	African Multinationals	Rest of World	Total
Angola	32	193	50	80	1,382	143	131	168	226	429	484	3,318
Benin	12	171	28	12	0	11	16	15	49	134	173	621
Botswana	29	77	66	16	4	5	194	166	182	939	567	2,245
Burkina Faso	21	264	28	10	0	4	74	34	55	114	398	1,002
Burundi	26	36	7	5	0	0	6	9	16	93	89	287
Cameroon	30	538	26	56	4	42	112	88	95	183	324	1,498
Cape Verde	10	16	5	66	537	137	19	8	81	104	76	1,059
Central African Republic	2	77	17	9	0	7	16	6	8	39	58	239
Chad	8	102	8	0	0	3	8	11	14	73	78	305
Comoros	0	38	0	0	0	0	3	7	9	33	9	99
Congo	46	289	19	69	9	26	63	68	52	138	373	1,152
Cote d'Ivoire	83	784	45	144	13	66	163	109	230	308	569	2,514
Democratic Republic of Congo	188	138	37	80	5	12	138	64	128	238	500	1,528
Equatorial Guinea	9	100	4	7	18	148	24	48	20	27	62	467
Eritrea	10	9	0	5	0	0	14	0	11	3	29	81
Eswatini	11	27	28	11	6	0	82	48	91	525	88	917
Ethiopia	23	13	20	70	0	6	83	38	103	103	293	752
Gabon	13	464	25	47	9	29	91	77	74	109	324	1,262
Gambia	14	25	13	3	5	9	31	12	20	118	78	328
Ghana	92	215	83	89	16	80	257	297	1,586	667	1,353	4,735
Guinea	23	178	9	16	12	23	81	70	48	137	305	902
Guinea-Bissau	1	14	2	3	40	21	0	9	9	51	39	189
Kenya	52	174	174	107	21	52	429	467	621	942	1,395	4,434
Lesotho	3	19	26	4	0	0	89	31	349	465	74	1,060
Liberia	27	30	62	29	0	14	142	171	158	1,734	1,389	3,756
Madagascar	41	513	47	32	3	25	74	54	84	205	288	1,366
Malawi	3	39	31	14	6	0	140	104	101	302	234	974
Mali	12	226	25	20	8	11	59	37	53	162	388	1,001
Mauritania	31	106	18	9	0	49	25	19	52	102	226	637
Mozambique	24	131	71	160	1,316	92	279	121	225	692	839	3,950
Namibia	64	67	91	45	21	82	197	103	309	1,077	674	2,730
Niger	2	134	12	11	0	4	38	38	22	64	139	464
Nigeria	134	362	182	201	31	61	492	723	1,404	1,103	1,677	6,370
Rwanda	45	34	23	0	1	1	47	27	56	243	188	665
Sao Tome and Principe	2	9	0	0	86	12	1	1	5	81	41	238
Senegal	61	668	34	229	25	157	80	154	123	186	455	2,172
Sierra Leone	11	36	16	14	8	14	98	37	35	144	208	621
Somalia	0	23	1	0	0	0	3	0	5	17	19	68
South Africa	466	1,240	1,458	727	138	424	2,211	3,341	14,445	5,560	7,128	37,138
South Sudan	3	6	0	1	0	0	14	5	6	39	24	98
Sudan	3	35	7	10	0	0	44	9	218	142	337	805
Tanzania	31	88	95	61	9	14	308	200	271	650	1,164	2,891
Togo	22	166	32	14	1	7	26	19	29	95	138	549
Uganda	18	79	48	31	20	12	184	176	158	547	537	1,810
Zambia	28	74	52	23	0	6	287	163	246	869	686	2,434
Zimbabwe	8	60	56	26	8	9	229	145	445	598	355	1,939
Total	1,774	8,087	3,081	2,566	3,762	1,818	7,102	7,497	22,527	20,584	24,872	103,670

Notes: This table presents the number of parent firm-country-year observations by the African country in which the parent has subsidiaries, as well as by the parent's home country.

Table D.4: DHS Survey Waves

Country	Years of Survey Rounds									
Burkina Faso	1998	1999	2003	2010						
Benin	2001	2011	2012	2017	2018					
Democratic Republic of Congo	2007	2013	2014							
Cote d'Ivoire	1998	1999	2011	2012						
Cameroon	2004	2011	2018	2019						
Ghana	1998	1999	2003	2008	2014					
Guinea	1999	2005	2012	2018						
Kenya	2003	2008	2009	2014						
Liberia	2007	2013								
Lesotho	2004	2005	2009	2010	2014					
Mali	2001	2006	2012	2013	2018					
Malawi	2000	2004	2005	2010	2015	2016				
Mozambique	2009	2011	2015							
Namibia	2000	2006	2007	2013						
Nigeria	2003	2008	2013	2018						
Sierra Leone	2008	2013	2019							
Senegal	2005	2010	2011	2012	2013	2014	2019			
Togo	1998	2013	2014							
Tanzania	1999	2003	2004	2007	2008	2009	2010	2011	2012	2015	2016
Uganda	2001	2006	2011	2016						
Zambia	2007	2013	2014	2018	2019					
Zimbabwe	1999	2005	2006	2010	2011	2015				

Notes: This table presents the years of DHS surveys waves used in the empirical analysis.The sample is restricted to DHS survey rounds (i) with available geographic data to match individual respondents' locations to the multinational firm subsidiary locations and (ii) in regions (10 km grid-cells) surveyed at least once before and after the U.K. tax rate reduction.

Table D.5: U.K. Tax Rate Reduction and Foreign Firm Presence in Sub-Saharan Africa

	(1)	(2)	(3)	(4)
	Log. Number Subsidiaries			
	Full Sample		OECD Firms	
U.K. Firm × 2009	-0.000	-0.014	-0.015	0.024
	(0.05)	(0.06)	(0.05)	(0.07)
U.K. Firm × 2010-2011	0.194***	0.148*	0.139**	0.206**
	(0.06)	(0.09)	(0.05)	(0.08)
U.K. Firm × 2012-2014	0.279***	0.255***	0.198***	0.317***
	(0.06)	(0.10)	(0.05)	(0.09)
U.K. Firm × Post 2014	0.296***	0.309***	0.209***	0.387***
	(0.06)	(0.10)	(0.04)	(0.09)
Obs.	93,510	19,966	49,980	12,644
Adj. R2	0.567	0.759	0.448	0.585
Balanced Presence	No	Yes	No	Yes
Firm Controls	Yes	Yes	Yes	Yes
Firm Country Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes

Notes: This table reports coefficient estimates of a difference-in-difference specification from Equation 5.1, which tests the effect of the U.K. tax rate reduction on the natural logarithm of the number of subsidiaries of a firm in a given sub-Saharan country (*Log. Number Subsidiaries*) for the sample period 2005 to 2018. *U.K. Firm* is a binary variable equal to one if the parent firm is incorporated in U.K. Separate *Post* indicators are included for the periods 2009, 2010-2011, 2012-2014, and after 2014, with effects measured relative to years prior to 2009. Columns (1) and (2) present results using all firms in our sample. Columns (3) and (4) present results restricting the sample of control firms to OECD parent firms. The models in odd-numbered columns include all firm-by-African country-year observations. The models in even-numbered columns only include firm-by-African country-year observations for firms that already had a subsidiary presence in a given African country in the period preceding the U.K. tax rate reduction (balanced presence). Firm data are from BvD Orbis. Standard errors are clustered at the firm level and presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table D.6: U.K. Tax Rate Reduction and Foreign Subsidiaries in Sub-Saharan Africa

	(1)	(2)	(3)	(4)	(5)	(6)
Panel (A)	*Log. Number Subsidiaries*					
U.K. Firm × *Post*	0.172***	0.173**	0.119**	0.198***	0.116***	0.122**
	(0.05)	(0.07)	(0.05)	(0.07)	(0.04)	(0.06)
Obs.	96,148	20,276	95,955	20,206	78,867	18,887
Adj. R2	0.542	0.678	0.628	0.720	0.537	0.687
Balanced Presence	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	.	.	Yes	Yes
Country-Pair FE	No	No	Yes	Yes	No	No
Year FE	Yes	Yes	.	.	.	.
Industry-Year FE	No	No	No	No	Yes	Yes
Country-Year FE	No	No	Yes	Yes	No	No

Panel (B)	*Log. Number Subsidiaries*					
	Foreign		UK vs. US		UK vs. JP	
U.K. Firm × *Post*	0.185***	0.249***	0.240***	0.205**	0.264***	0.400***
	(0.04)	(0.06)	(0.05)	(0.08)	(0.09)	(0.11)
Obs.	67,518	13,701	13,865	3,439	7,918	1,924
Adj. R2	0.428	0.449	0.423	0.394	0.396	0.334
Balanced Presence	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports coefficient estimates of a difference-in-difference specification from Equation 5.1, which tests the effect of the U.K. tax rate reduction on the natural logarithm of the number of subsidiaries of a firm in a given sub-Saharan country (*Log. Number Subsidiaries*) for the sample period 2005 to 2018. *U.K. Firm* is a binary variable equal to one if a the parent firm is incorporated in U.K. *Post* is a binary indicator equal to one for years after 2009. Panel (A) presents results using all firms in our sample. Panel (B) presents results restricting the sample of control firms to multinational firms, US multinational firms, and Japanese multinational firms. The models in odd-numbered columns include all firm-by-African country-year observations. The models in even-numbered columns only include firm-by-African country-year observations for firms that had an existing subsidiary presence in a given African country in the pre-period (balanced presence). Firm data are from BvD Orbis. Standard errors are clustered at the firm level and presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table D.7: Tax Reforms in OECD Countries and Multinational Firm Presence in Sub-Saharan Africa

	(1)	(2)	(3)
	Log. Number Subsidiaries (Africa)		
Tax Reform	0.25***	0.14***	0.12*
	(0.06)	(0.05)	(0.07)
Obs.	4,717	4,378	3,916
Adj. R2	0.025	0.842	0.847
Balanced Presence	No	No	No
Firm Controls	No	Yes	Yes
Firm Country Controls	No	Yes	Yes
Firm FE	No	Yes	Yes
Industry-Year FE	No	No	Yes
Year FE	Yes	Yes	.

Notes: This table presents coefficient estimates of staggered difference-in-differences regressions estimating the effect of tax reforms in OECD countries on the natural logarithm of the total number of subsidiaries of a firm in sub-Saharan Africa (*Log. Number Subsidiaries (Africa)*). The tax changes include Germany (2008), Canada (2008), Japan (2012), and Spain (2015). *Tax Reform* is an indicator variable equal to one for years after the tax reform in the respective country. Standard errors are clustered at the firm level and presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table D.8: Orbis Data Coverage of Multinational Firm Subsidiaries and Home Country FDI Outflow

	(1)	(2)	(3)	(4)
	Log. Number Subsidiaries			
Outward FDI Flow	0.165***	0.180***		
	(0.03)	(0.03)		
Outward FDI Stock			0.049***	0.050***
			(0.01)	(0.01)
Obs.	4,014	4,014	4,022	4,022
Adj. R2	0.050	0.087	0.214	0.252
Year FE	No	Yes	No	Yes

Notes: This table reports coefficient estimates of regressions estimating the relation between outward FDI from an OECD country to a sub-Saharan African country using data from OECD.Stat and the natural logarithm of the total number of multinational firm subsidiaries in a given sub-Saharan country from Orbis. *Log. Number Subsidiaries* refers to the total number of subsidiaries in a sub-Saharan country owned by multinational firms from one OECD country. *Outward FDI Flow* refers to the pairwise outward FDI flow from an OECD country to a sub-Saharan country during a calendar year. *Outward FDI Stock* refers to the pairwise outward FDI stock that an OECD country holds in a sub-Saharan country at the end of a calendar year. Robust standard errors are clustered at the country-pair and presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table D.9: Variation in the Effect of the U.K. Tax Rate Reduction on Foreign Subsidiary Presence based on African Statutory Tax Rates

	(1)	(2)
	Log. Number Subsidiaries	
	Lower Tax Rate	Higher Tax Rate
U.K. Firm × Post	0.230*	0.194***
	(0.12)	(0.05)
Difference	0.036	
	(0.13)	
Obs.	2,696	91,325
Adj. R2	0.515	0.549
Firm FE	Yes	Yes
Country-Year FE	Yes	Yes

Notes: This table reports coefficient estimates of difference-in-differences regressions estimating the effect of the U.K. tax rate reduction on the natural logarithm of the number of subsidiaries of a firm in a given sub-Saharan country (*Log. Number Subsidiaries*) for the sample period 2005 to 2018. *U.K. Firm* is a binary variable equal to one if the multinational parent is incorporated in U.K. *Post* is a binary indicator equal to one for years after 2009. The sample is partitioned at the African country level based on whether the subsidiary is located in a country with a lower corporate tax rate than the U.K. Standard errors are clustered at the firm level and presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table D.10: U.K. Tax Rate Reduction and the Likelihood of Foreign Subsidiary Presence in Sub-Saharan Africa

	(1)	(2)	(3)	(4)	(5)	(6)
Panel (A)	*New Subsidiary (0/1)*					
U.K. Firm × Post	0.012***	0.010***	0.011***	0.010***	0.016***	0.012***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Obs.	756,431	756,431	610,125	756,373	729,880	594,291
Adj. R2	0.060	0.103	0.073	0.102	0.116	0.114
Firm Controls	No	No	No	No	Yes	Yes
Firm Country Controls	No	No	No	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-Pair FE	No	No	No	Yes	No	Yes
Year FE	Yes	.	.	.	.	.
Industry-Year FE	No	No	Yes	No	No	Yes
Country-Year FE	No	Yes	No	Yes	Yes	Yes

Panel (B)	*New Subsidiary (0/1)*					
Control Group:	Foreign	OECD	Empires	UK vs. FR	UK vs. US	UK vs. JP
U.K. Firm × Post	0.011***	0.013***	0.022***	0.045***	0.016***	0.028***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Obs.	402,453	285,133	159,052	96,737	96,448	68,697
Adj. R2	0.091	0.088	0.067	0.083	0.085	0.102
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

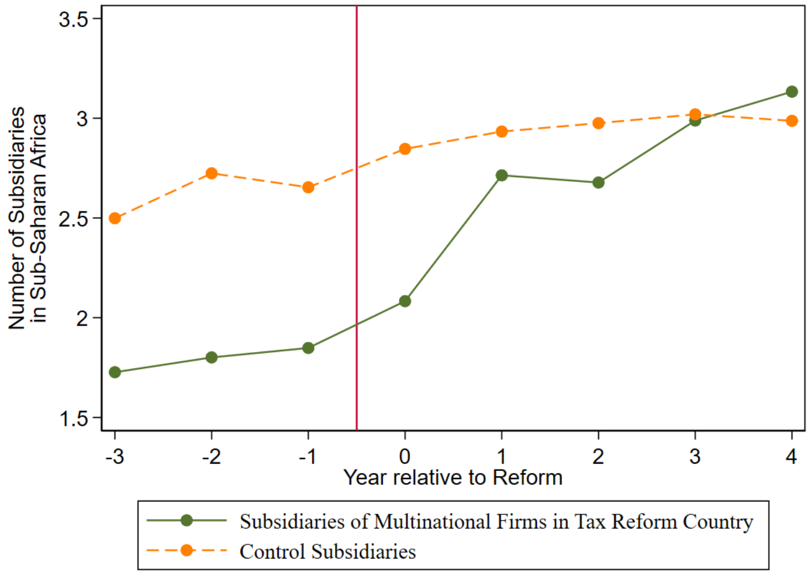
Notes: This table presents coefficient estimates of the linear probability model estimating the probability that a newly founded subsidiary in sub-Saharan African countries is U.K.-owned following the announcement of the U.K. tax rate reduction. *New Subsidiary* is a binary variable equal to one in the year the sub-Saharan African subsidiary was founded. *U.K. Firm* is a binary variable equal to one if a multinational firm is headquartered in U.K. *Post* is a binary indicator equal to one for years after 2009. Panel (A) presents results using all firms in our sample. Panel (B) presents results restricting the sample of control subsidiaries to those of foreign headquartered multinational firms (Column (1)), firms headquartered in OECD countries (Column (2)), firms headquartered in former colonial empires (Belgium, France, Germany, Italy, Portugal, Spain in Column (3)), French multinational firms (Column (4)), US multinational firms (Column (5)), or Japanese multinational firms (Column (6)). Standard errors are clustered at the firm level and presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table D.11: U.K. Tax Rate Reduction and U.K. Investment into Other Developed Nations

	(1)	(2)	(3)	(4)	(5)
	Log. Number Subsidiaries				
	France	*Germany*	*Ireland*	*OECD w/o UK*	*UK*
U.K. Firm × Post	0.047	0.071***	0.042*	0.075***	-0.100***
	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)
Obs.	704,700	1,170,370	129,342	18,214,475	1,465,856
Adj. R2	0.846	0.858	0.805	0.570	0.863
Firm FE	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes

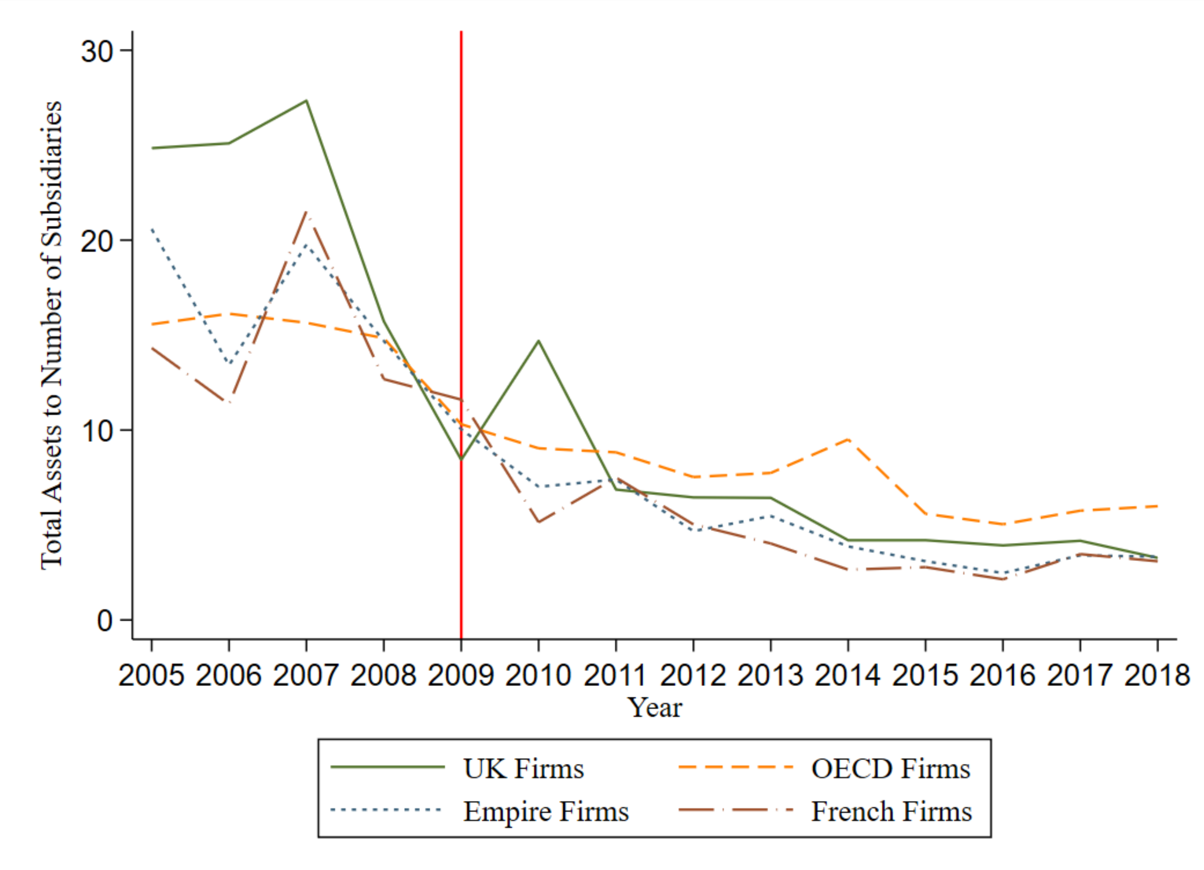
Notes: This table reports coefficient estimates of difference-in-differences regressions estimating the effect of the U.K. tax cut on the natural logarithm of the number of subsidiaries of a firm in a given country (*Log. Number Subsidiaries*) for the sample period 2005 to 2018. *U.K. Firm* is a binary variable equal to one if the multinational parent entity is incorporated in U.K. *Post* is a binary indicator equal to one for years after 2009. The sample consists of multinational firms incorporated in an OECD country. Column (1) presents results for the effect on of the U.K. tax rate reduction on investment in France, measured based on the number of subsidiaries in France. Columns (2) through (5) present results studying investment into Germany, Ireland, all OECD countries excluding U.K., and the U.K., respectively. Firm data are from BvD Orbis. Standard errors are clustered at the firm level and presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Figure D.1: Tax Reforms in OECD Countries and Multinational Firm Presence in Sub-Saharan Africa



Notes: This figure plots the development of the mean number of subsidiaries of multinational firms in sub-Saharan Africa in years relative to calendar years of major corporate income tax reforms in the multinational parent's country of incorporation. Treated subsidiary observations (green solid line) belong to multinational firms with parent entities that are incorporated in one of four OECD countries with a large corporate income tax reduction, including Germany (2008), Canada (2008), Japan (2012), and Spain (2015). Control observations (orange dotted line) refer to the mean number of subsidiaries in sub-Saharan Africa in the same calendar year belonging to firms incorporated in other OECD countries that do not experience a tax reform in the respective event years.

Figure D.2: Orbis Data Coverage of Multinational Firms and Firm Size Over Time



Notes: This figure plots the mean ratio of consolidated total assets to the number of subsidiaries in sub-Saharan Africa for multinational firms from 2005 to 2018. The mean ratio of U.K. multinational firms (green solid line) is compared to the mean ratio of multinational firms with parent entities incorporated in OECD countries (orange dashed line), multinational firms with parent entities incorporated in former colonial empire countries (blue dotted line), and French multinational firms (maroon dash-dotted line) in sub-Saharan African countries. The red vertical line marks the U.K. tax cut announcement. Firm data are from BvD Orbis.

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