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DISSERTATION

Corruption, Tax Abuse, and Financial Secrecy

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Declaration of Authorship

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Prague, October 28, 2020

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Abstract

This dissertation is composed of three papers that focus on different aspects of the relationship between the public sector and individuals who do not comply with the norms and regulations set by the government. I classify the ways in which individuals do so into two categories—corruption and tax abuse. Corruption, defined as the abuse of entrusted power for private gain, results in individuals obtaining more benefits from the public sector than is intended. Tax abuse, on the other hand, is defined as contributing less to the public sector than is intended. The last chapter of the dissertation focuses on financial secrecy, which I argue is the key facilitator of the two channels.

In Chapter 2, I show that companies that donate money to Czech political parties subsequently report abnormally high profits, pointing to preferential treatment that these firms enjoy as a result of their political connections: I conservatively estimate that the connected firms outperform their non-connected but otherwise similar competitors by 8 to 12% following the establishment of the connection, which is a higher effect than found previously for more developed economies. Importantly, however, I find that the effect virtually vanishes for non-connected firms that work closely with the public sector. This suggests that other forms of connections, such as personal ties, and those established at subnational levels of government, such as the regional and municipal level, are likely to have played a significant role in Czechia during its post-transition period.

In Chapter 3, we focus on international tax abuse by multinational corporations. Specifically, we ask which countries' tax revenues are affected most by international profit shifting of multinational corporations and how much. We begin by observing that the higher the share of foreign direct investment from tax havens, the lower the reported rate of return on this investment. We argue that the reported rate of return is lower due to profit shifting and provide illustrative country-level estimates of the scale of profit shifting for as many countries as possible, including low-income ones. This enables us to study the distributional effects of international corporate profit shifting. We compare our results with four other recent studies that use different methodologies to estimate tax revenue losses due to profit shifting.

Chapter 4 focuses on financial secrecy. We develop the Bilateral Financial Secrecy Index which quantifies the financial secrecy supplied to individual countries by secrecy jurisdictions. We then evaluate two major recent policy

efforts by comparing them with the results of the index. First, we focus on the blacklisting process of the European Commission and find that most of the important secrecy jurisdictions for EU member states have been identified by the lists. Second, we link the results to data on active bilateral automatic information exchange treaties to assess how well-aimed are the policymakers' limited resources. We argue that while low-secrecy jurisdictions' gains are maximized if a large share of the received secrecy is covered by automatic information exchange, high-secrecy jurisdictions aim not to activate these relationships with countries to which they supply secrecy. Our results show that secrecy jurisdictions successfully keep their most prominent relationships uncovered by automatic information exchange, and activating these relationships may thus be an effective tool to curb secrecy.

JEL Classification	D72, H7, D22, F21, F23, H25, F36, F63, F65, H26, O16
Keywords	corruption, tax havens, tax abuse, profit shifting, financial secrecy, secrecy jurisdictions

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Abstrakt

Tato disertační práce se skládá ze tří článků, které se zaměřují na různé aspekty vztahu mezi veřejným sektorem a jednotlivci, kteří nedodržují normy a předpisy stanovené vládou. Způsoby, jakými to jednotlivci dělají, rozděluji do dvou kategorií - korupce a vyhýbání se placení daní. Korupce, definovaná jako zneužití svěřené moci k soukromému prospěchu, má za následek, že jednotlivci získají více výhod z veřejného sektoru, než je zamýšleno. Vyhýbání se placení daní je na druhé straně definováno jako příspívání do veřejného sektoru méně, než je zamýšleno. Poslední kapitola disertační práce se zaměřuje na finanční tajemství, které je klíčovým zprostředkovatelem těchto dvou kanálů.

V kapitole 2 ukazuji, že společnosti, které darují peníze českým politickým stranám, následně vykazují neobvykle vysoké zisky, což poukazuje na preferenční zacházení, které si tyto firmy užívají v důsledku svých politických vazeb: konzervativně odhaduji, že propojené firmy vykazují o 8 až 12% vyšší zisky než jim podobní, nenapojení konkurenti. To představuje vyšší efekt, než jaký byl dříve odhadnut v rozvinutějších ekonomikách. Důležité však je, že mé výsledky ukazují, že tento efekt prakticky zmizí u společností, které nejsou napojené, ale úzce spolupracují s veřejným sektorem. To naznačuje, že v České republice během post-transformačního období pravděpodobně hrály významnou roli jiné formy politických vazeb, například osobní vazby či vztahy navázané na nižší než státní úrovni správy, jako je regionální a obecní úroveň.

V kapitole 3 se zaměřujeme na mezinárodní zneužívání daní nadnárodními společnostmi. Konkrétně se ptáme, které země jsou nejvíce negativně ovlivněny mezinárodním přesouváním zisků nadnárodních společností a do jaké míry. Začneme pozorováním, že čím vyšší je podíl přímých zahraničních investic z daňových rájů, tím nižší je vykazovaná míra návratnosti těchto investic. Předpokládáme, že uváděná míra návratnosti je nižší v důsledku přesouvání zisků a uvádíme ilustrativní odhady rozsahu přesouvání zisků na úrovni jednotlivých zemí pro co nejvíce zemí, včetně rozvojových zemí. To nám umožňuje studovat distribuční efekty mezinárodního přesouvání zisků společností. Nakonec porovnáváme naše výsledky se čtyřmi dalšími nedávnými studiemi, které používají různé metodiky k odhadu ztrát daňových výnosů v důsledku posunu zisku.

Kapitola 4 se zaměřuje na finanční tajemství. V této kapitole vytváříme Bilaterální index finančního tajemství, který kvantifikuje finanční tajemství dodávané do jednotlivých zemí z různých zemí, které toto finanční tajemství

poskytují. Poté vyhodnocujeme úspěch dvou hlavních politických iniciativ v boji proti finančnímu tajemství jejich porovnáním s výsledky indexu. Nejprve se zaměřujeme na proces vytváření černých listin Evropské komise a zjišťujeme, že většina důležitých jurisdikcí v oblasti finančního tajemství pro členské státy EU byla na seznamech identifikována. Poté porovnáváme výsledky indexu s daty o aktivních bilaterálních dohodách o automatické výměně informací, abychom posoudili, jak dobře jsou zaměřeny omezené zdroje jednotlivých států. Tvrdíme, že zatímco zisky jurisdikcí s nízkým tajemstvím jsou maximalizovány, pokud je velká část přijatého tajemství pokryta automatickou výměnou informací, jurisdikce s vysokým finančním tajemstvím mají za cíl neaktivovat tyto vztahy se zeměmi, kterým poskytují tajemství. Naše výsledky ukazují, že jurisdikce poskytující finanční tajemství úspěšně udržují své nejvýznamnější vztahy napokryté automatickou výměnou informací a aktivace těchto vztahů tak může být účinným nástrojem k omezení negativních efektů finančního tajemství.

Klasifikace JEL	D72, H7, D22, F21, F23, H25, F36, F63, F65, H26, O16
Klíčová slova	korupce, daňové ráje, daňové úniky, přesouvání zisků, finanční tajemství

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Chapter 1

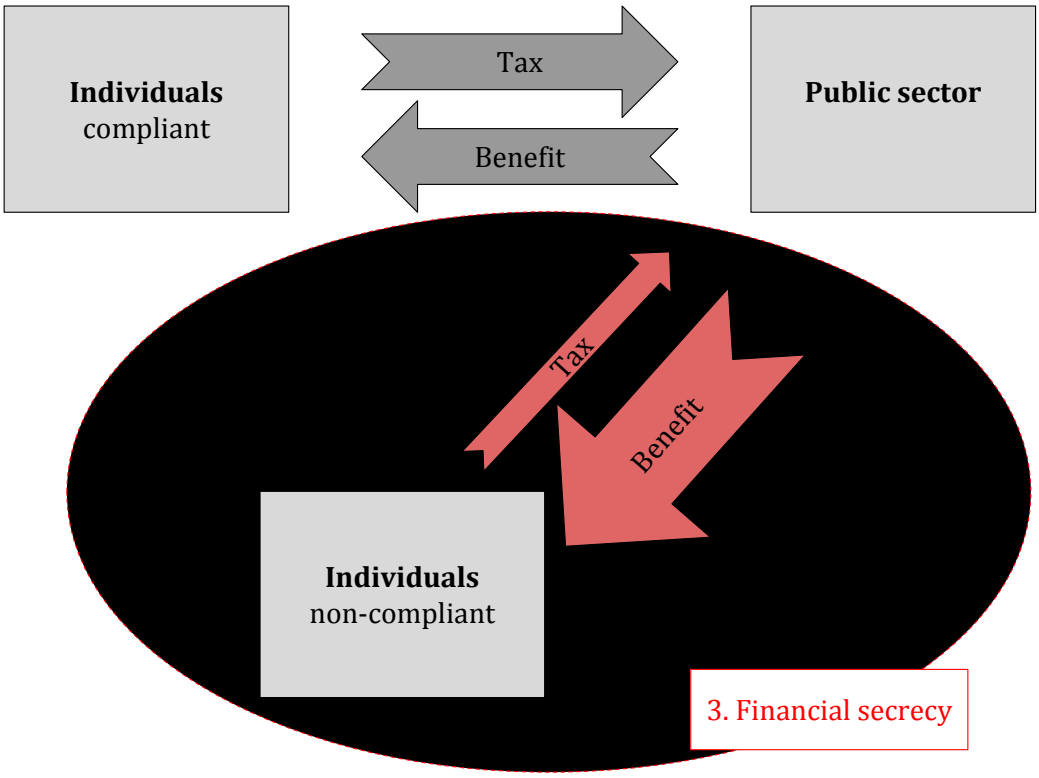
Introduction

When Wilbur Ross was appointed Commerce Secretary on February 28, 2017, he had failed to report his financial stake in the shipping firm Navigator Holdings—which put Ross, whose job it was to negotiate international trade deals on behalf of the US government, in a position of serious conflict of interests (Alexander 2018). On October 26, 2017, the New York Times contacted the U.S. Department of Commerce as part of an investigation of the Paradise Papers, a leak of confidential documents from the offshore law firm Appleby, which is known for facilitating business in low-tax and high-secrecy jurisdictions. The Paradise Papers included information about Ross' stake in Navigator Holdings, with potentially disastrous consequences for his position as Commerce Secretary as well as for the firm itself. On November 1, 2017, Ross shorted stock in Navigator Holdings, waited until the Paradise Papers were published five days later, and then exited his position, making a hefty profit. Following a public outcry, he transferred his ownership shares to an opaque trust for his children, thereby bypassing laws that prevent beneficial ownership of companies that could pose a risk of conflict of interests.

The relationship between individuals and the public sector is a remarkably intricate one. The modern *homo oeconomicus* is modelled in economics to have two 'virtues': (i) the infinite ability to make rational decisions, and (ii) the will to attempt to maximize their utility for private gain (Mill 1874). In the presence of a public sector and under any reasonable assumption on the shape of the social welfare function, however, the latter characteristic misaligns the individual's objectives with those of society as a whole. Indeed, it often makes the two orthogonal—individuals are often found to fail to pay their fair share of tax, and abuse of political power for private gain is all too common.

In this thesis I present my work focused on the relationship between individuals and the public sector. In particular, I am interested in the ways in which individuals fail to uphold their part of the social contract (Rousseau 1839)—in a broad sense, when they cheat in their dealings with the public sector, effectively stealing from other members of the society. The top part of Figure 1.1 depicts the relationship between the public sector and individuals who comply with the norms and regulations that their society has agreed upon. Primarily through taxes, which take on various shapes, they contribute to the public coffers, for which they, in return, receive benefits—infrastructure, social insurance, direct welfare transfers, education, or national security.

Figure 1.1: Relationship between individuals and the public sector in the presence of non-compliant individuals



Source: Author.

The bottom part of Figure 1.1 represents the existence of a group of individuals who do not comply with the social contract, reaping private gains at the expense of everyone else. The ways they may do so can be classified into two types. First, they may receive more benefits from the public sector than they are intended to. This channel is called corruption, defined in its broadest sense

as the abuse of entrusted power for illegitimate private gain. Corruption—and, specifically for the purposes of this thesis, political corruption—includes all types of bribery, extortion, and graft of elected or appointed government officials and their relatives or acquaintances.

The second channel in which non-compliant individuals effectively steal from other individuals is by contributing less in tax to the public coffers than they are intended to. To do so, individuals, often through the use of complex corporate structures, use loopholes and regulatory arbitrage (both domestic and international) to avoid or evade paying tax. In the title of this thesis, I intentionally use the term tax abuse, which includes both tax evasion (customarily defined as engaging in an activity that reduces one's tax liability and is in violation of the law) and tax avoidance (exploiting loopholes in the tax law in order to reduce one's tax liability in a way that is legal, but was not intended by the legislators). Denis Healey, former UK Chancellor of the Exchequer, once famously said that "the difference between tax avoidance and tax evasion is the thickness of a prison wall." The reasons why I do not like to distinguish between the two terms are twofold. First, the distinction is often not clear and can only be determined in court. Second, in a broad sense of the social contract, the distinction is meaningless—any activity that leads to lower tax due than was intended is a violation of the social contract, and thus constitutes our second channel.

I argue that the bulk of the activities that constitute these two channels is enabled by financial secrecy. In a recent survey of existing literature on tax compliance, Alm (2019) concludes that there are three main paradigms for the social contract to motivate individuals to comply with its provisions: (i) the enforcement paradigm, which underlines the importance of frequent audits and stiff penalties; (ii) the service paradigm, which acknowledges the role of enforcement but also recognizes the role of the tax administration as a facilitator and a provider of services to taxpayer-citizens; and (iii) the trust paradigm, which is based on the notion that individuals are more likely to respond either to enforcement or to services if they believe that the government generally and the tax administration specifically are honest (Alm 2019, p. 370). Of the three, the crucial paradigm is the enforcement paradigm, which is composed of two aspects, detection and punishment. While punishment is relatively easy to control (the government may simply decide to increase the penalties), detection is the tricky part. Information asymmetry is vast, audit is costly, and privacy protection often prevents complete transparency. However, there is an emerg-

ing literature that documents how financial transparency, when implemented carefully, solves the problem of detection.

In the remainder of this introduction, I summarize the three chapters of this thesis. Each chapter consists of a paper that is representative of the broader work that I have done in that particular area and which I also briefly describe in this introduction. Chapters 2 and 3 each pertain to one of the two channels through which non-compliant individuals abuse their relationship with the public sector at the expense of everyone else. Chapter 4 presents one of my contributions to the ongoing fight against financial secrecy which allows these channels to exist.

Corruption

In Chapter 2, titled *Value of Political Connections*, I show that firms that donate money to political parties in Czechia subsequently report abnormal profits. I started my work in the area of financing of political parties in 2014 when I joined the efforts of Econlab, a Prague-based NGO focused on public sector data analytics, and its PolitickeFinance.cz (Political Finance) project. Until 2017, information on private donations to political parties was only available in paper form as part of the parties' annual reports and they were physically stored in the Parliamentary library. In 2013, Econlab led a project that digitalized this data for the years 2006–2012 and published it online. When I started working on the project that ultimately resulted in the paper that forms Chapter 2 (Palanský 2020), in 2015 I spent several months taking photos of tables of donors and then retyping them into a spreadsheet, thereby digitalizing data on donations made in years 1995–2005 and 2013–2014. The resulting database contained information on the universe of over 56 thousand donations made by natural and legal persons to Czech political parties between 1995 and 2014.

Similarly to a broad array of existing literature (which I summarize in Section 2.2), I then use corporate donations to political parties as a measure of political connections. For each connected firm, I filter out from the universe of all companies ever registered in Czechia those that are similar to the connected firm in a number of observable characteristics. In my relatively strict matching procedure (as compared to the more widely used propensity score matching), I find one or more similar and non-connected peers for a total of 1,334 connected firms. I argue that for these firms I largely overcome the inherent endogeneity issue of corporate donations by including prior profitability

in my filter. I then focus on estimating the effect of political connections on the ultimate measure of firm performance—its financial profitability. Comparing the profitability of connected firms with their similar but non-connected peers, I find that the connected ones enjoy between 8 and 12% higher profits in the three years following the establishment of the connection. This effect, consistently with the notion that political connections are more important in countries with weaker institutions, is higher than previously found in Denmark or Italy (Amore and Bennedsen 2013; Cingano and Pinotti 2013). This paper represents the first empirical evidence on the effects of corporate political connections on firm profitability in a post-transition period.

While this paper does not (aim to) directly detect corruption, its results do provide robust evidence of connected firms obtaining additional benefits which exceed the cost they incur by making monetary contributions to politicians' campaigns. The channels through which they obtain these benefits are the focus of a large existing literature, including a number of ongoing projects of mine. Public procurement is consistently shown to be an important channel through which benefits are obtained, especially on lower-than-national levels of government. This includes evidence that uses data on corporate political donations in Czechia (Palanský 2014; Palanský et al. 2015; Palguta and Pertold 2017; Titl and Geys 2019). Other literature, which I review extensively in Section 2.2, points to other channels' varying importance in different settings—state grants, access to credit, stock market value premiums, and regulatory outcomes have all been shown to be affected by political connections.

I argue that transparency can provide an important means of public control in this area. The efforts of Econlab, including early results from Chapter 2, have helped to shape the current legislation on political party financing which introduces several measures that help the enforcement paradigm. A new Office for the Supervision of Financing of Political Parties has been set up in 2017. The Ofice publishes lists of donors online, independently checks party spending on which parties are newly required to report in more detail using transparent bank accounts, and oversees new limits on campaign spending and private donations.

Tax Abuse

In the area of tax abuse, my work is focused mainly on tax havens, i.e. jurisdictions that attract people and companies by offering them the opportunity to

bypass their local legislation and pay low or no tax. In chapter 3, titled *International Corporate Profit Shifting* and co-authored with Petr Janský (Janský and Palanský 2019a), we focus on estimating the scale of international profit shifting by multinational corporations to tax havens. We estimate how much money is shifted from individual countries and how much individual governments lose on tax revenue as a result. To do so, our paper uses the so-called foreign direct investment approach, as pioneered by UNCTAD (2015), and extends it in a number of ways. We find that around $420 billion USD is shifted from the 79 countries in our sample, resulting in $125 billion USD foregone in tax revenue. We discuss the distributional effects of international corporate profit shifting and compare our results with other existing estimates of its scale.

Apart from answering the question of how much money is actually at stake, I aim to answer two more research questions in my work. The first is which tax havens are most important. To this objective, I have worked on the first edition of the Corporate Tax Haven Index (Tax Justice Network 2019) which quantifies how much each jurisdiction acts as a tax haven and combines this data with the scale of economic activity by multinational corporations in that jurisdiction. The resulting ranking highlights which tax havens are most important. In a related paper (Janský and Palanský 2019c), we contribute to the ongoing conceptual and empirical discussions of how to assess the importance of individual tax havens. We conceptually distinguish between two areas of regulatory arbitrage – tax avoidance and financial secrecy—as well as between four different concepts of being of importance to other countries. Ultimately, we provide guidance on which indicators of tax havens and corresponding data sources may be used to what purpose and when, and we apply some of these in the paper's empirical part.

The third research question which I aim to answer in the area of tax abuse is how to best eliminate tax havens. In an ongoing project with Petr Janský and Jiří Skuhrovec, we show that firms that supply public procurement tenders in Europe are disproportionately often linked to tax havens via their ownership structures. We find that tenders are more likely to be won by firms linked to tax havens when they are less transparent and less audited. Since public procurement accounts for a large share of the market, we argue that governments could use their market power to incentivize firms not to use tax havens by discriminating against such firms in the tender auctions, thereby restoring fair competition.

Altogether, the case for transparency in eliminating tax abuse is simple

to make—detection of tax abuse is impossible without transparency. Policy measures to increase transparency which are currently being discussed (or have recently been implemented), such as public country-by-country reporting or beneficial ownership registers, will provide an effective means of making sure that everyone pays the tax they are supposed to pay.

Financial Secrecy

In my work, I argue that financial secrecy is the single most important facilitator of corruption and tax abuse. I am involved in a number of projects that aim to increase financial transparency. First, I have collaborated with the Tax Justice Network on the last two editions of the Financial Secrecy Index (FSI)[1], which ranks jurisdictions based on how much they contribute to the global problem of financial secrecy. The index has been published biannually since 2009 and has been very influential in policy debates around increasing financial transparency.

In Chapter 4, which is co-authored by Petr Janský and Markus Meinzer and is titled *Secrecy Jurisdictions and the Countries They Harm*, we extend the FSI to create a ranking of the most important secrecy jurisdictions for each country individually, in what we call the Bilateral Financial Secrecy Index (BFSI). We then evaluate two major recent policy efforts by comparing them with the results of the BFSI. First, we focus on the blacklisting process of the European Commission and find that most of the important secrecy jurisdictions for EU member states have been identified by the lists. Second, we link the results to data on active bilateral automatic information exchange treaties to assess how well-aimed are the policymakers' limited resources. We argue that while low-secrecy jurisdictions' gains are maximized if a large share of received secrecy is covered by automatic information exchange (AIE), tax havens aim not to activate these relationships with countries to which they supply secrecy. Our results show that secrecy jurisdictions successfully keep their most prominent relationships uncovered by AIE, and activating these relationships may thus be an effective tool to curb secrecy.

In an ongoing project, we use the six existing editions of the FSI to analyze the development of financial secrecy over time (Janský and Palanský 2019a). We find that financial secrecy has decreased on average—i.e. that financial

[1]For more information, visit the FSI website

transparency has improved—by at least 2–9% between 2011 and 2018. Most of the observed improvement comes from the advent of automatic information exchange. In a related paper, co-authored by Petr Janský and Tereza Palanská, we use the compiled panel data set of financial secrecy to test whether investors react to the changing landscape of financial secrecy by relocating their assets to jurisdictions that remain, or newly become, more financially secretive relative to other countries. We indeed find evidence of a significant positive effect of a change in relative secrecy on the value of third-country investors' assets. Importantly and in line with our theoretical predictions, we find that the elasticity is higher the higher is the change in relative secrecy, pointing to the heterogeneous benefits that different investors gain from using financial secrecy.

Curbing financial secrecy will not only eliminate the two channels in which non-compliant individuals contribute less to the public sector and obtain more benefits from it than they are intended to, but it will also help eliminate illegal economic activity which is now hidden under the veil of secrecy. This includes most of the shadow economy, money laundering, trade in drugs and arms, and human trafficking. I believe that the world will be a better place without these elements, and I hope that my research will contribute to help achieve that world.

Chapter 2

Value of Political Connections

2.1 Introduction

Corporate political connections and their effects have been a widely discussed topic in both academia and media for some time. From personal ties (friendships, relationships etc.) to more 'economic' links, such as campaign contributions or the provision of discounted services, connections between firms and politicians imply increased risks of conflicts of interest, corruption, rent–seeking, and discriminatory favorable treatment of the connected firms. Indeed, recent empirical literature has shown that in various settings, firms that are connected to political parties seem to enjoy significant benefits as compared to other, non–connected firms (Faccio 2006; Goldman et al. 2009; Cooper et al. 2010; Wu et al. 2012; Amore and Bennedsen 2013; Acemoglu et al. 2016). These benefits may take on diverse shapes: from rather indirect channels, such as legislation skewed in favor of the connected firms (De Figueiredo and Edwards 2007) or preferential access to finance (Claessens et al. 2008; Blau 2017), to more direct ones, such as influenced outcomes of public procurement auctions (Baltrunaite 2020) or stock market value premiums (Goldman et al. 2013).

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In this paper, I focus on one important and so far little studied type of political connections—direct corporate donations to political parties—and assess their effect on the ultimate measure of firm success: financial profitability. Specifically, I hypothesize that firms see monetary donations to political parties as investment which may pay off via abnormal revenues, which outweigh the costs of establishing these connections. As a result, connected firms report, on average, higher profits than non–connected firms. I thus do not aim to establish which specific channels are used by the connected firms to benefit from being connected; I refer to existing literature that presents such evidence in various settings. What I do estimate is the overall, compound effect of these connections on measures of financial performance of the connected firms.

To do so I analyze the case of Czechia in its post-transition period. Czechia is now a relatively stable, multiparty, parliamentary democracy which has, however, undergone a turbulent economic transition during the period that I study—between 1995 and 2014. Our current understanding of the effects of political connections, which I detail in the following section, is based primarily on case studies from countries whose institutions were generally more developed during the studied time periods, although there is voluminous literature suggesting that the effects might be different across settings. My focus on Czechia adds to this literature by representing the first such analysis for a post-transition period in a former Soviet bloc country. I address fundamental policy questions such as: Do firms view monetary donations to political parties as investment? Do connections established this way ultimately lead to higher profits of connected firms as compared to their non–connected competitors? How important are connections between business and politics in a post-transition setting?

Answering these questions requires an adequately careful approach to handle the inherent potential of endogeneity of the studied type of political connections and firm performance: firms that perform better financially are more likely to donate money to political parties, simply because they are more likely to be able to afford it. In this paper, I develop a conservative dynamic matching procedure that pairs connected firms with their non-connected but otherwise similar peers based on a number of observable firm-, industry-, regional-, and time-specific characteristics which are hypothesized to predict the likelihood of firms becoming connected. I argue that I thereby largely eliminate the self-selection problem which arises when using financial donations to political parties as a proxy for the connectedness of firms. I also estimate a number

of models that are likely to be more sensitive to this selectivity bias and compare their results with the ultimately preferred estimates that stem from the matching exercise.

I find that Czech connected firms do indeed perform significantly better than their non-connected peers following the establishment of a connection, with an estimated difference of 1.6 and 1.1 percentage points (7.8 % and 11.5 %) in terms of returns on equity and returns on assets, respectively. This translates into an estimated average return on investment into connections of around 900-1,100%. This represents an economically significant effect which is higher than those found by other studies of similar kind which focus on settings in which connections between business and politics are likely to be less important. However, I find that the effect virtually vanishes when we consider only firms that work closely with the public sector. These results suggest that while corporate donations to political parties are associated with better financial performance of the connected firms, other forms of political connections are also likely to be important, especially at the subnational levels of government. Overall, the results of this study are in line with some previous research in that they suggest that the relatively more direct channels, such as preferential treatment in public procurement auctions, are among those that contribute most to the advantage that politically connected firms may enjoy.

I present the contribution of this paper as threefold. First, as far as I know, this study is the first to focus systematically on the effects of corporate donations to political parties, rather than individual candidates or campaigns, on direct measures of financial performance of the donating firms. Second, I study the effects of political connections in a post-transition period, a setting that has so far been largely ignored in the empirical literature, although there is a considerable amount of theoretical and empirical work suggesting that the value of political connections in such environments may be substantial. Third, my results are in support of this work, and suggest that the value of political connections in countries that have relatively weak institutions is higher than in more developed economies. From this stems the recommendation to increase transparency of the relationships between businesses and politicians, thereby lowering their risks.

The remainder of this paper proceeds as follows. Section 2.2 details the mechanism through which connections are hypothesized to have an effect on financial performance and reviews the existing empirical literature from various other settings. Section 2.3 describes the empirical methodology that I use to

examine the effects of corporate donations on firm performance in Czechia and formulates the specific hypotheses that I test to find evidence for these effects. In Section 2.4, I present the used data sources—primarily, a novel data set on political donations made in Czechia between 1995 and 2014, and data on financial performance of all firms registered in Czechia during that time period. Section 2.5 presents the results of the analysis and describes the performed robustness checks. Finally, in Section 2.6, I discuss the implications of the results.

2.2 Related Literature and Background

A growing body of research suggests that firms connected to politicians enjoy significant benefits in various areas. Khwaja and Mian (2005) show that Pakistani firms associated with politicians enjoy better access to credit; De Figueiredo and Edwards (2007) find significant influence of private money on regulatory outcomes in the US telecommunications legislation; Goldman et al. (2013) document a significant positive effect of political connections of American S&P 500 firms on the allocation of public funds through procurement spending; similar results are obtained by Auriol et al. (2016) for Paraguay; Claessens et al. (2008) find that connected firms have substantially increased their bank leverage as compared to a non-connected control group after the 1998 and 2002 elections in Brazil. Cingano and Pinotti (2013) show that in Italy, connected firms enjoy an increase in domestic sales following the establishment of the connection; in Malaysia, Adhikari et al. (2006) showed that firms with political connections enjoy significantly lower effective rates than other firms; Duchin and Sosyura (2012) show that politically connected firms in the US are more likely to receive Troubled Asset Relief Program funds.

In a seminal contribution, Snyder (1990) formulates a simple model of a political donor's decision to become politically connected, suggesting that donors view political connections merely as investment. Naturally, these donors expect this investment to pay off in case their supported politician has, or comes to have, the political power to use public money to do so. Building on the theoretical framework outlined by Krueger (1974); Shleifer and Vishny (1994); Banerjee (1997); Acemoglu and Verdier (2000) and others, researchers have used various case studies to show that the value of connected firms does indeed increase following the establishment of a connection, and decreases following a sudden termination of such a relationship. Ferguson and Voth (2008); Cooper

et al. (2010); Goldman et al. (2009) and Johnson and Mitton (2003) all provide supporting evidence for the notion that establishing political connections helps add value to firms in terms of increased stock market returns, and, conversely, Fisman (2001); Jayachandran (2006) or Acemoglu et al. (2016) document abnormal negative returns of firms connected to politicians who lost their position or political capital. There is also some evidence that these investments only pay off in the short-term (Fan et al. 2007; Hersch et al. 2008; Aggarwal et al. 2012), pointing to the need of using more direct measures of firm performance.

In this paper I ask whether connections via direct monetary contributions to political parties help firms realize abnormal profits following the establishment of these connections. In a simplified model, a firm (which maximizes its profit π defined as the difference between its revenue r and costs c) that decides to donate money to a political party thereby increases its costs c by the amount of the donation, c_{don}. Since the firm expects this investment to pay off, it expects to receive additional revenue r_{don} (on top of what I call 'routine revenue', r_r) which it would not receive had it not become connected. In these terms, my main hypothesis in this paper is that $r_{don} > c_{don}$, implying $\pi_{don} > 0$, i.e. that there is a positive difference in profitability between connected and non-connected firms. Using data on Czech firms and their donations to political parties, I estimate the size of this difference. In so doing, I also indirectly estimate the size of the additional revenues that connected firms receive through the various channels mentioned above, since these revenues become the only unknown variable in the connected firm's profit equation:

$$\pi_r + \pi_{don} = r_r + r_{don} - c_r - c_{don} \tag{2.1}$$

Since we know that, by definition, $\pi_r = r_r - c_r$, Equation 2.1 simplifies to $\pi_{don} = r_{don} - c_{don}$, where c_{don} is the average donation in the sample and π_{don} is the average profit derived from being connected, a parameter that I estimate in this paper. This also allows me to derive an estimate of the overall additional revenue that firms are able to secure as a result of being connected.

Indeed, the existing (albeit scarce) research suggests that connected firms do indeed report abnormally high revenues. Cingano and Pinotti (2013) show that in Italy, connected firms enjoy a revenue premium, yielding an equivalent change in profits, of 5.7 % on average. Most of this increase is represented by domestic sales, with additional evidence pointing to potentially influenced outcomes of public procurement auctions. Amore and Bennedsen (2013) find

that even in a famously low-corruption economy such as Denmark, political connections boost firms' operating profits by 3.25 % on average, mainly due to a sharp increase in sales. The effect is stronger at the local governmental level, again pointing to the public procurement channel.

In general, public procurement seems to be among the most important channels through which firms benefit from their connections to politicians, with recent increased availability of data on public procurement leading to a range of convincing evidence from countries around the world. Witko (2011) shows that in the US, firms that had contributed to politicians subsequently received more public procurement contracts. In Lithuania, Baltrunaite (2020) finds that a ban on corporate contributions to parties decreased the donors' probability of winning a contract. Using regression discontinuity design and electoral results in Brazil, Boas et al. (2014) find that donating to a candidate that subsequently won office boosted the value of received contracts of the donors. Perhaps most related to this paper, Titl and Geys (2019) and Palguta and Pertold (2017) document that the public procurement channel is likely strong in Czechia as well.

In this paper I do not aim to estimate the importance of individual channels through which connected firms obtain the additional revenue, but rather the overall effect on the ultimate measure of firm success—financial profitability. This has at least three advantages. First, there are many potential sources of additional revenue for the connected firms which are virtually impossible to systematically measure or observe in the existing data. These include public procurement contracts of smaller values, some state subsidies, or regulatory outcomes that favor connected firms. Considering instead the firms' financial profitability allows to estimate the overall effect of all these channels combined. Second, higher revenue does not necessarily mean that the firm will make more profit, as increased revenues (for example through more supplied public procurement contracts) may bring increased costs. In my approach I test directly for the effect of connections on profitability, thereby assessing the true return on the investment that the donations represent. Third, an advantage of considering firm performance indicators based on financial profitability ratios as reported by individual firms rather than the more commonly used stock market returns is that they are less perception-driven, a concern that is relevant for the study of the effect of political connections on stock market returns, as connections are never perfectly observable. While accounting measures are far from being superior in capturing firm performance as compared to other, multi-

dimensional measures, the choice of these firm performance indicators arises as a consequence of considering all registered firms regardless of their ownership structure and legal status.

The other side of the coin to an analysis of the effects of political connections on business outcomes is the definition of political connections. Mainly due to unavailability of reliable data, the empirical literature defines political connections in different ways. The pioneering work by Faccio (2006) is the first rigorous international study to focus on a large scale on personal ties between politicians and firms. Specifically, Faccio (2006) identifies a firm as connected if one of the company's large shareholders or top officers is a member of parliament, a minister, a head of state or a close relative of a top official. The results of her study show that a firm's stock prices tend to increase significantly after a businessperson from the firm enters politics. Numerous researchers corroborated on these results using data on personal ties between politicians and firms in individual countries (Johnson and Mitton 2003; Khwaja and Mian 2005; Leuz and Oberholzergee 2006; Niessen and Ruenzi 2010; Goldman et al. 2013; Kostovetsky 2015; Baturo and Mikhaylov 2016; Amore and Bennedsen 2013).

The second commonly used measure of political connections, and one that I also use in this paper, are monetary campaign contributions to candidates or parties. There are two main advantages of using monetary contributions as indicators of being politically connected as compared to using personal ties. First, they carry a time stamp, which enables us to focus only on effects that are pronounced around or after the establishment of such connections. Moreover, some firms may become connected to different candidates or parties over time (or more of them at the same time, with overlapping intervals). However, we shall stay aware that the connection may have well been established much sooner than the donation was made. Similarly, the benefit a firm may obtain from being connected may accrue at a different time than the contribution, and we thus must treat the time stamp with caution. Second, and importantly for this paper, as opposed to personal ties which can only serve as binary variables, using the actual value of donations enables capturing the economic cost and importance of the connection.

Campaign contributions can either be made to individual candidates (as in the US, for example) or to political parties as a whole (as is the case in Czechia). Many studies use donations to individual candidates (Snyder 1990; Ansolabehere et al. 2004; Jayachandran 2006; De Figueiredo and Edwards 2007; Cooper et al. 2010; Witko 2011; Claessens et al. 2008), making their effects ar-

guably relatively well understood. However, little attention has so far been given to contributions to political parties[1], despite the fact that there are reasons to think that connections to a political party may be more efficient than connections to individual candidates (Miettinen and Poutvaara 2014; 2015). Importantly, this includes potential network effects within parties and their contributors, as discussed by Stark and Vedres (2012); Desmarais et al. (2015), or Grossmann and Dominguez (2009). In particular, firms that form such networks may benefit from the politicians' actions that are carried out primarily in favor of another network member. On the other hand, there are also channels through which connections may pay off to firms and at the same time effectively take away opportunities from all other firms, including those within the network. This includes the public procurement channel that I discuss above. In this paper, I take into account these effects and thereby contribute to the scarce empirical literature on this topic. Indeed, I know of no study that would assess whether firms that are connected within a network through direct financial contributions to political parties perform better financially.

The lack of reliable and voluminous data is a typical setback to the study of political connections. In this paper, I use a recently compiled data set covering all political donations made in Czechia between 1995 and 2014 as well as the best available data on economic performance of all registered firms in that time period. The data thus covers the post-transition period, which followed the establishment of market capitalism. According to the theoretical framework outlined by Rajan and Zingales (1995), economies in the early stages of capitalism are prone to be more relationship-based rather than market-based. As the economy develops, the role of connections is likely to decrease. This notion is supported by multiple studies focusing on empirical data in various stages of market-based capitalism (Adhikari et al. 2006; Faccio et al. 2006; Li et al. 2008; Boubakri et al. 2008). The nature of my data set and the political background of Czechia allows for a thorough analysis of the value of connections during the early stages of a capitalist economy, which, to my knowledge, has not been done before. I compare my results to studies from more developed economies and thus directly test the prediction of this strand of literature.

In addition to this, Czechia is an appealing case study for at least four reasons. First, as described above and in more detail in Section 2.4, the availability of data is exceptional in both its volume and the covered time period.

[1]The few papers that do use contributions to parties include the works that I reference above (Titl and Geys 2019; Boas et al. 2014; Baltrunaite 2020).

While most studies in this area focus only on publicly listed firms, which raises further concerns about self-selection, I draw on the universe of all registered firms in Czechia. To the extent that the choice of corporate form (public vs. private) and the decision to compete for government contracts or favorable legislation are correlated, studies that exclude private companies from the analysis are likely biased. The data set on political donations[2] is exceptionally comprehensive as well—political parties are obliged by law to disclose all received donations, and so any potential undeclared donations would have been made illegally. Second, during the studied period, the Czech law has not limited the amount of money donors may donate to political parties, and parties had not faced any campaign spending limits[3], which widens the potential for investment-motivated corporate political donations.

Third, taking into account various studies, surveys, and anecdotal evidence presented in the media which suggest that corruption and rent-seeking is a relatively widespread phenomenon in Czechia, the value of political connections is likely to be higher than in other countries (Lízal and Kočenda 2001; Faccio 2006; OECD 2013). Fourth, Czechia is a multiparty parliamentary democracy with relatively weak institutions[4], a setting in which the effect of corporate contributions to political parties on firm performance has so far not been explored. Importantly, Czechia has arguably undergone a similar transition as a number of other countries of the former Soviet bloc, and the effects that I estimate for Czechia are likely to apply for other countries with similar political systems, campaign finance systems, and institutions. The economic transition still represents an ongoing challenge to many countries in its various stages, and I believe the lessons learned from this analysis are applicable to these countries as well.

2.3 Research Design

In this section, I discuss the approach I use to assess the relationship between political connections and firm performance. My identification strategy relies

[2]For a thorough review of political financing in Czechia, see for example the works of Šimral (2015) and Palanský et al. (2015).

[3]This has changed in January 2017 when donations were capped at CZK 3 million per person (natural or legal) per year and limits on campaign spending were introduced.

[4]For example, likely the most widely used measure of institutional quality, the Worldwide Governance Indicators (see Kaufmann et al. (2011) for methodology), includes an indicator on "Control of corruption" which consistently places Czechia in the bottom 10 of OECD countries.

mainly on within-firm variation in performance, controlling for a number of firm-specific characteristics that are hypothesized to affect the likelihood of becoming politically connected, such as size, location, and previous profits. The construction of the models used in this paper is driven by one main concern: potential selectivity bias stemming from the fact that I use monetary corporate donations to political parties to define politically connected firms. Naturally, firms that perform well financially are more likely to be able to afford donating money to political parties, and the relationship between being connected and achieving superior financial results may thus be endogenous (Claessens et al. 2008; Faccio et al. 2006).

To overcome this issue to the largest possible extent, given data limitations, I develop a novel dynamic approach to matching connected firms with their similar, non-connected peers, and describe its main advantages: First, it is designed to account for firm- and sector-specific characteristics as well as time-varying conditions in an improved way as compared to the baseline pooled models which I explore later in this paper. Second, and importantly, I argue that by effectively excluding from the analysis those firms that significantly differ from the connected firms in their observable characteristics, the risk of self-selection bias is significantly reduced in the matching approach. In fact, in interpreting the results of the matching exercise I implicitly rely on the strong assumption that the scope of the observable characteristics adequately curtails any potential selectivity bias.

In the second part of this section I present a baseline cross-sectional model which aims to assess the effect of being connected on the performance of connected firms following the establishment of a connection, and I argue why this model is likely subject to the selectivity bias. In the third part I adjust the model so that it distinguishes between donations to parties present in the government and other parties. I explain in detail the inherent endogeneity concerns related to these models and discuss their implications for the interpretation of the results that these models yield.

2.3.1 Dynamic Matching

I use a dynamic matching approach to identify non-connected firms that are similar to their connected counterparts in a number of observable characteristics and compare the economic performance of both groups of firms, following

Faccio (2006); Dombrovsky (2008) and Boubakri et al. (2012b).[5] This approach is thus a standard subclassification exercise (as described for example by Rosenbaum and Rubin (1983)). The innovation over the methodology used by these researchers lies in the dynamic character of the matching which allows to mitigate the risks of estimation bias due to varying development of firms over time and due to the effects of business cycles on different types of companies and industries. The reason I match dynamically (and thus allow connected firms to match to different non-connected firms at different point in time) is that the time period I cover is significantly larger that in the studies mentioned above, and firms' various speeds of development are thus more likely to render an older match irrelevant. As far as I know, this is the first study that uses such dynamic subclassification matching to assess the effects of political connections on firm performance.

Let us define that a firm is politically connected in year t if it donated money to a political party in year $t-2$, $t-1$ or t. I use three consecutive years to identify connections because connections may take time to be exploited by firms and to be projected in their financial results (Acemoglu and Verdier 2000).[6] For each connected firm, I construct a dummy variable equal to 1 if at least one donation was made during the three years, and 0 otherwise. Then, I filter for similar firms using five criteria. First, I only keep firms that are registered as the same legal form of business entity. Second, I only keep firms which operate in the same industry sector based on their two-digit NACE classification (Dombrovsky 2008). Third, I filter out firms that operate in cities which are different in the size of their population by more than 20%.

[5] A different but related approach was taken by Boubakri et al. (2012a), who employ a two-stage regression model to first construct an instrumental variable estimating the probability of political connectedness of firms based on their location, size and other firm-specific characteristics. In the second stage, they use this variable to estimate the effects of political connectedness. There are, however, at least two reasons why this methodology is not suitable for this case study on Czech data. First, the longitudinal character of our data set does not enable the estimation of political connectedness based on firm characteristics, because for some firms, these vary significantly in time. Second, especially for an individual country study, this approach is not likely to resolve the endogeneity issue, since a brief look at the data reveals that better–performing firms are more likely to be larger in size, operate in relatively more capital–intensive industries, work closely with the public sector, and so on.

[6] This approach follows also from the notion that firms may view political campaign contributions as a form of short-term investment, as outlined for example by Hersch et al. (2008). As an illustration, let us suppose that a political donation made during 2010 is paid off by an influenced public procurement contract signed in 2011 and finished in 2012. Then, the full effect of the donation pronounced through the added profit made on the public procurement contract is not recorded in the financial result of the firm until the end of the financial year of 2012. For the results presented in this paper I use also different time periods as robustness checks (see below).

The fourth criterion concerns the size of the firm. Following Faccio et al. (2006) and Dombrovsky (2008) I use total assets as a proxy for firm size and filter out firms which differ by more than 20%. Fifth, I keep only firms that have declared similar profits in the previous year (again within a range of 20%). In light of the fact that monetary contributions are used as a proxy for political connections, including past profit as a matching criterion is especially important in reducing the likelihood of self-selection bias affecting our results. The filtering procedure employed here is relatively strict (as compared to the criteria used in other similar studies) in order to identify only firms that are very similar to each other in the observable characteristics that are hypothesized to predict being politically connected. Similarly, it is a more conservative approach than propensity score matching in that the criterion for a successful match is direct similarity rather than a similar average propensity.

By design of the filter, there may be none or more than one similar firms for each connected firm. In the former case, I disregard the connected firm from the analysis; in the latter, I take an average of the financial performance indicators across all matched non-connected firms. Using this matching procedure, I obtain pairs of connected and similar non-connected firms (or a set of non-connected firms) for each year.

Importantly, I employ the matching procedure individually for each year—this dynamic nature of the matching has at least two advantages over simple matching used by Dombrovsky (2008) or Faccio et al. (2006). Firstly, it accounts for the fact that firm characteristics, and thereby also their propensity to become politically connected, change significantly over time—two firms matched in year t may evolve significantly differently and therefore cannot be considered similar in year $t + 10$. Secondly, since I compare paired observations in each year individually, the changing overall economic situation does not distort the results.

I formulate the principal hypothesis tested in this section as follows: Firms that are connected to political parties through donations perform, on average, better than their non-connected but otherwise similar peers. In other words, I test whether there is a significant difference in return on equity (ROE) and return on assets (ROA) for connected and non-connected firms which are similar in terms of type of business entity, industry sector, location in which they operate, their size, and their previous profits. To do so, I employ two empirical techniques. First, I carry out a series of paired t-tests with the null hypothesis being that the means of the financial performance indicators of the two paired

samples are equal. Rejecting the null hypothesis would thus suggest that there is a statistically significant difference in the profitability of donating and non-donating but otherwise similar firms. Second, I run an event study of the effect of the contribution to a political party. Focusing on six years ahead and six years following the donations, I compare the means of financial performance indicators around the donations.

To assess whether the monetary value of donations themselves is important for the effect on firm profitability, I then divide the sample of connected firms to five quintiles based on the donations' value and run separate paired t-tests. I thereby test the hypothesis that a larger donation will increase the connected firms' profitability more than a smaller donation.

In the next part of the analysis, as discussed in Section 2.2, I hypothesize that firms that donate to the same political parties form networks of connected firms, potentially benefiting from being part of these networks, because policy actions and political decisions that favor one supporter of a party may benefit other supporters of this party (for example, via regulatory outcomes that favor a group of firms within the network). At the same time, however, these policy actions may act as substitutes rather than complements in the allocation of advantages across firms, which would predict a weaker effect of connections when taking into account partisan networks. To test this hypothesis, I carry out a series of tests for differences in performance of connected and non-connected firms which cluster the sample at the level of networks around individual political parties or groups of parties (for firms that donate to more than one party). Comparing these results to the paired t-tests described above, I estimate the strength and direction of these network effects.

To further investigate which types of firms are responsible for the potential differences in financial performance of connected and non-connected firms, I divide the sample in two different ways. First, I divide the non-connected, matched peers into those that do and those that do not work closely with the public sector (measured by *PubSec*, a binary variable equal to 1 if the firm has supplied at least 1 public procurement contract or has received at least 1 European grant since 2006; and 0 otherwise). If there is a difference in financial performance between these two groups of non-connected firms, other forms of connections that those identified in this paper are likely to play an important role. Second, I divide not only the non-connected firms, but also the connected firms by whether they work closely with the public sector, allowing for a more detailed analysis of the differences in financial performance of different groups

of similar firms.

2.3.2 Pooled Models

Due to the reasons outlined above, the matching exercise is likely to be a suitable approach to the analysis of the size of the effect of political connections. However, the effect should also be detectable in baseline cross-sectional pooled models. In this part I thus build a family of such models in which I consider the universe of all firms in Czechia and their reported financial results between 1993 and 2014. I hypothesize that on average, firms that are connected to political parties through donations perform significantly better than other, non-connected firms following the establishment of the connection.

To test this hypothesis, I use cross-sectional data on firm performance and consider a firm connected not only in the year in which the donation was made but also in two subsequent years. The reason why I build the model around multiple financial years is again that firms may be able to exploit their connections in different ways which vary in time that they take to project in the firms' financial reports. To capture these effects, I construct average measures of firm performance (ROE, ROA) over three consecutive years following the donation (including the year during which the donation was made) by assigning equal weights to observations at time t, $t+1$ and $t+2$. In so doing I disregard missing observations (not only in the inner part of the data set, but also on its edges defined by (i) the boundaries of the examined time period, (ii) existence of firms, and (iii) availability of data for each firm). Therefore, some data points, e.g. for the year 2013, are constructed as average values over two years only. I run additional robustness checks using three different periods around the time of the donation: (i) $t-2 \rightarrow t$; (ii) t only (i.e. no smoothing); and (iii) $t-1 \rightarrow t+1$. However, I prefer the smoothing period $t \rightarrow t+2$ because it best reflects the lagged reaction to connections established through donations—the benefits of being politically connected (such as obtaining more public procurement contracts) are likely to be projected in the firms' financial results with a delay.

Another issue to discuss here is whether to account for donations made to parties that were not in power during the year in which the donation was made. In this initial model, I do not differentiate between connections to parties in power and those not in power. The purpose is to first treat donations only as an indicator of closeness of the firm to politics—if a firm donates money

to a political party, it is hypothesized to thereby express interest in playing a role in politics, likely for own profit. In Section 2.3.3, I develop models that differentiate between donations to individual parties.

My first pooled model thus looks as follows:

$$Y_{Avg(t->t+2)} = \alpha + \beta_1 * Y_{t-1} + \beta_2 * DDon_t + \beta_3 * X + \gamma_t + \delta_s + \epsilon, \qquad (2.2)$$

where $Y_{Avg(t->t+2)}$ is the average of a firm performance indicator (ROE and ROA) over the years t to $t+2$; Y_{t-1} is the lag of the firm performance indicator; Don_t is a dummy variable equal to 1 for firms that donated money to a political party in year t, and 0 otherwise. X is a set of firm-specific control variables. Specifically, we include $PubInd$, a binary variable equal to 1 in case the firm operates in an industry which supplies public procurement of value above the median of all industries, and 0 otherwise (Amore and Bennedsen 2013); $PubSec$, a binary variable equal to 1 if the firm has supplied at least 1 public procurement contract or has received at least 1 European grant since 2006 (due to unavailability of data from previous years), and 0 otherwise; $LocSize$, a variable constructed by classifying cities in which firms are headquartered into 6 categories by population with boundaries set at 5, 20, 80, and 200 thousand and 1 million inhabitants; $FirmSize_t$, a variable controlling for the size of the firm at time t, constructed as the natural logarithm of the firm's total assets reported in year t; and $Leverage_t$, the ratio of liabilities to assets at time t. Lastly, γ_t are year-fixed effects, δ_t are industry sector-fixed effects (at the 2-digit NACE level), and ϵ is the error term. The coefficient of interest in the first model is β_2: a positive and statistically significant estimate would suggest that firms that donate money to political parties financially outperform other firms, controlling for a number of firm characteristics.

As an extension to the first model I replace $DDon_t$ by Don_t, which represents the actual value of the political donation made in year t:

$$Y_{Avg(t->t+2)} = \alpha + \beta_1 * Y_{t-1} + \beta_2 * Don_t + \beta_3 * X + \gamma_t + \delta_s + \epsilon \qquad (2.3)$$

This allows for the economic importance of the donation to be pronounced in the model, but naturally reduces our sample to only connected firms. I estimate this model to test the hypothesis that corporate donations to polit-

ical parties can be regarded as actual measures of the level of connectedness (and not only as a proxy variable for being politically connected). A significant positive estimate of β_2 in this model would suggest that higher donations may allow the donating firms to obtain more benefits from the politicians. A non-significant estimate could be explained by the argument that the officially declared corporate donations to political parties may not be the only connections at play.

Since a likely channel through which connections may benefit firms is through public procurement (see Section 2.2), and thanks to the exceptionally good coverage and quality of public procurement data in Czechia (see Section 2.4), I carry out an additional test in which I divide the sample into five quintiles based on the volume of public procurement supplied by firms within the same sector. The aim is to test whether the effects of connections are different across industries with different intensities of cooperation with the public sector.

A possible drawback of the approach taken in these models is that we may not be able to control for all firm characteristics which influence their profitability, such as managerial skills or particular market distortions that may significantly help firms succeed or fail. This issue could be partially solved by using a fixed-effects model with a varying intercept for each firm, however, our data set is not balanced and long enough to allow for this approach. Furthermore, these models are not robust to variation in favourability of the overall economic situation over time. In times of economic crises, the value of connections (as measured by the financial performance of the connected firms) may be different than in other times.

Last but certainly not least, likely the most pressing and inherent issue that pertains to these models is that firms that perform well financially may be more likely to donate money to political parties. On the other hand, donations are usually relatively small compared to the firms' profits, and there are thus many firms which could easily afford to donate but do not do so. In the matching strategy developed above, I take into account not only a number of firm characteristics but also previous profits of the matched firms, thus significantly reducing the risk of selectivity bias. I construct the models outlined in this section to test precisely for these effects. Significantly different results from these baseline models and from the matching exercise would suggest that endogeneity is indeed in play.

2.3.3 Party in Power Pooled Models

Lastly, I construct a family of models to assess the importance of connections to political parties which are in power as compared to connections to other parties. Only connected firms are thus considered in the models presented in this section. Thereby I partially solve the problem of endogeneity of political donations—since I am using firms that are connected through donations to parties which are not in the government as a control group, I overcome the issue of more successful firms being more likely to donate money to political parties. Claessens et al. (2008) used a similar argument in their difference-in-differences specification, comparing firms connected to the winning party with those connected to the losing party. A possible drawback of this approach is that different parties may be in power on different levels of government, while connections may be exploited at more government levels simultaneously.

I build the models in this section on the basis of the previous one but include a dummy variable $Power_t$ being equal to 1 when the donation was made to a party which was present in the national government in year t; and 0 otherwise.[7] I again construct two models, the first one including a dummy variable indicating whether or not firm i has donated money to a political party in year t and the second including the actual donated amount. An important caveat of the latter model is that there remains the issue of more successful firms being more likely to donate more money to political parties than less successful firms.

$$Y_{Avg(t->t+2)} = \alpha + \beta_1 * Y_{t-1} + \beta_2 * Power_t + \beta_3 * X + \gamma_t + \delta_s + \epsilon \qquad (2.4)$$

$$Y_{Avg(t->t+2)} = \alpha + \beta_1 * Y_{t-1} + \beta_2 * Don_t * Power_t + \beta_3 * X + \gamma_t + \delta_s + \epsilon \quad (2.5)$$

where $Y_{Avg(t->t+2)}$ is again the average of a firm performance indicator (ROE, ROA) over the years t to $t+2$; Y_{t-1}, Don_t, $Power_t$ and X represent the set of variables defined above and in the description of models formulated in Sections 2.2 and 2.3.

[7] This classification is somewhat tricky, because the composition of the government changes following the elections (and not on January 1). For the purposes of this paper, I classify as a governing party in year t the ones that have been in power at least 6 months of year t.

With Czechia being a relatively decentralised country, the lower levels of the Czech government enjoy significant autonomy in spending. It is therefore important to also distinguish whether a firm is connected to parties in power at the national level or those in power at other levels. A natural candidate is the regional level of government, however, considering this level of government brings about a number of other challenges given our measure of political connectedness. For example, if different parties are in power in different regions, but donations are recorded at the national level, one is not able to determine precisely in which region the firm is connected. To investigate further the effect of being connected to the party in power, I construct one additional model that exploits, similarly to Titl and Geys (2019) and Palanský (2014), the substantial shift in political power in the regional governments following the 2008 elections in which the incumbent Civic Democratic Party lost its position to the Czech Social Democratic Party in all 13 Czech regions. In this model, I restrict the sample to two election periods around this election and assess whether firms connected to the party in power have fared better than those connected to the other parties.

$$Y_{Avg(t->t+2)} = \alpha + \beta_1 * Y_{t-1} + \beta_2 * LocalPower_t + \beta_3 * X + \gamma_t + \delta_s + \epsilon \quad (2.6)$$

where $LocalPower_t$ is a dummy variable equal to 1 in case the donation was made to the party in power in year t, and 0 if it was made to another party, and the remaining notation stays as defined above.

2.4 Data

In this section, I describe the data sources used in the analysis and present some descriptive statistics. I use two main data sets. First, I use data on all donations to political parties made by legal persons in Czechia between 1995 and 2014. Second, I use data from a private database called Magnus, which is the most advanced data set on financial results and other information on Czech firms. I merge the data from these two sources and add other information about firms—such as their operating sector, size, location, or public procurement outcomes—from the Business Registry and other sources.

Political Donations Data

In Czechia, information on the financing of political parties is available to the public in the form of lists of donors attached to the parties' annual reports. Until 2017, when the new Office for the Supervision of the Financing of Political Parties and Movements (ÚDHPSH) was set up and started publishing the reports online, they were only available in the physical form in the Parliamentary library, which made computational analysis of the data incredibly tedious. EconLab, a Prague-based economic think-tank focused on public data analysis, has collected this data for the years 2006 onwards and published it online on the website of the project PolitickeFinance.cz (Political Finance), making it available for download and further analysis to other researchers, journalists and the general public. For the purposes of this paper, the database was extended to cover retrospectively the entire time period for which the reports are available, i.e. from 1995 onwards. As of March 2016, the database contained 56,696 donations of total value of more than CZK 3,06 billion. A summary of the database for parties present in the Chamber of Deputies of the Czech Parliament at the time of this writing is provided in Table 2.1.

Table 2.1: Summary of the database of corporate political donations in the Czech Republic, 1995-2014, parties present in the Chamber of Deputies as of March 2014.

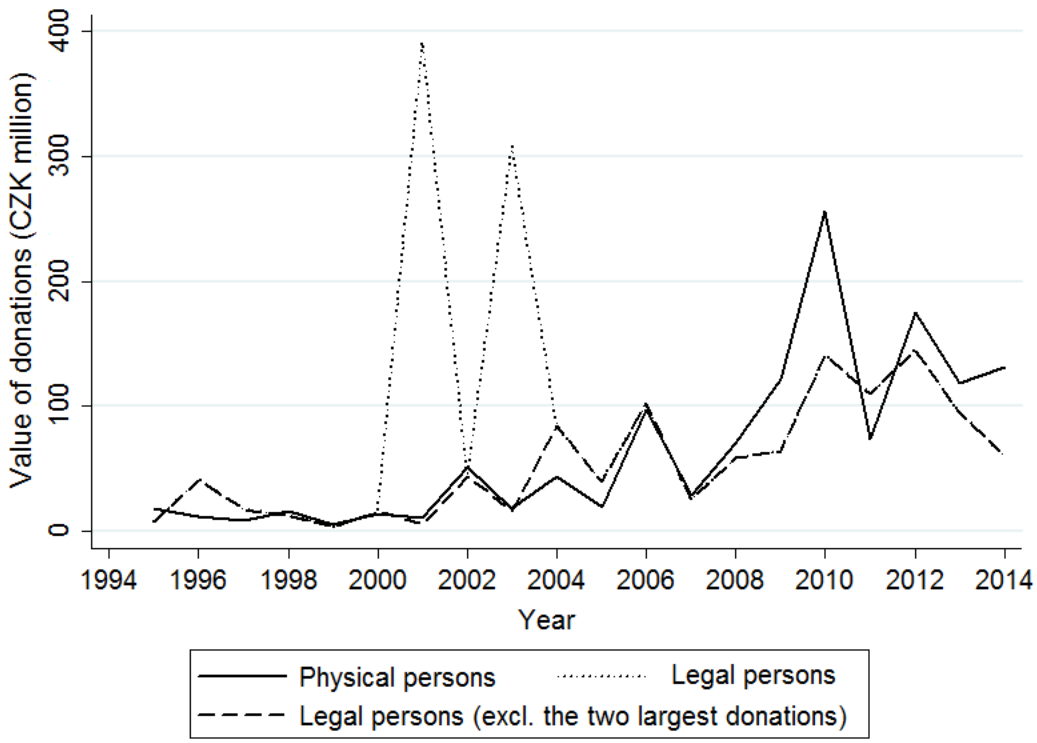
Party	**Number of donations**	**Sum of donations**	**Total donations per year***
ANO 2011	611	47,748,468	15,916,155.87
ČSSD	914	1,032,348,235	51,617,411.75
KDU-ČSL	826	44,708,752	2,235,437.61
KSČM	93	1,557,351	77,867.57
ODS	6,818	448,874,113	22,443,705.66
TOP 09	486	82,534,317	13,755,719.53
Úsvit	1	30,000	15,000
TOTAL	9749	1,657,801,237	106,061,298

*Sum of donations divided by the number of years in which the party existed.
Source: Author based on data from PolitickeFinance.cz.

Donations vary significantly in value over time. Since the late 1990's, their average value per year has increased markedly, as reported in Figure 2.1. The

increased importance of private money in politics during the transition and post-transition periods is well documented by Šimral (2015). In Figure 2.1 I include a line displaying the value of donations excluding the two largest non-monetary donations made by *Cíl, akciová společnost v Praze*[8] to ČSSD in 2001 and 2003, respectively, because they exceed the next donations in value by more than 1500 %.

Figure 2.1: Value of donations to Czech political parties over time.



Source: Author based on data from PolitickeFinance.cz.

The value of donations from both physical and legal persons has had an increasing trend during the examined time period. In the more recent years, the average total value of donations from physical persons slightly exceeds the value of legal persons' donations. We can clearly observe the peaks in years when major elections take place. The most important elections in Czechia, those to the Chamber of Deputies, took place in 1992, 1996, 1998, 2002, 2006, 2010 and 2013. Two elections (1998 and 2013) were snap elections, in which

[8] *Cíl, akciová společnost v Praze* is a company owned entirely by ČSSD and its reported main aim is to print and publish or sell advertisement materials. This company alone donated more than CZK 930 million to ČSSD over the examined time period.

the peak is not as significant. One larger peak is observed in 2012, which can be explained by the creation of ANO 2011, a party built by and around the Czech oligarch Andrej Babiš, which relied markedly on large donations in the first year of its existence. Political party financing in Czechia has been recently under more scrutiny from non-governmental organizations and academics who have analyzed potential risks of conflicts of interests at both the national and local level (Palanský et al. 2015).

Firm Performance Data

Data on firms' financial performance was obtained from *Bisnode Czech Republic*'s private database called Magnus, which is based on the Czech Document Registry of the Business Registry, run by the Ministry of Justice of the Czech Republic. Czech firms are obliged by law to publish their reports in this registry, although some of them do not do so on time (and may face fines for that). Magnus is then hand-cleaned and complemented by other sources of firm data. The data set used in this study includes the universe of all legal persons that have ever operated in Czechia. Financial data are available from the year 1993 onwards.

The data set includes three main variables that describe the financial performance of firms: Assets, Equity and Earnings Before Taxes. I choose to include the above-mentioned variables in this analysis because they allow for the creation of the most commonly used indicators of financial performance of firms—following Li et al. (2008), Amore and Bennedsen (2013) and numerous others, I construct two measures of firm performance: return on equity (ROE) and return on assets (ROA).

A few alterations to the data on Capital and Assets had to be made. Firstly, negative values of Assets, which were reported likely due to misguided accounting standards, are excluded from the analysis (this step reduces our sample by 0.14 % observations). Secondly, negative values of Capital, which were also most likely reported due to unusual accounting principles, are replaced by the 'Registered capital' which represents the reported initial capital of the company at the time of its foundation. This step alters approximately 21.75 % of observations. This may seem relatively significant, however, since Capital serves in our models only as a scaling variable in the construction of ROE, the explanatory power of the variable is maintained. Third, since extreme outliers in the data set would cause our estimations to be biased, I trim both firm

performance indicators—return on equity and return on assets. In doing so, I follow an approach common in the literature: dropping observations which fall outside the $\langle -1, 1 \rangle$ interval (see, for example, Beaver and Ryan (2000)). As a robustness check, I additionally test my models using 2 other trimming criteria (at the 1^{st} and the 99^{th} percentile values and at the 5^{th} and 95^{th} percentile values) and also 3 winsorization criteria (at the same boundaries that are used for trimming). I am inclined to use trimming rather than winsorization for my preferred results, due to the most likely reason that outliers are present in the data, which is misreporting or nonstandard reporting practices of one of the two variables used to construct the firm performance indicators. For the same reason, I consider it more appropriate to use trimming at the $\langle -1, 1 \rangle$ interval rather than using percentiles as trimming boundaries.

The final data set contains 474,749 firms and 2,595,654 yearly observations, averaging 5.46 years of data per firm. This is caused not only by the fact that many firms have only existed for a few years, but also by other factors—firms sometimes do not publish their annual reports in the Business Registry on time even though they are obliged to do so by law, and some documents are published in low quality which makes their inclusion in the Magnus database impossible. Despite these issues, the Magnus database presents the best available data on the financial performance of Czech firms. The database is summarized in Table 2.2.

Table 2.2: Descriptive statistics of the database of firm results

Variable	Obs	Mean	Std. Dev.	Min	Max
ROE	2,583,542	-2,112.75	1,027,881	-1.39E+09	3.11E+07
ROA	2,578,658	-228.17	394,576.50	-3.74E+08	4.40E+08
ROE, trimmed at $\langle -100, 100 \rangle$	2,067,086	9.66	34.03	-100	100
ROA, trimmed at $\langle -100, 100 \rangle$	2,463,095	2.41	23.54	-100	100
Assets (adjusted)	2,590,768	5.71E+07	2.13E+09	0	2.4E+09
Capital (adjusted)	2,593,405	27.1E+06	937E+06	0	814E+09
Leverage	2,581,871	14.47	2,990.81	-2,381,568	3,375,915
Earnings before tax	2,595,520	2,555,347	135E+06	-30.4E+09	64.9E+09

The donations database contains a total of 7,916 corporate donations made by 5,188 legal persons. This is, however, somewhat misleading, because many political parties include self-employed physical persons in the list of donating legal persons. Merging the two data sets, 5,044 donations made by 3,203 different firms were matched with an identification number of an existing firm. The remaining, unmatched donors are dropped. Regarding data on firms, I

use some information from the Magnus database itself (sector, total assets, and leverage ratios). I further add information about firms from other sources. Most importantly, I use EconLab's hand-cleaned internal database of firms and their details (such as law form, size, location), which is compiled from numerous different sources and also contains information on public procurement and European grants obtained by each firm since 2006.

2.5 Empirical Results

This section summarizes the results of tests outlined in Section 2.3. I present the results in three stages. First, I present the main results of this paper—those of paired t-tests comparing the means of firm performance indicators for matched connected and non-connected firms (as described in Section 2.3.1) and further extend the analysis to (i) estimate the monetary effect of connections, (ii) assess whether larger donations lead to better performance, (iii) account for the potential network effects, and (iv) divide the sample for firms that work closely with the public sector and those that do not. Second, I report the results of the estimates of the pooled models formulated in Section 2.3.2 using as explanatory variables both a dummy variable for donating firms and the actual value of donations made by each firm. I perform a series of tests to check the robustness of these results. Third, I add the effect of donating to a party which is in power at the time of the donation, as described in Section 2.3.3, in order to clarify whether the value of donations can be thought of as an actual measure of the level of connectedness or rather if we should view the donation as merely a proxy variable indicating closeness of the donating firms to politics. At the end of this section, I discuss how the results of the three main groups of models can be reconciled with the notion that firms may self-select themselves into the group of politically connected firms based on their financial performance.

The main results of this paper are based on the dynamic matching procedure described in Section 2.3.1. The matching is designed in a way that aims to reduce the endogenity concerns encountered in the pooled models to the minimum, given data limitations. However, it still implicitly assumes that the observable characteristics are enough to control for any selectivity bias that might arise when firms decide to become politically connected through donations to parties. In total, there are 4,896 firm-year observations for connected firms in the sample. Out of these, the matching exercise successfully matched

1,334 firm-years to at least one non-connected but otherwise similar firm(-year). Of these 1,334, 549 (41%) were matched with one non-connected firm, another 555 (41.6%) with 2-5 firms, and the remaining 230 (17.2%) with more than 5 firms. A histogram showing the number of successful non-connected matches for the connected firms is presented in Figure B.1 in the Appendix. A summary of the number of firm-year observations in each group after the matching is presented in Table 2.3.

Table 2.3: Summary of the number of firm-years in each group after the matching procedure.

	Not connected	Connected	Total
Not matched	2,585,306	3,562	2,588,868
Matched	5,452	1,334	6,786
Total	2,590,758	4,896	2,595,654

Not all of the matched firms have reported their financial results in the relevant year, which is why I report the number of observations used in each test below. Furthermore, I identified 12 (79) outliers for which the year-on-year difference in ROE (ROA) amounted to more than 100% which suggests either a mistake in the reported data or an unusual event that greatly affected the calculated profitability ratios, and I drop these from the test. To analyze the potential omitted variable bias that could generate selection into treatment (i.e. being connected), I report the balancing properties of the connected and non-connected firms before and after matching in Table A.1 in the Appendix. On average, the matched non-connected firms are smaller than an average non-connected firm, but significantly more profitable, pointing to the main advantage of the matching approach—that it excludes less profitable firms that are likelier not to be able to afford donating money to political parties. Among the connected firms, the matched ones are, on average, slightly more profitable than the unmatched ones, but not more than the non-connected matched firms.

In Table 2.4 I report the results of a paired t-test of equal means of the distributions of matched connected and non-connected firms. They suggest that connected firms reach significantly better results, as measured by both ROE and ROA. Specifically, connected firms (ROEc, ROAc) report on average 1.6 and 1.06 percentage points higher returns on equity and return on assets, respectively, than their non-connected but otherwise similar peers (ROEnc, ROAnc). This represents a 7.8% and 11.5% difference in terms of ROE and

ROA, respectively. These differences are statistically significant at the 5 and 1% levels of significance for ROE and ROA, respectively, and I argue that they represent strong evidence of the positive relationship between political connections and financial performance of the connected firms.

Table 2.4: Results of a paired t-test of equal means of financial performance indicators for connected and non-connected (but otherwise similar) firms.

Variable	Obs.	Mean	Std. Err.	95% Conf.	Interval	t-statistic	p-value
ROEc	1218	22.13	0.69	20.77	23.48		
ROEnc	1218	20.53	0.60	19.35	21.70		
Difference	1218	1.60	0.77	0.09	3.11	2.07	0.02
ROAc	1253	10.29	0.35	9.61	10.97		
ROAnc	1253	9.23	0.33	8.57	9.88		
Difference	1253	1.06	0.43	0.22	1.90	2.49	0.01

These effects are economically significant, too—compared to previous studies that focused on different settings, I find a higher effect. Specifically, Cingano and Pinotti (2013) report a 5.7% increase in profitability for Italy and Amore and Bennedsen (2013) a 3.25% increase for Denmark. These lower effects in countries with lower corruption levels and more developed institutions are consistent with the hypothesis that in Czechia during its post-transition period, political connections have had higher value than in more developed economies.

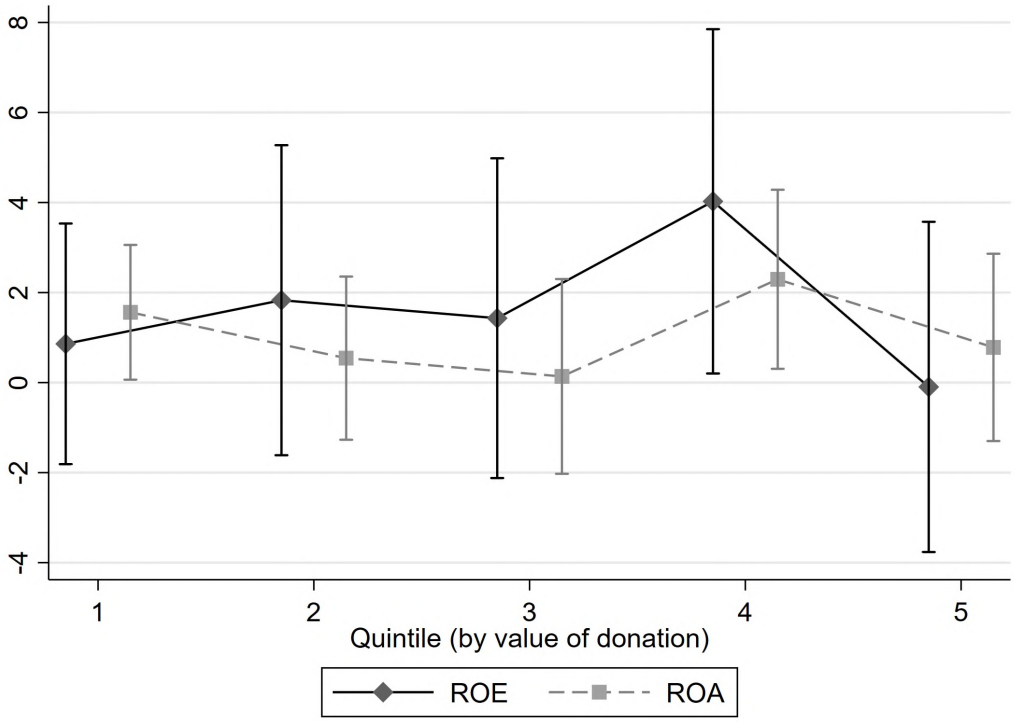
As outlined in Sections 2.2 and 2.3, the results in Table 2.4 allow us to quantify the average effect of donations not only on profitability, but also on revenues. We can calculate the average monetary effect of being connected on firm profits by multiplying the estimated marginal increase in ROE (ROA) by the average equity (assets) within the sample. For ROE, this effect is $1.6 *$ 34.6 million CZK = 554,000 CZK, and for ROA, it is $1.06*74.5$ million CZK = 790,000 CZK. The average donation in the sample is $c_{don} = 80,438$ CZK. Plugging these numbers to Equation 2.1 we obtain, for ROE, 790,000 CZK = $r_{don}^{ROE} - 80,400$ CZK which yields an average monetary effect of a donation on firm revenue of $r_{don}^{ROE} = 709,600$ CZK. For ROA, in a similar way, I arrive at an estimate of $r_{don}^{ROA} = 870,300$ CZK. Together, this puts the estimate of the average return to donating money to political parties at $r_{don}/c_{don} = 883\%$ and $1,082\%$ for the ROE and ROA versions, respectively.

I further split the sample used for the tests in Table 2.4 into three time periods to analyze the development of the effect over time. In Table A.2 in the

Appendix shows that the effect is observable in all three time periods, albeit with weaker statistical significance using the three subsamples. The estimates of the effect are especially large in the first period (before 2005) at 20.4%, then they decrease to 5.6% between 2006 and 2010 and increase again to 8.2% after 2011. I find that the lower estimate for the middle period is caused primarily by the year 2010 in which snap elections took place and the number of donating companies in 2010 is by far the largest across the whole studied time period. Excluding the year 2010 yields a statistically significant (at the 5% level) estimate of 9.1%.

Furthermore, arguably, these average effects, while allowing for statistically stronger estimates, may hide heterogeneity in the effects of donations of different values. To assess whether larger donations are associated with a higher difference in profitability, I divide the sample into 5 quintiles based on the donated amount. I present the results in Table 2.2. They show limited evidence for this effect: while all point estimates of the difference in profitability of connected and non-connected firms are positive (except for ROE in the fifth quintile), they are statistically significant at the 5 % level only in the fourth quintile and for ROA in the first quintile. This suggests, perhaps somewhat surprisingly, that the value of the donation is not as important as simply being connected, and is consistent with the notion discussed in Section 2.2 that donations do not directly translate into higher profit margins, but rather represent an investment which pays off in increased revenues, with varying potential effects on profitability.

Figure 2.2: Results of a paired t-test of equal means of financial performance indicators for connected and non-connected (but otherwise similar) firms, by quintiles based on total value of donations of the connected firms.



Source: Author.

In Table A.3 in the Appendix I present the results of a similar t-test to that of Table 2.4 but adjusted for partisan networks effects. This is done by the sample being clustered across groups of firms that are connected to the same party (or set of parties). The results do indicate a positive difference in profitability of connected firms as compared to their non-connected peers, however, the effects are smaller in size that those of the baseline test presented above, and are not statistically significant at the standard levels of significance. This is consistent with the notion that direct channels, which act rather as substitutes than complements within the partisan networks, are among the relatively more important.

I further proceed with the analysis of the differences between the connected and non-connected firms by dividing the non-connected, matched peers into those that do and those that do not work closely with the public sector (for the purposes of this paper, I will call such firms 'public firms'). Table 2.5 shows that on average, the profitability of connected firms is higher than that of the

non-public non-connected firms, by about the same margin as above for the sample of all matched non-connected firms. However, this effect vanishes if we consider only public non-connected firms, which report financial results that are not statistically significantly different from those of connected firms. This result suggests that working closely with the public sector brings an advantage similar to that of being politically connected, and also that other forms of political connections such as personal ties are likely to play a significant role.

Table 2.5: Results of a paired t-test of equal means of financial performance indicators for connected and non-connected (but otherwise similar) firms, public and non-public non-connected firms considered separately.

Variable	Obs.	Mean	Std. Err.	95% Conf.	Interval	t-statistic	p-value
ROEc	999	21.89	0.78	20.36	23.42		
ROEnc, PubSec=0	999	20.64	0.68	19.30	21.98		
Difference	999	1.24	0.89	-0.51	3.00	1.39	0.08
ROAc	1030	10.44	0.39	9.66	11.21		
ROAnc, PubSec=0	1030	9.57	0.39	8.81	10.33		
Difference	1030	0.87	1030	-0.12	1.86	1.72	0.04
ROEc	553	22.97	1.00	21.02	24.93		
ROEnc, PubSec=1	553	22.06	1.01	20.08	24.05		
Difference	553	0.91	1.20	-1.44	3.26	0.76	0.22
ROAc	571	10.24	0.49	9.29	11.20		
ROAnc, PubSec=1	571	9.75	0.55	8.67	10.83		
Difference	571	0.50	0.64	-0.76	1.75	0.78	0.22

I perform one more test to further analyze the differences in financial performance of connected firms wherein I separate the exercise into only public (and only non-public) connected firms and compare them with their public (non-public), non-connected peers. The results, presented in Table 2.6, show that within the sample of firms that do work closely with the public sector, the connected ones report only slightly better results than the non-connected ones, and these differences are not statistically significant at the standard levels of significance. Within the sample of firms that do not work closely with the public sector, however, I do find statistically significant differences for ROA (and non-significant only by a small margin for ROE), with estimates of larger magnitude than for the sample of all firms. In general, I conclude that donating money to political parties seems to indeed be associated with significantly better performance, but the effect is not detectable for cases when the non-connected firms work closely with the public sector.

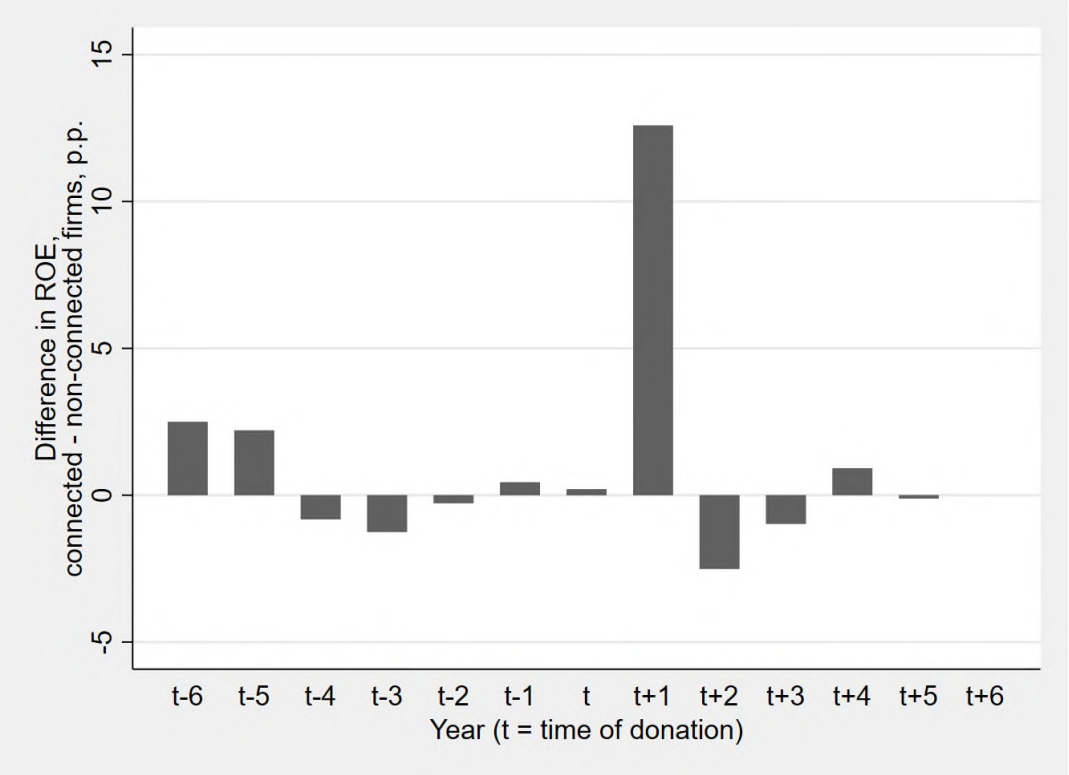
I recognize at least two potential explanations for this result. First, our measure of connections might not capture all existing connections. For example, personal ties have been shown in previous literature to be of high importance, and public firms may be more likely to cultivate these relationships than non-public firms. Second, it may be that working closely with the public sector is relatively more profitable than other activities, and public firms thus exhibit higher profits, by a margin similar to the one brought about by political connections.

Table 2.6: Results of a paired t-test of equal means of financial performance indicators for connected and non-connected (but otherwise similar) firms, public vs. non-public firms.

Variable	Obs.	Mean	Std. Err.	95% Conf.	Interval	t-statistic	p-value
ROEc, PubSec=0	633	21.59	0.99	19.64	23.54		
ROEnc, PubSec=0	633	20.17	0.84	18.53	21.82		
Difference	633	1.42	1.13	-0.81	3.64	1.25	0.11
ROAc, PubSec=0	651	10.93	0.52	9.91	11.95		
ROAnc, PubSec=0	651	9.17	0.48	8.22	10.11		
Difference	651	1.77	0.63	0.52	3.01	2.79	0.00
ROEc, PubSec=1	306	23.85	1.28	21.33	26.36		
ROEnc, PubSec=1	306	22.89	1.31	20.31	25.48		
Difference	306	0.95	1.61	-2.22	4.13	0.59	0.28
ROAc, PubSec=1	314	10.70	0.67	9.39	12.01		
ROAnc, PubSec=1	314	10.18	0.77	8.66	11.71		
Difference	314	0.52	0.90	-1.26	2.29	0.57	0.28

I further carry out an event study of the effect of contributions to political parties on the financial performance of connected firms around the time of the donation. I report on the results in Figure 2.3. I observe that in the 6 years prior to the contribution and also in the year of the contribution, the average difference in the return on equity between connected firms and their non-connected (but otherwise similar) counterparts is small, while in the first year following the donation, the difference is very large. Overall, these results support the validity of my main findings that political connections established through campaign contributions to political parties in Czechia indeed bring a significant advantage to the connected firms over their non-connected peers following the establishment of the connection.

Figure 2.3: Average difference in return on equity of connected vs. non-connected firms around the time of donation



Source: Author.

Let us now turn to the results of the baseline pooled models outlined in Section 2.3.2. Panels (1) and (2) in Table 2.7 present the results of the pooled model with a dummy variable *DDon* indicating that a firm is politically connected through donations. Since the sample for this model is very large (reaches 855 and 1,100 thousand observations for ROE and ROA, respectively), I report 95 % confidence intervals instead of t-statistics (Lin et al. 2013; Disdier and Head 2008). I observe that the fact that a firm is politically connected is indeed associated with superior financial performance, with a point estimate of 0.73 percentage point difference in ROE and 0.55 percentage points difference in ROA. Other factors with positive and statistically significant coefficients are firm size, lagged financial result, and *PubSec*, a binary variable indicating whether or not the firm has signed at least one public procurement contract or has received at least one European grant. On the other hand, the coefficient for *LocSize* shows a negative sign, suggesting that firms operating in larger cities tend to be less profitable as measured by ROE and ROA.[9]

[9]Although I do not explore the implications of this result in this paper, one possible

Table 2.7: Results of the pooled models, OLS.

	(1) ROE	(2) ROA	(3) ROE	(4) ROA
Lag of ROE	.291***		.305***	
	[.289,.294]		(18.1)	
Lag of ROA		.294***		.322***
		[.291,.296]		(13.4)
PubInd	5.95	1.77	-8.19**	-10.1***
	[-1.87,13.8]	[-1.47,5]	(-2.23)	(-5.41)
PubSec	2.35***	1.36***	2.17***	.666*
	[2.21,2.49]	[1.27,1.44]	(3.04)	(1.76)
LocSize	-.072***	-.243***	-.892***	-.371**
	[-.106,-.038]	[-.264,-.222]	(-3.38)	(-2.57)
FirmSize	.902***	.638***	-.266	-.156
	[.882,.923]	[.624,.653]	(-1.26)	(-1.18)
Leverage	4.3e-06	-2.7e-05	4.13**	-2.36***
	[-2.2e-06,1.1e-05]	[-8.4e-05,3.0e-05]	(2.23)	(-2.66)
DDon	.733**	.548***		
	[.116,1.35]	[.207,.89]		
ln(Don)			.197	.194*
			(.887)	(1.72)
Constant	-18***	-11.1***	17.8***	16.1***
	[-19.9,-16]	[-12.2,-9.94]	(3.2)	(4.98)
Year-fixed effects	Yes	Yes	Yes	Yes
Sector-fixed effects	Yes	Yes	Yes	Yes
Observations	855158	1099867	3772	4345
R^2	0.189	0.171	0.239	0.268

Notes: 95 % confidence intervals in brackets, robust t-statistics in parentheses.

* p < 0.1, ** p < 0.05, *** p < 0.01.

I conduct a series of tests to check the robustness of these results to some of the methodological choices described in Section 2.3.2 and their sensitivity to the inclusion of individual regressors. In Table A.4 in the Appendix, I compare the impact of using five alternative trimming and winsorization criteria, and I obtain expected results: the estimated coefficient for the effect of being politi-

explanation that would be in line with economic theory is that firms operating in larger cities generally face higher competition.

cally connected remains positive and statistically significant for the alternative outlier accommodation procedures. Due to high outliers, the coefficients for the binary variable indicating a connection are higher for criteria that affect lower numbers of observations, and even more so for winsorized samples as compared to trimmed ones. Table A.5 in the Appendix then shows the results of the estimation of the model represented by Equation 2.2 when using different smoothing periods for ROE, as described in Section 2.3. I observe that the results are fairly robust to this choice, with the estimated coefficient for the binary variable indicating a connection ranging between 0.7 and 1.5 percentage points in the return on equity. This suggests that the connections are likely to show a certain degree of persistence before and after the monetary contribution is made, or, alternatively, that the firm performance indicators can react with lags or leads to connections. I also analyze the sensitivity of the model to the inclusion of additional individual variables. The results are presented in Tables A.6 and A.7 in the Appendix for ROE and ROA, respectively, and show that the model is fairly robust in its specification—the inclusion of each additional variable increases the explanatory power of the model while not altering the directions of the estimated effects. Importantly, the effect remains positive and statistically significant in the model that excludes the lagged outcome, which has proven to be a strong predictor of firm outcomes, as expected.

Furthermore, I test whether the effects are different across industries with different intensity of cooperation with the public sector. To do so, I divide the pooled model into 5 quintiles based on the volume of public procurement they have supplied between 2006 and 2014. In Table A.8 in the Appendix, I present the results of estimating the model for these 5 groups individually. Although the level of statistical significance is lower for the individual groups' estimates, the effect of donations is generally a bit higher for firms operating in more procurement–intensive industries (i.e. in the higher quintiles), which is in support of the hypothesis tested in previous research that public procurement may be an important channel through which firms exploit their political connections in Czechia (Palanský 2014; Titl and Geys 2019).

The results of regressions outlined by Equation 2.3 which include the actual value of donations rather than a binary variable indicating a connection are presented in Panels (3) and (4) in Table 2.7. As the aim of this part of the analysis is to quantify the effect of donating more money (and not the fact that a firm donates), only donating firms are included in these models. The estimates of the effect of the donation value are positive for both firm perfor-

mance indicators, though not statistically significant at the standard levels of significance. These results suggest that the connections established through corporate donations to political parties may be, to a limited extent, regarded as a form of short-term investment. As discussed above, due to the potential endogeneity of the donated amount, I do not aim to establish causality in this analysis, but rather focus on describing the observed empirical relationship.

My second family of pooled models aims to shed more light on whether connections to parties present in the national government are more important than connections to other parties. To do so, I include in the model a binary variable indicating whether or not the donation was made to a party which was part of the government in the year of the donation. The results are presented in Table 2.8 and suggest, somewhat surprisingly, that being connected to the party in power is associated with lower financial performance in the years following the establishment of such connections. A possible explanation for this effect is that different parties are often in power at different levels of government. In Czechia, subnational levels of government (in which it is especially often the case that different parties are in power than in the national government) administer public procurement and grants of significant value, and coupled with the results reached above about public procurement being one of the likely sources of added value to connected firms, these results are far less surprising. To investigate this point more thoroughly, I perform a short additional test. Similarly to Palanský (2014) and Titl and Geys (2019), I exploit the substantial shift in political power in the regional governments following the 2008 elections. I restrict the sample to the two election periods surrounding this election and ask whether firms connected to the party that was in power at the regional level have fared better than firms connected to other parties. I report the results in panels (5) and (6) of Table 2.8. The coefficients for the variable $LocalPower$ are positive, though relatively less statistically significant, which is broadly in support of my argument, but a more detailed approach would be necessary to thoroughly analyze the effect of connections at the regional level.

Table 2.8: Results of the party-in-power models, OLS.

	(1) ROE	(2) ROA	(3) ROE	(4) ROA	(5) ROE	(6) ROA
Lag of ROE	.308***		.309***		.268***	
	(18.5)		(18.5)		(14.4)	
Lag of ROA		.321***		.321***		.306***
		(13.5)		(13.5)		(12.3)
PubInd	-6.46*	-8.74***	-6.66*	-8.97***	-11***	-12.2***
	(-1.75)	(-4.69)	(-1.81)	(-4.81)	(-3.21)	(-8.57)
PubSec	2.18***	.643*	2.18***	.638*	2.69***	.715
	(3.09)	(1.71)	(3.09)	(1.69)	(3.27)	(1.61)
LocSize	-.804***	-.346**	-.785***	-.336**	-.95***	-.508***
	(-3.1)	(-2.43)	(-3.02)	(-2.36)	(-3.09)	(-3.06)
FirmSize	-.189	-.102	-.175	-.099	-.289	.021
	(-.938)	(-.798)	(-.866)	(-.771)	(-1.2)	(.148)
Leverage	4.04**	-2.34***	4.04**	-2.35***	3.82*	-2.07**
	(2.22)	(-2.61)	(2.22)	(-2.63)	(1.66)	(-2.16)
Power	-1.92**	-1.13***				
	(-2.49)	(-2.59)				
ln(Don)*Power			-.164**	-.077*		
			(-2.21)	(-1.9)		
LocalPower					1.54	.552
					(1.43)	(.942)
Constant	19.5***	17.6***	19.6***	17.6***	35.1***	22.3***
	(3.71)	(5.64)	(3.71)	(5.62)	(7.91)	(8.41)
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3825	4410	3825	4410	2731	3172
R^2	0.240	0.269	0.240	0.268	0.233	0.268

Robust t-statistics in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Overall, the matching exercise, whose results are presented at the beginning

of this section and which is less sensitive to the selectivity bias discussed above, has shown higher differences in financial performance between connected and non-connected firms than the baseline models. I argue that this is due to the fact that in the matching, only firms that are similar to (and thus can be matched with) connected firms are included in the analyzed sample. The different estimates of the size of the effect of connections suggest that the endogeneity is indeed at play in the baseline pooled models. To investigate this point further, I rerun Equations 1 and 2 using a sample restricted only to firms that have been matched during the matching procedure. The results, reported in Table A.9 in the Appendix, show that within this sample, the model does not detect a statistically significant effect of connections. Similarly, within the sample of connected firms that have found a successful match, the size of the donation does not have a significant effect. These results underline the substantial differences in the samples analyzed in the baseline models and in the matching exercise, supporting the added value of the matching approach to the analysis of the effect of political connections on firm performance.

2.6 Conclusions

There is ample evidence in the academic literature that corporate political connections matter: they have been shown to add value to the connected firms through various channels, to various extent, and in various settings. In this paper, I corroborate on these existing studies by focusing on one particular setting which has not yet been analyzed in this context—I assess whether political connections established through direct corporate donations to political parties mattered for firms in Czechia, a small, multiparty parliamentary democracy, during its post-transition period between 1995 and 2014. Rather than focusing on specific channels through which connections may add value, I focus on the ultimate measure of firm success—reported profit. In particular, I hypothesize that connected firms, on average, financially outperform their non-connected but otherwise similar peers following the establishment of a connection. To test this hypothesis, I develop a conservative dynamic matching methodology that brings me closer to solving the endogeneity issue inherent in the type of studies such as this one. Based on a number of observable firm–level characteristics that are hypothesized to predict the likelihood of firms becoming connected, I match the connected firms with their non–connected peers individually in each

year, allowing to capture time–specific effects of the overall economic situation and the changes in firm characteristics over time.

I find robust evidence for the hypothesis that for Czech firms, being connected to political parties through donations does indeed pay off. The results suggest that the connected firms outperform their non-connected peers by around 1.6 and 1.1 percentage points (7.8 % and 11.5 %) in terms of returns on equity and returns on assets, respectively. This translates into an estimated average return on investment into connections of around 900-1,100%. Compared to other studies carried out in countries with more developed institutions and lower levels of corruption, the effect I find for Czechia during its post-transition period is higher. This is consistent with the hypothesis that in settings with weaker institutions, the value of corporate political connections is relatively high. I do not find convincing evidence for a higher effect of larger donations.

Moreover, and importantly, I find that the effect vanishes when we consider only those non-connected firms that I call 'public' (i.e. those that work closely with the public sector, measured by having supplied public procurement or having received public grants), suggesting that other forms of connections than those established through corporate political donations to parties (such as personal ties), and those established at lower levels of government (such as the regional level) might be at play. Within the group of public firms, nevertheless, connections through donations do seem to matter—donating firms that also work closely with the public sector are among the best–performing firms of all those analyzed.

To my knowledge, this study is the first to systematically document a significant advantage, in terms of financial performance, of firms connected to politics through donations to political parties in a post-transition setting, not only within the universe of all firms but also within a group of firms matched based on detailed firm characteristics that are implicitly allowed to evolve over time. The studied setting is particularly important in terms of this paper's contribution to the existing literature. The case of Czechia seems to be in line with some of the arguments in the political economy literature which suggest that political connections are of particular importance in post-transition periods during which institutions are weaker than in developed economies. This highlights the need for transparency of the relationships between business and politics, particularly in these settings.

A number of issues and areas that need further research remain. These

include finding other reliable proxy variables to estimate the firms' connectedness on a large scale. For example, it could be possible to match firm officials and electoral candidates (both successful and unsuccessful) in elections to different levels of government. Indeed, some of the results presented in this and other research point to a relatively high importance of being connected to parties at sub-national levels of government, where other connections than those measured by officially declared corporate contributions to political parties are likely to prevail. Another strand of research could focus specifically on firms that seem to derive the most added value from connections. One interesting area might be to consider only publicly listed firms and focus on their stock market returns as performance indicators in similar settings as the one explored in this study. An increased understanding of the role of corporate political connections will inevitably contribute to a more informed approach to mitigating rent-seeking, corruption, and cronyism of modern democracies.

Chapter 3

International Corporate Profit Shifting

3.1 Introduction

Profit shifting to tax havens by multinational enterprises (MNEs) and the tax avoidance related to this practice represent a crucial issue for the world economy. As we show in this paper's preferred estimates, around $420 billion in corporate profits are shifted from the 79 countries in our sample annually, amounting to almost 1 per cent of these countries' GDP. Using statistics from Tørsløv et al. (2018) for comparison, the shifted profits represent 6% of all corporate profits and 37% of MNEs' corporate profits. This estimate implies that at least $125 billion is lost in tax revenue, around 10% of all corporate tax revenue. Our methodology enables us to go beyond these aggregated figures and present estimates of the scale of profit shifting for individual countries.

Tax havens and profit shifting by MNEs have been receiving increasing

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attention from researchers, policymakers and the media alike, as documented by the recent studies cited in this paper. This is in part because it has become rather easy for MNEs to avoid paying corporate tax, but also, thanks to recent leaks of confidential documents and thorough investigative case studies, it has become relatively easy for the public to learn about it and for researchers to provide evidence of it. Yet, the scale of the tax revenue losses incurred by individual governments remains uncertain due to the inherent difficulties of estimating tax avoidance and due to gaps in the availability of relevant data, some of which are being addressed by recent proposals of the European Union (EU) and Organisation for Economic Co-operation and Development (OECD), and some of which are being overcome by innovative researchers. For example, Bilicka (2019) uses the United Kingdom's confidential corporate tax returns to learn how aggressively foreign MNEs are reducing their corporate tax liability, and Alstadsæter et al. (2019) use audit and leaked data from tax haven institutions to study tax evasion by wealthy individuals. While similar studies do provide rigorous evidence, they are limited in their scope and provide revenue loss estimates for only one or a handful of countries.

In this paper, in contrast, we provide estimates of the scale of profit shifting and the consequent tax implications for as many countries as possible; this requires us to sacrifice rigour to some extent for the sake of improved scope. Specifically, we estimate the scale of profit shifting and tax revenue losses using data on foreign direct investment (FDI). Our two most important data sources are the International Monetary Fund's (IMF) Coordinated Direct Investment Survey (CDIS), which contains bilateral FDI stock data for around 100 countries between 2009 and 2016, and the IMF's Balance of Payments Statistics (BOPS) containing data on FDI income for an even wider set of countries. We begin by observing that a higher share of investment from tax havens (or offshore financial centres—we use both terms interchangeably in this paper) is associated with a lower reported rate of return on inward FDI. We argue, in line with UNCTAD's (2015) World Investment Report, that this pattern is caused by profit shifting and we estimate its scale and the resulting tax revenue losses. Importantly, we present country-level estimates of profit shifting, which enables us to study its impact on the individual countries' government revenues and thus also the distributional impact of profit shifting. Indeed, our main research question in this paper is which countries' tax revenues are most affected by international corporate profit shifting.

We estimate the resulting tax revenue losses to understand which countries

are losing the most tax revenue relative to their economic size. We compare the results across country groups classified by income per capita and while we do identify some differences in the point estimates, we find that these differences are rarely statistically significant. We further compare our findings with four other studies that have reported country-level tax revenue loss estimates, each of which uses a different methodology: Tørsløv et al. (2018), Cobham and Janský (2018), Cobham and Janský (2019) and Clausing (2016). Most of these studies identify some differences across income groups, but the nature of these differences varies across the studies.

This paper's empirical contribution is presented in the following five stages. First, using new and updated data sources, we re-estimate and critically review the work of UNCTAD (2015) in what we call the baseline model. Second, we develop an extended model which improves on the baseline model in a number of aspects. Third, for the first time using this methodological approach, we report country-level estimated tax revenue losses. Fourth, we compare our results with four other recent studies that provide country-level tax revenue loss estimates and this is the first such comparison made. Fifth, we focus on the distributional impact of profit shifting and compare the revenue losses across countries using our estimates as well as those from the other four studies. These five specific stages altogether contribute to the expanding body of literature on profit shifting and tax havens. We make a contribution to the existing research in at least two specific areas. First, we contribute to the ongoing collective attempt to arrive at credible estimates of the scale of profit shifting. Despite the inherent difficulties, discussed for example by Fuest and Riedel (2012), of making such an estimation, a growing number of studies do make estimates of the scale of profit shifting, as our literature review below documents. However, a number of these focus on just one country, such as Gumpert et al. (2016) and Godar and Janský (forthcoming) on Germany or Zucman (2014) and Garcia-Bernardo et al. (forthcoming) on the United States. We develop estimates for a wide range of countries—indeed, for all countries for which we have available data. We see this study as a contribution to international policy debates, since only a limited number of similar estimates are available (and often not for as many countries), and we compare ours to four others that do exist.

Second, we contribute to the study of the heterogeneous impacts of international corporate tax avoidance. So far, most research looks at individual countries or, in the case of an international focus, often concentrates only on the division between developing and developed countries. For example, Fuest

et al. (2011) find that the effect of the host country corporate tax rate on the debt ratio of multinational affiliates in developing economies is larger than for affiliates in developed economies. A similar division is used by Johannesen et al. (2020), who link the tax aggressiveness of MNEs with the economic development of their host countries, but they also estimate models that exploit the cross-country variation in economic and institutional development. This more granular approach is needed and similar studies should reflect the country-specific characteristics. In this paper's preferred model, we perform our regression analysis using regional- and income-group fixed effects and carry out the rest of the estimation at the country level, at which we also present the results and discuss differences across income groups.

The remainder of this paper is structured as follows. We begin with a brief literature review of related research in Section 3.2 and an overview of the data used and basic descriptive statistics in Section 3.3. We describe our empirical methodology in Section 3.4 and present the results in Section 3.5, in which we also compare our estimates with those reached by other similar papers. Finally, Section 3.6 provides a discussion of the implications of the results and concludes.

3.2 Related Literature

In this section, we first discuss the main channels through which MNEs may effectively shift profits out of high-tax jurisdictions. Second, we briefly review recent literature related to the quantification of corporate profit shifting and the resulting tax revenue losses. Third, we sum up the results of a pioneering report by UNCTAD (2015). which developed an FDI-driven approach that we build upon in this paper. Last, before describing our data, we discuss the pros and cons of the data sets most frequently used in similar research and those used in this paper. For the sake of space, we provide only a brief literature review focused on the most relevant research. For more comprehensive reviews of academic literature on profit shifting, we refer to Dharmapala (2014), Clausing (2016) or Dowd et al. (2017).

Three main profit-shifting channels are usually recognized in the literature: debt shifting, the location of intangible assets and strategic transfer pricing. All three are motivated by the MNEs' assumed desire to reduce their global tax liabilities by artificially shifting their profits and assets, and thus tax bases, to countries with lower (effective) tax rates, sometimes referred to as tax havens.

First, in the case of the debt shifting channel, MNEs implement unnecessary loans at high interest rates from one MNE affiliate located in a low-tax jurisdiction to another profitable unit located elsewhere (Buettner and Wamser 2013; Desai 2005; Fuest et al. 2011; Huizinga and Laeven 2008). Second, intangible assets and intellectual property, such as brands or research and development, can be stationed artificially at a subsidiary in a tax haven, to which service fees are then paid by other parts of the MNE (Bryan et al. 2017; Dischinger and Riedel 2011; Seabrooke and Wigan 2015; Taylor et al. 2015). As discussed thoroughly by OECD (2017), pricing such intangible assets poses several major challenges, making it intrinsically difficult to disentangle profit-shifting effects from actual prices. The third main channel for profit shifting is to artificially inflate or deflate the prices of goods or services being transferred between the various foreign parts of an MNE in such a way as to minimize the tax burden faced in all the countries put together (Bartelsman and Beetsma 2003; Clausing 2003; Davies et al. 2018; Peralta et al. 2006).

The quantitative evidence of MNEs shifting profits and debt and locating their headquarters or intellectual property in such a way as to avoid tax is substantial. A number of studies have provided evidence of profit shifting, especially on how tax rate differentials affect reported pre-tax profits, and on the strategies MNEs employ to reallocate profits within their groups (Dharmapala and Riedel 2013; Dischinger and Riedel 2011; Hines and Rice 1994; Huizinga and Laeven 2008). Although the existing academic and policy studies provide useful guidance on what can be quantified, findings on the implications of tax avoidance for government revenue are rather limited. Among recent exceptions are Clausing (2009), Zucman (2014) and Guvenen et al. (2017), who provide estimates for the United States.

Indeed, for some much-studied countries such as the United States we do not expect the added value of our new estimates to be high, however, for many countries there are no estimates of profit shifting available and that is where we hope to make an important contribution. For developing countries, Johannesen and Pirttilä (2016) provide an overview and Johannesen et al. (2020) offer firm-level empirical results, whereas one of the recent examples of revenue estimates comes from Reynolds and Wier (2018) for South Africa. Furthermore, at least three international organizations have recently developed estimates of the budgetary impact of international corporate tax avoidance for most of the world economy: OECD (2015b), IMF's Crivelli et al. (2016) and UNCTAD (2015). Although these studies make strong assumptions to deal with

a lack of any realistic counterfactual data (i.e. what the tax base would be in the absence of profit shifting) and they do not publish country-level results, they do make valuable contributions to the research and have been influential in the policy debate.

We naturally build on a range of existing research in this paper, although we build upon one specific source more than on the others – UNCTAD's (2015) World Investment Report. In the report, the authors develop an FDI-driven approach (further detailed in Bolwijn et al. (2018a;b)) to measure the scale and economic impact of MNEs' tax avoidance schemes. Their investment perspective on tax avoidance puts the spotlight on the role of tax havens as major international investment players. They estimate that some 30 per cent of cross-border corporate investment stocks are routed through tax havens before they reach their destination as productive assets (Bolwijn et al. 2018b). Their preferred estimate of annual revenue losses for developing countries, the focus of their study, is $90 billion; extending that estimate globally results in $200 billion, or 8% of all corporate income tax, lost in government revenue in 2012. In this paper, we review their methodology and, using updated data sources, we extend it to help us better answer our research question. In particular, we present results at the country level and discuss the resulting distributional impacts of profit shifting.

Four other recent papers have presented country-level estimates of revenue losses due to profit shifting for many countries worldwide: Tørsløv et al. (2018), Cobham and Janský (2018), Cobham and Janský (2019) and Clausing (2016). We compare our findings with the estimates from these four studies in our results section. While they each include an answer to the question of the scale of profit shifting and tax revenue losses for many countries, there are important differences to consider. In particular, each of the four studies uses a distinct methodology (discussed in detail in the papers themselves or in chapter 4 of Cobham and Janský (2020)), which we now briefly describe.

First, Tørsløv et al. (2018) use national accounts and other data sources, such as balance of payments and foreign affiliate statistics, to estimate the misalignment between the locations of reported profit and real economic activity represented by employee compensation. Their methodology is quite straightforward and persuasive, further supported by additional findings on how the profits are shifted. They estimate that around 40% of MNEs' profits are artificially shifted to tax havens. This is the most recent study and also likely the most reliable source of estimates. In contrast, it is currently not very useful

for studying differences across different countries. Although it extends its estimates to the rest of the world, it only covers 37 individual countries, most of them developed, due to the limited availability of national accounts data.

Second, Cobham and Janský (2018) build on the methodology developed by IMF's Crivelli et al. (2016)[1] and provide a wide coverage of countries, including developing ones. This good country coverage, however, was enabled by their being less demanding with regard to the data, which renders the results less reliable. Specifically, they exploit data on all corporate income revenues, rather than only those paid by MNEs, and do not use any other information on MNEs. The methodology observes the effects of tax havens on other countries' tax bases and assesses the influence of tax havens by turning off these effects, which might include factors other than profit shifting and tax avoidance. These estimates thus capture a broader set of tax avoidance phenomena. As their main results, the authors report long-run estimates which take into account the lagged response of corporate tax base erosion, however, as argued by Tørsløv et al. (2018), this leads to estimates of shifted profits that are often higher than the amount of profits in MNEs. In the comparisons that we make in this paper, we generally consider the short-run, direct estimates as more comparable to the results of other studies, including this one.

Third, Cobham and Janský (2019) estimate misalignment between the locations of reported profit and real economic activity. They estimate how much additional tax certain countries would collect if MNEs' reported profits were fully aligned with their economic activity, and, inversely, in that hypothetical case how much less tax some other countries—including tax havens—would collect. Together with Tørsløv et al. (2018), Cobham and Janský (2019) are the only studies that empirically identify tax havens as part of their results. In contrast to the other studies' intended global coverage, Cobham and Janský (2019) only covers US-headquartered MNEs (US FDI accounts for about a fifth of global FDI) and information on many developing countries are suppressed in their data for confidentiality reasons. As acknowledged by more recent studies applying the same methodology at different data sets (e.g. Janský (2020;

[1]The results of IMF's Crivelli et al. (2016) were presented for the first time in their preliminary version in a report by IMF (2014). The report also includes another set of revenue loss estimates due to profit shifting that uses a distinct methodology based on differences in countries' so-called corporate income tax efficiency ratio relative to the average ratio in other countries. Although country-level estimates are presented for this latter methodology, they are perhaps even broader than those of IMF's Crivelli et al. (2016) in what they capture in addition to profit shifting. Because it is not possible to disentangle profit shifting from other factors, which are likely present, we do not include it in our comparisons.

forthcoming)), the identified misalignments are open to other interpretations than profit shifting and they are thus likely to be overestimates.

Fourth, Clausing (2016) derives her revenue effect estimates from the MNEs' profits' sensitivity to lower tax rates. A number of other studies have used a similar methodology for the case of the United States, but no other similar research paper has speculatively extended this methodology to other countries around the world or presented country-level results, as she does. The methodology used by Clausing (2016) has been recently critisized by Blouin and Robinson (2020) who argue that studies that use data on US MNEs from the Bureau of Economic Analysis to estimate the scale of profit shifting suffer from an upward bias due to inherent double counting of foreign income and to its misattribution to incorrect jurisdictions in the used data. Clausing (2020), on the other hand, argues that the proposed adjustments would omit some types of profit shifting, effectively introducing a downward bias. Clausing (2020) then provides estimates based on newly available data from country-by-country reports which do not suffer from the drawbacks highlighted by Blouin and Robinson (2020) and lead to results consistent with prior studies.

The data source that many of the recent profit-shifting studies aiming for a wide coverage of countries—including OECD (2015b) and Johannesen et al. (2020)—have used is the Orbis database, the largest commercially available database of company balance sheets. Orbis has the advantage of providing data that enable researchers to produce rigorous estimates about various profit-shifting channels, such as the choice of patent location within MNEs (Karkinsky and Riedel 2012). However, Orbis also has several quite well-known and substantive shortcomings. Most importantly, some countries' companies are more likely to be represented in Orbis than others. As Clausing (2016) and Alstadsæter et al. (2019) have argued, Orbis includes extremely limited information on companies from tax havens and any analysis based on its data thus excludes many observations of income-shifting behaviour. In fact, Tørsløv et al. (2018) find that only 17% of global profits of MNEs are observable in Orbis, and Cobham and Loretz (2014); Kalemli-Ozcan et al. (2015); Garcia-Bernardo et al. (forthcoming) document that Orbis coverage is severely limited among developing countries in particular. Therefore, as recently acknowledged by Garcia-Bernardo et al. (2017), the Orbis data is tilted against tax havens and developing countries, both of which are crucial for research such as ours. Instead of Orbis, we use country-level FDI statistics that have been employed in various recent studies ranging from Pérez et al.'s (2012) on illicit financial

flows as motives for FDI, to Akkermans (2017) considering the long-term effects of FDI.

The FDI data that we use in this paper are of quality that is sufficient for our purposes. On the one hand, the level of granularity of FDI data remains much lower than that of Orbis and some concerns about data quality are similar to those discussed for Orbis, especially when the data is reported by tax havens. We recognise that there are critical issues related to the data and we discuss at least a couple of them here. First, Haberly and Wójcik (2015) expressed concern about the representativity of FDI data. Second, FDI data are based on the immediate investor approach and do not enable us to distinguish whether an investor country is only a conduit or represents the real origin of the investment. Damgaard and Elkjaer (2017) empirically highlight the difference between using immediate counterparts and ultimate investing countries. Third, FDI data include information on the activity of the so-called special-purpose entities; we discuss this in the methodology section below. On the other hand, the extensive coverage of countries, and developing countries in particular, makes the FDI data superior to Orbis for our purposes. To the best of our knowledge, these are the most reliable data currently available, and we thus use them despite their limitations. We believe that both the Orbis and FDI data sets should be used for research into profit shifting and that their results can complement each other. Given the better country coverage of the FDI data, our empirical approach is apt for estimating the global distribution of profit shifting and the scale of the resulting tax revenue losses.

3.3 Data

Our approach uses the leading data sources with country-level information on FDI. We use bilateral data on FDI stocks from the IMF's Coordinated Direct Investment Survey (CDIS), which contains data for up to 112 countries between 2009 and 2016 (IMF 2019a). For stocks of direct inward investment, we use the variable 'Inward Direct Investment Positions, US Dollars (IIW_BP6_USD)'. Bilateral data are needed to calculate the share of FDI from tax havens, which is our main explanatory variable in the core regressions. In addition, to identify FDI attributable to special-purpose entities (SPEs) in the countries that allow these entities to exist, we use the OECD's Foreign Direct Investment Statistics,

which is the only source of FDI data that distinguishes between investment by SPEs and other firms, albeit for OECD countries only.[2]

The volume of the total global stock of international direct investment rose substantially over the observed time period, as shown for countries classified into income groups and regions in Figures B.2 and B.3 in the Appendix. While in 2009 the total global FDI stock amounted to $18.7 trillion, in 2016 it was $27.7 trillion—a 48% increase. All groups increased their FDI stock except one—the Middle East and North Africa lost 69% of its FDI stock, likely due to the combined effect of declining oil prices, the Arab spring and military conflicts in the region. The significant increase (by 1,382%) in South Asia's FDI stock between 2009 and 2015 is caused by the lack of data for India in 2009 – if we use India's 2010 value to compute the difference over the observed time period, we arrive at a modest 43% increase. We observe that the increase in total FDI stock was caused by investment from both tax havens (whose classification we explain in the following section) and other countries.

We also need information on FDI income, which we source from the IMF's Balance of Payments Statistics IMF (2019b). Specifically, we use three variables from this source: (i) the overall FDI income (the variable 'Current Account, Primary Income, Investment Income, Direct Investment, Debit, USD (BMIPID_BP6_USD)'); (ii) the equity component of FDI income (the variable 'Current Account, Primary Income, Investment Income, Direct Investment, Income on Equity and Investment Fund Shares, Debit, US Dollars (BMIPIDE_BP 6_USD)'; and (iii) the debt component of FDI income (the variable 'Current Account, Primary Income, Investment Income, Direct Investment, Interest, Debit, US Dollars (BMIPIDI_BP6_USD)'). We then compute the three corresponding rates of return on FDI (overall, equity component, and debt component) as the ratio of the corresponding FDI income to the total FDI stock in each country. While we believe that this is the best approach, it comes with four limitations. First, while investment from different countries may yield different returns across countries, the FDI income data are only available at country level (and not at a bilateral level), which hides some of the information that could potentially be used to obtain better estimates of the size of corporate profit shifting (for example by distinguishing between FDI income from tax havens and from other countries; such data is available for OECD countries,

[2]Only two countries that do not report this data in the OECD FDI Statistics and are known to allow SPEs are Luxembourg and Austria, for which we use the estimates of SPE shares provided by UNCTAD (2015) based on data from these countries' central banks.

but not for the majority of other countries, including developing ones). Second, although both sources (for FDI income and FDI stocks) that are combined into a single number (the rate of return on FDI) come from the IMF, they may use slightly inconsistent methodologies to identify what is classified as FDI. Third, while we use not only the overall FDI income, but also its equity component and its debt component, we divide all these three measures of FDI income by the same overall FDI stock, rather than the equity component and the debt component of the FDI stock. Fourth, FDI income in the IMF CDIS is recored on a net-of-tax basis, for which we have to adjust using an assumption on the applicable tax rate. Despite these data limitations, we believe that these sources provide us with as good information as there is on the true rate of return on FDI.

In addition to data on FDI, our methodological approach requires data sources that are auxiliary to the main analysis, including data on corporate tax rates from KPMG (2018) and the World Bank (2017), lists of tax havens from various sources, and data on GDP from the World Bank (2017), complemented, where missing, by data from the United Nations (2018) and the CIA (2018). To present the estimates in relative terms to tax revenues, we use the ICTD/UNU-WIDER's (2018) Government Revenue Dataset. We present summary statistics of the variables used in the analysis in Table A.10 in the Appendix.

3.4 Methodology

In this section, we describe the empirical strategy that we use to estimate the scale of profit shifting. Since profit shifting is inherently difficult to observe directly, the existing methodological approaches aim to indirectly shed more light on certain aspects of profit shifting. In this paper, we build on one such approach developed by UNCTAD (2015) and detailed by Bolwijn et al. (2018a), which we extend further to provide the answer to our main research question: which countries' corporate tax revenues are most affected by profit shifting? We begin by describing how we empirically test whether higher shares of FDI from tax havens are associated with lower reported rates of return on inward FDI. We then outline how we use these models to derive estimates of the scale of profit shifting and the resulting tax revenue losses. The final part of this section explains in detail how we define the share of FDI from tax havens in total inward FDI in each country, which we use as an input in the first part.

We begin by explaining the logic of the hypothesis that countries with a

higher share of foreign direct investment originating from tax havens tend to have lower rates of returns on FDI. The proposition central to our analysis is that a higher share of FDI from tax havens is associated with a higher tendency to shift profits to these tax havens, resulting in an artificially deflated reported rate of return on FDI. This proposition is in line with all three main channels of profit shifting recognised in the literature (i.e. debt shifting, trade mispricing, and location of intangibles). First, debt shifting to tax havens through inflated interest rate payments should lower the rates of return reported in countries that receive a lot of investment from tax havens. While there is some evidence that the debt shifting channel is not very important (Heckemeyer and Overesch 2017), no detailed quantification of the various channels seems yet to have been published. Second, when intangible assets are located in tax havens so that other countries need to pay for their use, this also lowers their reported rate of return. Third, if an MNE unit based in a tax haven artificially inflates exports to MNE units located in other countries, then this trade mispricing is consistent with lowering the rate of return in these other countries as well.

Therefore, we conclude that all these main channels of profit shifting should be reflected in the observed relationship between the rate of return on FDI and the share of FDI from tax havens. We acknowledge that there are some profit shifting schemes that would not show up in the FDI data (for example, a natural person that unofficially controls two separate entities that operate in two different countries may artificially shift profits across border through fictitious invoicing between these officially unrelated parties with no trace in the FDI data) or are otherwise not captured by our approach (such as the case, discussed below, when parent companies in non-havens shift profits to subsidiaries in tax havens). Overall, while some tax avoidance schemes remain beyond the reach of our empirical strategy, we argue that it is able to capture the three main channels.

Furthermore, our hypothesis is consistent with the observed heterogeneity in the scale profit shifting across countries, which can stem from country-level characteristics that are likely to affect profit shifting. For example, as countries implement different anti-profit shifting regulations, some schemes previously used to shift profit become unavailable, increasing the reported rate of return while at the same time decreasing the share of FDI coming from tax havens (as MNEs move away from these jurisdictions). The same mechanism applies to changes in tax enforcement capacity, governance institutions or corporate income tax rates. On the other hand, there may be other reasons for the

underlying relationship—more generally, that in case the share of FDI from tax havens is associated with the risk-profit characteristics of the underlying investments, this may drive the relationship even in the absence of profit shifing. On balance, we are not able to efficiently disentangle these effects, and we thus make the strong assumption that the relationship is due to profit shifting in line with UNCTAD (2015).

We empirically estimate the relationship between the rate of return on FDI and the share of FDI from tax havens using two models. For both models, we follow UNCTAD (2015) and we drop outliers displaying extreme values of rate of return or the share of investment from tax havens.[3] The first model is a baseline one that follows UNCTAD's (2015) methodology as closely as possible but uses updated data; the second is an extended model which, in addition to the year fixed effects and region-group fixed effects included in the baseline model, also includes income-group fixed effects and interaction terms with both regional- and income-group effects. More formally, in our baseline model, the regression to be estimated using ordinary least squares (OLS) with regional- and time-fixed effects is:

$$FDI\_ROR_{it} = \beta * share\_havens_{it} + \sum_{k=1}^{7} \phi_k d_{k,i} + \sum_{s=2009}^{2016} \delta_s z_{s,i} + \epsilon_{it}, \qquad (3.1)$$

where FDI_ROR_{it} it is the rate of return on FDI in country i in year t, $share\_havens_{it}$ is the share of FDI from tax havens in country i in year t, $z_{s,i}$ are year-fixed effects, and $d_{k,i}$ are regional-fixed effects based on the World Bank's classifications. The rationale behind using regional-fixed effects is that some regions share common characteristics that have significant effects on both the explanatory and dependent variables. We focus on two definitions of the rate of return—the overall rate of return and its equity component—and for both of these we hypothesise a negative relationship. In contrast, we expect a positive parameter estimate for the debt component since it is composed primarily of interest paid by the foreign affiliates to the parent, which is, in fact, a cost for the affiliates that is not subject to corporate income taxation.

[3]Specifically, we drop 13 observations with negative rate of return on FDI and 10 observations with rate of return on FDI higher than 0.8. We also follow UNCTAD (2015) in dropping from the analysis 8 countries with particular conditions that significantly influence the reported data on rate of return on FDI: Azerbaijan, Botswana, Macao, Russia, Nigeria, Kazachstan, Bhutan, and China. In addition, UNCTAD (2015) also drops Iceland, which, in our approach, is an SPE-country and is thus dropped for that reason.

In the second part of our empirical analysis, we propose an extended model:

$$\begin{aligned} FDI\_ROR_{it} = \alpha * share\_havens_{it} + \sum_{m=1}^{5} \beta_m * share\_havens_{it} * inc_{m,i} \\ + \sum_{k=1}^{7} \gamma_k * share\_havens_{it} * d_{k,i} \\ + \sum_{m=1}^{5} \zeta_m * inc_{m,i} + \sum_{k=1}^{7} \phi_k d_{k,i} + \sum_{s=2009}^{2016} \delta_s z_{s,i} + \epsilon_{it} \end{aligned} \tag{3.2}$$

where $inc_{m,i}$ are dummy variables for income groups (as per the classification by the World Bank), with the remaining notation the same as in the baseline model.

Our extended model makes two main innovations over the baseline model. First, we use a more granular definition for developing countries, which is based on the World Bank's classification of countries by income per capita. Specifically, we add controls for income groups in our model, using dummy variables in the full-sample regression, rather than splitting the sample for developing and developed countries and performing the regressions separately, allowing for improved explanatory power. Including income-group fixed effects is also a step towards a model with country-fixed effects, which would likely be a first-best model, but the data do not enable it because of the short period for which they are available and the low levels of variation in inward FDI stock and rate of return on FDI. While it would be desirable to account for countries' fixed effects in the estimation, the host country fixed-effects estimation results are not robust and mostly not statistically significant. Nevertheless, some specifications provide additional empirical support for our extended model (for example, a subsample of 69 countries for which all 7 yearly observations are available suggests a statistically significant negative estimate of the coefficient of a similar magnitude as reached by the estimates of our extended model). In addition, it would be possible to estimate a model with longer differences, however, large year-on-year deviations in FDI income and unclear long-term trends in the relationship would require a further set of strong assumptions. Given that the country-fixed effects model is not robust, we consider our extended model to be currently the best option for an estimation of the scale of profit shifting.

The second main innovation is that the extended model allows for effects that are heterogeneous across regions and income groups to influence the relationship between the share of FDI from tax havens and the rate of return

on FDI. This addition is enabled by interaction terms for income and regional groups with the share of FDI from tax havens. The regional and income-group effects are thus implicitly divided into those that affect the examined relationship and those that do not. The rationale behind this process is that the countries within these groups share some common characteristics that have a specific effect on the behaviour of the MNEs that route their investment through tax havens. Our approach allows us to capture these common effects. We also carry out a series of robustness tests to check the sensitivity of our results to the separate inclusion of regional- and income-group interaction terms.

While these two main innovations improve on the baseline model, there is an important assumption that we need to make for both the baseline and extended models. Namely, we argue that the negative relationship between the share of FDI from tax havens and the rate of return on FDI is due to profit shifting. There are at least two reasons to support this argument. First, as argued above, the observed negative relationship between the share of FDI from tax havens and the reported rates of return on FDI is consistent with all three main channels of profit shifting. Second, a wide body of literature has shown that profit shifting to tax havens leads to lower reported rates of return. For example, there is such evidence for developing countries (Fuest and Riedel 2012) as well as more recently for the United States (Wright and Zucman 2018). On the basis of these reasons, we argue that the reported rate of return is lower due to profit shifting. While we believe that our argument reflects the reality, we also recognize several factors that might lead to underestimation or overestimation, on balance most likely an underestimation, of the true size of the relationship.

First and foremost, we underestimate the scale of profit shifting by not capturing all tax avoidance schemes. The approach in this paper focuses on FDI from tax havens in other countries and it thus only captures profit shifting from subsidiaries located in non-havens to parent firms located in tax havens. Our estimates do not capture any profit shifting from parent companies in non-havens to subsidiaries in tax havens (e.g. the case of Apple Inc., headquartered in the United States with a subsidiary in Ireland). As a result, our estimates are likely underestimates of the true scale of overall profit shifting. Second, the rate of return in a hypothetical case of no profit shifting might be higher than what we assume it to be, which would lead to a downward bias. Every country has some FDI from tax havens, but our regression approach considers those with little FDI from tax havens as the implicit profitability benchmark

for those with more FDI from tax havens. In reality, a benchmark closer to the true value might be the profitability of otherwise comparable local companies (i.e. those with no profit shifting at all). If this is the case, and Tørsløv et al. (2018) provide some related empirical support for this view, we might be underestimating the scale of profit shifting. Third, our estimated relationship might suffer from the attenuation bias towards zero, also because our preferred specification is quite restrictive of the data. Any incomplete tax haven investment data coverage, as discussed above, might underestimate the share of FDI from tax havens and thus contribute to underestimation of the scale of profit shifting. Fourth, since data on FDI income is not available on a bilateral level, we instead use the FDI income information at the country level, averaging out the values – rather than having it (likely) lower for tax havens and higher for other countries – and this might lead to an underestimation of the relationship. Overall, in particular because of the first of these reasons, we believe that our results are lower-bound estimates.

On the other hand, we acknowledge that the reported rate of return might be lower due to reasons unrelated to profit shifting, potentially leading to an upward bias. There may be other reasons why FDI from tax havens yields lower rates of return than other FDI, such as its different sector-, risk- or expected yield-composition. To the extent that this is the case, we might overestimate the scale of profit shifting. Conversely, we also recognise that the reported rate of return might be higher due to reasons unrelated to profit shifting and, in that reverse case, similar logic would imply that we might be underestimating profit shifting.[4] This is an empirical question, but unfortunately the FDI data are only available at country-, rather than industry- and other disaggregated level, which prevents further improvement in the precision of the estimation in this respect. Overall, there does not seem to be a credible way to establish empirically the extent to which the negative relationship is or is not driven by profit shifting. We argue that our assumption on balance likely reflects the reality; nevertheless, this implies that we should be careful when interpreting and using these illustrative estimates of profit shifting.

[4]In case our empirical approach led to an overestimation of the scale of profit shifting, the extent of the overestimation would be proportional. For example, if profit shifting was responsible for only three quarters of the lower rate of return, then the scale of profit shifting would be lower by one quarter than that estimated in this paper. In the results sections we do not provide results with adjustments for this because we do not have any information in our data or the existing literature to derive these numbers using specific estimates and also because the adjustment for the potential overestimation is proportional and thus empirically straightforward on the basis of our results.

Once we obtain the estimate of the relationship between the share of FDI from tax havens and the rate of return on FDI, we can estimate how much profit is shifted and what the associated tax revenue losses are for the affected countries. Specifically, we multiply the responsiveness of the reported rate of return to the share of FDI from tax havens – a parameter estimated by the regression above – by the actual value of FDI from tax havens. Then, to arrive at an estimate of the associated tax loss, we transform the estimates of shifted profits to pre-tax values, an adjustment which is necessary because the original FDI data are after-tax. Finally, we multiply these estimates of pre-tax shifted profits by the relevant statutory corporate income tax rate (which implies that all the shifted profits would, had they not been shifted, have been liable to corporate income taxation; at the same time, if countries were to apply lower corporate income tax rates (for example by granting tax holidays to MNEs), they might not be able to collect the estimated amounts even if there was no profit shifting. In our results below, we report also the estimated tax revenue losses in case effective tax rates were applied to the shifted profits.). For the global baseline model, we do so in the same straightforward way as UNCTAD (2015), using total global values for FDI stock and average values for the share of FDI from offshore financial centres and the corporate tax rates.[5] In contrast, for the extended model, we use country-specific values for these variables whenever available. So, for example, we calculate the country-level estimates using specific corporate tax rates for each country rather than one estimate for all countries. This, together with the region- and income-group fixed-effects, makes the extended model more reliable than the basic model at the country level.

We now return to explaining how we define the share of FDI from tax havens that each country receives. In principle, it would be desirable to use relevant observable characteristics of tax havens and other investor countries, which could help us determine which individual tax havens are responsible for the tax revenue losses incurred by other countries. The inclusion of origin-country time-varying controls in the regression could enable us to control for characteristics of origin countries that might systematically vary between tax

[5]UNCTAD's (2015) approach can be summed up as follows (with their headline numbers for 2012 for developing countries in parentheses): corporate income tax revenues lost due to profit shifting for developing countries = average tax haven exposure of total inward FDI stock (46%) × reported FDI stock ($5,000 billion) × responsiveness of reported rate of return on tax haven investment (15.8%) × transforming the after-tax values to pre-tax values (1.25) × weighted average effective tax rate (20%) = $91 billion.

havens and other investor countries. In reality, however, there are hardly any data available other than FDI, GDP and some other basic economic variables for some of the tax havens in our analysis, and therefore it is not possible to control for any observable origin-country effects, either fixed or time-varying. Instead, we mostly rely on tax haven classifications and the available FDI-related data.

In particular, we identify tax havens, or offshore financial centres, in three categories, mostly following UNCTAD (2015). First, we use a list of 38 tax havens compiled by UNCTAD (2015) based on OECD's (2000) initial list of 41 jurisdictions.[6] The whole stock of FDI originating from these jurisdictions is considered offshore investment.[7] The second is a group of so-called self-declared special-purpose entity (SPE) countries. An SPE is an institutional unit set up to provide financial services to MNEs that allow it to transfer funds through a jurisdiction. These entities are sometimes called pass-through units or shell companies because the financial flows administered by these entities do not correspond to their actual economic activities in the SPEs' country of incorporation (OECD 2015a). Primarily, we use data published by the OECD to determine the average share of SPE-related FDI stock in the overall outward FDI of the 12 countries that report this data. Following UNCTAD (2015), for two other countries, we use data available from their corresponding central banks (Austria, 36%, and Luxembourg, 96%).

The final, third group of tax havens are 'other SPE countries', which do not declare themselves to be SPE-enabling countries, but seem to behave as such. We identify other SPE countries in the same way as UNCTAD (2015), proceeding in two steps. In the first step, we identify countries that have been successful in becoming important offshore financial centres. We classify

[6]Our list of 38 tax havens is the following: Anguilla, Antigua and Barbuda, Aruba, Bahamas, Bahrain, Belize, Bermuda, British Virgin Islands, Cayman Islands, Cook Islands, Cyprus, Dominica, Gibraltar, Grenada, Guernsey, Isle of Man, Jersey, Liberia, Liechtenstein, Malta, Marshall Islands, Mauritius, Monaco, Montserrat, Nauru, Netherlands Antilles, Niue, Panama, Saint Kitts and Nevis, Saint Lucia, Saint Vincent and the Grenadines, Samoa, San Marino, Seychelles, Turks and Caicos Islands, US Virgin Islands, Vanuatu.

[7]We acknowledge that this method partly relies on somewhat arbitrary decisions about the criteria for the dichotomous selection of tax havens, which have been criticised for example by Cobham et al. (2015). Indeed, we would prefer to use a continuous measure that does not rely on binary criteria, such as the difference between applicable corporate income tax rates in the two countries in question. The way we identify tax havens in three groups at least combines binary with continuous measures but, to our knowledge, there is currently no such single continuous measure for offshore investments. In addition, we carry out robustness checks using other lists of tax havens and reach similar results. Unfortunately, we currently do not see a suitable alternative metric that would enable us to move away from this partially dichotomous approach while being available for a wide range of countries.

a country as an 'other SPE country' if, as of 2016 data, it: (i) ranks in the first quartile in terms of inward FDI stock; and (ii) it has a ratio of inward FDI stock to GDP of more than 1. For 2016 data, we identify 26 countries complying with the first criterion and 20 with the second, with nine countries at the intersection of these two groups (complying with both criteria). Excluding tax havens and self-reported SPE countries (which were already handled in the first two groups) results in four countries being classified into the final 'other SPE countries' group. In the second step, we consider these four countries and calculate the level of investment implied by the size of their economy (based on a simple OLS cross-country regression of reported inward FDI on GDP in 2016). The difference between the actual FDI stock and the predicted FDI stock is then accounted towards the share of FDI from tax havens (Hong Kong, 89.9%, Ireland, 80.3%, Singapore, 85.7%, and Switzerland, 74.5%). Combined, the three categories contribute to how much each country receives in inward FDI from tax havens relative to its total inward FDI. This figure feeds into the regression at the beginning as an explanatory variable and we also begin the discussion of our results with it.

3.5 Results

We present our empirical results in this section. First, we present estimates of the baseline model using updated data sources. Second, we estimate the newly developed extended model and present its estimates of profit shifting and the resulting tax revenue losses. Third, we compare our results with four other similar studies and highlight their relevance for the cross-country distributional impact of international corporate profit shifting.

We begin with the results of the estimation of the baseline model. For both the rate of return and its equity component, we find a statistically significant negative relationship between the share of inward FDI originating from tax havens and the rate of return on FDI. We report the detailed results in Table A.11 in the Appendix.[8] Compared to the estimations carried out by UNCTAD (2015), we use data for all countries for which it is available in the period 2009-2016, increasing the sample from 265 to 614 observations. Follow-

[8]As an example, let us consider the parameter estimate of -.0713 for the independent variable, the share of FDI from tax havens, in the specification whose dependent variable is the equity component of FDI rate of return. This estimate implies that every one percentage point increase in the share of FDI from tax havens is associated with a lower rate of return by 0.0713 percentage point.

ing UNCTAD (2015) to ensure comparability, we then divide the sample into two groups—developing and developed countries—and estimate the model for three alternatives of the dependent variable: the overall rate of return on FDI and its equity and debt components. Our results suggest slightly smaller coefficients in absolute value, except for the model that only includes developed countries, where we find a slightly higher effect, statistically significant at the 1% level. Furthermore, we obtain statistically insignificant positive coefficients for the model that uses the debt component of the FDI rate of return as the dependent variable. This positive coefficient is in line with the notion that the debt component is composed primarily of interest paid by foreign affiliates to the parent, which is, in fact, a cost for the affiliates and thus an element that actually erodes the taxable base. In the remaining part of our analysis, including the extended model, we focus only on models that use the equity component of the rate of return or the overall rate of return itself, while preferring the equity component models due to their higher relevance for profit shifting by MNEs.

We use the baseline model's results to derive an estimate of the scale of profit shifting, both worldwide and for individual countries. Table A.12 in the Appendix summarizes the results for 2016. We use information on the total global exposure to tax haven investment (39.5% for all, 49% for developing and 35% for developed countries) and the total reported FDI stock ($16.68 trillion for all, $10.51 trillion for developed and $6.18 trillion for developing countries). One option to derive an estimate of profit shifting is to use the regression estimates for all countries from Table A.11. By doing so, using our preferred model with the equity component of the rate of return as a dependent variable, we arrive at a global estimate of $665 billion in shifted profits, and a corresponding $194 billion lost in tax revenues in 2016. Even in relative terms, these are large numbers – according to these estimates, around 0.9% of the world's GDP or 5.8% of all corporate profit is shifted to tax havens. Tørsløv, Wier, and Zucman (2018) report that the profits of foreign corporations in 2015 amounted to around $1.7 trillion and that close to 40% of these reported profits were shifted, and our estimates from the global baseline model reach similar numbers. Regarding tax revenue, the estimate of $194 billion in global corporate tax revenue foregone due to profit shifting means that 9.8% of the total corporate tax currently collected is avoided by MNEs.

While the obvious advantage of using this baseline model is that we obtain estimates of profit shifting and tax losses for all countries (except, naturally, for tax havens and SPE countries), one drawback is that it averages out significant

heterogeneity across countries. Therefore, we consider more granular options, starting with one that divides the sample into two groups – developing and developed countries. Our results for 2016, presented in detail in Table A.12 in the Appendix, show similar results to those reached by UNCTAD (2015) for 2012. While our estimated coefficient from the regression is slightly lower, the total FDI stock in developing countries increased from $5 trillion in 2012 to 6.18 in 2016, leading to estimates of similar magnitude – $83 and $95 billion lost in tax revenue in developing countries in 2016.[9] Using actual country-level inward FDI stock and corporate tax rates (rather than the averaged ones as indicated in column F of Table A.12) results in country-level estimates as presented in the first two columns of Table A.16 in the Appendix. These estimates use the same estimated coefficients for all countries and for the groups of developed and developing countries (in the second and third column, respectively). In our extended model, we use an even more granular level of fixed effects at the region-income group level to derive more precise estimates.

In the second part of the results section we turn to the results of our preferred, extended model. We begin with the regression results in Table 3.1 with two specifications that differ in their dependent variable: the overall rate of return versus only its equity component. In line with the hypotheses outlined above, we observe a statistically significant, negative relationship between the share of FDI from tax havens and the rate of return on FDI as well as its equity component. Importantly, the regressions in the extended model include controls for income-, region- and year-fixed effects and interactions of the main explanatory variable, the share of FDI from tax havens, with dummy variables indicating the regional and income-group classifications, which are included both separately and simultaneously. The coefficient estimates of the negative relationship across these specifications are of similar magnitude and statistical significance, as shown in Table A.13 in the Appendix. While simultaneous inclusion of the two sets of interaction terms improves the explanatory power of the model, it could potentially lead to multicollinearity. We therefore use an F-test and find that the coefficients for both groups of the interaction terms

[9] One speculative, and likely too optimistic, explanation for the lower estimated parameter estimates is that recent government efforts to curb profit shifting have already started to have an impact and we can observe that change in the estimates. Also speculatively, because of the statistically insignificant coefficients for developed countries, we derive the estimate of $46 billion of tax revenue losses for developed countries – this is to be interpreted with caution. If we combine it with the estimate for developing countries, a global estimate of $129–$141 billion is slightly lower than that of our first model, which used the same regression estimate for all countries.

are jointly statistically significant at the 1 per cent level of significance. Also, as supporting evidence, we observe in Figure 3.1 that the countries are distributed quite widely across income groups as well as regions, which is not consistent with the presence of multicollinearity. At the same time, it is important to highlight that none of the interaction terms is significant at the 5 per cent level of significance, which suggests that that there might not be large differences between countries from different income and regional groups. In our preferred specification we conservatively use standard errors clustered at the region-income group level, and we further report in Table A.14 in the Appendix the results of estimations that use robust standards errors and standard errors clustered at the country level, while keeping in mind the low heterogeneity in the explanatory variable over time which prevents a country-fixed effects approach, as discussed above. Indeed, the standard confidence intervals associated with some of our estimates for individual region-income pairs are relatively large and our point estimates should thus be interpreted with caution.

Having established that the results are robust to the inclusion of the interaction terms and do not suffer from multicollinearity, our preferred extended model is the one with all interaction terms simultaneously included (shown in Table 3.1 and in column 3 of Table A.13). The results of the estimation of this preferred extended model in the form of a summary of region-income group combinations are presented in Table A.15 and in Figure 3.1.[10] In Figure 3.1 as well as other figures we include estimates of 95% confidence intervals of the sums of the coefficients.[11] These confidence intervals indicate the level of uncertainty of our estimates.

Our empirical approach is only suitable for estimating non-haven countries' revenue losses, rather than tax havens' revenue gains. Therefore we exclude from further analysis the countries in those region-income groups for which the estimated parameter estimates are positive.[12] Our extended approach takes

[10] As an example, the estimate of -0.193 for Latin American lower middle income countries is a sum of the coefficients for the share of FDI from tax havens (-0.123) and for the interaction terms with the lower-middle income binary variable (0.0415) and the Latin American & Caribbean binary variable (-0.112).

[11] To estimate these confidence intervals, we calculate the standard errors of the combined distributions of the relevant estimated coefficients for each region-income group. We calculate the standard errors of the sum of the random variables, assumed to be normally distributed, using the standard formula (for the variance of the sum of three normally distributed random variables X, Y, and Z): $Var(X+Y+Z) = Var(X) + Var(Y) + Var(Z) + 2 * Cov(X,Y) + 2 * Cov(Y,Z) + 2 * Cov(X,Z)$.

[12] We recognise several potential reasons why we obtained positive parameter estimates for some country groups (which in the data include 13 countries). For example, our list of tax

advantage of the inclusion of region- and income-fixed effects and exploits the heterogeneity in the relationship between the rate of return and the share of FDI from tax havens across combinations of these classifications, thereby providing a more country-specific, and so more precise, estimate of the relationship for individual countries. Also, the negative relationship between the share of FDI from tax havens and the rate of return of FDI is fairly robust, with alternative classifications of tax havens yielding similar results. For example, it is statistically significant and negative for a list by Gravelle (2015) used by IMF's Crivelli et al. (2016).[13] We now use these robust estimates of the relationship to estimate tax revenue losses.

havens and SPE countries is the same for all countries, but in reality, each country's MNEs may use different tax havens with different intensity, resulting in an artificially deflated or inflated share of FDI from tax havens for such countries. A potential solution for future research might be to weigh the tax-haven FDI against a form of bilateral definition for tax havens, preferably defined as a continuous variable rather than a binary one. Alternatively, the data on bilateral FDI may be collected using different methodologies in different countries, as not all countries comply with the IMF's international standards for FDI reporting.

[13]In principle, it would be possible to proxy the contribution of each tax haven to the revenue losses by estimating the regression with the independent variable of FDI share from one specific tax haven. In an exploratory analysis, we find that for many countries the variability of FDI data over such a short time period is not high enough for us to estimate the responsiveness of the rate of return on FDI to changes in the country of origin of FDI. Still, for some tax havens, especially those with good data coverage such as the Netherlands–the largest FDI investor according to the data–the estimated relationship is statistically significant and negative and it thus holds at individual origin-country level. Future research could examine these patterns to reveal which tax havens most contribute to the estimated revenue losses.

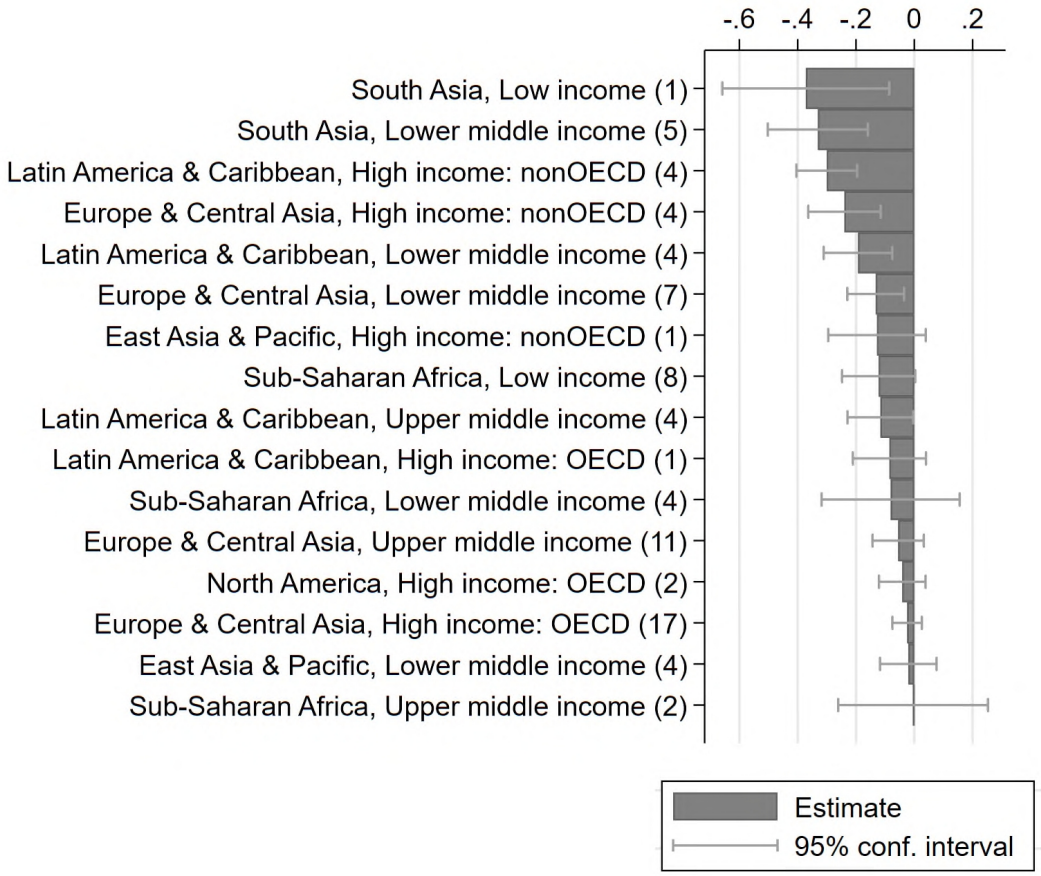
Table 3.1: Estimation results of the extended model

	(1)	(2)
	Rate of return	Rate of return – equity component
Share of FDI from tax havens (Share)	-0.158***	-0.123***
	(0.018)	(0.018)
Share * Low income	Omitted (=base)	Omitted (=base)
Share * Lower-middle income	0.0655	0.0413
	(0.140)	(0.132)
Share * Upper-middle income	0.207	0.118
	(0.142)	(0.137)
Share * High income: non-OECD	-0.0258	-0.0659
	(0.149)	(0.143)
Share * High income: OECD	0.189	0.149
	(0.146)	(0.137)
Share * Sub-Saharan Africa	Omitted (=base)	Omitted (=base)
Share * Europe and Central Asia	-0.0495	-0.0513
	(0.141)	(0.131)
Share * East Asia and Pacific	0.0936	0.0605
	(0.138)	(0.132)
Share * Latin America and Caribbean	-0.134	-0.112
	(0.147)	(0.144)
Share * Middle East and North Africa	0.205	0.212
	(0.163)	(0.159)
Share * North America	-0.0324	-0.0679
	(0.139)	(0.129)
Share * South Asia	-0.188	-0.25
	(0.220)	(0.212)
Constant	0.0639***	0.0622***
	(0.013)	(0.018)
Observations	631	614
R-squared	0.264	0.278
Income effects	Yes	Yes
Regional effects	Yes	Yes
Year effects	Yes	Yes

Source: Authors.

Note: Standard errors clustered at region-income level in parentheses. $***p < 0.01, **p < 0.05, *p < 0.1$.

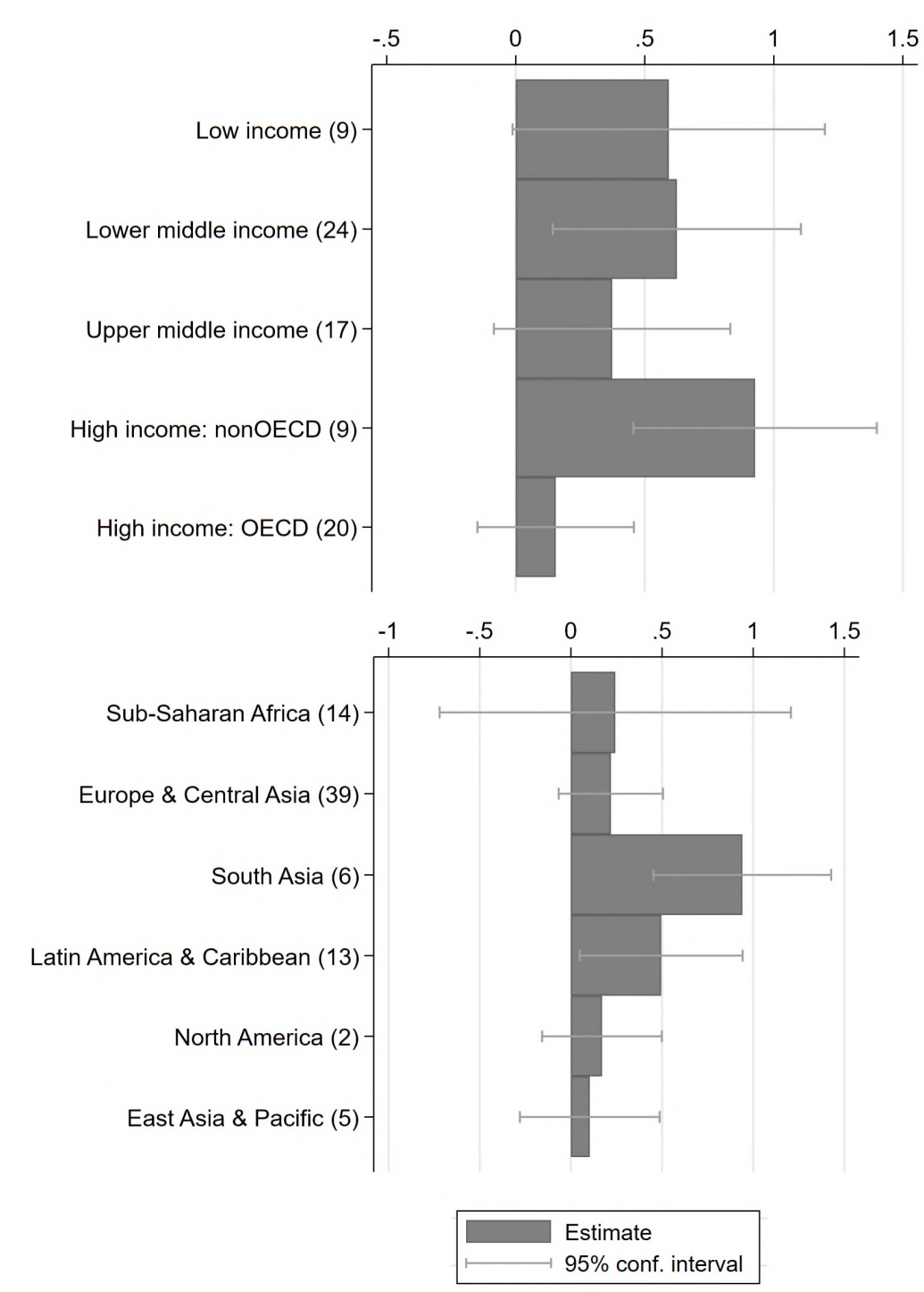
Figure 3.1: Estimated coefficient of the relationship between the share of FDI from tax havens and the rate of return on FDI



Source: Authors.

Note: The number of countries in each region-income pair is in parentheses.

Figure 3.2: Estimated tax revenue loss as a share of GDP, by income and region group, 2016



Source: Authors.

Note: The number of countries in each income or regional group is included in parentheses.

To estimate tax revenue losses, we follow the steps as applied above for the baseline model, but with information specific to each country. In particular, we use country-specific information on FDI stock, exposure to FDI from tax havens and corporate tax rates. This contributes to the two sources of heterogeneity in the country-level estimates that we present below: across different region-income groups and across countries within each region-income group. First, the differentiated regression estimates contribute to the differences across countries from different income-region groups. Second, the differences in the estimated tax revenue losses across countries within the same income-region groups are driven by the heterogeneity in FDI stock, tax haven exposure and corporate tax rates. While Figure 3.1 displays the first source of heterogeneity, Figure 3.2 and other results below capture both sources of heterogeneity together. In total, we obtain estimates of tax revenue losses for 79 countries; this represents a comparatively good country coverage, in particular for developing countries.

We now put the aggregate estimates in macroeconomic perspective. If we sum up the 79 country-specific estimates for our preferred specification using the equity component of the rate of return, the total profits of MNEs that were shifted out of these 79 countries in 2016 amounted to $420 billion, resulting in these countries incurring tax revenue losses of $125 billion.[14] We compare these estimates with relevant macroeconomic statistics from two sources. First, Tørsløv, Wier, and Zucman (2018)[15] report that the total corporate profits reported in the countries in our sample amounted to around $6,340 billion, implying that we estimate that around 6% of all corporate profits are shifted to tax havens. MNEs' profits from our 79 countries account for $1,122 billion, so this in turn implies that around 37% of the MNEs' profits are shifted. Reported tax revenues in the 79 countries in our sample amount to approximately $1,086 billion annually, implying an estimate of around 10% of corporate tax revenue lost due to profit shifting. Second, for the subsamples of 53 and 66 countries for which data is available in the ICTD/UNU-WIDER's Government Revenue Dataset for corporate tax revenue and total tax revenue, respectively,

[14]This estimate uses nominal corporate tax rates to derive tax revenue losses from shifted profits. If we use effective tax rates for the 40 countries for which these are available from Garcia-Bernardo et al. (2020), the sample total decreases from 94 to 68 billion USD annually.

[15]We use the statistics from Tørsløv et al. (2018) only for comparison and presentational purposes. While Tørsløv et al. (2018) only report country-specific statistics on corporate profits, MNE profits, and corporate tax revenue for 28 out of the 79 countries in our sample, they also report an aggregate number for the rest of the world. From this number, in order to enable comparisons, we interpolate country-specific statistics using GDP, assuming that the share of GDP of each country within this group is equivalent to its share of corporate profits, MNE profits, and corporate tax revenue.

our estimates imply that 8% of corporate tax revenue and 1% of total tax revenue is foregone. In terms of GDP, the shifted profits amount to close to 1%, and around 0.25% of GDP is foregone in corporate tax revenue. We present estimates of shifted profits and corporate tax revenue losses for 79 individual countries in Table A.16 in the Appendix, also as shares of corporate profits, profits of MNEs, corporate tax revenue, total tax revenue, and GDP. Overall, our estimates' wide coverage—especially for low and lower middle income countries—makes them particularly suitable for the study of the global distributional impact of international corporate profit shifting.

We compare estimated corporate tax revenue losses, relative to the countries' GDP and tax revenues, of country groups classified by income per capita. Figure 3.2 presents weighted averages for five income groups and seven regional groups classified by the World Bank. With the exception of the specific group of high income non-OECD countries[16], the point estimates suggest that low and lower middle income countries lose more tax revenue as a share of their GDP than high income OECD countries, but the confidence intervals are quite large and the differences between most income groups are not statistically significant. The exception of interest is the difference between the low income and high income OECD countries whose 95% confidence intervals in Figure 3.2 do not overlap. But generally, we find that there are almost no statistically significant differences across these groups.

In addition, we compare the point estimates for various groups of countries in Tables A.17—A.20 in the Appendix using a one-sided t-test for differences in simple averages of estimated tax revenue losses as a share of GDP and corporate tax revenue. In Table A.17 we report that there is a statistically significant difference in the average point estimates for low and lower middle income countries (around 0.6% of GDP) with respect to high income OECD countries (around 0.15% GDP). Similar results hold for these estimates expressed as shares of corporate tax revenue – we estimate that low (lower middle) income countries lose nearly 20 (12) percentage points more corporate tax revenue than high income, OECD countries. We report these results for three alternative country groupings (an aggregated World Bank income classification, which uses only

[16]A relatively high weighted average for the group of 10 high income non-OECD countries (0.88%) is driven to a large extent by the result of one country, Russia, which we estimate foregoes 1.3% of its GDP through corporate tax revenue loss due to profit shifting. Indeed, without Russia, the weighted average for high income non-OECD countries drops to a modest 0.39%, which is below the weighted averages of both low income and lower middle income countries.

three low-middle-high income groups instead of the original five, World Bank regional classification and UNCTAD development status classification) in Tables A.18—A.20, but conclude that, in general, the differences in the estimated tax revenue losses across groups of countries are mostly insignificant.

Figures B.4, B.5 and B.6 in the Appendix show the estimates of tax revenue losses as shares of GDP, shares of corporate tax revenue and total tax revenue, respectively, for all countries with available data, providing a clearer picture of which individual countries' losses contribute most to the aggregated numbers for income and regional groups that are displayed in Figure 3.2.[17]

In the final part of this section, we compare our estimates with those obtained by four other recent studies: Tørsløv, Wier, and Zucman (2018), Cobham and Janský (2018), Cobham and Janský (2019) and Clausing (2016). As we discuss in the related literature section above, these four studies use very different methodologies and data. While recognising the differences in empirical approaches and related difficulties, we make these comparisons to observe whether the inconsistent methodologies lead to similarly inconsistent results or not. We prefer to compare the estimates in relative terms, as we did for our main results discussed above, although we do provide a direct comparison in dollar values as well.

Each of the four studies provides estimates of MNE's profit shifting at the country level, but they differ in the extent to which they use extrapolations. Tørsløv et al. (2018) cover 37 countries at the country level and they estimate the total annual revenue loss of these countries at $165 billion. In addition, they provide estimates for other countries in the form of the 'Rest of the world' group, reaching a total estimate of $185 billion lost in tax revenue annually. Cobham and Janský (2018) provide estimates for the 102 countries for which they have data and for which they estimate their model. Cobham and Janský (2019) only briefly discuss the extrapolation of their estimates to non-

[17]We present the estimates of tax revenue losses here as shares of GDP because, in contrast to the other few suitable indicators for the relative size of the tax revenue losses such as total or corporate tax revenues, data on GDP is available for all countries in our sample. Generally, however, corporate tax revenues or total tax revenues are preferred to GDP for these comparisons; they would provide a more realistic perspective and better guidance from the tax revenue point of view, but their coverage is substantially worse than for GDP. Therefore, we believe that it is worth presenting the estimated losses in terms of the corporate tax revenues and total tax revenues, even if only for a subsample of countries. In Figures B.4-B.6 and Table A.16 in the Appendix we present our estimates at the country level as shares not only of GDP, but also of corporate tax revenue and total tax revenues, respectively, for all countries in our sample that have data on these tax revenues available for 2016 in the ICTD/UNU-WIDER's Government Revenue Dataset. The results show that significant shares of the countries' current tax revenues are relinquished due to profit shifting.

US-headquartered MNEs and prefer the sum of their country-level estimates. Clausing (2016) extends her US estimates to 24 other countries to cover 95% of the total profit share of Forbes Global 2000. In this paper's extended model, we carry out the estimation at income- and region-group level and then we apply these estimates at the individual country level for all 79 countries for which we have the underlying data. We then present results for the 79 countries and their sum only (in contrast, our baseline model estimates, similarly to those of UNCTAD, 2015, are extrapolated to the rest of the world on the basis of FDI data). For the comparisons across studies below we use their estimates for individual countries, as detailed in Table 3.2, which summarises the number of individual countries covered, the total estimated tax revenue losses and their averages as shares of GDP for each of the five studies.

Table 3.2: A summary of the five studies estimating the scale of profit shifting

	Number of countries	Total annual revenue loss of these countries (USD billion)	Average tax revenue loss (% GDP)
Our estimates	79	125	0.26%
Tørsløv et al. (2018)	37	165	0.26%
Cobham and Janský (2018)	102	90 / 494*	0.15% / 0.81%*
Cobham and Janský (2019)	34	133	0.21%
Clausing et al. (2016)	25	280	0.48%

Source: Authors on the basis of the cited studies.
*Notes: *For Cobham and Janský (2018) we present here short-run and long-run estimates, respectively. $90 billion (0.15% of GDP) is the short-run estimate, the long-run estimate amounts to $494 billion (0.81% of GDP). In this paper we use the short-run estimates as the preferred ones. See Cobham and Janský (2018) and our discussion in Section 3.2 and below for more details.*

The total annual revenue losses estimated by the five compared studies are all in the lower hundreds of billions of US Dollars. Our estimate of $125 billion is lower in magnitude than those reached by some of the comparison studies, which is in line with our estimates likely being lower bound estimates. It is also within the often-cited range of $100-$240 billion reported by OECD's Johansson et al. (2017). The long-run results of Cobham and Janský (2018),

whose estimates are quite rough, and those by Cobham and Janský (2019), who present estimates only for US-headquartered MNEs and are thus narrower in their coverage, live up to their descriptions as overestimates of profit shifting, as we discussed in the related literature section above. We believe that the relatively lower estimate by Tørsløv et al. (2018), slightly higher than ours, is likely closer to the true scale of profit shifting, at least for the covered countries.

Differences across income groups are identified by every study, but the nature of these differences varies across the studies. Figure 3.3 compares the various studies' results by showing the estimated tax revenue losses as weighted shares of GDP for the five income groups used above and also includes, in parentheses, the number of countries per income group for each of the studies. In the first such comparison made, we find that, for example, for high income OECD countries the estimates range from 0.15% of GDP in our results to around 0.54% of GDP in those by Clausing (2016). In theory, this might be driven by the differences in total revenue losses discussed above, but in practice it is not because there are also substantial relative differences across studies. To generalise, we can divide the studies into two groups according to their high-level findings in terms of income groups. The three studies by Tørsløv et al. (2018), Cobham and Janský (2019) and Clausing (2016) identify high income OECD countries as those most affected by profit shifting, but that is also the group of countries by far most represented in these studies.

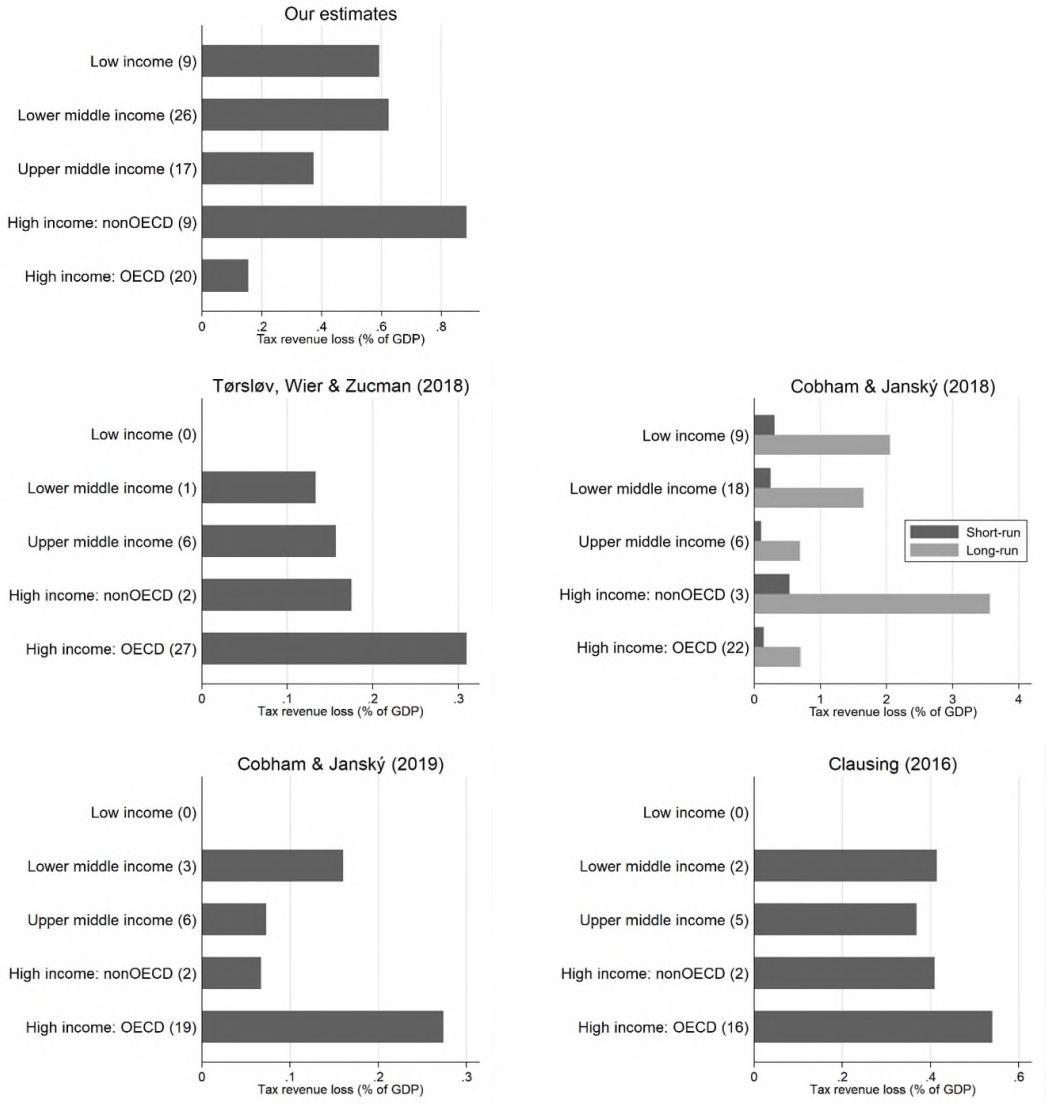
The results are different in the two studies with better country coverage. Our results as well as those by Cobham and Janský (2018) point to a similar pattern: that the tax revenues of low and lower middle income countries are likely affected as much, and possibly more, as those of high income OECD countries. For the results of Cobham and Janský (2018), in Figure 3.3 we also include their long-run estimates which are approximately four times higher and are constructed to reflect the long-run effects of corporate tax base erosion. As discussed in Section 3.2, however, we argue that their short-run estimates are more comparable to the results of the other studies and we thus use only these in the subsequent figures.

Another way to look at the results is through Figures B.8 and B.9, which show for each income group the share of the total profits shifted and the total tax revenue losses, respectively, as estimated by the studies. Since these are absolute numbers, it is not surprising that the higher-income economies' losses account for the bulk of global shifted profits and tax revenue losses. However, the two studies that do cover a number of low and lower middle income countries

suggest that these countries are indeed subject to significant profit shifting and incur large corporate tax revenue losses as a result. A similar picture is drawn by Figures B.10 and B.11, which show the sums of the shifted profits and tax revenue losses for each income group as estimated by each of the five studies. In addition, we estimate correlation coefficients, although they are not very informative.[18]

[18]We further analyse correlations between our results and the results in the four other papers and GDP per capita to shed more light on the relationship between countries' incomes and their estimated tax revenue losses resulting from profit shifting, and to compare our estimates more rigorously with those reported by similar studies. Table A.23 in the Appendix reports the unweighted correlation coefficients for tax revenue losses as shares of GDP. The interpretation of the differences in the correlation coefficients is complicated due to the substantial differences in country coverage, which we discuss in the main text. Overall, the estimated correlation coefficients vary across the five studies, and most of the correlation coefficients are not different from zero at the standard levels of statistical significance.

Figure 3.3: Estimated tax revenue loss as a share of GDP - weighted averages by income group, 2016



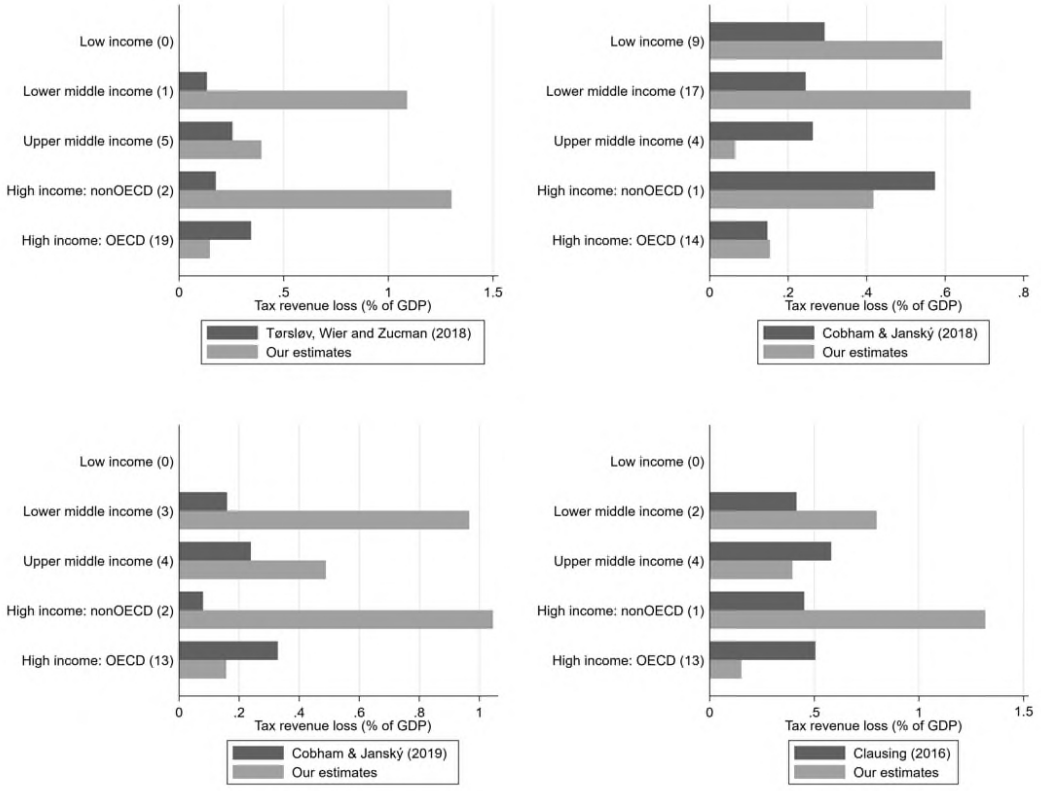
Source: Authors, data from Tørsløv et al. (2018), Cobham and Janský (2018), Cobham and Janský (2019) and Clausing (2016).
Note: The number of countries in each income group is included in parentheses.

Due to large differences in the coverage of countries, especially in some income groups, we also compare the results of the five studies using only common subsamples of covered countries. Figure 3.4 shows individual comparisons of our results with each of the other four studies, again with the numbers of countries that are covered by these pairs of studies in parentheses. In general, our estimates are lower for developed countries and higher for developing countries. Unfortunately, only eight countries (six of which are high income OECD coun-

tries) lie at the intersection of the samples of all the five studies, allowing a single direct comparison of the results of the five studies. The revenue losses of these eight countries as estimated by the compared studies are presented in Figure 3.5, but the studies seem to differ for this common sample as much as in their overall findings presented above.

We now discuss the likely reasons why there are differences between the studies. The various differences between this and the other four studies are difficult to reconcile and we argue that there are two main reasons behind this. First and foremost, the methodologies used in the five studies are very different and some of them, such as Cobham and Janský (2018)), are not very reliable, as we discussed in the related literature section. Second, there are important differences in the overall coverage of countries per study—ours covers 79, while Tørsløv et al. (2018) cover 37, Cobham and Janský (2018) 102, Cobham and Janský (2019) 30, and Clausing (2016) 25. Importantly, the number of countries included in the individual income groups varies greatly. For example, neither Tørsløv et al. (2018), nor Cobham and Janský (2019), nor Clausing (2016) have any low income countries in their sample and only a few lower middle income countries (1, 3, and 2, respectively), while our paper, as well as that by Cobham and Janský (2018), have a relatively good coverage of low (9 and 24, respectively) and lower middle income countries (24 and 29). Tables A.21 and A.22 provide a more detailed look at each study's coverage of countries and the economic activity measured by GDP in each income group.

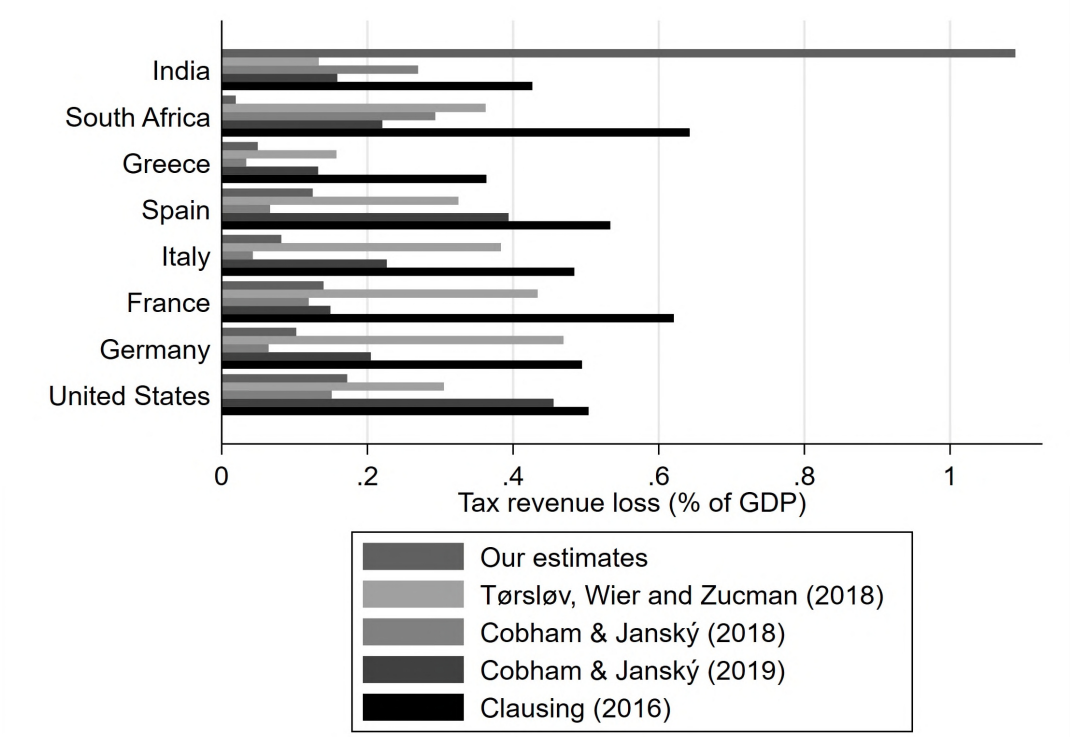
Figure 3.4: Total estimated tax revenue losses by income group, pairwise-consistent samples, 2016



Source: Authors, data from Tørsløv et al. (2018), Cobham and Janský (2018), Cobham and Janský (2019) and Clausing (2016).

Note: The number of countries in each income group is included in parentheses.

Figure 3.5: Estimated tax revenue losses – one consistent sample across all studies, 2016



Source: Authors, data from Tørsløv et al. (2018), Cobham and Janský (2018), Cobham and Janský (2019) and Clausing (2016).

3.6 Conclusions

In this paper, we have focused on quantifying the scale of profit shifting by MNEs and the resulting corporate tax revenue losses, using FDI data. We recognize the contribution of this work to the existing literature in the following five aspects. First, we have followed the so-called FDI approach, one of the leading methodological approaches to estimating the scale of profit shifting as pioneered by UNCTAD (2015), in what we call the baseline model, using new data to obtain updated estimates and confirming the robustness of the approach. Second, we have developed an extended model which innovates on the baseline model in a number of ways and has enabled us to obtain detailed estimates of the scale of profit shifting. Our preferred extended model estimates annual tax revenue lost by the 79 countries in our sample due to profit shifting at $125 billion. Third, we have used the extended model to arrive at tax revenue loss estimates. We provide estimates for a wide range of countries,

including a number of developing ones. We find that while OECD countries lose the least, low and lower middle income countries lose the most corporate tax revenue both relative to their GDP and relative to their corporate and total tax revenue. Fourth, we have compared our results with four other existing studies. Fifth, we have used our and other studies' estimates to observe differences in how various countries' government revenues are affected by profit shifting. All the existing studies identify differences across income groups, but the nature of these differences varies across the five studies.

We find, using our new estimates, that profit shifting and associated tax revenue losses are relatively high in most studied countries and across most income groups. At the same time, low income countries are more likely to be among those that are relatively less able to implement effective tools to reduce the amount of profit shifted out of their countries. In terms of policy recommendations, our work thus further corroborates the importance of the wider inclusiveness of initiatives such as the OECD's Base Erosion and Profit Shifting framework for the tax revenues that developing countries need. Our estimates might also assist policy makers in developing countries for which country-specific estimates of profit shifting scale were previously hard to come by. More generally, we provide estimates of the vulnerability to profit shifting for individual countries, and policy makers should pay close attention to their countries' specific situations.

We contribute to the existing literature with improved estimates of the scale of profit shifting using FDI data. Since the data do not enable us to precisely quantify the various biases of our estimates, as we have discussed, our estimates are only illustrative. Furhtermore, the currently available data do not allow methodologies that would likely be superior to the approach we take here (such as country-fixed effects models or models using longer differences). In addition to addressing these limitations, future research should further develop the empirical approach to reveal which havens are responsible for the estimated revenue losses. For example, future methodologies could relax some of the assumption we are forced to make in this paper: on the lists of tax havens, on the determinants of the relationship between the rate of return on FDI and the share of FDI coming from tax havens, or on corporate income tax rates. In addition, one promising new source of data that could improve the empirical strategy is the MNE country-by-country reporting data which was released in aggregate and anonymised form in July 2020 by the OECD and first analyses of this data are underway at the time of this writing in October 2020. However,

only once the CbCR is made publicly available in full and detailed form will researchers and policy makers find a comprehensive answer to what the true scale of profit shifting is.

Chapter 4

Secrecy Jurisdictions and the Countries They Harm

4.1 Introduction

Successive offshore leaks have shone a light on the enormous harm caused by financial secrecy on front page and in primetime news across the globe since 2013. The Panama Papers, for example, caused a number of politicians to resign and affected the valuation of firms around the world (O'Donovan et al. 2019). Not only tax-related offenses were demonstrated to thrive under the cloak of secretive shell companies and other financial secrecy vehicles, but also illegal drug and arms trafficking, grand corruption and money laundering operations by organised crime (Obermayer and Obermaier 2016). These revelations have strengthened governments' commitment to address the financial secrecy of secrecy jurisdictions with new policies and to bolster existing regulatory initiatives. These include blacklisting of tax havens by the European Commission and the cross-border automatic information exchange (AIE) on the capital income of non-residents. The AIE now operates on the basis of a multilateral

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framework agreement to which over 100 jurisdictions are signatories, resulting in a web of over 4000 bilateral information exchange relationships as of May 2020 (OECD 2020; Tax Justice Network 2020b). The political breakthrough for this system was announced on 19 April 2013 in a G20 communiqué (Meinzer 2018; G20 2013), shortly after the breaking of the Offshore Leaks on 2 April 2013 (ICIJ 2013). Despite the obvious nominal progress towards financial transparency, it is not clear how much real financial transparency has been achieved and how it is distributed across countries.

This paper addresses the question of how successful countries are in covering secrecy jurisdictions with their policies, in particular, AIE and blacklisting. In other words, do countries cover their most relevant secrecy jurisdictions with, for example, AIE, or do secrecy jurisdictions manage to keep the secrecy they supply to other countries uncovered by AIE? AIE and its role in addressing collective action problems caused by small island tax havens has been discussed intensely in the global tax governance literature. Equated with financial transparency and hallowed as ushering in a new era after neoliberalism, AIE has been found to be successful in reversing the downwards trend in capital income taxation, offering new tax policy options for (re)embedding globalisation (Hakelberg and Rixen 2020). At the same time, current research documents continued diversion of aid monies to tax havens (Juel Andersen et al. 2020) and Ahrens and Bothner (2019) find that "tax havens apparently did not suffer massive financial losses following AEI" while at the same time finding evidence for an overall decrease of tax evasion. On the other hand, detailed legal analyses and process tracing studies cast doubts on the AIE's effectiveness especially with regard to the impact on tax evasion of high-net worth individuals (Meinzer 2018; Knobel and Meinzer 2014). This has led to the likening of the AIE standard to a "sham" or double standard (Meinzer 2019) in an adapted typology of regulatory coordination (Drezner 2003). Given these divergent views, we aim to help to reconcile the apparent simultaneity of successful international tax cooperation and continued resistance by secrecy jurisdictions.

We answer our research question by introducing two innovations that allow us to analyse the role of power in the entire global information exchange network. First, we acknowledge that financial secrecy as well as tax havens are multi-dimensional phenomena that are not suitably captured by binary variables which underpins blacklisting initiatives. The binary nature of tax haven blacklists tends to omit power inherent in underlying definitions and terminologies, and thus risks reinforcing political biases in academic research. To

overcome this limitation, we apply a new dataset on the intensity of financial secrecy and combine it with bilateral portfolio asset data to determine the financial secrecy risks covered by information exchange networks of individual countries and by blacklisting approaches of the European Union. This new data set enables us to test the relative successes of both AIE and blacklisting policies in covering risks. The second innovation is that we use dyads rather than jurisdictions as the unit of analysis, reflecting the reality that each tax haven is relevant for a different set of countries. Combining the two innovations, we arrive at three main findings.

In our first main finding, more secretive jurisdictions manage to keep more of the secrecy they supply to other countries uncovered by AIE, indicating a successful strategy of selective resistance. Among those highly secretive jurisdictions, the role of OECD dependent territories is salient. Rather than being a specialty of the particular US information exchange system, the Foreign Account Tax Compliance Act (FATCA; Hakelberg and Schaub (2018)), we show that hypocrisy lies at the core of the OECD AIE system. Second, OECD countries are better than other countries at covering their most relevant secrecy jurisdictions with AIE. Third, the EU member states are better at covering their most relevant secrecy jurisdictions with AIE rather than with their blacklisting.

The remainder of this paper is structured as follows. Section 4.2 reviews the literature on global tax governance and derives three hypotheses that we empirically test in the subsequent sections. Section 4.3 introduces the data used, explains the construction of the Bilateral Financial Secrecy Index and the research design to test the hypotheses. Section 4.4 presents the main findings and we conclude in Section 4.5.

4.2 Closing the back door to power in international tax haven research

A key puzzle permeating international political economy is if tax havens can escape coercive measures by powerful countries, and if so, why that is the case. Rational choice institutionalism has been a favoured methodological lens to approach the topic of international tax competition and tax havens. Assuming the rational design of international institutions, early research had undertaken to explain why collective action against tax havens has been apparently fail-

ing despite theoretic modelling indicating an overarching interest of powerful states to coerce or induce cooperation by tax havens. Dehejia and Genschel (1999) argued that a defection problem in international tax cooperation is exacerbated by the different size of jurisdictions. Small jurisdictions would in this asymmetric prisoner's dilemma not only have an incentive to defect to dodge cooperation, but—more importantly—would in the first place not find any cooperative arrangement desirable (p. 411). In addition, the feasibility of paying off tax havens for cooperation would be complicated by what the authors called an "outside world constraint" (p. 419). This weakest link problem has been further analysed by Elsayyad and Konrad (2012) who modelled the perverse incentives for those tax havens holding out longest resulting in higher profitability of their tax haven business, increasing the disincentives for cooperation.

With a similar methodological approach enriched by historical process tracing, Rixen (2008) provided an additional explanation for the lack of success of the OECD's efforts before 2008 to counter tax havens in their harmful tax competition initiative. He argued that the confrontation inherent in the asymmetric prisoner's dilemma was not resolved through sanctions or compensations to induce cooperation because of path dependency of the OECD tax work. The OECD's deep involvement in the establishment and management of thousands of bilateral tax treaties focused on preventing so-called double taxation between sovereign states constrained any reform proposal to piecemeal approaches that would leave the treaty set up largely unaffected – an approach that is insufficient to address the issues.

From a constructivist theoretical perspective, Sharman (2004; 2006) attributed the failure of the OECD 1998 harmful tax competition initiative to microstate tax havens successfully mobilising shared regulatory norms in their favour. By alluding to the cartel interests of OECD member states to protect their financial centres against smaller non-member competitors, they successfully fended off the attempts by the OECD to impose tax rules on them as hypocritical. Webb (2004) complements Sharman's account by pointing out the role of liberal economic ideology and of the principle of fiscal sovereignty to which small island tax havens and libertarian non-governmental actors successfully appealed in discussions with the new US government. The Bush administration finally withdrew its support from the existing OECD initiative in May 2001 and ended up truncating its scope to transparency and information exchange.

This shift towards information exchange and away from blacklisting has conceptually been backed up by contributions arguing that blacklisting tax havens has accumulated a long poor track record spanning decades, and as a policy has low chances in the future, in addressing the underlying problems of tax evasion and avoidance (Meinzer 2016; Cobham et al. 2015; Lips and Cobham 2018). The key reason for blacklist failures are argued to be power relations in shaping the listing efforts that would always end up settling for the smallest common denominator and ensuring removal of any dependencies of great powers.

Since then, different strategies employed by tax havens to resist the global regulatory efforts have been scrutinised. Eccleston and Woodward (2014) identified as dysfunctional policy transfer how the OECD designed and promoted a system for bilateral information exchange upon request since 2002. Informed by the literature on the politics of bureaucracies, the authors take this system as a case for a lowest common denominator standard that is failing to meet intended outcomes, yet is rolled out by an international organization because "[...] 'success' is often judged in terms of reaching an international agreement rather than its ultimate effectiveness [...]." (p. 227). The OECD policies for information exchange invited mock compliance by tax havens because they allowed a standard that is inadequate in principle to deal with the problem of tax evasion to be rolled out nonetheless (Woodward 2016). Woodward argues this problem largely persisted even after the tightening of the system through a peer review mechanism in the wake of the reforms since 2009 after the global financial crisis. Crasnic (forthcoming) identified four different strategies of resistance by small state tax havens in the face of regulatory regimes, including the new regime of automatic transmission of information. Categorised by the intensity and visibility of the resistance strategies employed, she argues that the resolve and capability determine whether jurisdictions engage in submission, foot-dragging, rejection or disruption. Also economists have evaluated the system for bilateral information exchange upon request. For instance, Bilicka and Fuest (2014) found that tax havens would not systematically undermine tax information exchange by picking mainly irrelevant partner jurisdictions for exchange agreements, while Johannesen and Zucman (2014) assessed the impact of the same type of information exchange agreements on foreign bank deposits of rich countries' residents, finding a modest relocation of assets to tax havens not covered by a treaty with the jurisdiction of residency of the depositor.

The advent of AIE at the OECD and G20 levels since 2013 has been overwhelmingly heralded in international political economy literature as a game changer in the global fight against tax havens. Emmenegger (2017) attributes the breakthrough for AIE by the determination of the US since the Obama administration 2009 to secure information access on accounts held in Switzerland both in future and retrospectively. On both accounts, the US has been successful in overcoming traditional Swiss resistance to compromising its bank secrecy rules because it used its structural importance within the international financial system to threaten Swiss banks with credible enough sanctions for them to lobby for concessions in Switzerland. These legislative reforms then paved the way for others, including the EU and the OECD, to exact AIE from Switzerland, especially after the Swiss proposal for an alternative system of bilateral anonymous tax agreements had been stopped in the German legislature in 2012 (Meinzer 2018). Also Palan and Wigan (2014) confirm a potential watershed moment in international tax cooperation through the new US FATCA law implementing the US AIE system going forward. They interpret this law as indicative of a potential wider shift towards unilateralism in international tax governance that could prove superior to multilateralism which is notoriously hampered by the smallest common denominator of all jurisdictions. Lips (2019) instead argued that success in shifting towards unilateralism is confined to the sphere of individual's tax evasion as opposed to corporate tax avoidance.

The case of the US was also examined by Hakelberg and Schaub (2018), who found empirical evidence that it had successfully coerced smaller tax havens into an information exchange regime. After the passing of FATCA in 2010, they observe a substantial fall of international bank deposits in a handful of tax havens when compared to non-havens. Yet they also observe an important effect of an asymmetry in the US FATCA law. This law succeeded not only in obtaining information to counter tax evasion of its residents and citizens in any possible tax haven worldwide, but at the same time abstained itself from participation and reciprocating the same information to non-resident investors in the US financial system. The corresponding US agreements implementing FATCA ensure full information for the US tax administration on assets held abroad, but reciprocate at best only a trickle of information about assets held in the US by foreign persons. For example, bank accounts controlled by shell companies or trusts are excluded from US reporting (Knobel 2016; Meinzer 2018). Some of the international agreements implementing FATCA even contain the explicit acknowledgement that the US needs to further amend its laws

to reciprocate the information exchange. As an effect of this hypocrisy of countering tax havens, Hakelberg and Schaub (2018) showed how deposits in the US financial system grew much faster than in other countries after FATCA was enacted (2010-2014). The authors thus identify a redistributive impact of FATCA suggesting that the US's "[...] refusal to reciprocate the AEI increases its attractiveness for capital that had formerly been hidden in jurisdictions making up the tax haven group." (p. 11-12).

Since the rolling out of the Common Reporting Standard (CRS) from 2013 onwards, a key discussion in international tax governance has been about the effectiveness of this new multilateral AIE regime. The evidence and conclusions about its impact and effectiveness are more heterogenous and ambiguous than with FATCA. For example, Ahrens and Bothner (2019) in contrast to Hakelberg/Schaub do not find evidence for a continuing shift of assets from havens into the US after the introduction of the CRS (p. 11). While they identify a relative decline of assets in tax havens compared to non-havens, overall "tax havens apparently did not suffer massive financial losses following AEI" (p. 13). The effect of a relative decline of assets invested in tax havens compared to non-havens was found to be triggered only by the unilateral announcement of the introduction of AIE by a given jurisdiction, and not by the actual enacting of a specific bilateral exchange agreement, nor by these agreements entering into force, nor by the actual first exchanges of data happening.

Furthermore, the overall research design of Ahrens and Bothner (2019), and their approach for controlling for treaty circumvention in particular, is highly sensitive to the list of tax havens used, constrained by the limited data available from those tax havens, and furthermore misses the risk of circumvention via golden visas entirely, which might be the most relevant risk of circumvention. For example, Lebanon, Labuan (Malaysia), Mauritius and the United Arab Emirates—the latter two among the fastest rising secrecy jurisdictions and corporate tax havens in the world—are missing from this list. If bank deposits in response to the introduction of the CRS were shifted to be held in accounts opened in the name of shell companies registered in any of these jurisdictions, no circumvention would be identified in their research methodology. Furthermore, risky golden visa regimes are offered by many more jurisdictions not included in their list, for example Ireland, Guatemala or the Dominican Republic (Knobel and Heitmüller 2018). These regimes enable the circumvention of the CRS by allowing natural persons to feign a change of residency vis a vis banks when opening bank or brokerage accounts. The data would then be sent to the

jurisdiction that offered the golden visa, many of which do not tax foreign investment income at all or not comprehensively. The original jurisdiction of residency where the natural person continues to reside in contrast would not receive the information about the hidden foreign investment income anymore, facilitating the ongoing offshore tax evasion AIE was designed to conquer.

Finally, Hakelberg and Rixen (2020) explain an observed reversal in the downward trend of tax rates on dividend investment income with the introduction of tax information exchange in general, and with AIE in particular. Their findings show that the exit threat, which tax havens had successfully projected into policymakers' minds in the past, appears to have lost some of its luster, and no longer constrains them from increasing tax rates on investment income. They attribute these changes to the new information sharing cooperation for enforcing personal income taxes on cross border investment income, and interpret it as an important marker for departure from a neoliberal era of anticipatory obedience in tax policy making when policymakers felt as having no alternative except to lower tax rates. Yet this study does neither prove the effectiveness of the AIE regime itself, as it does not measure more directly either its outputs or direct impact on tax revenues.

To the contrary, there is evidence casting doubt on the effectiveness of the CRS regime. Persistent private sector advertisements by law and asset management firms (MacFarlane 2020), detailed legal analyses as well as process tracing studies on the design of the AIE system suggest both that loopholes in CRS system are available and that they are promoted by firms specializing in catering to high net worth individuals (Meinzer 2018; Knobel and Meinzer 2014). For example, golden visas, the carve out for active companies, low levels of sanctions for willful misreporting as well as the absence of public registers of beneficial and legal owners of shell companies, trusts and foundations open possibilities for misreporting and abuse. The lack of public statistics about the system's output performance and the OECD's reluctance to make them mandatory are reasons for skepticism about the system's impact.

Most recently, a study by the World Bank has analysed the relationship of aid disbursements and deposits from the recipient jurisdictions in 16 tax havens between 1990 and 2018 and found that aid monies disbursed to the poorest countries in the world coincide with haven deposit increases (Juel Andersen et al. 2020). Many of these tax havens are part of or controlled by the OECD minority world (10 out of 16), and a particular strong association between aid disbursements and deposits was identified for OECD members Switzerland

Table 4.1: A typology of international tax policy coordination

		Divergence of preferences between core (OECD) and peripheric (non-OECD) states	
		High conflict	**Low conflict**
Divergence of preferences among core (OECD) states	**High conflict**	Sham or double standards (or none/privatised)	Rival standards
	Low conflict	Club standards	Harmonized standards

Source: Meinzer (2019) on the basis of Drezner (2003).

and Luxembourg. Importantly, the authors do not see any change in rate of aid diversion to tax havens in the period 2009-2018 which is marked by more information exchange, indicating that the information exchange may not be successful at least for lower income countries.

Further to the empirically driven observations and arguments, there are also theoretical grounds on which to expect rather a "sham" or "double standard" or at best a "club standard" governing international tax cooperation. Table 4.1 shows a typology of international tax policy coordination adapted from Drezner's typology of regulatory coordination (Drezner 2003). According to this typology inspired by dependency theory, we would expect a sham or double standard to emerge in the realm of the taxation of cross-border financial investment and related income. There is substantial heterogeneity of preferences among OECD members between those invested in the haven business and others, as well as between OECD and non-OECD countries based on former colonial patters of capital flight and illicit financial flows (Ndikumana and Boyce 2011). We suggest that this typology is a more fruitful framework of analysis for international tax governance than the widespread binary categorisation of tax havens and non-havens, and the related implicit proposition of homogenous interests among those countries classified as non-havens. Our contribution addresses the puzzle of the apparent simultaneity of successful coercion and continued resistance by tax havens as observed in the realm of AIE by employing a novel approach and dataset, and drawing on the analytical framework informed by dependency theory as embodied in the typology below.

Our research design builds on two innovations. First, we replace binary tax haven categories with more nuanced data on a country's level of financial secrecy offered by jurisdictions to non-resident investors. Assessing each

country's laws and regulations transparently allows robust comparisons of the intensity of financial secrecy across jurisdictions and places each on a spectrum of secrecy, overcoming an artificial binary distinction between tax havens and others. This approach allows a more timely, nuanced and multidimensional view of financial secrecy by taking into consideration various secrecy tools that plausibly can help bypass or are out of scope of AIE, yet enable tax evasion and illicit financial flows. At the same time, this approach allows loosening and replacing the assumption of homogeneous interests of non-havens for objectively verifiable criteria about a jurisdiction's investment in the financial secrecy business. The false and oversimplifying dichotomy between tax havens and others has been argued to lie at the core of the failure of policies countering tax havens for decades, and we argue that it has had a similar debilitating effect on much research on this subject (Palan 2003; Cobham et al. 2015; Meinzer 2016), omitting the potentially crucial role of power vested in the OECD club and their network of satellites in maintaining colonial types of economic domination and extraction.

Second, we change the unit of analysis from a single jurisdiction to bilateral dyads, taking into account that each tax haven is relevant for a different set of countries. For example, while Cyprus has been a favourite tax haven for Russian depositors and combines both low taxation and high secrecy (Pelto et al. 2004; Ledyaeva et al. 2015), Mauritius has been notoriously secretive and important for multinational enterprises active in India and African countries as well as for African oligarchs (Beer and Loeprick 2020; Fitzgibbon 2019), and relevant for incorporating shell companies. Our dataset for the first time systematically captures jurisdictional dyads, opening up new avenues for econometric hypothesis testing. By combining nuanced data on a jurisdiction's level of secrecy with bilateral investment data in the Bilateral Financial Secrecy Index (BFSI), we quantify which secrecy jurisdictions create most risks for illicit financial flows to individual countries, including those stemming from tax evasion (Economic Commission for Africa 2018; Abugre et al. 2019).

We use this dataset in combination with bilateral data on the global network of information exchange relationship to answer our research question of how successful countries are in covering secrecy jurisdictions with their policies, AIE in particular, and, in this way, to help to reconcile the apparent simultaneity of successful coercion and resistance by tax havens. Based on the literature and our analytical framework, we expect tax havens to fight for excluding their most relevant secrecy counterparts from their information exchange network.

Conversely, we expect more powerful countries to more successfully coerce tax havens to exchange information with them (Hakelberg and Schaub 2018). We test these hypotheses by checking how much of any country's BFSI is or is not currently covered by active AIE relationships. After testing if secrecy jurisdiction are successful in resisting, we then set out to find any pattern of selective resistance. To this end, we flip the analytical prism around and focus not on the supply side of secrecy, but on jurisdictions exposed to secrecy risks. Our analytical framework suggests that powerful OECD countries will be more successful in covering with AIE those relationships that exposes them to the greatest risk than non-OECD members and lower income countries. If confirmed, these two hypotheses could point to a strategy of selective resistance by secrecy jurisdictions towards less powerful jurisdictions as an explanation for the hitherto contradictory findings. Finally, we use the approach to test how blacklisting tax havens is comparing with AIE in covering relevant jurisdictions. We compare the European Union's recent greylist and blacklist with the EU's network of AIE relationships. We expect a much higher share of covered BFSI through the AEI than through black- and greylisting exercises, because of the smallest common denominator approach and likely political bias inherent in the latter.

4.3 Dyadic analysis: the Bilateral Financial Secrecy Index

In this section we introduce the BFSI as an innovate dataset enabling dyadic analyses with nuanced and verifiable data on the intensity of financial secrecy. We proceed by explaining its underlying data sources, its construction methodology and some illustrative results. We then introduce the additional data sources for the policy analysis and our approach for using the BFSI for evaluating policies.

4.3.1 Construction of the BFSI: data and methodology

The Bilateral Financial Secrecy Index is composed of two parts which follow the methodology of the original Financial Secrecy Index. First, the qualitative part of the BFSI is composed of secrecy scores which measure the intensity of financial secrecy of each jurisdiction, and which we source from the original

FSI in its 2020 edition (Cobham et al. 2015; Tax Justice Network 2020a). This dataset overcomes blind spots of previous research which so far has paid scarce attention to domestic policies that may affect the functioning of the AIE system, such as the availability of golden visas, secretive trusts and shell companies, luxury free ports to hide wealth or secretive real estate investment options. This landscape of potentially very harmful secretive instruments is highly dynamic not only because of new jurisdictions establishing secrecy hubs, but also because of reactions to reforms undertaken. Policy makers have in recent years responded to various leaks by bolstering their domestic rules and regulations beyond AIE, for example in the realm of identifying the true owners of legal vehicles, the so-called beneficial owners (Knobel et al. 2020).

The secrecy scores capture this dynamic landscape up to date as of September 2019, ranging from 0 (least secretive) to 100 (most secretive). They are calculated as arithmetic averages of 20 key financial secrecy indicators (KFSIs) which are grouped around four broad dimensions of secrecy: (1) ownership registration (five indicators); (2) legal entity transparency (five indicators); (3) integrity of tax and financial regulation (six indicators); and (4) international standards and cooperation (four indicators). While the 2015 version of the 20 indicators and secrecy scores has been peer reviewed (Cobham et al. 2015), the 2018 version differed substantially (for details of changes see Tax Justice Network (2018)) but was subject to a statistical audit by the Joint Research Centre of the European Commission and is largely consistent with the 2020 version. A detailed description of the secrecy scores and each of its indicators is provided by Tax Justice Network (2020a).

The individual indicators are, for the most part, unilateral (i.e. they do not differ by partner jurisdiction). The only indicators that are bilateral in nature are KFSIs 18 (AIE), 19 (bilateral treaties) and 20 (international legal cooperation). We adjust these KFSIs specifically for intra-European relationships, where cooperation is more intensive than is portrayed by the original secrecy scores, resulting in corresponding lower secrecy levels among EU members. However, since most European countries had already scored low on these indicators, these adjustments have not had a substantial effect. We report both the original secrecy scores as well as their adjusted values in Table A.31 in the Appendix. Despite their imperfections, we consider the secrecy scores of the FSI the best available indicators of financial secrecy and the academic as well as policy debate seem to support this view (see, for example, Clark et al. (2015)).

Second, in the quantitative part of the index we replace unilateral global

scale weights (GSW) used in the FSI with bilateral scale weights (BSW). Whereas the FSI used exports of financial services of each jurisdiction derived and partially extrapolated from multiple sources to calculate its share of the global total (Tax Justice Network 2020a), we replace this with portfolio investment data which is available bilaterally. We use data on cross-border portfolio assets from the IMF's Coordinated Portfolio Investment Survey (CPIS) for 6,623 bilateral relationships in 2018. While there are reasons in favour of using CPIS assets and of using liabilities (as discussed in e.g. (Cobham et al. 2015, p. 293)), we choose to use assets because of their better coverage and suitability to analyse the role of small secrecy jurisdictions which often do not directly report under CPIS, and would thus be missing as destinations for hiding assets.

It is important to realize that not all of the flows captured by the bilateral scale weights are secrecy-related. Therefore, a high BSW alone is not an indicator of any wrongdoing; quite the contrary, jurisdictions with high BSWs could be congratulated on their success in international trade and investment. Rather, the bilateral scale weights are an indicator of the potential for a jurisdiction to be an important secrecy haven for another country, if secrecy is chosen by that jurisdiction in the range of policy areas (which is what we capture by the secrecy scores). Therefore, the Index is a measure of risk rather of actual secrecy-related activity and this distinguishes it from actual estimates of the scale of secrecy-related activity, such as those provided by Zucman (2013).

To estimate the scale of these risks, in the next step, we combine the value of assets held by residents of country i in secrecy jurisdiction j with the secrecy score of secrecy jurisdiction j. Therefore, we estimate the BFSI only for countries that report to the IMF CPIS the data on the value of their citizen's portfolio assets in countries for which secrecy scores are available. We use the best available data, while keeping in mind their weaknesses. For example, the CPIS data includes portfolio investment by households, but also by companies and banks with the latter two likely dominating at least some of the bilateral relationships. Also, the CPIS might not cover the whole scale of economic and investment activities relevant for financial secrecy and might thus lead to imprecise results by omitting important activity. However, we argue that CPIS is the best available data source for individuals' holdings of financial assets which we would like to capture in the BFSI. The approach to quantify the scale weights of the FSI builds on a methodology pioneered by the IMF in 2007 for defining offshore financial centres (Zoromé 2007).

Other data, such as that on foreign bank deposits, foreign direct investment

(FDI), or trade, could be used to construct BSW as well. The closest alternative to the CPIS is the Bank for International Settlements (BIS) Locational Banking Statistics (LBS) data. The LBS include data at the bilateral level for 31 reporting countries, including some notorious tax havens, and start in early 2000s for most country pairs. However, this data has its own weaknesses: some of the most important secrecy jurisdictions do not report this data; and it only includes bank deposits, not the portfolio asset classes of equities, bonds, and mutual fund shares that households entrust to offshore banks (which are found by Zucman (2013) to only account for about one-quarter of offshore financial wealth). In addition, as Alstadsæter et al. (2018) argue, the use of anonymous shell corporations makes it increasingly hard to identify the beneficial owners of the wealth held offshore, which is an issue that also pertains to the CPIS data. Other alternative proxy variables for the strength of the economic relationship that might be relevant for financial secrecy are data on trade in services and foreign direct investment. As a form of robustness test, we construct the BFSI with BSW based on data on foreign bank deposits (from the BIS LBS), FDI (from the IMF's Coordinated Direct Investment Survey), and trade in financial services (from the World Trade Organization). In Table A.24 in the Appendix we report that the correlation coefficients between the 4 different versions of BFSI that each use different data to estimate the BSW are high. In Table A.25 in the Appendix, we provide descriptive statistics for the four variables as well as for the original and adjusted secrecy scores.

In combining the qualitative measure of secrecy intensity and the quantitative measure of bilateral economic extensity, we aim at maintaining consistency between the FSI and the BFSI. We use CPIS portfolio assets to estimate the shares of each country's total portfolio investment in a jurisdiction as a ratio to the total global cross-border portfolio investment (it considers all portfolio investment regardless secrecy). More formally, for each country i and each partner jurisdiction j, we define the BSW as:

$$\text{BSW}_{ij} = \frac{\text{Cross-border portfolio assets (true or approximated)}_{ij}}{\text{Sum of all global cross-border portfolio assets (true or approximated)}_{ij}}$$

for each country i and each partner jurisdiction j. We then define the BFSI, using secrecy scores from the 2018 FSI and the same transformation as in the FSI, as:

$$\text{Bilateral Financial Secrecy Index}_{ij} = \text{Secrecy score}_{j}^{3} * \sqrt[3]{\text{Bilateral Scale Weight}_{ij}}$$

While there are multiple relevant options on how to combine the two components, we use the cube/cube-root formula to construct the BFSI in the same way as in the original FSI for two reasons. First, we aim to maintain methodological consistency to the largest possible extent to ensure comparability with the FSI as a widely used and established measure of financial secrecy. Second, as confirmed in a statistical audit of the FSI methodology (Becker et al. 2016; Tax Justice Network 2018), the statistical properties of the distributions of the two variables are fundamentally different, preventing the use of a simpler multiplicative formula. The statistical properties of the variables in the BFSI are very similar to those of the FSI, and thus require a transformation before combination. Cubing the secrecy score and taking a cube root of the scale weight highlights the importance of harmful secrecy regulations in contributing to global financial secrecy.

The BFSI represents a snapshot view of the secrecy world and estimates which jurisdictions are important for other countries. While the two components of the index may influence each other (for example, if countries implement information exchange agreements, this impacts portfolio flows and might direct them towards other countries (and recent empirical evidence suggests that this is indeed the case (Johannesen and Zucman 2014; Ahrens and Bothner 2019)). We argue that while the BFSI cannot be used to estimate these effects (at least not until a sufficient time series of the index is available, see (Janský and Palanský 2019b) for a thorough discussion of the usability of FSI data over time), it can be used to evaluate the progress of existing policies (as we do in this chapter) as well as by policymakers to pinpoint the most important secrecy jurisdictions for their country specifically.

4.3.2 Results of the BFSI

In total, we estimate the BFSI for 82 countries, with 131 different counterpart secrecy jurisdictions supplying secrecy to them. Due to data on portfolio investment not being available for all relationships between countries and secrecy jurisdictions, we estimate the BFSI for only 5,657 country pairs of the 82*131=10,742 possible ones. We find that a relatively small number of rela-

tionships is responsible for a large share of the total global sum of the values of the BFSI: the top 50 relationships are responsible for 9.61% of all global secrecy as measured by the BFSI. In Table A.25 in the Appendix we provide a list of the fifteen relationships with the highest BFSI values.

We illustrate the BFSI by presenting the data of the top ten secrecy jurisdictions for Germany and the United States in Table A.26 in the Appendix. We observe that there is substantial heterogeneity in which secrecy jurisdictions are most important for the two countries: while for Germany, the Netherlands and Luxembourg are the most important suppliers of secrecy, for the United States these are the Cayman Islands and Switzerland. In addition to estimating which secrecy jurisdictions are important for individual countries, the BFSI can also be used to analyse important secrecy jurisdictions for various groups of countries. For example, in Table 4.2 we explore the differences across groups of countries by per capita income (according to World Bank's classification; since there is no data available to estimate the BSW for any of the low-income countries, we only compare the remaining four income groups). Five jurisdictions are included among the top 10 jurisdictions for all four income groups – the United States (which top the list for every income group), Hong Kong, the Netherlands, the Cayman Islands, and Switzerland. United Arab Emirate is in the top 10 for three lower-income groups and thirteenth for OECD countries. These results show that these several major global financial centres are responsible for most of the secrecy faced by countries across income levels.

Table 4.2: Top ten secrecy jurisdictions and BFSI results for four income groups

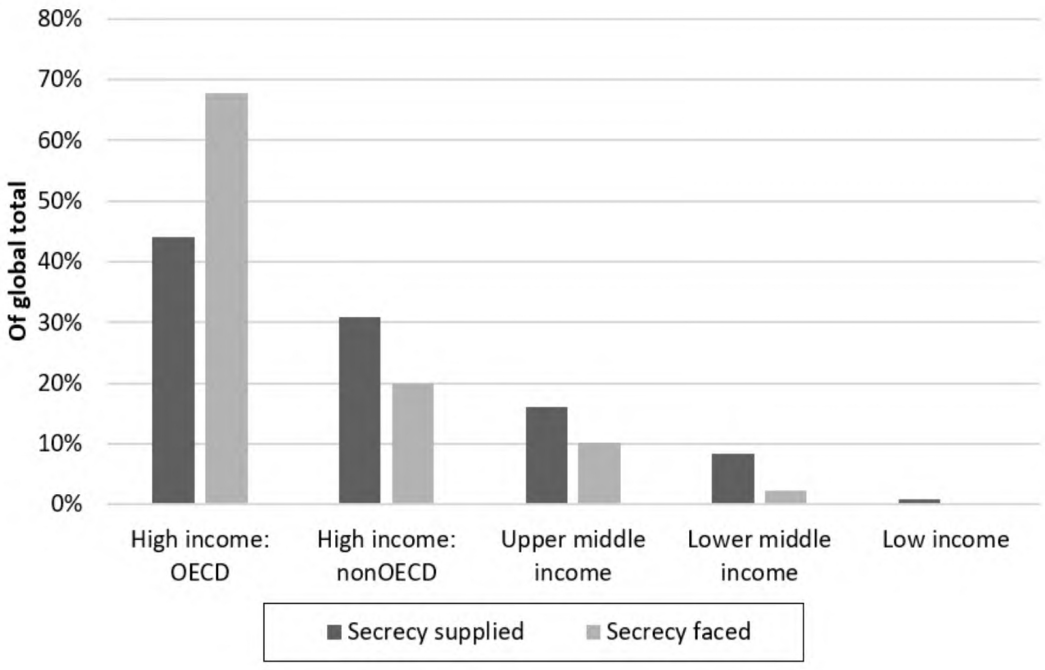
Rank	Lower middle income	BFSI	Upper middle income	BFSI	High income: nonOECD	BFSI	High income: OECD	BFSI
1	United States	498	United States	2051	United States	3098	United States	8556
2	Netherlands	305	Cayman Islands	1370	Cayman Islands	2747	Cayman Islands	7798
3	UAE	280	Hong Kong	1291	UAE	1881	Switzerland	6790
4	Cayman Islands	274	Switzerland	1154	Switzerland	1726	Netherlands	6679
5	Hong Kong	271	Luxembourg	1030	Bermuda	1563	Luxembourg	5937
6	China	205	Singapore	900	BVI	1492	Germany	5127
7	Switzerland	200	UAE	854	Hong Kong	1472	Japan	4546
8	Saudi Arabia	193	Bermuda	848	Luxembourg	1464	Hong Kong	4452
9	Luxembourg	189	Netherlands	825	China	1462	Taiwan	4235
10	Thailand	186	BVI	670	Netherlands	1415	UAE	4046

Source: Authors.

We can reverse the analysis and look at countries and country groups that

supply the most secrecy to other jurisdictions by summing up the BFSI values for secrecy-supplying jurisdictions (see Table A.25 in the Appendix). In Figure 4.1 (and Table A.28 in the Appendix), we compare the shares of secrecy risks both supplied and faced by each income group. In total, OECD countries face 67.7% of the global secrecy, while only supplying 44%, with all the remaining income groups supplying more secrecy than they face. Table A.28 in the Appendix then shows these results in more detail in the form of a matrix of shares of secrecy supplied by income groups in columns to income groups in the rows. We find that the bulk of the excess secrecy that OECD countries face is supplied by OECD overseas countries and territories, primarily those associated with the United Kingdom. This finding supports the notion of prevailing hypocrisy in international tax governance not only as regards the US, but of the entire OECD as a club of global rule setters which tolerates and seeks to benefit from secrecy business in its backyard.

Figure 4.1: Shares of global secrecy supplied and received by each income group



Source: Authors.

We provide a similar breakdown by regional groups in Table A.30 the Appendix. We observe that some secrecy jurisdictions specialize in supplying secrecy to countries that are close to them geographically, such as Saudi Arabia to South Asia or Panama to Latin America. We again derive a matrix of

shares of total global secrecy among regional groups (shown in Table A.30 and Figure B.13 in the Appendix) and find that Europe & Central Asia supplies 33.9% and faces 53.4% of the total global secrecy. We also find that Europe & Central Asia and North America are among the regions that face more secrecy than they supply, with jurisdictions from Latin America & the Caribbean supplying most of this difference. By doing so, we essentially create a single ranking of jurisdictions in terms of how much secrecy they provide to other countries – an objective of the original FSI. The results of the summed BFSI and the original FSI are indeed quite similar, with a correlation coefficient of 0.892. It is thus no surprise that the same secrecy jurisdictions come out at the top – the United States together with the Cayman Islands and Switzerland make up the top 3 in both rankings.

As a robustness check for our methodological choice to use data on portfolio assets to construct bilateral scale weights, we recalculate the original FSI using data on portfolio assets to calculate global scale weights. We again find similar results: the correlation coefficient between the summed BFSI and the FSI using CPIS data is 0.983; and between the original FSI and the FSI using CPIS data, it is 0.885. We report the results for individual jurisdictions of the summed BFSI as well as the original FSI and the FSI using CPIS data in Table A.31 in the Appendix and we compare the summed BFSI with the original FSI in Figure B.12 in the Appendix. We find that, among the most important secrecy jurisdictions, the two indices differ most for Bermuda and the Netherlands on the one hand (where the BFSI suggests that these are more important than is portrayed by the FSI), and Singapore, Hong Kong, and Switzerland on the other (where the BFSI suggests that these are less important than is portrayed by the FSI). This is roughly in line with the perceived role of Singapore, Hong Kong and Switzerland as financial centres that export large amounts of financial services while not being proportionately important globally as destinations of portfolio investment. One advantage of the summed BFSI over the original FSI is that it allows to analyse which jurisdictions face the most secrecy from individual suppliers of secrecy. Table A.27 in the Appendix shows ten countries that face the most secrecy from each of the top three suppliers of secrecy – the United States, Cayman Islands, and Switzerland. The results show that the Cayman Islands are a particularly important supplier of secrecy for the United States, Japan, and Hong Kong, and that Switzerland is particularly important for the United States.

4.3.3 Research design for assessing automatic information exchange and blacklisting

After establishing the BFSI, we now introduce data sources for two policies – AIE and the EU's blacklists. For AIE, we use bilateral data available on the OECD's Automatic Exchange Portal (OECD 2018), displaying all activated relationships between pairs of jurisdictions under the CRS. In most of our empirical analysis, we use data on relationships as of January 4, 2018. In addition, we support some of our findings by analysing the development of these relationships over time. Notwithstanding other conditions, an exchange relationship is activated whenever two jurisdictions either conclude a bilateral competent authority agreement or list each other under the multilateral competent authority agreement (MCAA) in its Annex E (Meinzer 2017). However, Annex E is not made public. This prevents us from directly observing countries' preferences for activating – or not – exchanges with any given jurisdiction. Therefore, only pairs of countries can be observed which have chosen each other in Annex E or have otherwise concluded a bilateral agreement. A further complicating factor is the absence of harmonised deadlines for the submission of countries' exchange preferences and the fact that many jurisdictions have committed to exchange only in 2018 or some even later (OECD 2017). Furthermore, the updates on the OECD data portal are made without clear timelines. Therefore, our data sample is composed of snapshots in time which can be complemented by future analyses of the development of the AIE network over time.

Three complicating factors concern the multilateral agreements. The first consists of the possibility for jurisdictions to voluntarily choose only to send, but not receive, tax information. These jurisdictions enlist in Annex A and will not be receiving any information. Moreover, the banks in any participating jurisdiction will not be required to report accounts held or controlled by people resident in those jurisdictions. The risk of this tactic clearly consists in notorious tax havens attempting to lure foreign residents into taking up fake residency or citizenship there, with tax information exchanges falsely being classified as belonging to an Annex A jurisdiction resident, which will thus not be collected nor exchanged by the banks at all. The second problem consists in the data protection assessments the OECD is currently performing on entrants to the AIE mechanism, the outcomes of which remain confidential. As long as the OECD diagnosed weaknesses in data protection, the jurisdiction in question would not be eligible to receive any data under competent authority

agreements. There is no way to differentiate between the first and second type of asymmetric data provision (Annex A or data protection concerns).

The third complicating factor of the multilateral agreements is the EU directive on AIE (Council of the European Union 2014), which does not provide for non-reciprocal information exchanges and which overrides any EU member's preference as expressed in Annex A of the MCAA, and which also might override the data protection assessments of the OECD. In addition to the EU member states, there are specific treaties between the EU as a whole and six non-EU members in place which very likely only allow for reciprocal exchanges. The countries concerned are Switzerland, Liechtenstein, San Marino, Andorra, Monaco and Saint-Barthelemy (Commission 2017). As a result, we observe in the data that some jurisdictions (Cyprus, Romania) are exchanging information reciprocally with the EU and a handful of third countries covered by EU-equivalent treaties, but not with the rest of the world. While it is impossible for us to know the reasons for sure, it is likely that data protection concerns explain Romania's exclusion, and Annex A might explain Cyprus' asymmetry.

Furthermore, in the data we use from OECD, the United States are absent because they do not participate in AIE. We apply this fact consistently in this paper and it implies that below we observe the United States as having 0% of its financial secrecy covered by AIE (and also all of the other countries' secrecy from the United States not covered by AIE). However, due to the application of FATCA, the United States do receive comparable information from almost every country in the world (but they do not share similar data with their partners). If we were to extend our definitions and data sources to cover both AIE and, in the special case of the United States, FATCA, this might be beneficial for the BFSI as a general risk assessment tool, from which the United States would emerge as a country actually being very successful in obtaining information from other countries. We prefer to rely on CRS only for consistency, but the special case of the United States should be kept in mind when interpreting the results.

The last group of data we use in our empirical analysis are lists of tax havens published by the EU. On December 5, 2017, after years of political pressures and negotiations, the European Commission published a blacklist of 17 non-cooperative jurisdictions (European Commission 2017). The blacklist is a result of a screening process that has covered 92 jurisdictions. 72 of these were asked to address deficiencies, and 47 of them committed to "improve transparency, stop harmful tax practices, introduce substance requirements or implement

OECD BEPS" (European Commission 2017), and were put on a grey list. Eight countries were given more time to address the deficiencies as they had recently been hit by natural disasters. Finally, the remaining 17 jurisdictions were blacklisted as non-cooperative. Since then, the European Commission has updated the list many times (see European Commission (2020) for details) as jurisdictions implement (or don't) the required measures. In our analysis, we use three versions of the lists – the initial one from December 2017, one from December 2018, and one from March 2019 – to assess how well the lists align with the results of the BFSI.

We divide our assessment of the two policies into three parts. First, we test our hypothesis that secrecy jurisdictions (i.e. countries that score high on the FSI or one of its two components) successfully defend their 'business model' by avoiding or at least delaying the activation of AIE relationships with countries to which they supply secrecy (as opposed to countries that lose out due to the secrecy jurisdictions' secrecy; these countries aim to cover as much of the received secrecy as possible by AIE). To that objective, we construct, for each country i, the share of supplied (or—for the purposes of the second part of our analysis—faced) secrecy that is covered by activated AIE relationships on the sum of the country's total supplied/faced secrecy, defined as follows:

$$\text{Share of received/supplied BFSI covered by AIE}_i = \frac{\sum_{j=1}^{k} BFSI_{ij}}{\sum_{l=1}^{m} BFSI_{il}}$$

where k is the number of jurisdictions j with which country i has an activated AIE relationship, and m is the number of partner jurisdictions l for which the BFSI is estimated for country i.

An important caveat to consider in this part of the analysis is that the secrecy scores themselves include an indicator on automatic exchange of information. In particular, Key Financial Secrecy Indicator 18 (KFSI-18; see Tax Justice Network (2020a)) focuses on AIE. Since the final secrecy scores of a jurisdiction is calculated as the arithmetic average of 20 KFSI, for the purposes of this part of the analysis, we derive an alternative set of secrecy scores which exclude KFSI 18. These alternative secrecy scores are thus constructed as arithmetic averages of 19 KFSIs. We do this to prevent potential endogeneity of secrecy scores, and thus FSI, when assessing the relationship between FSI and the ratio of received and supplied BFSI covered by AIE. We report these adjusted secrecy scores for each jurisdiction in Table A.31 in the Appendix.

To assess the relationship between secrecy and the share of supplied/faced BFSI covered by active AIE relationships, we estimate the following model:

$$\text{Share of supplied/faced BFSI covered by active AIE}_i = \alpha + \beta * secrecy_i + \gamma * X_i + \epsilon \tag{4.1}$$

where $secrecy_i$ represents either the secrecy score or the global scale weight or the value of the FSI for country i; X_i is a set of income and regional group binary variables; and ϵ an error term. We hypothesize that $\beta < 0$, i.e. that countries that are more secretive manage to avoid or delay the activation of AIE relationships with countries to which they supply secrecy (as measured by the BFSI).

Second, we test whether countries, in their efforts to counter the negative effects of secrecy, conclude bilateral AIE agreements with their most important suppliers of secrecy. In this part of the analysis, we thus focus on countries that face secrecy (rather than those that supply it). We hypothesize that richer and more powerful countries are better than other countries at covering their most relevant secrecy jurisdictions with AIE. We test the hypothesis using the model specified in Equation 4.1 where $Share\_covered_i$ is the share of faced secrecy that country i has managed to cover by active AIE relationships.

Third, we use the BFSI for testing the hypothesis that blacklists are less effective in covering risks than more specific policies such as AIE. We test this hypothesis for the case of the European Union by establishing the most harmful secrecy jurisdictions for this political bloc as a whole. We estimate the extent to which lists of non-cooperative jurisdictions published by the European Commission and AIE arrangements cover the secrecy that the EU faces. To do so, we construct the share of secrecy faced by the EU from blacklisted countries on the EU's total faced secrecy as follows:

$$Share\_covered_{(EU,t)} = \frac{\sum_{j=1}^{k} BFSI_{EU,j}}{\sum_{l=1}^{m} BFSI_{EU,l}}$$

where $Share\_covered_{(EU,t)}$ is the share of faced secrecy that is covered by the black- or grey-list at time t; k is the number of jurisdictions j included on the lists at time t; and m is the number of non-EU jurisdictions l for which the BFSI is estimated for the EU. We only include non-EU countries in m because by European Commission's design, the lists automatically omit EU member countries. We then use this metric to compare the blacklisting with AIE in

terms of how successful these policies have been in covering the most relevant secrecy jurisdictions.

4.4 Results

We start with the supply side of financial secrecy and investigate if more secretive jurisdictions manage to keep more of the secrecy they supply to other countries uncovered by AIE. We then turn to the receiving end of financial secrecy and test whether OECD countries are better than other countries at covering and coercing their most relevant secrecy jurisdictions with AIE. Finally, we evaluate whether EU member states are better at covering their most relevant secrecy jurisdictions with AIE or with blacklisting.

4.4.1 Tax havens successfully resisting coercion?

We hypothesize that secrecy jurisdictions would aim not to activate AIE relationships with countries to which they supply secrecy. Hong Kong is a clear example of a secrecy jurisdiction that supplies substantial secrecy to other countries and has been very slow in activating AIE relationships. Within the EU, in Figure B.14, we observe that three EU member states have a particularly low number of activated AIE relationships – Cyprus, Romania, and Bulgaria. Figure 4.2 shows the relationship between the share of supplied secrecy covered by active AIE treaties and the secrecy score. We observe that for countries that have engaged in at least one AIE relationship, there is a negative correlation between the share of supplied secrecy covered by active AIE and the secrecy score, suggesting that more secretive jurisdictions are less likely to activate AIE relationships, or at least are more likely to postpone activating these relationships, with countries that are significant destinations for their secrecy. The relationship is partly driven by highly secretive OECD dependencies which have so far managed to cover only relatively low shares of the secrecy they supply to other countries, although there are many other jurisdictions with high secrecy scores in similar positions. OECD countries, on the other hand, are among those that have covered most of the secrecy they face.

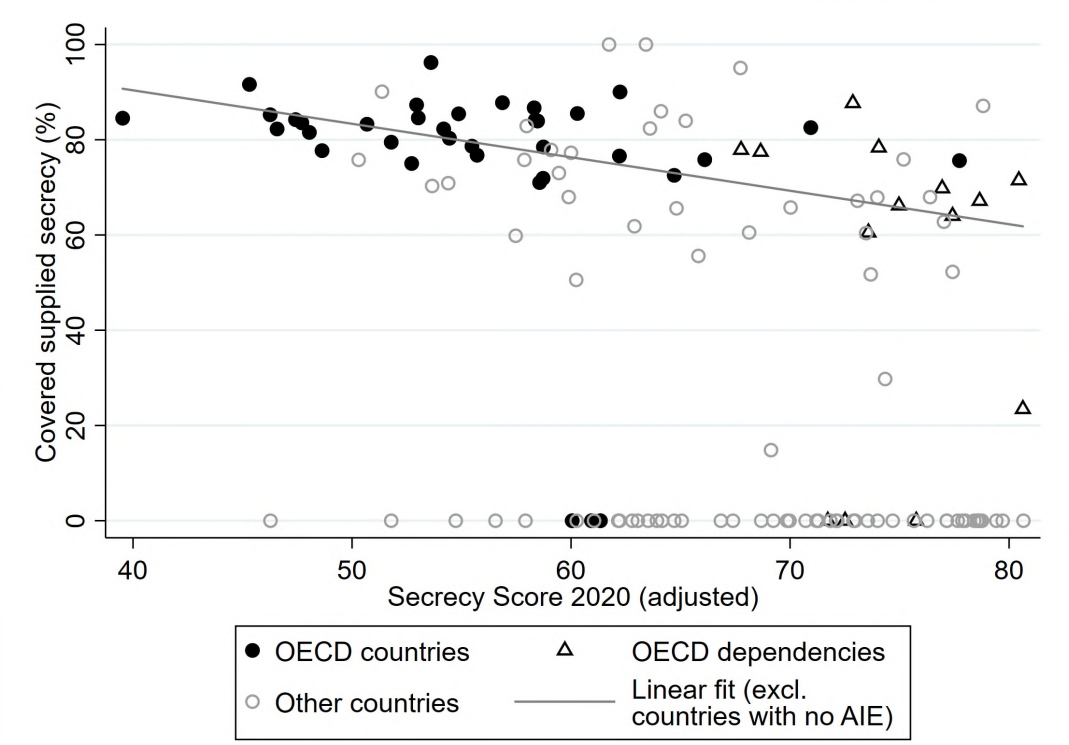
In addition to this trend, there is a cluster of jurisdictions in the bottom right corner of the graph that have high secrecy scores and, at the same time, have not yet disclosed any AIE exchange partners.[1] We recognize three possible

[1] In total, there are 55 countries for which we have secrecy scores and at the same time they

explanations for the position of these countries. First, these jurisdictions aim to gain from their secrecy by attracting wealth from abroad, and so far they have been successful in avoiding the activation of any AIE relationships (either by delaying the activation of the signed treaties or by not signing any treaties). For the majority of these countries, Figure 4.2 provides evidence consistent with this explanation. Second, these jurisdictions' foreign activities may be very small and it is thus not on their policymakers' agendas to negotiate AIE treaties at all. Third, if the jurisdiction's foreign activities are indeed very small, it may be the case that it is not on the agenda of policymakers of other countries to activate AIE relationships with these jurisdictions. There is a theoretically possible fourth explanation: that some of these countries have activated some AIE relationships, but there is no data available on portfolio assets between these countries, which is why the share of covered supplied secrecy would be zero. However, empirically, there is no such case.

have not activated any AIE relationships as of January 4, 2018. The five largest suppliers of secrecy in this group, as measured by the FSI, are the United States, United Arab Emirates, Taiwan, Panama, and Thailand.

Figure 4.2: Relationship between the share of supplied Bilateral Financial Secrecy Index value covered by active automatic information exchange treaties and secrecy scores from the Financial Secrecy Index 2020



Source: Authors.

More formally, Table 4.3 presents the results of the estimation of the regression models characterized by Equation 4.1. We find that there is a negative and statistically significant relationship between the secrecy score and the share of supplied BFSI covered by active AIE, including when controlling for income and regional effects. The results suggest that an increase of 1 point in the secrecy score is associated with a roughly 0.7 (0.36 when controlling for regional and income effects) percentage point lower share of BFSI covered by activated AIE treaties. Our findings thus suggest that high-secrecy jurisdictions are aware of which countries they supply their secrecy to, and have so far been successful in avoiding or delaying the activation of AIE relationships with these countries, engaging in a strategy of selective resistance.

Table 4.3: Estimation of the relationship between the share of supplied BFSI covered by active AIE treaties as of Jan 4, 2018, and the Financial Secrecy Index 2020, adjusted Secrecy Score, and Global Scale Weight

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Secrecy score 2020	-0.703***	-0.362*	-0.72***	-0.36*				
(adjusted)	(0.149)	(0.196)	(0.155)	(.197)				
Global scale weight			-53.35	-82.9	-15.29	-83.47		
2020			(57.29)	(52.43)	(60.87)	(50.72)		
Financial Secrecy							-0.008	-0.007
Index 2020 (adjusted)							(.007)	(.006)
Regional groups	No	Yes	No	Yes	No	Yes	No	Yes
Income groups	No	Yes	No	Yes	No	Yes	No	Yes
No. of observations	78	78	78	78	78	78	78	78
R-squared	0.22	0.35	0.23	0.37	0	0.34	0.03	0.34

Source: Authors.

Note: Robust standard errors in parentheses. $***p < 0.01, **p < 0.05, *p < 0.1$. *Secrecy scores (and, consequently, the Financial Secrecy Index) are adjusted for intra-EU relationships and exclude Key Financial Secrecy Indicator 18 on automatic information exchange.*

To further examine the relationship and to provide more insight into which secrecy jurisdictions manage to avoid AIE in part or entirely, we run the regressions with the FSI value and the global scale weights instead of or in addition to the secrecy score. In doing so, we assess whether the negative relationship found above is driven by secrecy, by the scale of cross-border activity, or both. We present the results in columns 3-8 in Table 4.3 and show the relationship graphically in Figure B.15 in the Appendix for the FSI value (top panel) and for the GSW (bottom panel). While the coefficients are negative, we do not find statistically significant evidence for the hypothesis that the FSI value and the global scale weights are associated with the share of supplied BFSI covered by AIE treaties. These results suggest that secrecy scores are more important indicators of a tendency of jurisdictions to delay the activation of important AIE relationships that the scale of the jurisdictions' cross-border financial activity.

Overall, we find that jurisdictions with high secrecy scores manage to keep more of the secrecy they supply to other countries uncovered by AIE and engage in selective resistance. Our results thus point to AIE being of high importance to secrecy jurisdictions, and future policy efforts should stress the development of AIE relationships with the most secretive tax havens and the

need for true multilateralism as opposed to allowing bilateral cherry-picking. Our findings further suggest that OECD-controlled secrecy jurisdictions not necessarily succeed in dodging relevant exchange relationships more successfully than non-OECD dependent secrecy jurisdictions. However, we observe that OECD dependencies prevent a far higher share of their secrecy risks from being covered by AIE than OECD members themselves. This lends support to OECD hypocrisy in outsourcing dirty secrecy business to its dependencies, and to the hypothesis of a sham standard (see Table 4.1 above) designed by the OECD that would secure secrecy business for tax havens, even though the OECD members themselves would accept higher transparency.

4.4.2 Automatic information exchange: powerful countries successfully coercing tax havens?

Next, we focus on the receiving end of financial secrecy and to what extent the countries affected by secrecy jurisdictions are able to cover them with AIE. We turn to testing whether countries, in their efforts to counter the negative effects of secrecy, conclude bilateral AIE agreements with the most important secrecy jurisdictions. Figure B.14 A3 in the Appendix shows the share of BFSI accounted for by countries which are covered by existing activated AIE treaties versus the number of AIE relationships set up with these jurisdictions (as of four dates between January 2018 and October 2019). We observe substantial heterogeneity in countries' success in activating AIE relationships with their specific most important secrecy jurisdictions: while some countries, such as New Zealand, Poland, or Greece, had already by January 2018 covered around 90% of the secrecy risks they faced, other countries have covered much less despite having activated similar numbers of AIE relationships.

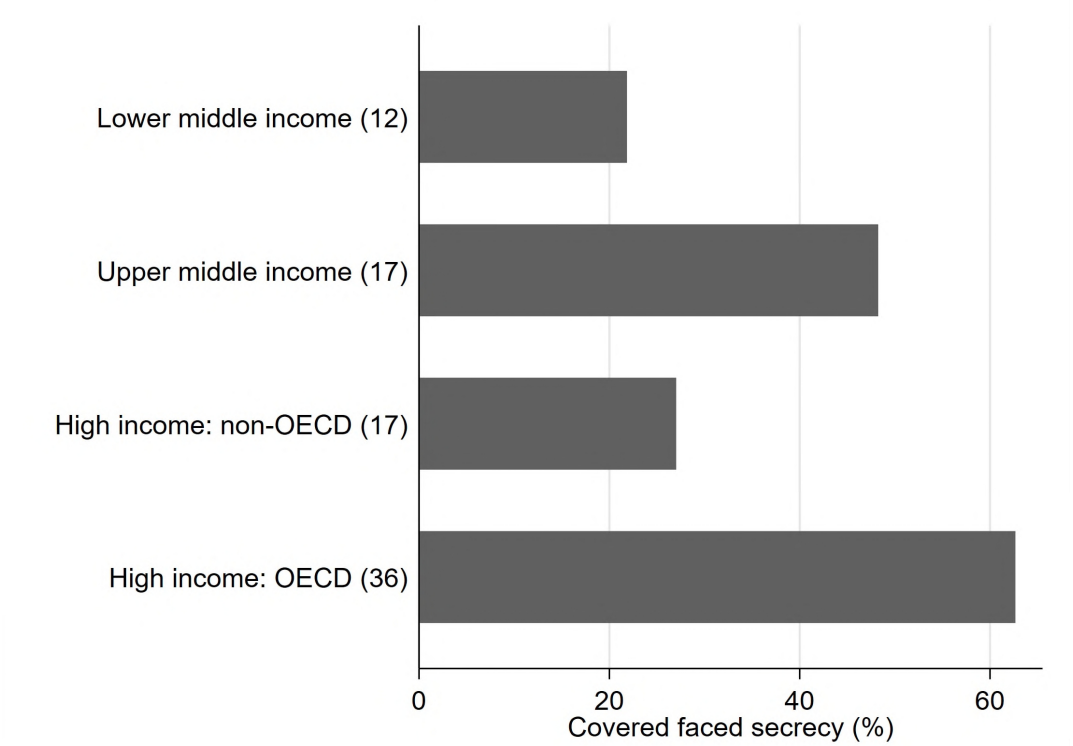
This straightforward comparison between the share of secrecy covered by AIE and the number of active AIE relationships can help us identify cases in which the attention and resources of policymakers regarding AIE might not be directed to the jurisdictions which harm their countries the most. For example, in January 2018, Malaysia, while having activated 73 AIE relationships, has covered only 53.9% of the secrecy it faced. In particular, Malaysia did not have an AIE relationship with 5 out of its top six largest secrecy suppliers. In contrast, New Zealand has also activated 73 AIE relationships, but has covered 89.1% of the secrecy it faced. China, Brazil, Argentina and Colombia were in similar situations as Malaysia. While the network of AIE relationships

has improved substantially between January 2018 and October 2019 and most countries now cover most of the secrecy they face, we argue that the BFSI can guide future policies.

In line with our discussion in Section 4.2, we expect that more powerful countries would be better than other countries at covering their most relevant secrecy jurisdictions with AIE. The one group of more powerful countries in tax matters are OECD member states which enjoy privileges around decision making in international tax matters and standard setting. Figure 4.3 shows the share of covered faced secrecy by income group. On average, high-income OECD member states succeed to cover substantially more secrecy risks than any other income group (above 60% vs. between 20% and 50%).[2] Overall, we conclude that powerful (OECD member) countries more successfully than others cover their most relevant secrecy jurisdictions with AIE. In the typology of tax standards, this observation lends support to AIE being a club standard primarily imposed for the club member's benefit (see Table 4.1 above).

[2]While Figure 4.3 shows a weighted average, the findings are similar for unweighted averages. A two-sided t-test of arithmetic sample means between OECD and non-OECD countries reveals a statistically significant difference of 41 percentage points in the share of faced BFSI covered by active AIE treaties. Further in support of our argument that membership in the OECD is a strong predictor of the share of covered faced secrecy, Figure B.16 in the Appendix shows a scatter plot between secrecy covered and GDP per capita, suggesting that the relationship solely with income per capita levels is not clear-cut and there are outliers across all income groups.

Figure 4.3: Share of faced Bilateral Financial Secrecy Index covered by active automatic information exchange treaties, by income group



Source: Authors.

Notes: Numbers in parentheses indicate the number of countries in each income group.

4.4.3 EU's tax haven blacklists

Finally, we turn to examining and comparing the EU tax haven blacklisting approach with the EU's AIE network. Using the BFSI, we find that 30.1% of the secrecy faced by EU countries is supplied by other member states (most importantly by the Netherlands and Luxembourg).[3] Regarding secrecy jurisdictions outside the EU, Table 4.4 shows the top 15 suppliers of secrecy to the EU member states together with an indication of whether the jurisdiction is included in the black or the grey list published by the European Commission. Out of the 17 blacklisted jurisdictions in the original blacklist from December 2017, only

[3]Separately, another useful observation can be made from the results of the BFSI if we focus on one particular group of jurisdictions that is often pointed to as harmful to the financial transparency in the EU—the British overseas countries and territories. We find that of the total secrecy received by the EU member states from the outside world, the British overseas countries and territories are responsible for 13.2%.

twelve have secrecy scores available, and these are together responsible for only 5.9% of the BFSI faced by the EU member states. Comparing the results of the BFSI with the grey list, we find that the original list, which included 47 jurisdictions (of which we have secrecy scores for only 25), covered 26.9% of secrecy faced by the EU. The black and grey lists from December 2018 have covered 0.3% and 33.6% of faced secrecy, while those from December 2019 have covered 0.35% and 15.9%, respectively. We find that ten of the top fifteen BFSI jurisdictions that supply secrecy to the EU have at least at one point been included on the lists (i.e. they have been identified by the European Commission as in need of addressing deficiencies), with the United States, Japan, Canada, Singapore, and China missing.[4] The United Arab Emirates and South Korea have moved from the black list to the grey list only in the January 2018 update. Overall, while the EU has, to a large extent, succeeded in identifying the most potentially harmful jurisdictions according to the BFSI, as of December 2019, only four of these remain on the grey list, and none are on the black list.

[4]While the British Virgin Islands were missing in the original lists from December 2017, their assessment was only delayed because of the devastating consequences of the hurricane Irma (European Commission 2020)

Table 4.4: Top 15 secrecy jurisdictions (excluding countries within the EU) for EU member states and their presence on three editions of the EU's black list and grey list

				Lists of non-cooperative jurisdictions		
Rank	Country	BFSI	Secrecy Score	2017/12	2018/12	2019/12
1	United States	6,515	63			
2	Cayman Islands	4,954	76			
3	Switzerland	4,289	74			
4	Japan	3,041	63			
5	Bermuda	2,344	73			
6	Jersey	2,238	66			
7	Guernsey	2,082	71			
8	Hong Kong	2,020	66			
9	UAE	2,009	78			
10	Canada	1,987	56			
11	BVI	1,973	71			
12	Singapore	1,691	65			
13	South Korea	1,638	62			
14	China	1,571	60			
15	Thailand	1,517	73			

Source: Authors.

Note: The editions of the black and grey lists for the years 2017-2019 are those that were effective in December of each year and published on December 5, 2017, December 4, 2018, and November 14, 2019, respectively.

When comparing the success of EU's tax haven blacklists with the EU's AIE network in covering secrecy risks, our expectation discussed in Section 4.2 is that AIE will be more successful than blacklisting. To enable comparison, we need to consider that the EU's blacklists omit other EU member states by definition. After adjusting the faced secrecy risks by removing any provision of secrecy from within the EU, we find that the EU member states have covered 58.7% of the secrecy they face from countries outside the EU by AIE relationships. In contrast, the blacklisting exercise has covered between 15.9% to

33.6%. We thus confirm and conclude that the EU member states are indeed more successful in covering their most relevant secrecy jurisdictions with AIE than with their blacklisting – even after adjusting for the fact that the blacklist has a political bias at its core by ignoring any secrecy risks from within the EU.

4.5 Conclusion

In this paper we address the question of how successful countries are in covering secrecy jurisdictions with their policies, in particular, AIE and blacklisting. We thus contribute to solving a pressing dilemma in international tax governance: how to reconcile the apparent simultaneity of successful international tax cooperation and continued resistance by tax havens. While we point out various loopholes in the AIE policy framework (i.e. CRS), our key findings rest on the assumption that the AIE system reduces financial secrecy and associated risks for illicit financial flows. In the policy era of AIE as designed by the OECD, we find evidence that particularly secretive jurisdictions successfully can and do apply a strategy of selective resistance in dodging relevant information exchange relationships. We interpret our results as in support of the hypothesis of a hypocrisy of OECD countries, in that its controlled secrecy jurisdictions - above all of the former colonial powers UK and the Netherlands – are found to be using selective resistance far more intensely than OECD member states. This complements earlier findings of US hypocrisy in international tax governance (Hakelberg and Schaub 2018) and supports the notion of AIE being a sham standard.

Our second main finding shows that those countries belonging to the OECD, the most powerful club of countries in the tax world, more successfully than others cover their most relevant secrecy jurisdictions with AIE. Thus, under the cherry-picking system, they seem to be better able to cover and coerce suppliers of secrecy to cooperate with them than non-members of the club. This observation lends support to the AIE system being a club standard that benefits primarily OECD members. Our third and final finding confirmed the poor covering and political bias in blacklisting tax havens when comparing the recent EU blacklists with the AIE system. Despite the network of AIE relationships requiring bilateral agreement and thus - formally at least – consent between two partners while the black- and greylists do not require consent between the listing and the listed, the share of risks covered by the AIE policy

is more than twice as high as by the black- and greylist. This finding supports our critical discussion of tax haven blacklists as the basis for both policy making and academic research. We suggest future research on tax havens to replace those lists with, or at least include as a robustness test the inclusion of, more nuanced and objectively verifiable secrecy scores for individual's cross-border taxation or haven scores for corporate income taxation. Relying on tax haven lists is fraught with too many biases, omissions and time lags.

While international political economy scholars have previously found AIE to be successful in rolling back neoliberalism, we are more cautious about its ultimate effectiveness and underline the urgent need for detailed statistics on the CRS to ensure public accountability and further probing research. Until such data becomes available, the dyadic secrecy risk approach presented in the paper opens additional questions and avenues for further research: how would the findings of selective resistance, OECD power and hypocrisy change if the US FATCA was included in the analysis? While FATCA is highly asymmetrical even in its reciprocal version, there are many countries not even obtaining any reciprocity from the US. In addition, we suggest that the typology of international tax cooperation presented in Table 4.1 is a fruitful framework of analysis for bringing power back into the scholarly debate on international tax governance, which could be tested, for example, with the use of a gravity model. The framework also allows systematic analysis of the relationship between countries at various levels of income per capita, including of patterns and histories of colonial exploitation.

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Appendix A

Tables

Table A.1: Balancing properties of the samples of connected and non-connected firms, before and after matching

Non-connected firms	Obs	Mean	Std. Dev.	Min	Max
Unmatched					
ROE	2,573,352	-2,121.0	1,029,914	-1,390,000,000	31,100,000
ROA	2,568,447	-229.1	395,360	-374,000,000	440,000,000
ROE, trimmed at $\langle -100, 100 \rangle$	2,057,723	9.6	34.1	-100	100
ROA, trimmed at $\langle -100, 100 \rangle$	2,452,923	2.4	23.6	-100	100
Assets (adjusted)	2,580,555	56,800,000	2,130,000,000	0	2,400,000,000,000
Capital (adjusted)	2,583,192	26,900,000	938,000,000	0	814,000,000,000
Leverage	2,571,661	15	2,997	-2,381,568	3,375,915
Earnings before tax	2,585,306	2,530,786	135,000,000	-30,400,000,000	64,900,000,000
Total donations	0				
Matched					
ROE	5,446	-57	3,847.6	-282,867	5,547
ROA	5,452	9.3	21.7	-431.7	677.0
ROE, trimmed at $\langle -100, 100 \rangle$	5,059	21.7	27.4	-99.5	100
ROA, trimmed at $\langle -100, 100 \rangle$	5,432	9.6	16.1	-97.3	97.3
Assets (adjusted)	5,452	37,300,000	101,000,000	268,000	2,680,000,000
Capital (adjusted)	5,452	17,700,000	56,100,000	0	1,680,000,000
Leverage	5,452	0.6	0.4	-4.5	11
Earnings before tax	5,452	3,256,762	17,000,000	-497,000,000	797,000,000
Total donations	0				

Connected firms	Obs	Mean	Std. Dev.	Min	Max
Unmatched					
ROE	3,546	-13.7	633.5	-15,528	10,584.8
ROA	3,559	5.4	34.8	-1,502.9	114.6
ROE, trimmed at $\langle -100, 100 \rangle$	3,161	17.9	29.1	-99	100
ROA, trimmed at $\langle -100, 100 \rangle$	3,540	6.8	16.9	-99.9	96.6
Assets (adjusted)	3,561	289,000,000	1,040,000,000	0	31,400,000,000
Capital (adjusted)	3,561	135,000,000	519,000,000	0	13,700,000,000
Leverage	3,558	0.6	0.7	-0.7	29.9
Earnings before tax	3,562	18,200,000	92,200,000	-640,000,000	2,670,000,000
Total donations	3,562	241,526.3	5,279,273	1	303,000,000
Matched					
ROE	1332	-21.5	640.6	-19500	1991.9
ROA	1334	9.6	14.3	-54.8	91.6
ROE, trimmed at $\langle -100, 100 \rangle$	1267	21.4	25.6	-90	99.3
ROA, trimmed at $\langle -100, 100 \rangle$	1334	9.6	14.3	-54.8	91.6
Assets (adjusted)	1334	71300000	185000000	309000	3010000000
Capital (adjusted)	1334	32400000	80600000	0	1270000000
Leverage	1334	0.5	0.3	-0.5	2.4
Earnings before tax	1334	5629133	18200000	-28400000	466000000
Total donations	1334	84272.9	264676.4	200	3250000

Table A.2: Results of a paired t-test of equal means of rate of return for connected and non-connected (but otherwise similar) firms; by time period

Variable	Obs.	Mean	Std. Err.	95% Conf. Interval		t-statistic	p-value
Period: before 2005							
ROEc	97	31.13	2.43	26.31	35.95		
ROEnc	97	25.85	2.33	21.23	30.46		
Difference	97	5.28	2.92	-0.52	11.08	1.81	0.04
Period: 2006-2010							
ROEc	746	23.08	0.909	21.30	24.87		
ROEnc	746	21.85	0.76	20.34	23.36		
Difference	746	1.23	1.04	-0.81	3.28	1.18	0.12
Period: 2011-2014							
ROEc	375	17.87	1.13	15.64	20.11		
ROEnc	375	16.51	1.01	14.54	18.48		
Difference	375	1.36	1.17	-0.94	3.67	1.16	0.12

Table A.3: Results of a t-test, clustered at the party network level, of equal means of financial performance indicators for connected and non-connected (but otherwise similar) firms.

Variable	Obs.	Clusters	Mean	Std. Err.	95% Conf.	Interval	t-statistic	p-value
ROEc	1230	20	21.28	2	17.08	25.49		
ROEnc	1230	1	20.53	2.99				
Difference	1230		0.76	3.6	-6.78	8.29	0.21	0.42
ROAc	1230	20	10.08	1.08	7.83	12.34		
ROAnc	1230	1	9.44	1.61				
Difference	1230		0.64	1.94	-3.41	4.69	0.33	0.37

Table A.4: Results of the pooled model with return on equity as the dependent variable, using different trimming and winsorization procedures, while using the preffered smoothing period t $\rightarrow$ t+2; OLS. All models include year- and industry-fixed effects.

	(1) ROE, trimmed at $\langle -1, 1 \rangle$	(2) ROE, winsorized at $\langle -1, 1 \rangle$	(3) ROE, trimmed at 1st and 99th percentile	(4) ROE, winsorized at 1st and 99th percentile	(5) ROE, trimmed at 5th and 95th percentile	(6) ROE, winsorized at 5th and 95th percentile
Lag of ROE	.291***	.254***	.288***	.273***	.323***	.301***
	[.289,.294]	[.252,.256]	[.282,.293]	[.267,.278]	[.32,.326]	[.298,.303]
PubInd	5.95	12.4**	3.85	46**	-12.1	6.8
	[-1.87,13.8]	[2.21,22.5]	[-23.6,31.3]	[10.2,81.8]	[-35.4,11.2]	[-13.3,26.9]
PubSec	2.35***	5.65***	26.9***	37.5***	7.39***	15.9***
	[2.21,2.49]	[5.45,5.86]	[25.9,28]	[35.9,39.2]	[7.09,7.68]	[15.4,16.4]
LocSize	-.072***	-.62***	-4.63***	-7.15***	-1.02***	-2.5***
	[-.106,-.038]	[-.671,-.569]	[-4.89,-4.36]	[-7.53,-6.78]	[-1.1,-.936]	[-2.62,-2.37]
FirmSize	.902***	1.13***	-1.61***	-6.84***	1.8***	.199***
	[.882,.923]	[1.1,1.16]	[-1.79,-1.44]	[-7.12,-6.56]	[1.75,1.85]	[.122,.277]
Leverage	4.3e-06	4.5e-06	1.7e-04	-1.7e-04	5.4e-05	4.5e-05
	[-2.2e-06,1.1e-05]	[-2.5e-05,3.4e-05]	[-1.3e-04,4.6e-04]	[-5.8e-04,2.5e-04]	[-1.5e-05,1.2e-04]	[-4.6e-05,1.4e-04]
DDon	.733**	1.34***	7.94***	15.8***	2.92***	5.77***
	[.116,1.35]	[.457,2.21]	[2.97,12.9]	[9.08,22.5]	[1.7,4.14]	[3.72,7.82]
Constant	-18***	-19.8***	57.4***	145***	-30.2***	5.49*
	[-19.9,-16]	[-22.7,-17]	[43.4,71.5]	[122,167]	[-34.2,-26.3]	[-.812,11.8]
Observations	855158	1167312	1127516	1167312	994719	1167312
R^2	0.189	0.147	0.116	0.115	0.175	0.157

95 % confidence intervals in brackets. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table A.5: Results of the pooled model with return on equity as the dependent variable, using different smoothing periods, while using the preffered trimming procedure at $\langle -1, 1 \rangle$; OLS. All models include year- and industry-fixed effects.

	(1) ROE, smoothed $t \to t+2$	(2) ROE at t	(3) ROE, smoothed $t-1 \to t+1$	(4) ROE, smoothed $t-2 \to t$
Lag of ROE	.291***	.352***		
	[.289,.294]	[.349,.355]		
Lag (2) of ROE			.26***	
			[.257,.262]	
Lag (3) of ROE				.246***
				[.243,.248]
PubInd	5.95	8.85	3.3***	7.94
	[-1.87,13.8]	[-1.75,19.5]	[1.21,5.4]	[-1.69,17.6]
PubSec	2.35***	1.54***	1.87***	1.53***
	[2.21,2.49]	[1.37,1.72]	[1.72,2.02]	[1.37,1.7]
LocSize	-.072***	-.103***	-.074***	-.093***
	[-.106,-.038]	[-.144,-.063]	[-.111,-.037]	[-.133,-.052]
FirmSize	.902***	1.13***	1.12***	1.28***
	[.882,.923]	[1.1,1.15]	[1.09,1.14]	[1.26,1.31]
Leverage	4.3e-06	8.2e-06**	1.2e-05***	-3.8e-08
	[-2.2e-06,1.1e-05]	[4.0e-07,1.6e-05]	[5.5e-06,1.8e-05]	[-2.2e-05,2.2e-05]
DDon	.733**	.992***	1.32***	1.56***
	[.116,1.35]	[.244,1.74]	[.705,1.94]	[.907,2.21]
Constant	-18***	-22.4***	-21.9***	-25.1***
	[-19.9,-16]	[-24.7,-20]	[-24,-19.8]	[-27.4,-22.8]
Observations	855158	855158	681503	553164
R^2	0.189	0.186	0.188	0.186

95 % confidence intervals in brackets. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table A.6: Sensitivity analysis for the pooled model with return on equity as the dependent variable; OLS. All models include year- and industry-fixed effects.

	(1) ROE	(2) ROE	(3) ROE	(4) ROE	(5) ROE	(6) ROE	(7) ROE	(8) ROE
DDon	6.24***	3.28***	3.27***	1.95***	1.94***	.732**	.733**	1.24***
	[5.58,6.91]	[2.67,3.89]	[2.66,3.88]	[1.34,2.56]	[1.32,2.55]	[.116,1.35]	[.116,1.35]	[.552,1.92]
Lag of ROE		.304***	.304***	.301***	.301***	.291***	.291***	
		[.302,.306]	[.302,.306]	[.299,.303]	[.299,.303]	[.289,.294]	[.289,.294]	
PubInd			6.2	6.09	6.03	5.95	2.84***	4.47***
			[-1.67,14.1]	[-1.73,13.9]	[-1.8,13.9]	[-1.87,13.8]	[.889,4.79]	[2.39,6.56]
PubSec				4.22***	4.23***	2.35***	2.35***	3.25***
				[4.09,4.36]	[4.09,4.36]	[2.21,2.49]	[2.2,2.49]	[3.09,3.4]
LocSize					-.064***	-.072***	-.072***	-.205***
					[-.097,-.03]	[-.106,-.038]	[-.106,-.038]	[-.239,-.171]
FirmSize						.902***	.902***	1.75***
						[.881,.923]	[.882,.923]	[1.73,1.77]
Leverage							4.3e-06	-4.1e-06
							[-2.2e-06,1.1e-05]	[-2.9e-05,2.1e-05]
Constant	-2.71***	-1.56**	-.081	-1.28	-.995	-17.9***	-18***	-32.7***
	[-4.03,-1.39]	[-2.95,-.178]	[-1.99,1.83]	[-3.18,.628]	[-2.91,.917]	[-19.9,-16]	[-19.9,-16]	[-34.9,-30.6]
Observations	1189894	861810	860429	860429	860429	855185	855114	1179817
R^2	0.039	0.180	0.181	0.183	0.183	0.189	0.189	0.064

Notes: 95 % confidence intervals in brackets. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table A.7: Sensitivity analysis for the pooled model with return on assets as the dependent variable; OLS. All models include year- and industry-fixed effects.

	(1) ROA	(2) ROA	(3) ROA	(4) ROA	(5) ROA	(6) ROA	(7) ROA	(8) ROA
DDon	3.77***	2.18***	2.18***	1.4***	1.36***	.544***	.548***	.855***
	[3.4,4.14]	[1.84,2.52]	[1.84,2.52]	[1.06,1.74]	[1.02,1.7]	[.203,.886]	[.207,.89]	[.482,1.23]
Lag of ROA		.303***	.303***	.301***	.301***	.294***	.294***	
		[.301,.306]	[.301,.306]	[.299,.304]	[.298,.304]	[.291,.297]	[.291,.296]	
PubInd			2.36	2.07	1.89	1.77	1.77	2.26
			[-.926,5.65]	[-1.19,5.32]	[-1.36,5.15]	[-1.47,5]	[-1.47,5]	[-1.14,5.65]
PubSec				2.59***	2.59***	1.36***	1.36***	1.96***
				[2.51,2.67]	[2.51,2.67]	[1.28,1.44]	[1.27,1.44]	[1.87,2.04]
LocSize					-.217***	-.244***	-.243***	-.375***
					[-.238,-.196]	[-.265,-.223]	[-.264,-.222]	[-.396,-.355]
FirmSize						.638***	.638***	1.08***
						[.623,.653]	[.624,.653]	[1.06,1.09]
Leverage							-2.7e-05	-7.9e-05
							[-8.4e-05,3.0e-05]	[-2.1e-04,4.8e-05]
Constant	-2.69***	-2.24***	.608	-.121	.838	-11.1***	-11.1***	-17.8***
	[-3.54,-1.85]	[-3.11,-1.36]	[-.471,1.69]	[-1.2,.962]	[-.251,1.93]	[-12.2,-9.94]	[-12.2,-9.94]	[-19,-16.6]
Observations	1408046	1101866	1100056	1100056	1100056	1100056	1099867	1405017
R^2	0.032	0.163	0.163	0.165	0.165	0.171	0.171	0.052

Notes: 95 % confidence intervals in brackets. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table A.8: Results of the pooled model which divides the sample into 5 quintiles based on the volume of public procurement supplied by firms within the same sector between 2006 and 2014.

	(1) ROE	(2) ROE	(3) ROE	(4) ROE	(5) ROE
Lag of ROE	.296***	.308***	.285***	.284***	.218***
	(114)	(135)	(114)	(126)	(39.8)
PubSec	1.36***	3.44***	2.24***	2.39***	2.21***
	(8.79)	(20.7)	(15.6)	(16.8)	(7.87)
LocSize	.145***	-.116***	-.074*	-.186***	.061
	(3.47)	(-3.62)	(-1.91)	(-5.88)	(.694)
FirmSize	.661***	.773***	1.01***	1.17***	.801***
	(28.1)	(40)	(44.3)	(57.1)	(13)
Leverage	-3.6e-04***	3.3e-06	7.8e-06***	3.3e-05*	-1.0e-03*
	(-3.27)	(.255)	(2.62)	(1.85)	(-1.68)
DDon	.71	.85	.837	.198	1.54*
	(1.05)	(1.1)	(1.32)	(.308)	(1.8)
Constant	-13.3***	-16.2***	-26.5***	-21.7***	-17.5***
	(-11.5)	(-18.9)	(-16.3)	(-27.8)	(-12)
Year-fixed effects	Yes	Yes	Yes	Yes	Yes
Sector-fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	171767	239543	180071	228785	36372
R^2	0.202	0.205	0.178	0.177	0.130

Notes: Robust t-statistics in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table A.9: Results of the pooled models in the matched sample, OLS.

	(1) ROE	(2) ROA	(3) ROE	(4) ROA
Lag of ROE	.349***		.348***	
	(21.3)		(9.48)	
Lag of ROA		.401***		.316***
		(15.1)		(8.38)
PubInd	24.3***	15.5***	13.2	5.3
	(2.91)	(3.35)	(1)	(1.13)
PubSec	.904	.157	1.52	.063
	(1.43)	(.45)	(1.15)	(.095)
LocSize	-.153	-.193	-1.09**	-.524**
	(-.714)	(-1.6)	(-2.17)	(-2.04)
FirmSize	1.35***	.656***	.01	-.164
	(5.77)	(4.89)	(.022)	(-.643)
Leverage	-2.39**	-7.29***	-.406	-9.59***
	(-2.12)	(-4.93)	(-.124)	(-6.53)
DDon	-.074	-.23		
	(-.108)	(-.64)		
ln(Don)			.198	.38**
			(.449)	(2.12)
Constant	-17.2	-5.75	24.4**	16.9***
	(-1.38)	(-.99)	(2.41)	(3.22)
Year-fixed effects	Yes	Yes	Yes	Yes
Sector-fixed effects	Yes	Yes	Yes	Yes
Observations	6326	6764	1267	1334
R^2	0.143	0.274	0.224	0.302

Notes: Robust t-statistics in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table A.10: Summary statistics of the used variables

Variable	Count	Mean	Std. Dev.	Min	Max	Source
Rate of return on FDI (%)	631	8.25	5.52	0.06	38.88	IMF BoP
Rate of return on FDI - equity component (%)	614	7.6	5.55	0	37.64	IMF BoP
Rate of return on FDI - debt component (%)	542	0.72	0.77	0	6.97	IMF BoP
Share of FDI from tax havens	631	0.2803	0.1329	0	0.701	IMF CDIS
Inward FDI stock (USD billion)	631	157,512.9	375,594.7	132.16	3,711,057	IMF CDIS
GDP (USD billion)	628	669,165.9	2,020,984	245.56	18,600,000	WB, UN, CIA
Nominal corporate tax rate (%)	631	23.45	8.37	0	40.69	KPMG, WB
Total corporate tax revenue (% of GDP)*	66	21.06	6.83	7.63	45.88	GRD
Total tax revenue (% of GDP)*	53	2.58	1.05	0.2	5.5	GRD

Source: Authors; data from IMF's CDIS and UNCTAD's FDI database.

*Note: *This data is for 2016 only; in our analysis, it is used only to present results.*

Table A.11: Results of the baseline model

Dependent variable:	FDI rate of return			Equity component of FDI rate of return			Debt component of FDI rate of return		
	All	Developing	Developed	All	Developing	Developed	All	Developing	Developed
Share of FDI from tax havens	-.0642***	-.0792***	-0.0277	-.0713***	-.0899***	-0.0276	0.0018	-0.0021	0.0027
	(0.020)	(0.028)	(0.017)	(0.020)	(0.028)	(0.017)	(0.003)	(0.004)	(0.004)
Regional effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	631	420	211	614	410	204	542	346	196
R-squared	0.158	0.185	0.163	0.189	0.206	0.147	0.122	0.086	0.168

Source: Authors; data from IMF's CDIS and UNCTAD's FDI database.

*Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table A.12: Estimating the scale of profit shifting and tax revenue losses, 2016

		A	B	C = (-1)*A*B	D	E = D*C	F	G = E/(1-F)	
	Model	Estimate from the regression	Exposure to tax haven investment	Estimated profitability gap	Reported FDI stock (USD trillion)	Simulated profit shifting (after-tax, USD billion)	Average corporate tax rate weighted by FDI income	Simulated profit shifting (pre-tax, USD billion)	Tax revenue losses (USD billion)
All countries	Our results – ROR	-0.0642	0.3949	0.0254	16.68	423.67	0.2923	598.66	174.99
	Our results – ROR equity	-0.0713	0.3949	0.0282	16.68	470.38	0.2923	664.66	194.28
Developing countries	UNCTAD (2015) – ROR	-0.115	0.46	0.053	5 (data for 2012)	265	0.2	331.25	66.25
	UNCTAD (2015) – ROR equity	-0.158	0.46	0.072	5 (data for 2012)	360	0.2	450	90
	Our results – ROR	-0.0792	0.49	0.0388	6.18	239.78	0.258	323.15	83.37
	Our results – ROR equity	-0.0899	0.49	0.0441	6.18	272.54	0.258	367.3	94.76
Developed countries	Our results – ROR	-0.0277	0.35	0.0097	10.51	101.95	0.3125	148.29	46.34
	Our results – ROR equity	-0.0276	0.35	0.0097	10.51	101.95	0.3125	148.29	46.34

Source: Authors' construction; UNCTAD (2015).

Table A.13: Sensitivity analysis of the inclusion of interaction terms in the extended model

Dependent variable: ROR - equity component	(1)	(2)	(3)
Share of FDI from tax havens (Share)	-0.112***	-0.128***	-0.123***
	(0.039)	(0.012)	(0.018)
Share * Low income		Omitted (=base)	Omitted (=base)
Share * Lower middle income		0.0463	0.0413
		(0.067)	(0.132)
Share * Upper middle income		0.1000*	0.118
		(0.056)	(0.137)
Share * High income: non-OECD		-0.134**	-0.0659
		(0.064)	(0.143)
Share * High income: OECD		0.139***	0.149
		(0.048)	(0.137)
Share * Sub-Saharan Africa	Omitted (=base)		Omitted (=base)
Share * Europe and Central Asia	0.0366		-0.0513
	(0.045)		(0.131)
Share * East Asia and Pacific	0.134**		0.0605
	(0.055)		(0.132)
Share * Latin America and Caribbean	-0.104		-0.112
	(0.096)		(0.144)
Share * Middle East and North Africa	0.269**		0.212
	(0.104)		(0.159)
Share * North America	0.067		-0.0679
	(0.041)		(0.129)
Share * South Asia	-0.228		-0.25
	(0.182)		(0.212)
Constant	0.0787***	0.0843***	0.0622***
	(0.020)	(0.018)	(0.018)
Observations	614	614	614
R-squared	0.264	0.23	0.278
Income effects	Yes	Yes	Yes
Regional effects	Yes	Yes	Yes
Year effects	Yes	Yes	Yes
F – test for joint significance	46.2		54.18
of regional group interactions	[0]		[0]
F – test for joint significance		23.45	19.45
of income group interactions		[0]	[0]

Source: Authors.

Note: Standard errors in parentheses, p-values in brackets. $***p < 0.01, **p < 0.05, *p < 0.1$.

Table A.14: Estimation results of the extended model – robust standard errors vs. standard errors clustered at the country level

	(1)	(2)	(3)	(4)
	ROR	ROR	ROR equity component	ROR equity component
Share of FDI from tax havens	-0.158**	-0.158*	-0.123*	-0.123
(Share)	(0.067)	(0.079)	(0.064)	(0.074)
Share * Low income	Omitted (=base)	Omitted (=base)	Omitted (=base)	Omitted (=base)
Share	0.0655	0.0655	0.0413	0.0413
* Lower-middle income	(0.141)	(0.231)	(0.134)	(0.219)
Share	0.207	0.207	0.118	0.118
* Upper-middle income	(0.148)	(0.244)	(0.143)	(0.233)
Share	-0.0258	-0.0258	-0.0659	-0.0659
* High income: non-OECD	(0.152)	(0.246)	(0.149)	(0.236)
Share	0.189	0.189	0.149	0.149
* High income: OECD	(0.148)	(0.240)	(0.142)	(0.230)
Share * Sub-Saharan Africa	Omitted (=base)	Omitted (=base)	Omitted (=base)	Omitted (=base)
Share	-0.0495	-0.0495	-0.0513	-0.0513
* Europe and Central Asia	(0.135)	(0.230)	(0.129)	(0.219)
Share	0.0936	0.0936	0.0605	0.0605
* East Asia and Pacific	(0.136)	(0.228)	(0.131)	(0.216)
Share	-0.134	-0.134	-0.112	-0.112
* Latin America and Caribbean	(0.138)	(0.239)	(0.134)	(0.230)
Share	0.205	0.205	0.212	0.212
* Middle East and North Africa	(0.136)	(0.239)	(0.131)	(0.229)
Share	-0.0324	-0.0324	-0.0679	-0.0679
* North America	(0.137)	(0.230)	(0.133)	(0.221)
Share	-0.188	-0.188	-0.250*	-0.25
* South Asia	(0.142)	(0.241)	(0.137)	(0.231)
Constant	0.0639***	0.0639***	0.0622***	0.0622***
	(0.016)	(0.019)	(0.017)	(0.019)
Observations	631	631	614	614
R-squared	0.264	0.264	0.278	0.278
Standard errors	Robust	Clustered at country level	Robust	Clustered at country level
Income effects	Yes	Yes	Yes	Yes
Regional effects	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes

Source: Authors.

Note: Standard errors in parentheses. $***p < 0.01, **p < 0.05, *p < 0.1$.

Table A.15: Results of the estimation of the extended model – summary of region-income group combinations

Region	Income group	ROR method	ROR – equity component method	No. of countries
South Asia	Low income	-0.346**	-0.372**	1
		(0.223)	(0.215)	
South Asia	Lower middle income	-0.281***	-0.331***	5
		(0.196)	(0.191)	
Latin America & Caribbean	High income: nonOECD	-0.317***	-0.3***	4
		(0.032)	(0.037)	
Europe & Central Asia	High income: nonOECD	-0.233***	-0.24***	4
		(0.053)	(0.054)	
Latin America & Caribbean	Lower middle income	-0.226***	-0.193***	4
		(0.055)	(0.068)	
Europe & Central Asia	Lower middle income	-0.142***	-0.133***	7
		(0.029)	(0.025)	
East Asia & Pacific	High income: nonOECD	-0.09	-0.128	1
		(0.056)	(0.072)	
Sub-Saharan Africa	Low income	-0.158**	-0.123*	8
		(0.018)	(0.018)	
Latin America & Caribbean	Upper middle income	-0.084	-0.116**	4
		(0.060)	(0.067)	
Latin America & Caribbean	High income: OECD	-0.103*	-0.085	1
		(0.063)	(0.069)	
Sub-Saharan Africa	Lower middle income	-0.092	-0.081	4
		(0.137)	(0.128)	
Europe & Central Asia	Upper middle income	0	-0.056	11
		(0.047)	(0.042)	
North America	High income: OECD	-0.002	-0.041	2
		(0.025)	(0.026)	
Europe & Central Asia	High income: OECD	-0.019	-0.025	17
		(0.019)	(0.020)	
East Asia & Pacific	Lower middle income	0.001	-0.021	4
		(0.023)	(0.031)	
Sub-Saharan Africa	Upper middle income	0.049	-0.004	2
		(0.139)	(0.133)	

Source: Authors.

Note: Standard errors in parentheses. $***p < 0.01, **p < 0.05, *p < 0.1$.

Table A.16: Estimated tax revenue losses and their share on GDP. Global model, developed and developing countries model, and extended model's rate of return on equity method, 2016

	Global model	Developed and developing countries model	Extended model						
Country	Tax revenue loss (USD million)	Tax revenue loss (USD million)	Shifted profits (USD million)	Shifted profits as % of all corporate profits	Shifted profits as % of foreign corporations' profits	Tax revenue loss (USD million)	Tax revenue loss as % of GDP	Tax revenue loss as % of corporate tax revenue	Tax revenue loss as % of total tax revenue
Sint Maarten	7.8	3	94.9	1.74	14.91	32.7	8.95		
Mozambique	226.5	285.8	1216.8	0.74	6.35	389.4	3.53		17.59
Curacao	10.6	4.1	202.4	0.43	3.72	44.5	1.42		
Croatia	208.7	263.3	3509.6	0.46	3.93	701.9	1.37		5.84
Russia	5030.3	6346.6	84599.9	0.29	2.28	16920	1.32	41.16	5.93
Zambia	216.5	273.2	705.4	0.23	1.93	246.9	1.18		9.12
India	5335.9	6732.2	71598.2	0.19	9.26	24780.1	1.09		
Honduras	83.3	105.1	752.7	0.23	2	225.8	1.04		5.34
El Salvador	91.7	115.7	828.9	0.23	1.99	248.7	1.04	32.22	6.64
Macao	220.8	85.7	3303.8	0.49	4.19	396.5	0.87	56.26	3.22
Belgium	9828.8	3813.9	10025.7	0.12	0.31	3407.7	0.73	21.17	2.39

Uganda	99.4	125.4	569.7	0.16	1.36	170.9	0.71		5.67
Georgia	47.4	59.8	587.6	0.27	2.35	88.1	0.61	19.7	2.37
Pakistan	367.4	463.5	5331.6	0.13	1.1	1706.1	0.61		
Brazil	6695.7	8447.9	32117.7	0.12	1.08	10920	0.61	19.27	2.66
Latvia	48.5	18.8	1087.3	0.26	0.89	163.1	0.59	34.89	2.7
Chile	1146.5	1446.5	5723.6	0.08	0.58	1373.7	0.55	12.85	3.17
Kazakhstan	951.7	1200.8	3709.1	0.18	1.55	741.8	0.54		
Mexico	2832.1	3573.2	15396.3	0.05	0.66	4618.9	0.43	11.96	
Argentina	549.5	693.3	6617.3	0.08	0.69	2316.1	0.42	14.25	1.71
Lithuania	49.6	19.2	1112.3	0.17	1.49	166.8	0.39	24.01	2.24
Ukraine	183.4	231.4	1894.5	0.14	1.17	341	0.37	14.47	1.45
Bolivia	44.4	56	481.3	0.09	0.82	120.3	0.35	8.04	1.35
Serbia	141.2	178.1	733.6	0.13	1.1	110	0.29	15.09	1.14
Costa Rica	99.9	126	542.9	0.04	0.49	162.9	0.29	10.6	2.15
Nigeria	998.4	1259.6	3794.5	0.06	0.54	1138.3	0.28	29.21	
Armenia	14.3	18	132.8	0.08	0.72	26.6	0.25	9.8	1.26
Sri Lanka	42.1	53.2	1304.4	0.11	0.92	195.7	0.24	17.21	1.93
Bhutan	1	1.3	15.7	0.05	0.41	4.7	0.21		
Guatemala	53.4	67.4	579.5	0.06	0.49	144.9	0.21	5.96	2.03
Cape Verde	3	3.7	13.5	0.06	0.47	3.4	0.21		1.04
Venezuela	133.9	169	1660.4	0.04	0.33	564.5	0.2	99.34	1.38
Bulgaria	127.6	161	994.4	0.12	1.07	99.4	0.19	8.7	0.88
Mali	14.6	18.4	83.7	0.04	0.34	25.1	0.18		1.2
United States	55242	21435.8	80188.8	0.04	0.52	32075.5	0.17	7.68	0.87

Bangladesh	79.8	100.7	1483	0.04	0.38	370.7	0.17	10.19	2.03
Moldova	5.8	7.4	90.5	0.09	0.77	10.9	0.16	6.41	0.74
Romania	371.1	468.2	1807.7	0.06	0.55	289.2	0.15		
Portugal	894.1	346.9	1476.1	0.06	0.31	310	0.15	4.92	0.6
Canada	3865.4	1499.9	8469.5	0.06	0.18	2244.4	0.15	4.57	0.54
France	9932.1	3854	10341	0.06	0.33	3443.6	0.14	6.84	0.49
Czech Republic	782	303.4	1426.9	0.04	0.08	271.1	0.14	3.84	0.72
Slovak Republic	358.4	139.1	564.8	0.05	0.1	124.3	0.14	3.66	0.74
Sweden	1859.5	721.5	2930.5	0.05	0.12	644.7	0.13	5	0.37
Spain	4457.3	1729.6	6181.6	0.04	0.3	1545.4	0.12	5.48	0.57
Norway	1132.1	439.3	1570	0.02	0.23	392.5	0.11	3.08	0.38
United Kingdom	7846.1	3044.6	13601.6	0.03	0.19	2720.3	0.1	3.66	0.38
Germany	10263.2	3982.5	11972.9	0.02	0.28	3558.3	0.1	5.14	0.44
Belarus	62.5	78.8	270.6	0.04	0.33	48.7	0.1	3.75	0.4
Estonia	68.2	26.5	118.3	0.03	0.08	23.7	0.1	5.79	0.44
Togo	2.5	3.2	24.8	0.04	0.32	4.3	0.1	5.2	0.45
Cote d'Ivoire	31.2	39.3	142.2	0.03	0.22	35.5	0.1		
Albania	14.6	18.4	75.6	0.04	0.37	11.3	0.1	9.44	0.52
Azerbaijan	46.2	58.3	180	0.03	0.27	36	0.1		
Kosovo	3.3	4.2	61.4	0.06	0.53	6.1	0.09		0.38

Myanmar	196.7	248.2	229.2	0.02	0.21	57.3	0.09		1.19
Indonesia	2766.4	3490.3	3223.2	0.02	0.2	805.8	0.09		0.83
Macedonia	11.4	14.4	88.8	0.06	0.48	8.9	0.08	4.65	0.5
Guinea-Bissau	0.6	0.7	7	0.04	0.34	1	0.08		0.89
Montenegro	4.6	5.8	40	0.06	0.53	3.6	0.08	6.9	0.31
Italy	4388.9	1703	4846	0.02	0.37	1521.7	0.08	3.84	0.27
Poland	1087.3	1371.8	1984.1	0.02	0.1	377	0.08	4.31	0.39
Kyrgyz Republic	2.8	3.5	51.6	0.05	0.43	5.2	0.08	2.97	0.37
Denmark	660.8	256.4	1041.4	0.02	0.2	229.1	0.07	2.77	0.16
Tajikistan	2.6	3.2	34	0.03	0.28	4.8	0.07		0.33
Burkina Faso	4.4	5.5	46.8	0.03	0.23	7.5	0.07		0.44
Nepal	2.3	2.9	59.1	0.02	0.16	11.8	0.06	1.49	0.3
Slovenia	71.8	27.9	146.5	0.04	0.12	24.9	0.06	3.49	0.25
Paraguay	8.6	10.9	140.5	0.03	0.29	14	0.05	1.87	0.41
Greece	274.7	106.6	328.4	0.01	0.24	95.3	0.05		0.18
Finland	339.4	131.7	588.4	0.02	0.14	117.7	0.05	2.22	0.16
Bosnia and Herzegovina	9.5	12	74	0.03	0.25	7.4	0.04	2.9	0.19
Philippines	452.2	570.5	439	0.01	0.08	131.7	0.04	1.17	0.32
Turkey	419.4	529.2	1634.7	0.01	0.45	326.9	0.04	2.3	0.21
Solomon Islands	1.1	1.3	1	0.01	0.05	0.3	0.03	0.48	0.09
Niger	1	1.3	5.9	0.01	0.04	1.8	0.02		

South Africa	948.4	1196.6	204	0	0.02	57.1	0.02	0.35	0.07
Benin	0.9	1.2	8.1	0.01	0.05	1.6	0.02		0.15
Botswana	20.2	25.5	5.5	0	0.02	1.2	0.01		
Palau	0	0				0	0		0
Kuwait	0	0				0	0		0
West Bank and Gaza	0.6	0.2							
Japan	1896.7	736							
South Korea	1063.7	412.7							
Morocco	83.7	105.6							
New Zealand	318.5	123.6							
Thailand	1116.3	1408.4							
Mongolia	35.3	44.5							
China	39284.1	49564.2							
Malaysia	1153.9	1455.9							
Australia	3203.4	1243							
Israel	577.2	224							
Total	193632.4	139760.3	419526.6			124576.7			

Source: Authors.

Table A.17: Differences in the estimates of tax revenue losses as shares of GDP and corporate tax revenue across income groups

As shares of GDP				
	Low income [9]	Lower middle income [24]	Upper middle income [17]	High income: nonOECD [9]
Lower middle income [24]	0.17 p.p. (0.26)			
Upper middle income [17]	0.35 p.p. (0.11)	0.18 p.p.** (0.04)		
High income: nonOECD [9]	-1.2 p.p. (0.12)	-1.37 p.p.** (0.01)	-1.54 p.p.** (0.01)	
High income: OECD [20]	0.37 p.p.* (0.08)	0.2 p.p.** (0.02)	0.02 p.p. (0.35)	1.56 p.p.** (0.01)
As shares of corporate tax revenue				
Lower middle income [24]	7.82 p.p. (0.27)			
Upper middle income [17]	18.19 p.p. (0.1)	10.37 p.p.** (0.02)		
High income: nonOECD [9]	-56.72 p.p. (0.13)	-64.55 p.p.** (0.01)	-74.92 p.p.** (0.01)	
High income: OECD [20]	19.47 p.p.* (0.07)	11.65 p.p.** (0.01)	1.28 p.p. (0.3)	76.2 p.p.** (0.01)

Source: Authors.

Note: p-values in parentheses. $***p < 0.01, **p < 0.05, *p < 0.1$*.; number of countries in each income group in brackets. The difference (in percentage points) is calculated as "Estimate for the income group in the row" – "Estimate for the income group in the column" and t-test is used.*

Table A.18: Differences in the estimates of tax revenue losses as shares of GDP across regional groups

As shares of GDP				
	Sub-Saharan Africa [14]	Europe & Central Asia [39]	South Asia [6]	Latin America & Caribbean [13]
Europe & Central Asia [39]	0.22 p.p. (0.1)			
South Asia [6]	0.07 p.p. (0.43)	-0.16 p.p. (0.14)		
Latin America & Caribbean [13]	-0.73 p.p. (0.15)	-0.96 p.p.** (0.01)	-0.8 p.p. (0.21)	
North America [12]	0.31 p.p. (0.33)	0.08 p.p. (0.36)	0.24 p.p. (0.22)	1.04 p.p. (0.28)
East Asia & Pacific [5]	0.24 p.p. (0.3)	0.02 p.p. (0.46)	0.17 p.p. (0.24)	0.97 p.p. (0.19)
As shares of corporate tax revenue				
Europe & Central Asia [39]	11.61 p.p.* (0.08)			
South Asia [6]	1.11 p.p. (0.48)	-10.5 p.p.* (0.06)		
Latin America & Caribbean [13]	-33.13 p.p. (0.17)	-44.74 p.p.** (0.01)	-34.24 p.p. (0.25)	
North America [12]	16.42 p.p. (0.32)	4.81 p.p. (0.32)	15.31 p.p. (0.21)	49.55 p.p. (0.28)
East Asia & Pacific [5]	11.7 p.p. (0.3)	0.09 p.p. (0.49)	10.59 p.p. (0.22)	44.83 p.p. (0.21)

Source: Authors.

Note: p-values in parentheses. $***p < 0.01, **p < 0.05, *p < 0.1$*.; number of countries in each income group in brackets. The difference (in percentage points) is calculated as "Estimate for the regional group in the row" – "Estimate for the regional group in the column" and t-test is used.*

Table A.19: Differences in the estimates of tax revenue losses as shares of GDP and corporate tax revenue across three income groups

As shares of GDP		
	Low income [9]	Middle income [41]
Middle income [41]	0.24 p.p. (0.12)	
High income [29]	-0.12 p.p. (0.42)	-0.36 p.p.* (0.09)
As shares of corporate tax revenue		
Middle income [41]	12.12 p.p. (0.12)	
High income [29]	-4.17 p.p. (0.44)	-16.3 p.p. (0.11)

Source: Authors.

Note: p-values in parentheses. $***p < 0.01, **p < 0.05, *p < 0.1$*.; number of countries in each income group in brackets. The difference (in percentage points) is calculated as "Estimate for the income group in the row" – "Estimate for the income group in the column" and t-test is used.*

Table A.20: Differences in the estimates of tax revenue losses as shares of GDP and corporate tax revenue across three groups of countries based on development status

As shares of GDP		
	Developing [39]	Transition [16]
Transition [16]	0.39 p.p. (0.16)	
Developed [24]	0.43 p.p.* (0.08)	0.04 p.p. (0.33)
As shares of corporate tax revenue		
Transition [16]	19.97 p.p. (0.14)	
Developed [24]	20.86 p.p.* (0.09)	0.9 p.p. (0.42)

Source: Authors.

Note: p-values in parentheses. $***p < 0.01, **p < 0.05, *p < 0.1$*.; number of countries in each income group in brackets. The difference (in percentage points) is calculated as "Estimate for the income group in the row" – "Estimate for the income group in the column" and t-test is used.*

Table A.21: Number of countries covered by each study in each income group

Study	Low income	Lower middle income	Upper middle income	High income: nonOECD	High income: OECD	Total number of countries
Our estimates	9	24	17	9	20	79
Tørsløv, Wier and Zucman (2018)	0	1	7	2	27	37
Cobham and Janský (2018)	24	29	20	7	22	102
Cobham and Janský (2019)	0	3	10	3	18	34
Clausing (2016)	0	2	5	2	16	25

Source: Authors; results from the cited studies.

Table A.22: Share of GDP covered by each study in each income group

Study	Low income	Lower middle income	Upper middle income	High income: nonOECD	High income: OECD	All countries
Our estimates	26%	82%	25%	45%	77%	62%
Tørsløv, Wier and Zucman (2018)	0%	38%	85%	26%	95%	83%
Cobham and Janský (2018)	76%	77%	69%	12%	95%	81%
Cobham and Janský (2019)	0%	43%	83%	46%	91%	82%
Clausing (2016)	0%	53%	83%	38%	82%	77%

Source: Authors; results from the cited studies.

Table A.23: Correlations between GDP per capita and estimated tax revenue losses as shares of GDP

	GDP per capita	Our estimates	Tørsløv, Wier and Zucman (2018)	Cobham and Janský (2018)	Cobham and Janský (2019)	Clausing (2016)
GDP per capita	1					
	[161]					
Our estimates	-0.0569	1				
	(0.618)					
	[79]	[79]				
Tørsløv, Wier and Zucman (2018)	0.2994*	-0.1714	1			
	(0.072)	(0.393)				
	[37]	[27]	[37]			
Cobham and Janský (2018)	-0.3553***	0.5503***	0.2642	1		
	(0)	(0)	(0.212)			
	[102]	[45]	[24]	[102]		
Cobham and Janský (2019)	-0.0679	0.027	0.3876*	0.0467	1	
	(0.707)	(0.907)	(0.056)	(0.825)		
	[33]	[21]	[25]	[25]	[34]	
Clausing (2016)	0.3274	0.0654	0.4113*	0.3983	0.0331	1
	(0.110)	(0.784)	(0.051)	(0.127)	(0.896)	
	[25]	[20]	[23]	[16]	[18]	[25]

Source: Authors; results from the cited studies.

Note: p-values in parentheses. $***p < 0.01, **p < 0.05, *p < 0.1$*.; number of observations in brackets.*

Table A.24: Correlation table for different versions of the Bilateral Financial Secrecy Index which use different data sources to calculate bilateral scale weights

	BFSI (IMF CPIS)	**BFSI using BIS LBS**	**BFSI using WTO trade in financial services**	**BFSI using IMF CDIS**
BFSI (IMF CPIS)	1			
	(6743)			
BFSI using BIS LBS	0.7908***	1		
	(3860)	(7438)		
BFSI using WTO trade in financial services	0.8255***	0.8156***	1	
	(536)	(459)	(558)	
BFSI using IMF CDIS	0.7359***	0.7433***	0.8171***	1
	(3387)	(2498)	(470)	(4482)

Source: Authors.

Notes: Number of observations in parentheses. $***p < 0.01, **p < 0.05, *p < 0.1$.

Table A.25: Descriptive statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max
CPIS assets (USD million)	12,924	3,990	42,800	0	1,690,000
Bank deposits (USD million)	4,907	4,140	30,800	0	969,000
Exports of financial services (USD million)	526	151.2	649.2	0	8,928
Foreign direct investment (USD million)	5,314	6,310	42,800	0	959,000
Secrecy Score	133	63.9	10.2	37.6	79.8
Secrecy Score (adjusted for intra-EU relationships)	133	63.8	10.2	37.6	79.8
Secrecy Score (adjusted for intra-EU relationships and excluding KFSI 18)	133	65.4	9.9	39.5	80.7

Source: Authors.

Table A.26: The most harmful secrecy relationships, as estimated by the Bilateral Financial Secrecy Index

Supplier of secrecy	**Receiver of secrecy**	**Secrecy score**	**Bilateral scale weight**	**BFSI**
Cayman Islands	United States	76	3.3%	1408
Cayman Islands	Japan	76	1.7%	1132
Cayman Islands	Hong Kong	76	0.9%	923
Switzerland	United States	74	0.9%	859
United States	Cayman Islands	63	3.2%	788
United States	Japan	63	2.9%	767
United States	Luxembourg	63	2.1%	687
United States	United Kingdom	63	2.0%	676
Japan	United States	63	1.9%	668
United States	Canada	63	1.9%	665
United States	Ireland	63	1.9%	658
Bermuda	United States	73	0.5%	642
Netherlands	United States	67	0.9%	640
Cayman Islands	Luxembourg	76	0.2%	595
Bermuda	Hong Kong	73	0.3%	529

Source: Authors.

Table A.27: Top ten secrecy jurisdictions and BFSI results for Germany and the United States

Rank	**Germany**	**SS**	**BSW**	**BFSI**	**United States**	**SS**	**BSW**	**BFSI**
1	Netherlands	67	0.5%	505	Cayman Islands	76	3.3%	1408
2	United States	63	0.8%	487	Switzerland	74	0.9%	859
3	Switzerland	74	0.1%	410	Japan	63	1.9%	668
4	Luxembourg	55	1.2%	377	Bermuda	73	0.5%	642
5	Cayman Islands	76	0.0%	330	Netherlands	67	0.9%	640
6	France	50	0.8%	236	Canada	56	1.9%	464
7	Japan	63	0.1%	214	Hong Kong	66	0.3%	431
8	Austria	57	0.2%	202	Taiwan	66	0.3%	408
9	United Arab Emirates	78	0.0%	192	Curacao	75	0.1%	406
10	Canada	56	0.1%	184	British Virgin Islands	71	0.1%	387

Source: Authors; secrecy scores from the Financial Secrecy Index 2020.

Table A.28: Top destinations of secrecy supplied by three largest suppliers of secrecy: the United States, Cayman Islands, and Switzerland

Rank	**United States**	**BFSI**	**Cayman Islands**	**BFSI**	**Switzerland**	**BFSI**
1	Cayman Islands	788	United States	1408	United States	859
2	Japan	767	Japan	1132	Luxembourg	473
3	Luxembourg	687	Hong Kong	923	United Kingdom	419
4	United Kingdom	676	Luxembourg	595	Germany	410
5	Canada	665	Ireland	502	Norway	361
6	Ireland	658	United Kingdom	492	Ireland	356
7	Netherlands	528	Switzerland	472	Japan	347
8	Norway	491	Netherlands	426	Canada	337
9	Germany	487	China	414	France	329
10	Bermuda	469	Australia	406	Netherlands	314

Source: Authors.

Table A.29: Shares of secrecy supplied by income groups in the columns to income groups in the rows

Income group	**High: OECD**	**High: nonOECD**	**Upper middle**	**Lower middle**	**Low**	**Total**
High: OECD	31.0%	19.4%	0.7%	5.8%	10.9%	67.7%
High: nonOECD	7.9%	6.9%	0.1%	1.6%	3.3%	19.8%
Upper middle	4.2%	3.7%	0.1%	0.8%	1.4%	10.2%
Lower middle	1.0%	0.7%	0.0%	0.1%	0.4%	2.2%
Low	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
Total	44.0%	30.8%	0.8%	8.3%	16.1%	100.0%

Source: Authors.

Table A.30: Top ten most important secrecy jurisdictions for seven regional groups

Rank	East Asia & Pacific	Europe & Central Asia	Latin America & Caribbean	Middle East & North Africa	North America	South Asia	Sub-Saharan Africa
1	Caymans	USA	USA	USA	Caymans	USA	USA
2	USA	Caymans	Switzerland	Caymans	Switzerland	UAE	Switzerland
3	Bermuda	Netherlands	Caymans	UAE	USA	Saudi Arabia	Bermuda
4	BVI	Switzerland	Netherlands	Qatar	Japan	China	Caymans
5	Hong Kong	Luxembourg	Luxembourg	Switzerland	Netherlands	Qatar	Hong Kong
6	Switzerland	Japan	Japan	Jordan	Bermuda	Netherlands	Guernsey
7	China	Bermuda	Bermuda	Netherlands	Hong Kong	Hong Kong	Singapore
8	Netherlands	Germany	Panama	Japan	South Korea	Caymans	Nigeria
9	Singapore	France	BVI	Egypt	Taiwan	Mauritius	UK
10	UAE	Jersey	Canada	Hong Kong	Canada	Japan	Luxembourg

Source: Authors.

Table A.31: Shares of global secrecy supplied by regions in the columns to regions in the rows

Region	**East Asia & Pacific**	**Europe & Central Asia**	**Latin America & Caribbean**	**Middle East & North Africa**	**North America**	**South Asia**	**Sub-Saharan Africa**	**Total**
East Asia & Pacific	5.2%	4.6%	3.6%	1.5%	2.4%	0.5%	0.6%	18.3%
Europe & Central Asia	9.3%	21.3%	9.3%	4.6%	5.1%	1.0%	2.9%	53.4%
Latin America & Caribbean	1.1%	2.2%	2.3%	0.5%	1.2%	0.1%	0.2%	7.5%
Middle East & North Africa	0.9%	1.7%	0.9%	1.3%	0.5%	0.1%	0.2%	5.7%
North America	2.9%	3.5%	2.9%	1.1%	1.0%	0.3%	0.7%	12.3%
South Asia	0.2%	0.2%	0.1%	0.1%	0.1%	0.0%	0.0%	0.7%
Sub-Saharan Africa	0.4%	0.5%	0.3%	0.2%	0.2%	0.1%	0.3%	2.1%
Total	20.0%	33.9%	19.3%	9.3%	10.5%	2.1%	4.9%	100%

Source: Authors.

Table A.32: Secrecy scores, their adjusted versions, aggregate results of the BFSI, and their comparison with FSI

	Country	Secrecy score 2020	Secrecy score 2020∗	Secrecy score 2020†	Total inward assets, USD billion, 2018	Total BSW (%)	Global scale weight 2020 (2018 data, %)	Summed BFSI 2020	Contribution (summed BFSI 2020, %)	FSI 2020	Contribution (FSI 2020, %)	FSI 2020 using CPIS data	Contribution (FSI 2020 using CPIS data, %)
1	United States	60	60	58	11976	21.98	22.30	14581	5.33	1298	4.12	1292	3.96
2	Cayman Islands	72	72	76	3927	7.21	3.79	12662	4.63	1268	4.02	1571	4.81
3	Switzerland	76	76	76	1102	2.02	4.50	10424	3.81	1590	5.04	1218	3.73
4	Netherlands	66	66	70	2179	4.00	0.90	9539	3.48	599	1.90	984	3.01
5	Luxembourg	58	58	61	3851	7.07	12.13	8936	3.26	976	3.10	815	2.50
6	Hong Kong	71	71	70	659	1.21	4.17	7657	2.80	1244	3.95	823	2.52
7	Germany	59	59	62	2523	4.63	5.17	7395	2.70	769	2.44	741	2.27
8	United Arab Emirates	84	84	84	91	0.17	0.14	7295	2.66	661	2.10	698	2.14
9	Bermuda	73	73	76	610	1.12	0.04	6695	2.45	282	0.89	872	2.67
10	Japan	61	61	62	2329	4.27	2.24	6582	2.40	624	1.98	774	2.37
11	Taiwan	76	76	74	363	0.67	0.50	5948	2.17	743	2.36	818	2.50
12	Thailand	80	80	79	136	0.25	0.13	5499	2.01	551	1.75	692	2.12
13	Singapore	67	67	68	398	0.73	4.58	5396	1.97	1082	3.43	587	1.80
14	France	52	52	54	3203	5.88	2.52	5208	1.90	404	1.28	536	1.64
15	China	60	60	62	1062	1.95	0.51	5135	1.88	373	1.18	583	1.79
16	British Virgin Islands	69	69	71	262	0.48	0.38	5092	1.86	503	1.59	546	1.67

Table A.32: Secrecy scores, their adjusted versions, aggregate results of the BFSI, and their comparison with FSI

	Country	Secrecy score 2020	Secrecy score 2020∗	Secrecy score 2020†	Total inward assets, USD billion, 2018	Total BSW (%)	Global scale weight 2020 (2018 data, %)	Summed BFSI 2020	Contribution (summed BFSI 2020, %)	FSI 2020	Contribution (FSI 2020, %)	FSI 2020 using CPIS data	Contribution (FSI 2020 using CPIS data, %)
17	Ireland	51	51	53	2122	3.89	2.66	4771	1.74	388	1.23	440	1.35
18	Guernsey	72	72	76	144	0.26	0.52	4562	1.67	659	2.09	526	1.61
19	Jersey	65	65	69	341	0.63	0.38	4516	1.65	438	1.39	517	1.58
20	Turkey	68	68	68	135	0.25	0.14	4472	1.63	354	1.12	425	1.30
21	Canada	55	55	56	1646	3.02	1.75	4392	1.60	426	1.35	511	1.57
22	Malaysia	72	72	74	122	0.22	0.07	4188	1.53	335	1.06	487	1.49
23	South Korea	59	59	62	626	1.15	0.36	4173	1.52	314	1.00	464	1.42
24	Russia	64	64	63	197	0.36	0.26	3663	1.34	361	1.15	402	1.23
25	Australia	51	51	54	1064	1.95	0.61	3480	1.27	244	0.78	360	1.10
26	United Kingdom	42	42	45	4457	8.18	17.37	3425	1.25	424	1.34	330	1.01
27	Indonesia	61	61	62	218	0.40	0.05	3383	1.24	189	0.60	368	1.13
28	Bahamas	85	85	84	27	0.05	0.04	3323	1.21	429	1.36	478	1.46
29	Panama	77	77	75	53	0.10	0.27	3321	1.21	626	1.99	444	1.36
30	Curacao	75	75	76	111	0.20	0.00	3221	1.18	106	0.34	531	1.62
31	Italy	49	49	52	1361	2.50	0.92	3140	1.15	254	0.81	354	1.08
32	Mexico	54	54	57	377	0.69	0.03	2833	1.03	108	0.34	306	0.94
33	Spain	48	47	50	1135	2.08	0.77	2828	1.03	214	0.68	299	0.91

Table A.32: Secrecy scores, their adjusted versions, aggregate results of the BFSI, and their comparison with FSI

	Country	Secrecy score 2020	Secrecy score 2020∗	Secrecy score 2020†	Total inward assets, USD billion, 2018	Total BSW (%)	Global scale weight 2020 (2018 data, %)	Summed BFSI 2020	Contribution (summed BFSI 2020, %)	FSI 2020	Contribution (FSI 2020, %)	FSI 2020 using CPIS data	Contribution (FSI 2020 using CPIS data, %)
34	Austria	56	56	57	340	0.62	0.56	2760	1.01	310	0.98	322	0.98
35	Mauritius	72	72	75	45	0.08	0.02	2608	0.95	223	0.71	356	1.09
36	Chile	62	62	61	87	0.16	0.04	2569	0.94	169	0.54	273	0.84
37	India	52	52	55	594	1.09	1.16	2569	0.94	317	1.00	310	0.95
38	Denmark	53	52	54	416	0.76	0.15	2530	0.92	166	0.53	285	0.87
39	Philippines	65	65	66	75	0.14	0.09	2508	0.92	270	0.86	311	0.95
40	Poland	57	57	60	139	0.26	0.15	2487	0.91	215	0.68	258	0.79
41	Norway	52	52	54	329	0.60	0.55	2372	0.87	243	0.77	250	0.77
42	Israel	63	63	63	100	0.18	0.19	2347	0.86	314	0.99	310	0.95
43	Finland	53	53	55	315	0.58	0.09	2343	0.86	142	0.45	263	0.80
44	South Africa	56	56	59	237	0.44	0.18	2281	0.83	216	0.69	288	0.88
45	Romania	66	66	68	28	0.05	0.06	2147	0.78	232	0.74	225	0.69
46	Saudi Arabia	70	70	70	21	0.04	0.05	2146	0.78	279	0.88	250	0.77
47	Sweden	45	45	48	741	1.36	1.01	2137	0.78	204	0.65	224	0.69
48	Bahrain	78	78	78	11	0.02	0.11	2125	0.78	491	1.56	273	0.84
49	Brazil	49	49	50	428	0.79	0.16	2094	0.76	138	0.44	234	0.72
50	Portugal	55	55	58	141	0.26	0.08	1962	0.72	152	0.48	224	0.69

Table A.32: Secrecy scores, their adjusted versions, aggregate results of the BFSI, and their comparison with FSI

	Country	Secrecy score 2020	Secrecy score 2020∗	Secrecy score 2020†	Total inward assets, USD billion, 2018	Total BSW (%)	Global scale weight 2020 (2018 data, %)	Summed BFSI 2020	Contribution (summed BFSI 2020, %)	FSI 2020	Contribution (FSI 2020, %)	FSI 2020 using CPIS data	Contribution (FSI 2020 using CPIS data, %)
51	Kenya	80	80	79	6	0.01	0.04	1878	0.69	378	1.20	250	0.77
52	Liberia	80	80	79	20	0.04	0.02	1784	0.65	277	0.88	360	1.10
53	Ukraine	69	69	68	20	0.04	0.04	1667	0.61	246	0.78	237	0.73
54	New Zealand	56	56	58	76	0.14	0.10	1639	0.60	179	0.57	198	0.61
55	Paraguay	84	84	84	3	0.01	0.00	1626	0.59	159	0.50	238	0.73
56	Venezuela	69	69	67	10	0.02	0.00	1593	0.58	105	0.33	183	0.56
57	Belgium	44	44	46	600	1.10	1.56	1589	0.58	213	0.68	190	0.58
58	Dominican Republic	72	72	70	12	0.02	0.01	1560	0.57	147	0.47	223	0.68
59	Greece	58	58	61	44	0.08	0.02	1524	0.56	119	0.38	181	0.55
60	Marshall Islands	73	73	73	19	0.03	0.04	1510	0.55	275	0.87	271	0.83
61	Liechtenstein	78	78	82	8	0.01	0.01	1472	0.54	241	0.76	250	0.76
62	Cyprus	61	61	63	20	0.04	0.55	1440	0.53	404	1.28	166	0.51
63	Hungary	55	55	58	49	0.09	0.05	1437	0.52	133	0.42	158	0.48
64	Lebanon	72	72	72	10	0.02	0.51	1421	0.52	644	2.04	214	0.66
65	Isle of Man	64	64	67	15	0.03	0.09	1347	0.49	249	0.79	168	0.52
66	Czechia	53	53	56	57	0.11	0.09	1317	0.48	145	0.46	151	0.46

Table A.32: Secrecy scores, their adjusted versions, aggregate results of the BFSI, and their comparison with FSI

	Country	Secrecy score 2020	Secrecy score 2020∗	Secrecy score 2020†	Total inward assets, USD billion, 2018	Total BSW (%)	Global scale weight 2020 (2018 data, %)	Summed BFSI 2020	Contribution (summed BFSI 2020, %)	FSI 2020	Contribution (FSI 2020, %)	FSI 2020 using CPIS data	Contribution (FSI 2020 using CPIS data, %)
67	Malta	61	61	64	12	0.02	0.71	1238	0.45	426	1.35	136	0.42
68	Costa Rica	69	69	70	7	0.01	0.01	1205	0.44	169	0.54	160	0.49
69	Croatia	59	59	62	12	0.02	0.02	1106	0.40	119	0.38	124	0.38
70	Iceland	60	60	63	13	0.02	0.03	1073	0.39	140	0.44	132	0.41
71	Uruguay	61	61	62	13	0.02	0.03	1034	0.38	148	0.47	140	0.43
72	Slovakia	55	54	56	33	0.06	0.05	1015	0.37	128	0.41	140	0.43
73	Guatemala	73	73	72	2	0.00	0.00	948	0.35	124	0.39	134	0.41
74	Ghana	62	62	64	10	0.02	0.00	903	0.33	69	0.22	133	0.41
75	Macao	68	68	67	5	0.01	0.24	901	0.33	425	1.35	139	0.43
76	Barbados	74	74	75	5	0.01	0.02	804	0.29	231	0.73	183	0.56
77	Puerto Rico	77	77	76	1	0.00	0.00	804	0.29	151	0.48	137	0.42
78	Bolivia	80	80	79	1	0.00	0.00	743	0.27	95	0.30	130	0.40
79	Latvia	57	57	60	6	0.01	0.11	730	0.27	196	0.62	93	0.28
80	US Virgin Islands	73	73	72	2	0.00	0.00	687	0.25	102	0.32	119	0.36
81	Gibraltar	71	71	74	2	0.00	0.00	668	0.24	107	0.34	116	0.35
82	Bulgaria	54	54	57	6	0.01	0.02	658	0.24	91	0.29	77	0.24
83	Trinidad and Tobago	65	65	63	2	0.00	0.00	610	0.22	28	0.09	92	0.28

Table A.32: Secrecy scores, their adjusted versions, aggregate results of the BFSI, and their comparison with FSI

	Country	Secrecy score 2020	Secrecy score 2020∗	Secrecy score 2020†	Total inward assets, USD billion, 2018	Total BSW (%)	Global scale weight 2020 (2018 data, %)	Summed BFSI 2020	Contribution (summed BFSI 2020, %)	FSI 2020	Contribution (FSI 2020, %)	FSI 2020 using CPIS data	Contribution (FSI 2020 using CPIS data, %)
84	Tanzania	73	73	72	1	0.00	0.00	572	0.21	129	0.41	103	0.32
85	Lithuania	47	47	49	13	0.02	0.02	497	0.18	59	0.19	63	0.19
86	Seychelles	75	75	78	1	0.00	0.00	456	0.17	125	0.40	97	0.30
87	Aruba	76	76	76	1	0.00	0.00	446	0.16	148	0.47	109	0.33
88	Slovenia	42	42	44	22	0.04	0.01	429	0.16	35	0.11	54	0.16
89	Belize	75	75	76	0	0.00	0.00	394	0.14	86	0.27	67	0.20
90	Estonia	51	51	53	3	0.01	0.02	385	0.14	79	0.25	51	0.16
91	Montenegro	63	63	61	1	0.00	0.00	384	0.14	53	0.17	54	0.17
92	Monaco	78	78	80	0	0.00	0.00	330	0.12	83	0.26	81	0.25
93	Antigua and Barbuda	87	87	88	0	0.00	0.00	320	0.12	55	0.17	98	0.30
94	Botswana	69	69	67	0	0.00	0.00	296	0.11	39	0.13	56	0.17
95	Anguilla	78	78	80	2	0.00	0.01	280	0.10	195	0.62	160	0.49
96	Maldives	81	81	80	0	0.00	0.00	241	0.09	75	0.24	84	0.26
97	Grenada	77	77	77	0	0.00	0.00	174	0.06	45	0.14	52	0.16
98	Turks and Caicos Islands	77	77	79	0	0.00	0.00	144	0.05	98	0.31	55	0.17
99	Brunei	84	84	83	0	0.00	0.00	131	0.05	86	0.27	64	0.20

Table A.32: Secrecy scores, their adjusted versions, aggregate results of the BFSI, and their comparison with FSI

	Country	Secrecy score 2020	Secrecy score 2020∗	Secrecy score 2020†	Total inward assets, USD billion, 2018	Total BSW (%)	Global scale weight 2020 (2018 data, %)	Summed BFSI 2020	Contribution (summed BFSI 2020, %)	FSI 2020	Contribution (FSI 2020, %)	FSI 2020 using CPIS data	Contribution (FSI 2020 using CPIS data, %)
100	Andorra	66	66	66	0	0.00	0.00	122	0.04	35	0.11	48	0.15
101	Samoa	78	78	79	0	0.00	0.00	102	0.04	116	0.37	41	0.13
102	St. Vincent and the Grenadines	70	70	71	0	0.00	0.00	94	0.03	21	0.07	42	0.13
103	St. Lucia	78	78	79	0	0.00	0.00	79	0.03	22	0.07	51	0.16
104	Dominica	77	77	76	0	0.00	0.00	69	0.03	62	0.20	39	0.12
105	Gambia	77	77	75	0	0.00	0.00	48	0.02	35	0.11	38	0.12
106	Cook Islands	75	75	75	0	0.00	0.00	37	0.01	45	0.14	17	0.05
107	San Marino	64	64	67	0	0.00	0.00	25	0.01	24	0.08	24	0.07
108	Montserrat	78	78	80	0	0.00	0.00	20	0.01	17	0.05	14	0.04
109	Nauru	67	67	66	0	0.00	0.00	18	0.01	26	0.08	18	0.06
110	St. Kitts and Nevis	77	77	77	0	0.00	0.00	9	0.00	153	0.48	8	0.02

Source: Authors.

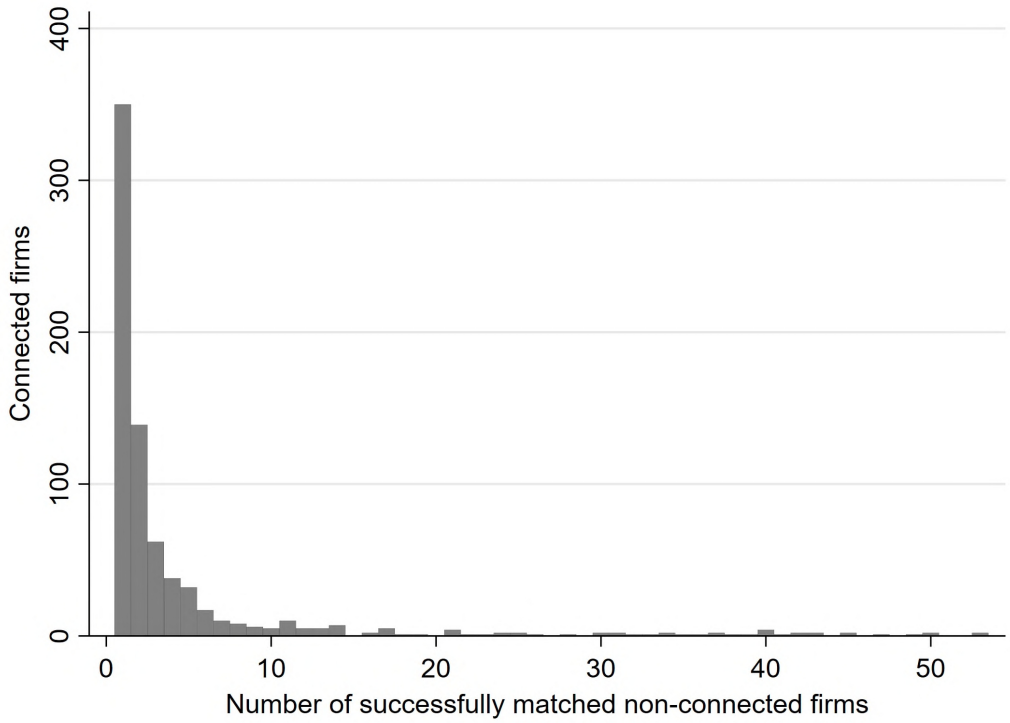
Notes: ∗ These secrecy scores are adjusted for intra-European relationships (see Section 4.3.1).

† *These secrecy scores are adjusted for excluding KFSI 18 on automatic information exchange (see Section 4.3.3).*

Appendix B

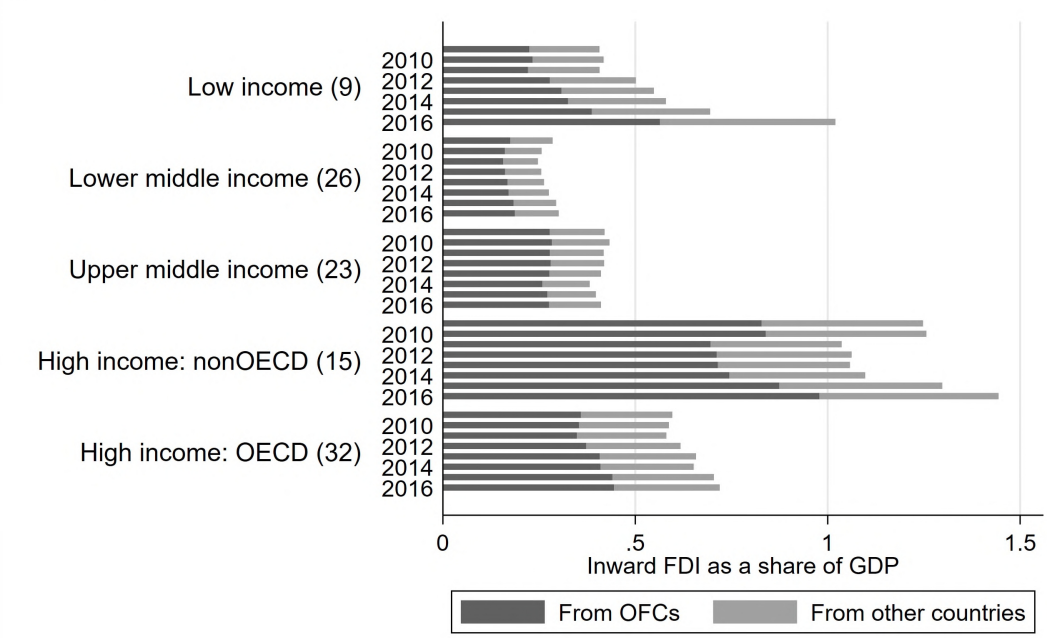
Figures

Figure B.1: Histogram of the number of successful matches of non-connected firms to connected firms.



Source: Author.

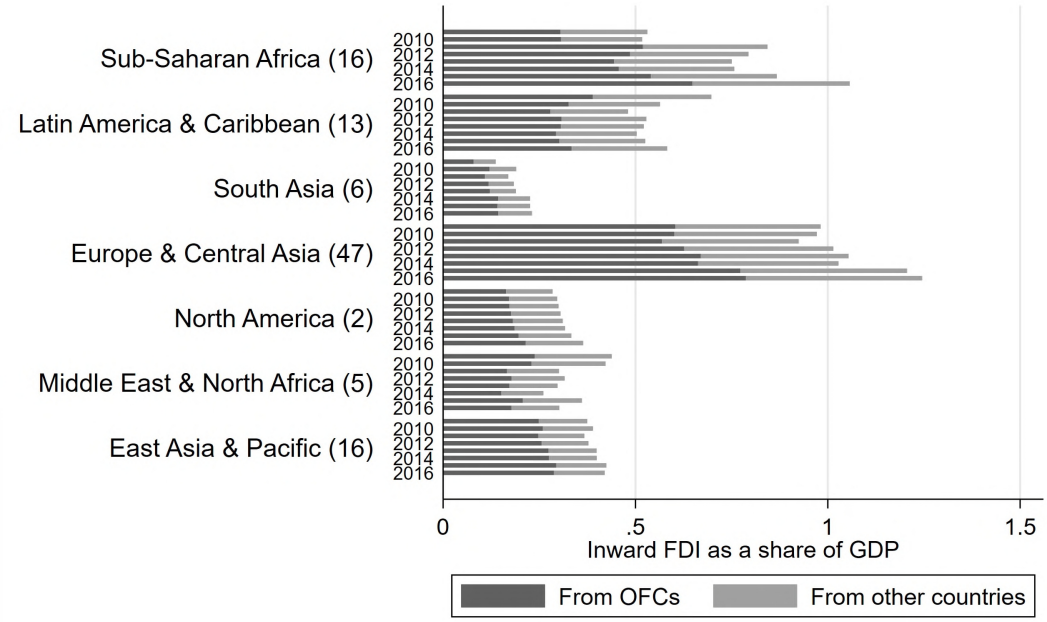
Figure B.2: Development of the volume of total inward FDI stock between 2009 and 2016 (as a share of GDP; by income group and origin)



Source: Data from IMF's CDIS; classification by the World Bank; authors' construction.

Note: The classification of 'offshore financial centres' is defined in Section 3.4. The number of countries in each income group in the data for the year 2016 is included in parentheses.

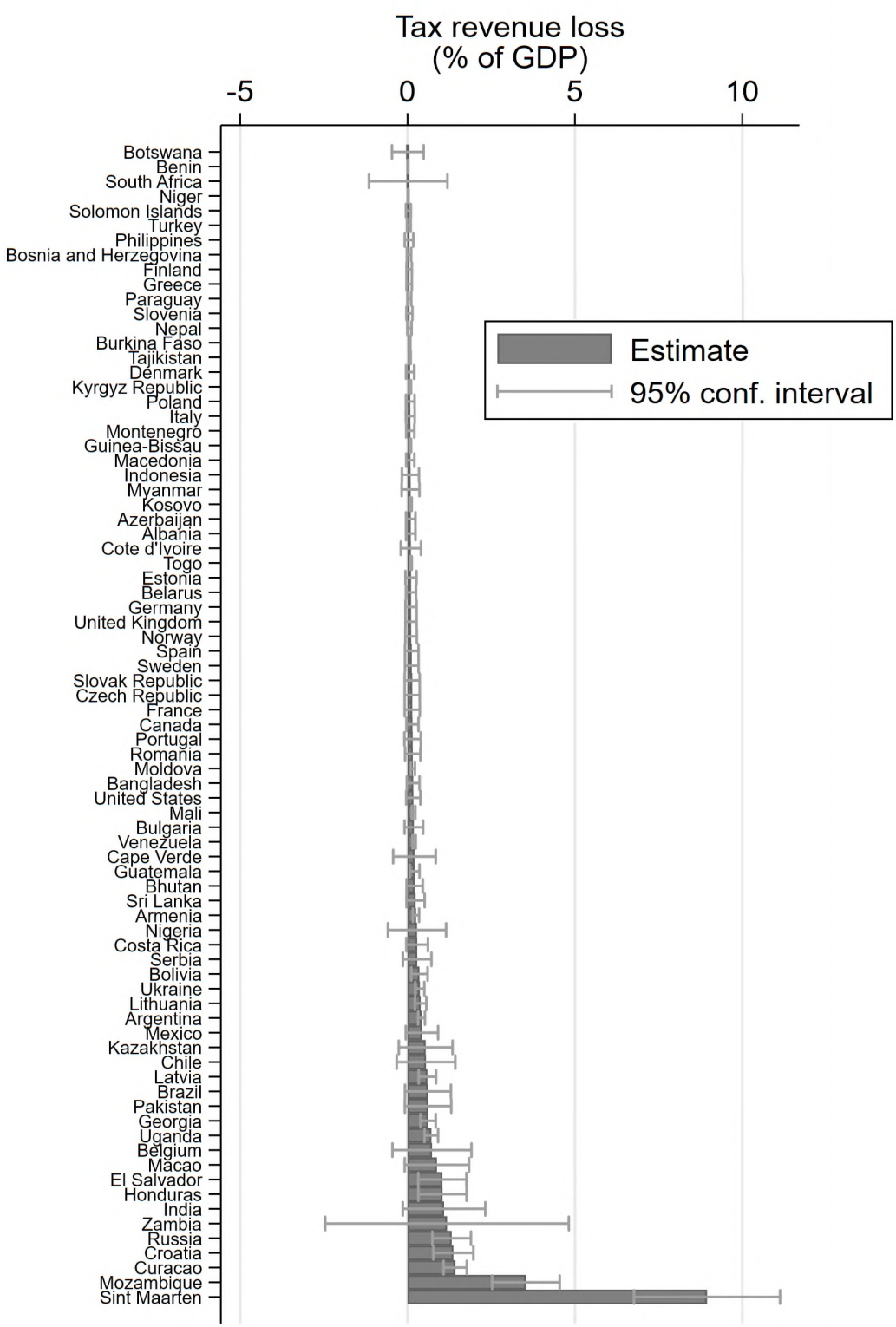
Figure B.3: Development of the volume of total FDI stock between 2009 and 2016 (by region and origin)



Source: Data from IMF's CDIS; classification by the World Bank; authors' construction.

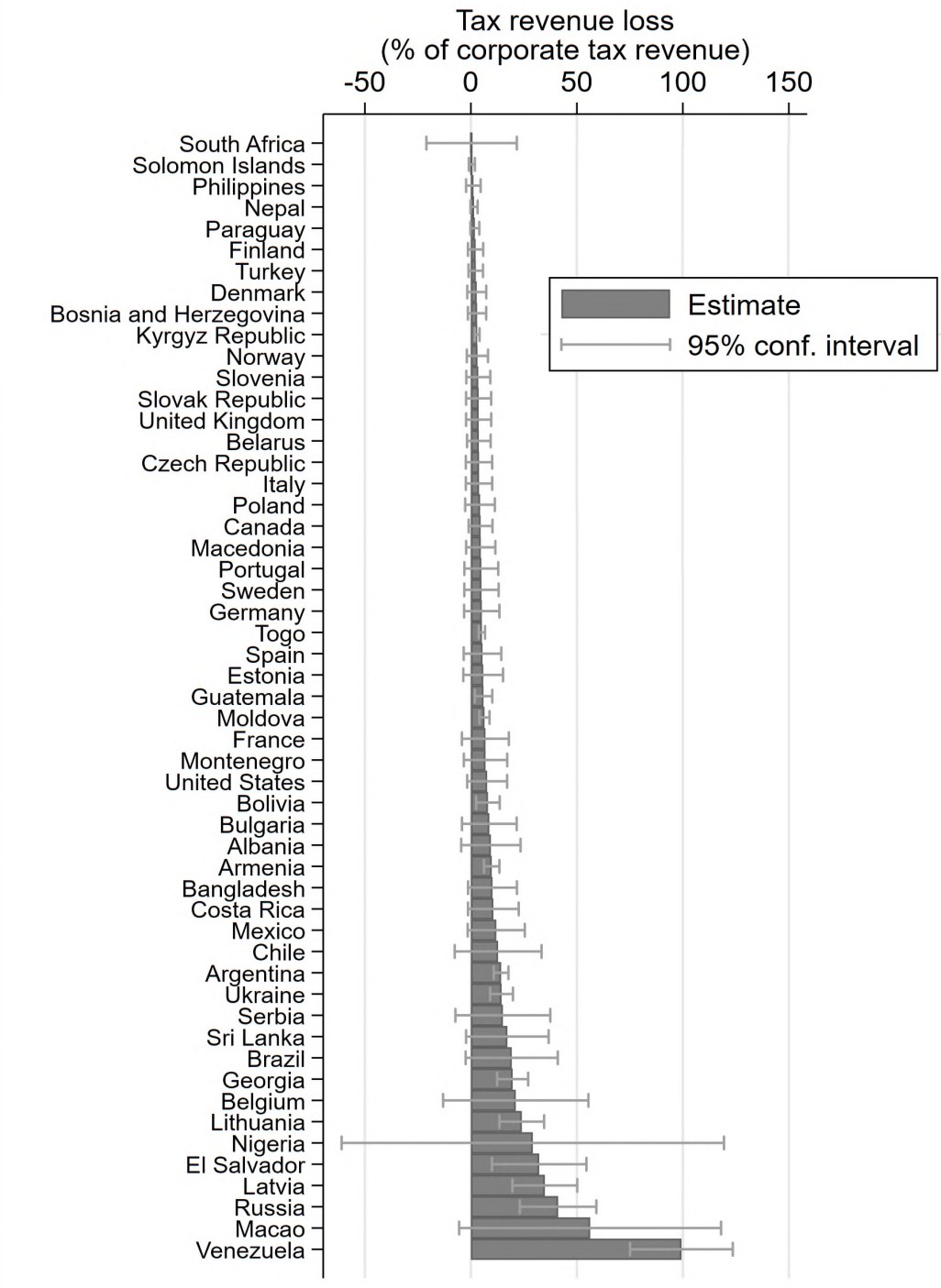
Note: The classification of 'offshore financial centres' is defined in Section 3.4. The number of countries in each regional group in the data for the year 2016 is included in parentheses.

Figure B.4: Estimated tax revenue loss as a share of GDP, 2016



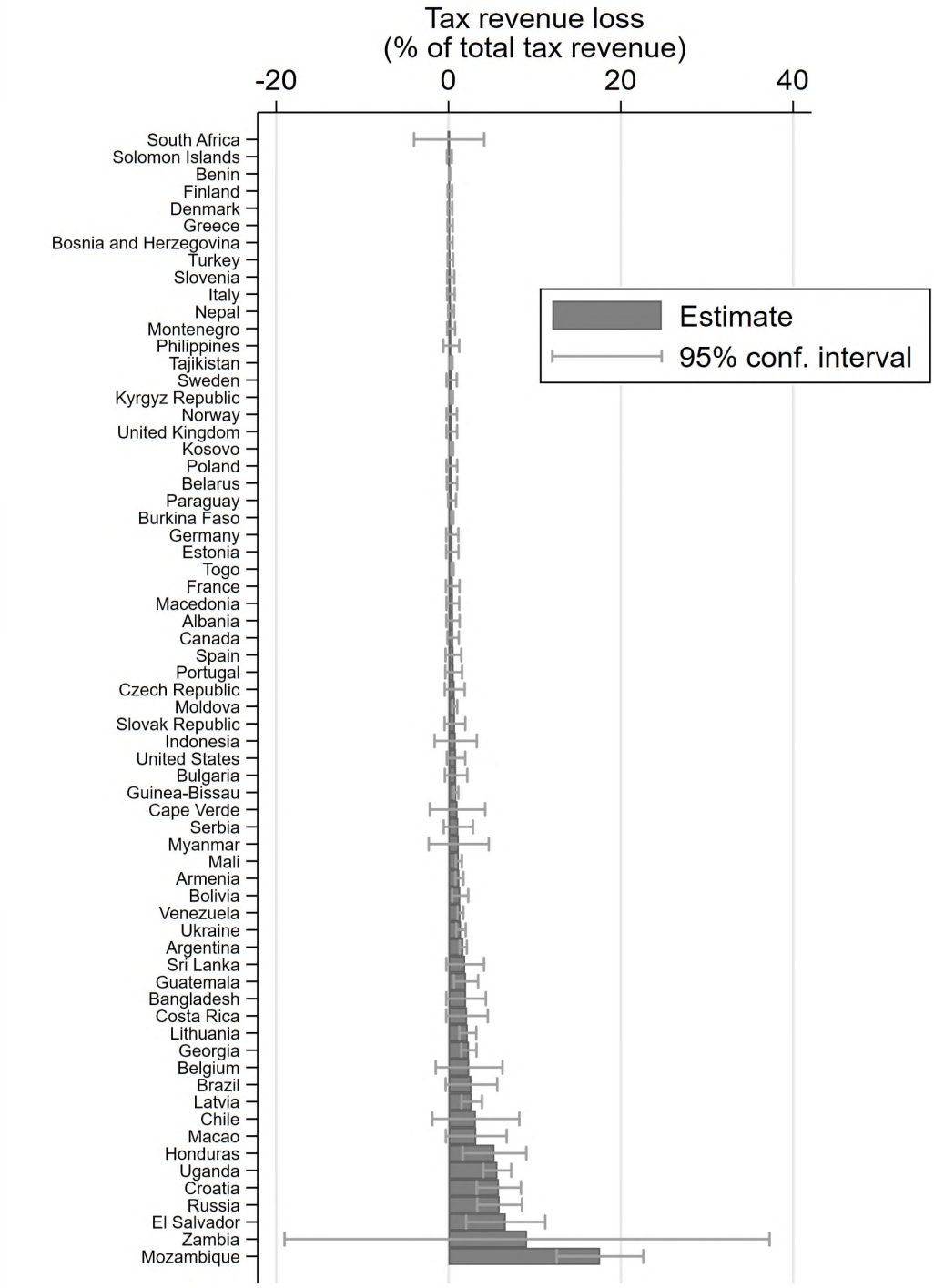
Source: Authors, data on GDP from the World Bank, the United Nations, and the CIA World Factbook.

Figure B.5: Share of estimated tax revenue losses on corporate tax revenue, 2016



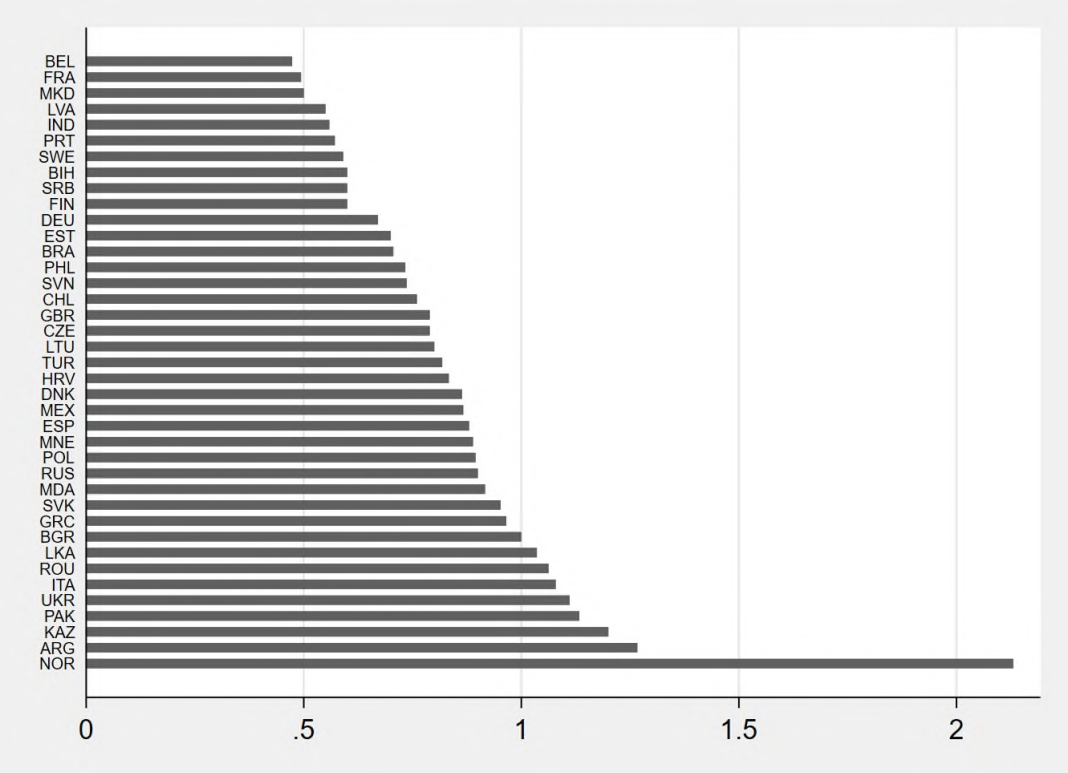
Source: Authors, data on corporate tax revenue from the Government Revenue Dataset.

Figure B.6: Share of estimated tax revenue losses on total tax revenue, 2016



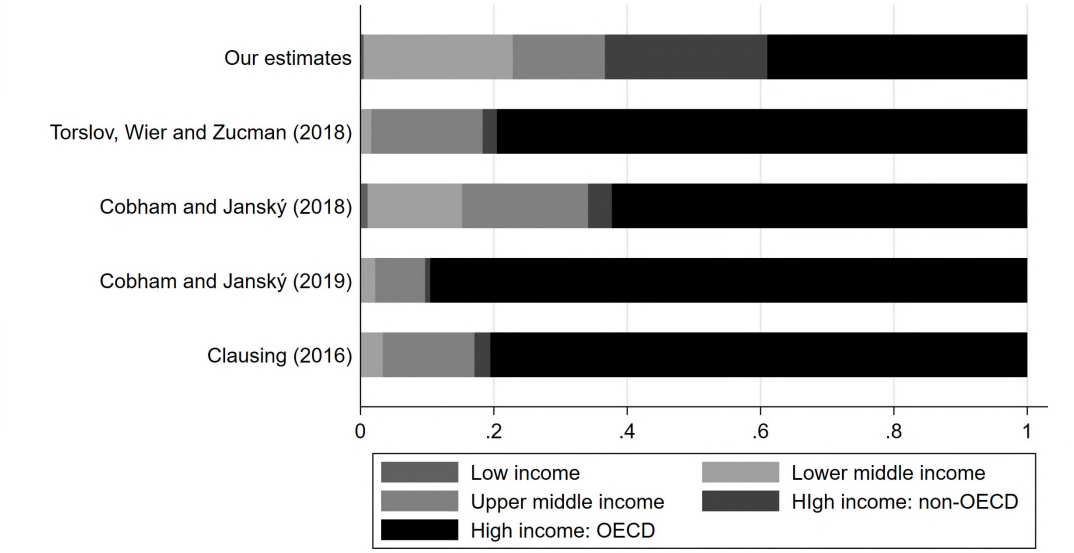
Source: Authors, data on tax revenue from the Government Revenue Dataset.

Figure B.7: Ratio of estimated tax revenue losses using effective and nominal corporate income tax rates.



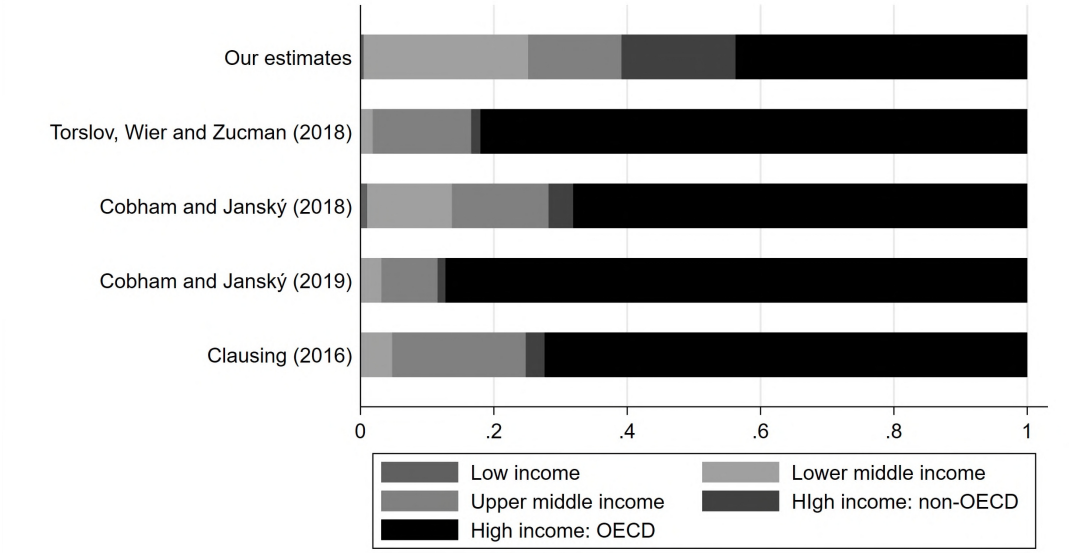
Source: Authors.

Figure B.8: Share of shifted profits on total global shifted profits, by income group, 2016



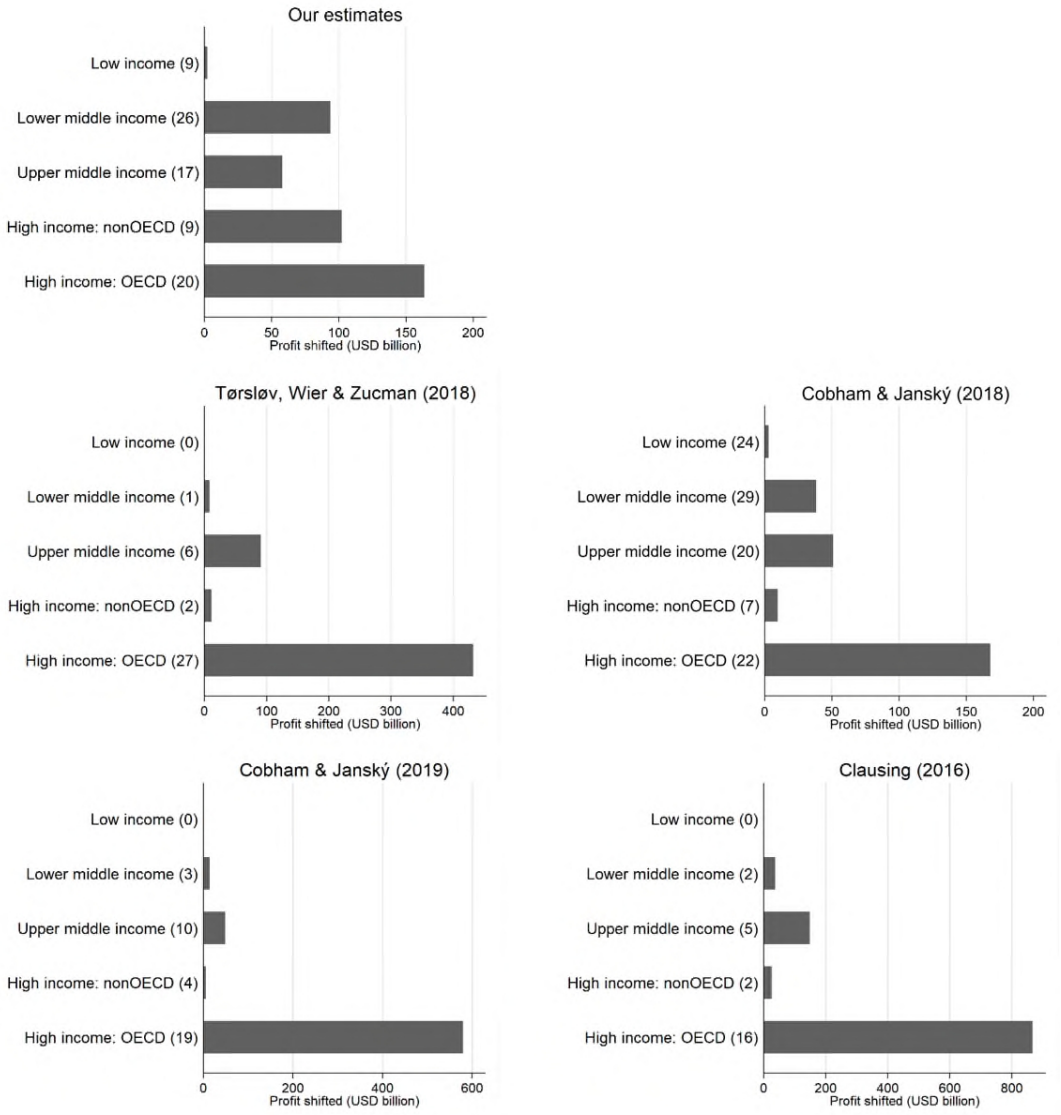
Source: Authors, data from Tørsløv et al. (2018), Cobham and Janský (2018), Cobham and Janský (2019) and Clausing (2016).

Figure B.9: Share of estimated tax revenue losses on total global estimated revenue losses, by income group, 2016



Source: Authors, data from Tørsløv et al. (2018), Cobham and Janský (2018), Cobham and Janský (2019) and Clausing (2016).

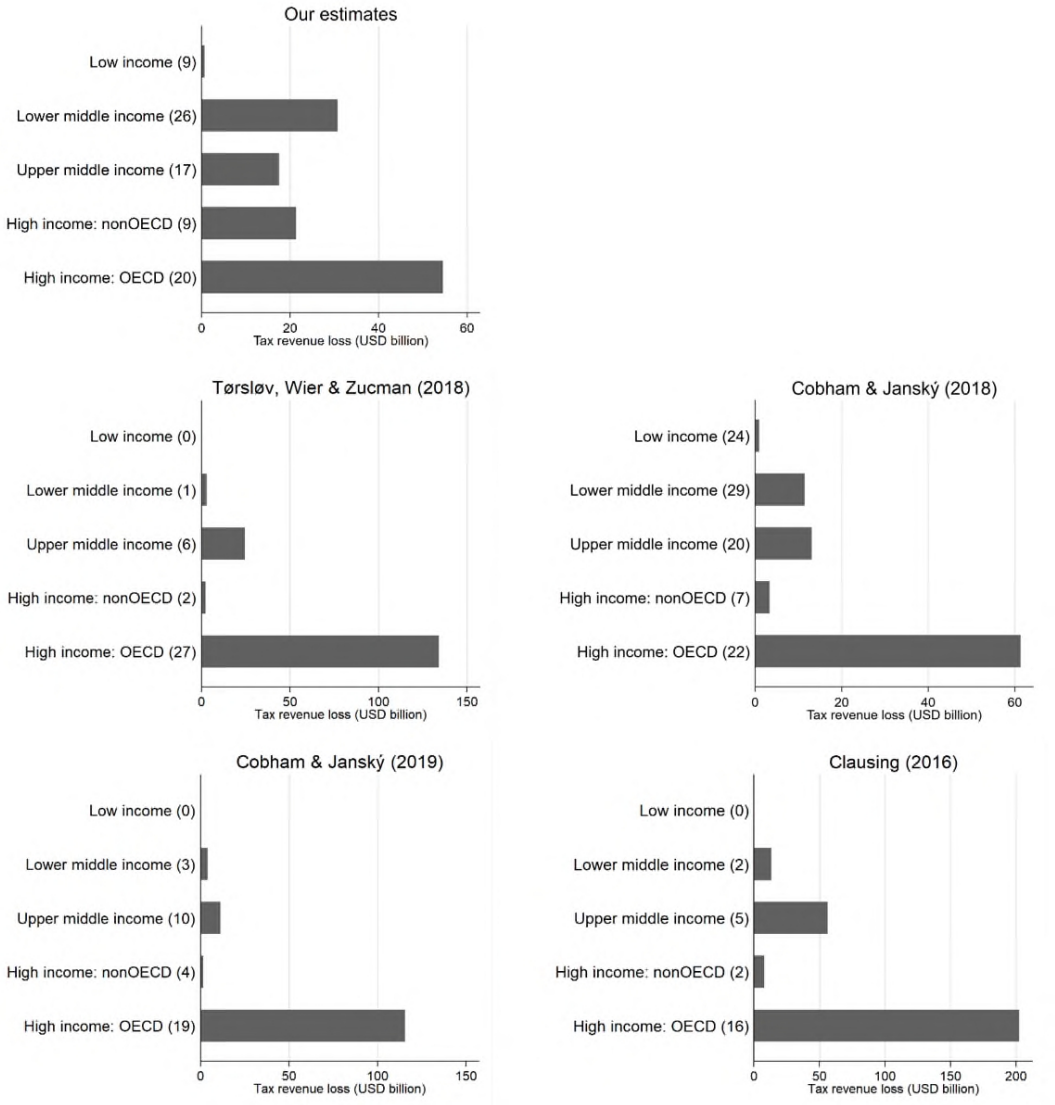
Figure B.10: Total estimated profit shifted out of countries by income group, 2016



Source: Authors, data from Tørsløv et al. (2018), Cobham and Janský (2018), Cobham and Janský (2019) and Clausing (2016).

Note: The number of countries in each income group is included in parentheses.

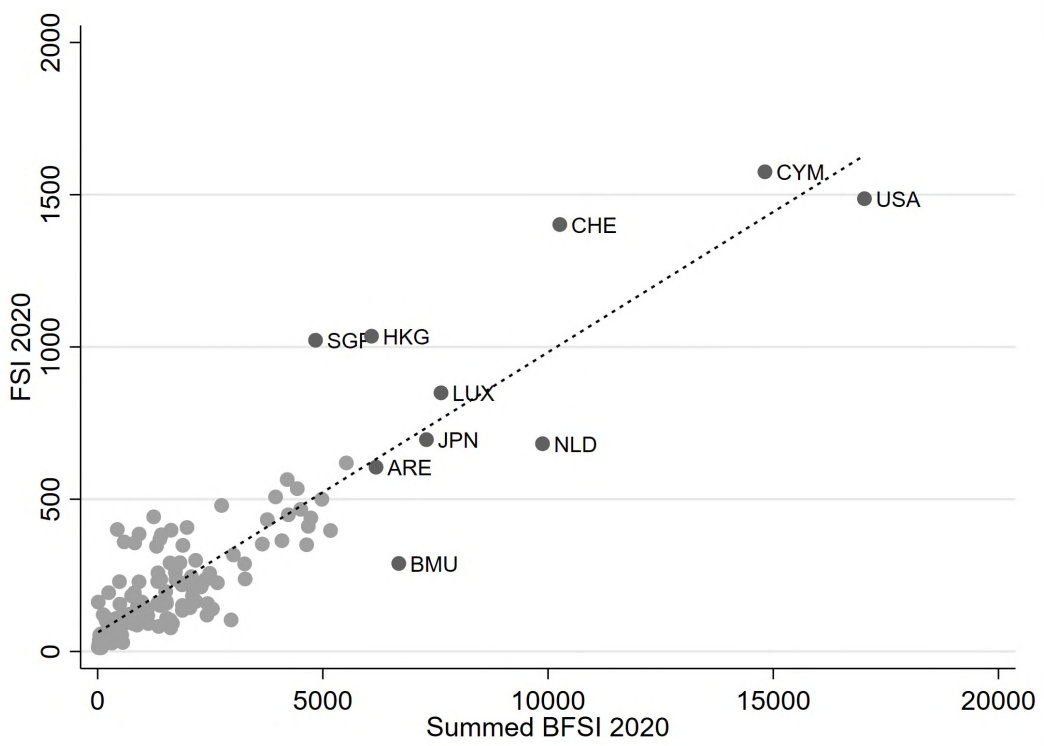
Figure B.11: Total estimated tax revenue losses by income group, 2016



Source: Authors, data from Tørsløv et al. (2018), Cobham and Janský (2018), Cobham and Janský (2019) and Clausing (2016).

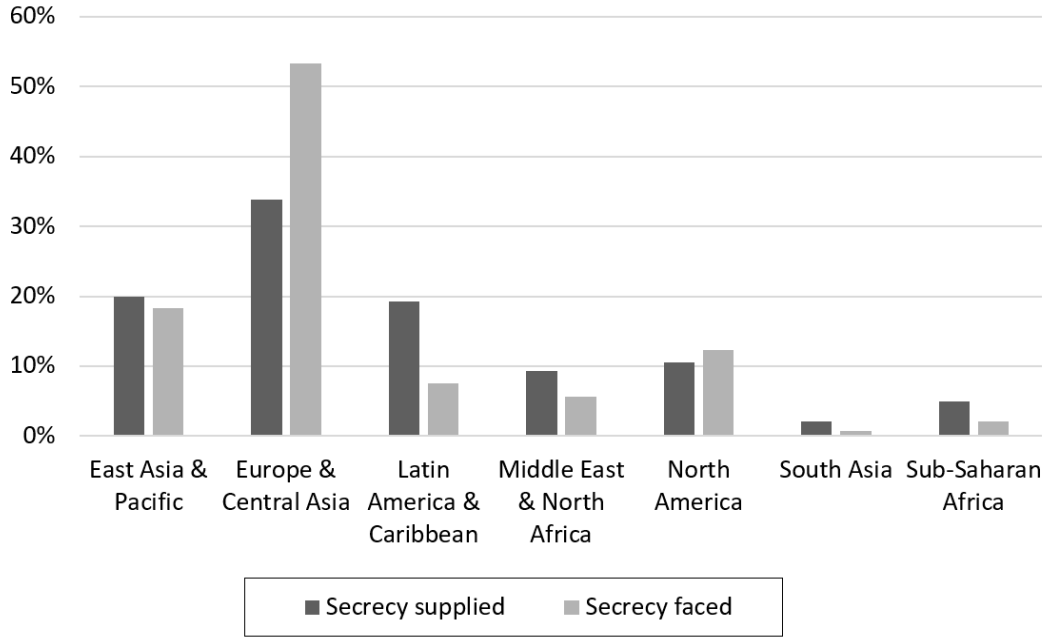
Note: The number of countries in each income group is included in parentheses.

Figure B.12: Comparison of summed BFSI 2020 and the original FSI 2020



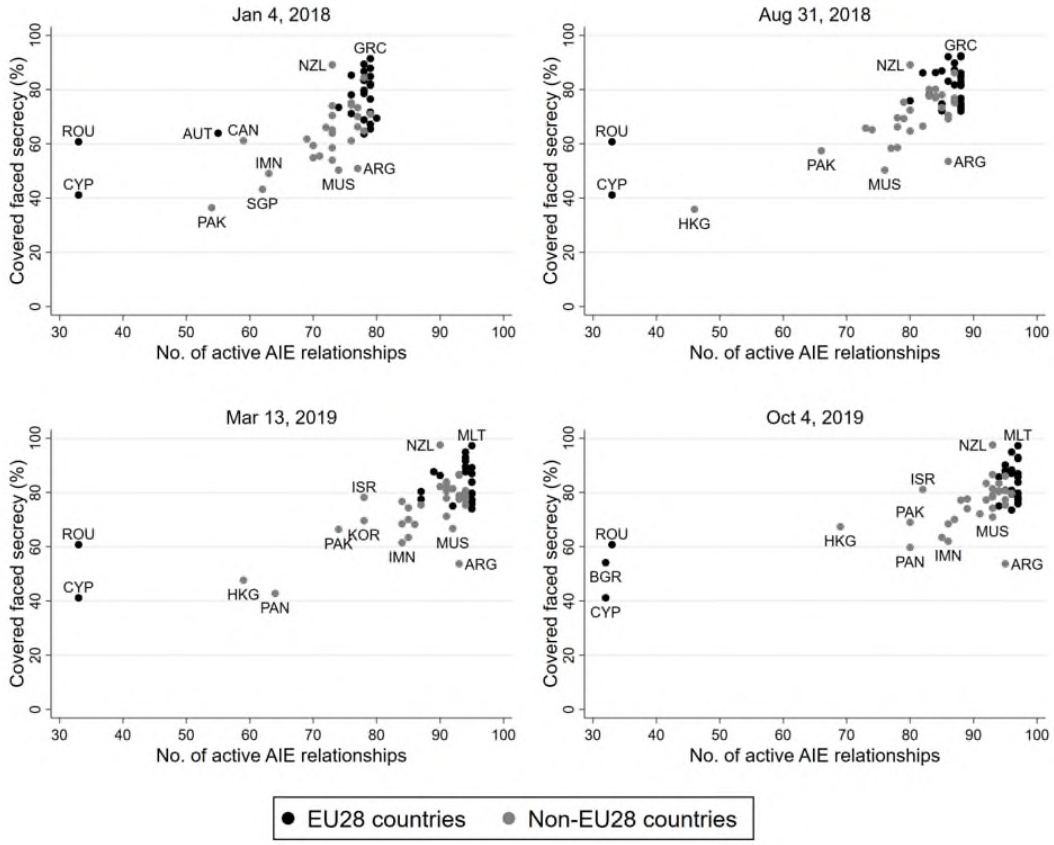
Source: Authors.

Figure B.13: Shares of global secrecy supplied and received by regional groups



Source: Authors.

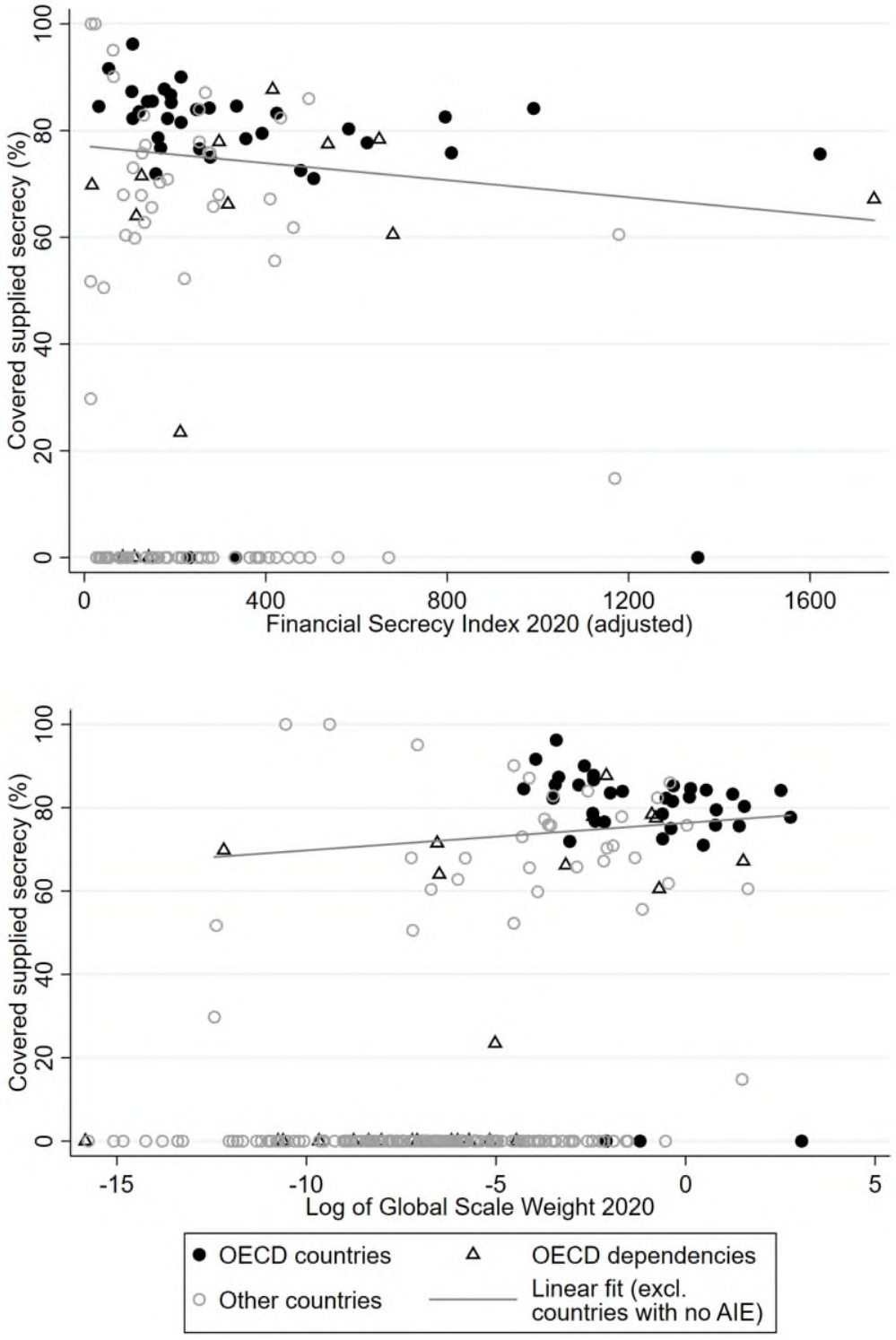
Figure B.14: Share of received secrecy covered by currently activated automatic information exchange relationships vs. the number of activated automatic information exchange relationships



Source: Authors.

Notes: In this figure we only show countries for which we have BFSI scores for at least 10 counterpart countries.

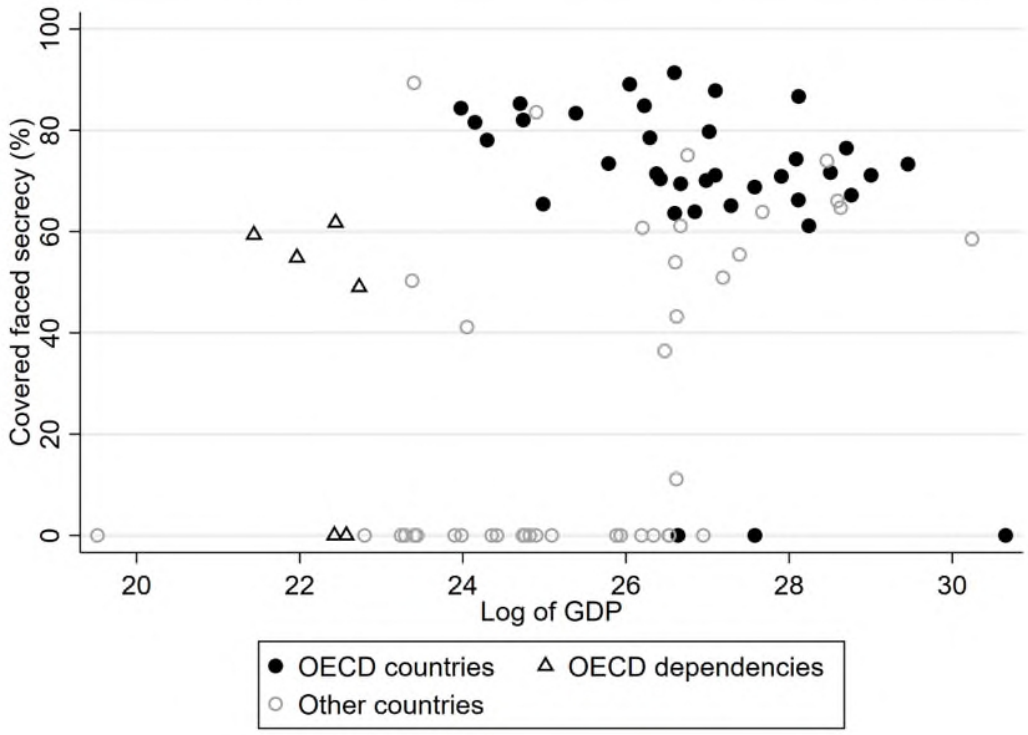
Figure B.15: Relationship between the share of supplied secrecy covered by automatic information exchange treaties and the Financial Secrecy Index 2020 (top) and its Global Scale Weights (bottom)



Source: Authors.

Notes: FSI values are constructed using Secrecy Scores adjusted for intra-EU relationships and by excluding Key Financial Secrecy Indicator 18 on automatic information exchange (see Section 4.3).

Figure B.16: Relationship between GDP and the share of faced secrecy covered by automatic information exchange



Source: Authors.

Appendix C

Responses to reviewers' comments

C.1 Nadine Riedel

Mr Palansky's thesis tackles questions related to corruption, tax abuse and financial secrecy. It comprises four chapters. Chapter 1 provides motivation and background for the topic and gives an overview of the analyses to come. The studies presented in Chapter 2-4 are original contributions to the economic literature.

C.1.1 Chapter 2: Value of Political Connections

In Chapter 2, "Value of Political Connections", the author empirically analyses the financial return that Czech firms derive from donations to political parties. The analysis is based on a comprehensive, partly hand-collected, data set spanning the years 1995-2014. The data contains donations from the universe of Czech firms as well as financial performance measures and some firm characteristics (including sector, assets and leverage).

To overcome endogeneity concerns, the author employs a "dynamic matching procedure" where in each period donating firms are matched with observationally similar firms based inter alia on industry classification and prior profitability. The analysis reveals that donating firms earn more than 1 percentage point higher returns on equity and assets corresponding to a 7.8% and 11.5% difference.

The chapter is well written and among the first papers to present evidence on the value of political donations in a transition economy. There is some scope for improvement, however. With regard to empirical methodology, I found the "dynamic matching approach", where donating firms are matched

to a possibly different control firm in every period, rather ad hoc and would appreciate if the author could provide references to the established literature. Moreover, the stable unit treatment value assumption (SUTVA) is likely to be violated in this context as e.g. a change of regulations that benefits donors might simultaneously hurt or benefit non-donors (SUTVA is also violated in the context of the allocation of procurement contracts, for example). Moreover, while matching allows leveling out observed differences across firms, unobserved differences may remain and may bias the effect of interest. It might hence be worthwhile to think about more credible research designs that tackle these concerns: one might, for example, match firms that donated to the winner of subsequent elections with firms that donated to losers (possibly restricting attention to close elections).

Author: Thank you for these comments. I have incorporated them into the revised version in the following ways. First, I have extended the comparison of the approach I take with similar studies, explaining in more detail how and why it differs, in light of the sample of data that I use. In particular, I newly link the approach to the seminal contribution by Rosenbaum and Rubin (1983). This discussion is in Section 2.3.1.

Second, regarding the SUTVA assumption, I agree that this is a potential issue and it was also brought forth by a reviewer at *Public Choice*. In response to this concern, I included a series of tests for differences in the performance of connected and non-connected firms which cluster the sample at the level of networks around individual political parties. I discuss these tests in the penultimate paragraph of Section 2.3.1. While I do agree that the current analysis of these effects is limited and does not allow to answer this concern definitively, I believe that moving significantly towards that goal would require a different empirical approach.

Third, and related to that, the avenue of research that you mention (i.e. focusing on close elections) is what I believe to be very promising. One step that I do take in the paper hints, however, at the (rather low) usability of Czech data for this purpose, and is described in Section 2.3.3: "I construct a family of models that assess the importance of connections to political parties which are in power as compared to connections to other parties.". The results do not suggest strong effects of being connected to the party in power at the national level, but they do broadly confirm these effects to be present at the regional level, which has been used in this context before in

the literature by Titl and Geys (2019) and by myself in my Bacheler Thesis (Palanský 2014). By far the largest variation to study these effect can be found at the municipal level, at which more than 6 thousand councils are elected every four years. I am currently supervising a thesis at our Institute that uses this data in combination with data on political connections, and I hope to exploit this opportunity in more detail in the future.

Some of the results also warrant a more thorough discussion. I was surprised, for example, that the effect vanishes in the subsample of firms that work closely with the public sector. On top of that, the data permits additional, and in my view interesting, heterogeneity analysis: for example, does the size of the treatment effect depend on the size of the coalition government (i.e. the number of parties in the ruling coalition) or the identity of the dominant party? How did the treatment effect evolve over time (this might be particularly interesting in a transition economy)?

Author: In response to this comment, I have extended the discussion of the results in Section 2.5. In particular, I added a new separate paragraph just before Table 2.6 which focuses on the possible eplanations of the vanishing effect for public firms.

Regarding the development of the treatment effect over time, I newly included an additional sample split (into three time periods) for the headline results from Table 2.4—they are reported in Table A.2 in the Appendix and described in a new paragraph in between Table 2.4 and Figure 2.2. In short, I find that the effect was strongest in the first time period, but has been observable to some extent throughout the whole studied time period.

Regarding the analysis of coalitions, unfortunately I believe that the variation is too small in the Czech case to enable a test of this effect: there have only been 4 different coalitions over the studied period, with 3 of them led by the strongest Civic Democratic Party (together with smaller parties which generally do not receive as many donations).

C.1.2 Chapter 3: International Corporate Profit Shifting

Chapter 3 of the thesis, "International Corporate Profit Shifting", contributes to a flourishing literature that aims to quantify tax-motivated profit shifting by multinational firms to low-tax countries. There are broadly speaking two

strands of literature which assess this question – one using micro-level data, the other using macro-level data. Both strands have strength and weaknesses. Micro data allows for research designs with high internal validity, that allow quantifying the size of shifting activity from plausibly causal relationships. Data quality is an issue with many of these studies, however, and if high-quality micro-data is available, it is often limited to individual countries (although, by now, several of such studies exist). Macro data, in turn, is available for many countries, including less developed and tax haven economies that are poorly represented in many micro datasets. Empirical identification is, in turn, more challenging and studies tend to rely on stronger identification assumptions. Micro and macro data analyses, moreover, differ in terms of the estimated size of shifting effects, with macro studies commonly yielding much larger estimates.

Mr Palansky uses macro data to quantify shifting activities. His analysis relies on country level data on foreign direct investments (FDI) and FDI income. Profit shifting volumes are quantified by estimating the effect of the share of inward FDI from haven economies on FDI's reported rate of return. From this, the author obtains estimates for aggregate shifting volumes and related revenue losses. A particular focus is on testing for heterogeneity of revenue losses across countries. In line with prior studies, estimated shifting volumes are large, where less developed countries are identified to be particularly vulnerable.

The chapter is well written and the analysis is competently conducted. There is a strong interest of policymakers and academics in understanding the size and structure of profit shifting activities and the paper offers insights in this regard. Assessing potential heterogeneity of shifting activities across countries – one particular aim of the paper - is also clearly of relevance. Again, there is some room for improvement, however.

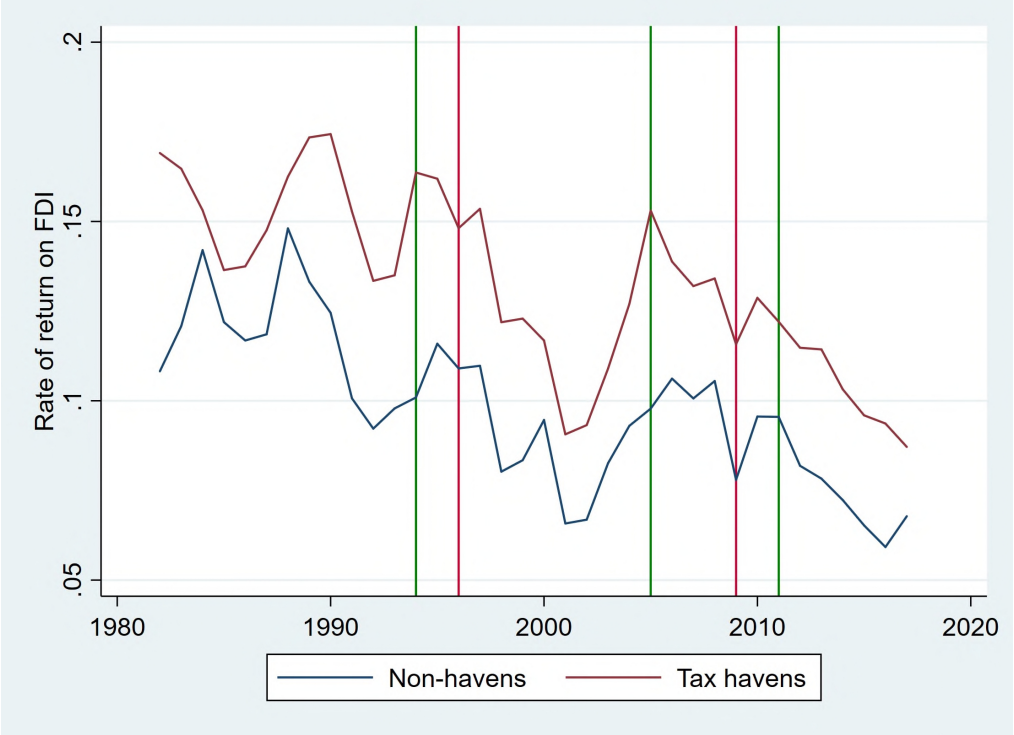
One main concern is empirical identification. The estimates rely on cross-sectional comparisons of inward FDI from haven economies and reported rates of return. There are multiple reasons why rates of returns may differ across countries, including differences in industry structure, human capital, public institutions, governance structure, health environment. If these factors correlate with tax haven exposure, the authors' estimates may be biased. It is impossible to assess the direction or importance of these biases, making the estimates hard to interpret. Even the inclusion of country fixed effects would not eliminate all concerns as changes in tax haven FDI and return rates may also be driven by underlying variation in third variables. One option might be to turn to particular reforms – e.g. the tightening of anti-profit shifting rules – and show that

pre-trends in the dependent variable are okay and then determine how rates of returns (and tax haven FDI) evolve after the policy changes. Moreover, the sensitivity of results to using richer and different sets of control variables could be assessed.

Author: Thank you, I agree that this is an issue and that our underlying assumption that the negative relationship between rate of return on FDI and the share of FDI coming from tax havens is relatively strong. In response to this comment, we included a more detailed discussion of the reasons why we think this assumption is plausible, while at the same time recognizing in more detail the reasons why it may not be an overly accurate representation of reality. This discussion now takes up paragraphs 2-4 in Section 3.4.

I similarly agree that examining individual reforms to get closer to the identification of a causal effect is a very promising research opportunity and I would love to have the chance to explore it more in the future. I am part of one ongoing project at our Institute which examines the effects of changes in transfer pricing regulations on the rates of return of US firms in tax havens. While that project is still in a very preliminary phase, there are some promising reforms to be analyzed in this context, for example the 1996 check-the-box regulations, as seen in Figure C.1.

Figure C.1: Rate of return on the U.S. FDI in tax havens and non-tax havens following important reforms in transfer pricing regulations



Source: Palanská (forthcoming)

Lastly, regarding the additional control variables, while in this paper we rely on the strong assumption described above (which effectively means that we assume that no other variables are at play), I believe that relaxing this assumption is an important improvement that future methodologies should incorporate. In response to this comment, we now include these ideas in the last paragraph of Section 3.6 where we propose avenues for future research.

Based on the link between haven FDI and rates of returns, the author uses back-of-the-envelope estimates to determine revenue losses. For that purpose, estimated changes in after-tax returns are transformed in pre-tax returns and the calculated tax base loss is then multiplied by the country's statutory tax rate to arrive at the revenue loss. This appears quite rough to me. Using statutory tax rates might e.g. induce non-negligible bias, given that many countries, in particular in the less developed world, make extensive use of tax holidays.

Author: I agree that using statutory tax rates is a relatively rough step in the estimation and an upward bias of some extent is likely a consequence of this methodological choice. The reason we chose to use statutory corporate income tax rates is twofold. First, the alternative (i.e., effective tax rates) is not available for as many countries in sufficient quality; I discuss this issue in more detail in one of my responses below. Second, the bias is likely to be a problem primarily in countries with high differences between statutory and effective tax rates, which are generally tax havens, most of which are captured by our list of tax havens—and for these countries, we do not provide estimates of tax revenue losses due to profit shifting. At the same time, this bias is only introduced in the very last step of the calculation in which we move from estimates of shifted profits (which do not suffer from this potential bias) to tax revenue losses.

To see how large this bias is in our estimates, I recalculated the estimated tax revenue losses using effective tax rates from Garcia-Bernardo et al. (2020)—they are available for 40 countries out of the 79 in our sample. These 40 countries are estimated to lose 94 billion USD in our headline results; using effective tax rates brings this total down to 68 billion USD.

A new Figure B.7 which we included in response to this comment shows, at the country-level, the ratio of the estimated tax revenue losses if we use effective vs. nominal rates. In addition, we now include a discussion of this potential source of bias in Section 3.4, arguing that countries that make extensive use of tax holidays might not be able to collect the full amount of the estimated tax losses even if profit shifting disappeared, and include the global totals in the discussion of our results in Section 3.5.

I like the focus of the study on heterogeneity of profit shifting across countries, but I lacked some theoretical guidance on why we might expect shifting to be heterogeneous in first place. Including a discussion along these lines would be a plus. Differences might, e.g., root in differences of anti-profit shifting legislations, in tax enforcement capacity, governance institutions, statutory corporate tax rates.

Author: Thank you, in the revised version we have included a discussion of this reasoning in a new separate paragraph in Section 3.4. I agree that this is important guidance on how the mechanism works, since the underlying assumption—that the negative relationship between rate of return on FDI

and the share of FDI coming from tax havens is due to profit shifting—is a crucial part of our analysis.

Moreover, some methodological choices could be better defended. While the author, for example, shows that his estimates are robust to applying different standard definitions of haven economies, I wondered why to focus on havens in first place. Tax havens differ quite substantially in their regulations and services. Some do not target corporates at all. And MNEs may, in turn, also shift to low-tax economies that do not appear on tax-haven list (as e.g. suggest by a recent paper by Liu and Schmidt-Eisenlohr).

Author: I agree that this is relevant criticism of our approach and we have considered other options when developing it. While the preferred approach would be to use differences in the relevant tax rates, we ultimately decided not to use these due to the inherent difficulties of data on corporate income tax rates. In particular, backward-looking data on effective tax rates, which would be most relevant for this purpose, is not sufficiently available at the coverage we need (although recent studies (such as Garcia-Bernardo et al. (2020)) aim at improving this situation). Forward-looking effective tax rates are another option and these are increasingly available for a wide range of countries, such as those provided by the Corporate Tax Haven Index by Tax Justice Network (2019) published in May 2019, and can thus be a good alternative for future research. The last option, statutory tax rates, are less relevant especially for some tax havens (such as Luxembourg) where the difference between statutory and effective rates is substantial; these important tax havens would thus be missed by an approach that would use statutory rates.

After the publication of the article that makes up Chapter 3 (in June 2019), I supervised a Master's thesis of Lukáš Nepivoda (available here: link) in which he used the 'Lowest Available Corporate Income Tax' (LACIT) rate from the Corporate Tax Haven Index to determine which jurisdictions can, in theory, be used by each country's MNEs to shift profit in order to reduce that MNEs' tax burden, regardless of whether these jurisdictions are placed on any lists. While this was not the only methodological innovation made in that study, the results were very similar to the original ones that we present in Chapter 3, with generally only slightly higher estimates for developed countries. Interestingly, one of the findings

of that thesis was that the elasticity is stronger with increasing differences in LACIT rates used in the definition of the 'share of FDI coming from tax havens'.

I believe that the improvements in datasets on corporate income tax rates over the coming years will give rise to an improved re-estimation of our approach which will not rely on dichotomous lists of tax havens. In response to this comment, we explained this methodological choice in more detail in Section 3.4.

It might also be helpful to discuss in more detail how the dependent variable – financial income from FDI – is defined. How does it deviate from taxable income? How may that affect the results? This is of particular relevance as recent work points to problems with some of the more prominent shifting estimates because of data issues (Blouin and Robinson, 2019).

Author: Thank you, I believe that this is an important discussion which we have omitted in our paper and instead we had referred to the original methodology by UNCTAD (2015). FDI income in the IMF CDIS is recorded net of taxes (IMF 2013, Chapter 2, par. 2.19), and for this reason we make an adjustment for the simulated profit shifting (as you mention in one of your comments above); this step was until now perhaps best described in the paper by the inclusion of column G in Table A.12. The same procedure is applied for our headline country-level estimates.

Regarding the critique by Blouin and Robinson (2020), as I describe in more detail in my response to a comment by Prof. Johannesen below and newly also in Section 3.2, the data that we use does not suffer from the bias that these authors highlight for the case of US Bureau of Economic Analysis data.

In response to this comment, I have now included this more detailed discussion of the definition of FDI income (in Section 3.3).

C.1.3 Chapter 4: Secrecy Jurisdictions and the Countries They Harm

In Chapter 4, "Secrecy Jurisdictions and the Countries they Harm", the author develops a "Bilateral Financial Secrecy Index" that quantifies which secrecy jurisdictions harm individual countries the most. The index is constructed based

on a country-specific Financial Secrecy Index (FSI) (defined in previous research), which is then linked with bilateral information on portfolio investment used to proxy for the scale of the relationship between two jurisdictions. As a main finding, the analysis shows that different countries are exposed to and harmed by different secrecy jurisdictions. The author argues that the index helps evaluating policymakers' existing efforts to fight financial secrecy.

Again, the chapter is well-written and the index will certainly attract interest in the policy sphere and might indeed help to evaluate countries' exposure to certain secrecy jurisdictions. Nevertheless a more thorough discussion of the approach and results would have been helpful. The index value depends on the level of secrecy of the partner country (constructed in prior work) as well as bilateral portfolio flows between countries. Not all of these flows are secrecy-related and the share may (non-linearily) vary in the level of secrecy. Moreover, portfolio-investments are an endogenous variable which is affected by policy choices. If countries e.g. implement information exchange agreements or enact other measures to counter the use of these jurisdictions, this impacts portfolio flows and might direct them towards other countries. Some more guidance on how to think about that would be helpful - also with regard to the interpretation of the results. In the final part of the AIE-analysis it might be nice to disentangle level and selection effects: do countries with high levels of financial secrecy have less AIE agreements and direct agreements towards unimportant partner countries? Or just one or the other?

Author: Thank you, this chapter has been thoroughly revised since the pre-defense, taking into account very useful comments from not only you and other reviewers of this disseration, but also from four reviewers at the journal *Regulation and Governance*, from which we have now received a request for further minor revisions and the paper is close to being accepted for publication there.

In particular, in response to your comments, we now provide a more thorough discussion of the results and a number of checks to support the robustness of our preferred results. First, we use other data sources to construct alternative versions of the Index and we compare these versions by looking at their correlation coefficients in Table A.24. Second, we newly discuss a version of the BFSI that is aggregated at the country-level and so is directly comparable to the original FSI. We compare the two in Figure B.12 and we report the complete results for each country in Table A.32.

Third, we now explicitly discuss in Section 4.3.1 the rationale behind using bilateral portfolio flows which include legitimate economic activity (and not only secrecy-related activity). Fourth, we now provide more guidance on how to think about the BFSI (in Section 4.3.1), explaining that the index can be viewed as a snapshot of reality rather than a tool that could be used to analyze the development of the secrecy world or the consequences of changes in policies. Fifth, we have now amended the AIE-analysis (Section 4.4.2) by reporting in more detail whether the main result is driven by less AIE relationships or these relationships being directed towards less important (in terms of their secrecy supply) countries. In short, we find evidence for both effects.

In addition, we have now positioned the framing of the paper much closer to the international political economy literature (for the purposes of its publication in *Regulation and Governance*), we updated the data to its newest version (2020) and amended the analysis in a number of ways, including a more detailed analysis of OECD countries and their dependencies' role in the offshore secrecy world (see Figures 4.2, B.15 and B.16 and the discussion in Section 4.4.1).

Finally, let me stress that I enjoyed reading the thesis. Chapter 2-4 are original contributions to the literature. The studies are based on rich data and the analyses are competently conducted. The work also reveals the author's excellent institutional knowledge and knowledge of the existing literature. Furthermore, I very much like the author's open discussion of shortcomings of his methodological approaches (although it would have been nice if some more of these could have been tackled). This thesis is defendable from my perspective. The papers are publishable in economic journals (although unlikely the top ones).

I made a number of suggestions for improvement. I do not think that necessarily all of these must be tackled. Some more discussion along the sketched lines would, from my perspective, be enough.

C.2 Niels Johannesen

C.2.1 Chapter 2: Value of Political Connections

The chapter aims to identify empirically firms' financial return to their investment in political connections. The analysis uses a manually collected dataset on

the political contributions of Czech firms as well as their financial performance. The empirial strategy is to compare the return to equity of «connected firms», i.e. firms that have made contributions to political parties in recent years, to matched firms that have made no such contributions but are similar in dimensions such as size, location, industry and, importantly, pre-contribution profitability.

The main result indicates that there is a sizable return to party contributions: connected firms have returns that are significantly larger, in an economical and statistical sense, than matched firms making no contributions. The estimated return to political connections in the Czech Republic is higher than estimates from low-corruption economies suggesting that the strength of political institutions is an important determinant of the scope for rent seeking through political connections.

My main suggestion for improvements is the following: an alternative and possibly more transparent method for estimating the return to political contributions would be the event study method where the event is the contribution and a vector of event time dummies captures dynamics in profitability before and after the contribution relative to other firms (either firms that make contributions in other years or firms that never make contributions).

Author: Thank you for very much for this suggestion, I believe that this is indeed a viable approach. In response to this comment, I carried out a preliminary event study of contributions and I believe I have found strong and insightful results. I report on them in a new Figure 2.3 and describe them in a new paragraph just above it. In short, I find preliminary evidence of a strong effect concentrated in the first year following the contribution. Unfortunately, I won't be able to include these results in the article on which Chapter 2 is based since it is already published in a journal, but I will try to use it in the future in my stream of research on political connections.

The chapter is very well executed. It has a clear research question and a well-explained methodology. It is generally well written and it is easy to follow the exposition. The relevant literature is cited appropriately and it is clear how the chapter relates to other work in the field.

I find that the chapter makes a clear contribution to the literature on political connections by:

- creating a high–quality dataset on political contributions for the Czech Republic
- using solid methodology to provide credible estimates of financial returns to policial contributions in a post-transition economy
- proposing refinements of this methodology

The chapter is certainly publishable in a respectable academic journal.

C.2.2 Chapter 3: International Corporate Profit Shifting

The chapter sets out to measure the global scale of corporate profit shifting, regional differences in exposure to profit shifting and associated revenue losses. The main data source is publicly available on foreign direct investment (FDI) at the country-level. Following the methodology developed by UNCTAD (2015), the chapter estimates the empirical relation between the share of a country's FDI coming from tax havens and the average return on the FDI. It is well-known that many corporate tax avoidance techniques shift profit from high-tax economies to tax havens. Theoretically, this trafic will raise the share of FDI coming from tax havens and lower the recorded return to FDI. Under strict assumptions, the relation between the two variables thus yields a baseline return on FDI in countries with no exposure to tax havens, which can be used to infer how much exposure to tax havens erodes FDI returns and thus corporate tax bases.

The results suggest that gradients vary considerably across countries in different geographical regions and a different income levels. Transforming the estimated gradients into revenue losses, the chapter finds global losses in excess of $100 billion, which is consistent with other recent studies using different methodologies.

The main limitation of the empirical exercise is that it relies on macro-data and therefore cannot control for possibly confounding factors. For instance, if the share of tax havens in FDI correlates with the risk-profit of the underlying investments, it may create a correlation between with returns on FDI even in the absence of profit shifing.

Author: Thank you, I agree that the underlying assumption that we make in this step of the analysis is crucial. In response to this comment, we have now extended the part of the chapter in which we discuss the reasons why

we think the assumption could be plausible as well as explain the possible reasons for the assumption not to be met (paragraphs 2-4 in Section 3.4 are now devoted to it).

I think the empirical identification could be enhanced by controlling for country fixed factors. Currently, the estimates are identified by both cross-sectional variation and time variation while it is possible – at least in principle, to include country fixed effects and identify from time variation only. In practice, it may be that the year-to-year variation is too limited for a standard panel model. In that case, it may be possible to estimate a model in longer differences, e.g. 2010-2018, correlating the change in the share of haven FDI and the change in FDI returns within countries.

Author: I agree that the country-fixed effects model would be preferred, and we had considered that option in the final published version of the article, describing our findings in Section 3.4 and concluding that, as you predict: "... the data do not enable it because of the short period for which they are available and the low levels of variation in inward FDI stock and rate of return on FDI."

Naturally, the time period for which the relevant data is available is expanding in time, and after the publication of the article in June 2019, at our Institute I supervised a Master's thesis of Lukáš Nepivoda (available here: link) in which he explored the country-fixed effects model in more detail. For a smaller sample of countries, he used it to derive new estimates of foregone tax revenue. The results were very similar to the original ones that we present in Chapter 3, with generally slightly higher estimates for developed countries. I believe that the country-fixed effects model will be a good option to use for new estimates that will be created in the future, perhaps even allowing for an analysis of the development of profit shifting over time.

Regarding the model with longer differences, I believe that it is an interesting possibility for future research. Two issues to solve would be that (i) there are significant year-on-year deviations in FDI income, and some averaging over several years is likely to be necessary; and (ii) it is unclear that there would be linear long-term trends in the relationship between FDI rate of return and the share of FDI coming from tax havens. For these reasons, a further set of strong assumptions would be required

for this approach.

In response to this comment, we newly discuss the potential of the model with longer differences in Sections 3.4 and 3.6.

Overall, what I recognize as a serious drawback of the FDI approach data-wise is that many of the structures that allow profit shifting today had been implemented prior to the start of the data sample (i.e., 2009). Therefore, the power of estimating the elasticity only based on data starting at that time is limited—however, unfortunately, older data is not (and likely will not ever be) available.

Ultimately, the uncertainties are forgivable because the question of revenue losses from profit shifting is challenging and yet of first-order importance in international taxation. This is another good chapter. It is well written and very thorough in explaining the analytical steps and interpreting the results. The comparison to the related literature is very useful. However, it would be useful if the authors could discuss whether the methodology is robust to the general critique delivered in Blouin and Robertson (2019, «Double Counting Accounting: How Much Profit of Multinational Enterprises is Really in Tax Havens»)

Author: Thank you, I agree that Blouin and Robinson (2020)'s contribution to the discussion of estimates of the scale profit shifting is very important and I have been following this discussion (e.g. by Clausing (2020) or Garcia-Bernardo et al. (forthcoming)) that emerged after the publication of the article that makes up Chapter 3.

The critique of Blouin and Robinson (2020) is that some prior studies that used data on US MNEs from the Bureau of Economic Analysis to estimate the scale of profit shifting by US MNEs (such as (Clausing 2016)) suffer from an upward bias due to inherent double counting of foreign income and to its misattribution to incorrect jurisdictions in the used data. Clausing (2020), on the other hand, argues that the proposed adjustments would omit some types of profit shifting, effectively introducing a downward bias. In addition, Clausing (2020) then provides estimates based on newly available data from country-by-country reports which do not suffer from the drawbacks highlighted by Blouin and Robinson (2020) and which lead to results consistent with prior studies.

Since our approach uses data on FDI which is published by the IMF in a

standardized manner for countries around the world, our paper is not prone to this criticism. Furthermore, as we report in Section 3.5, our estimates are in line with other studies that have used different data sources. This includes new estimates which rely on global country-by-country reporting data released by the OECD in July 2020 (which I have collaborated on and which are yet to be published).

In response to this comment, I have added this discussion of Blouin and Robinson (2020)'s critique in Section 3.2.

I find that the chapter makes a clear contribution to the literature on profit shifting by:

- providing estimates of profit shifting and revenue losses, globally and by country groups, applying a clear and transparent methdology
- comparing to other estimates using different methodologies and thus shedding light on where the remaining uncertainties are.

The chapter is definitely publishable in a respectable academic journal.

C.2.3 Chapter 4: Secrecy Jurisdictions and the Countries They Harm

The goal of the chapter is to develop a bilateral measure of financial secrecy: an index capturing the importance of the opportunities for secrecy provided by a specific haven to specific country. The exercise relies on macro-data on cross-border financial positions from the IMF as well as the Financial Secrecy Index (FSI) which expresses the de jure transparancy of a jurisdiction.

The Bilateral Financial Secrecy Index (BFSI) thus combines the financial secrecy embedded in the laws of a counterpart jurisiction (FSI) and the relative importance of the counterpart jurisdiction in the external financial flows.

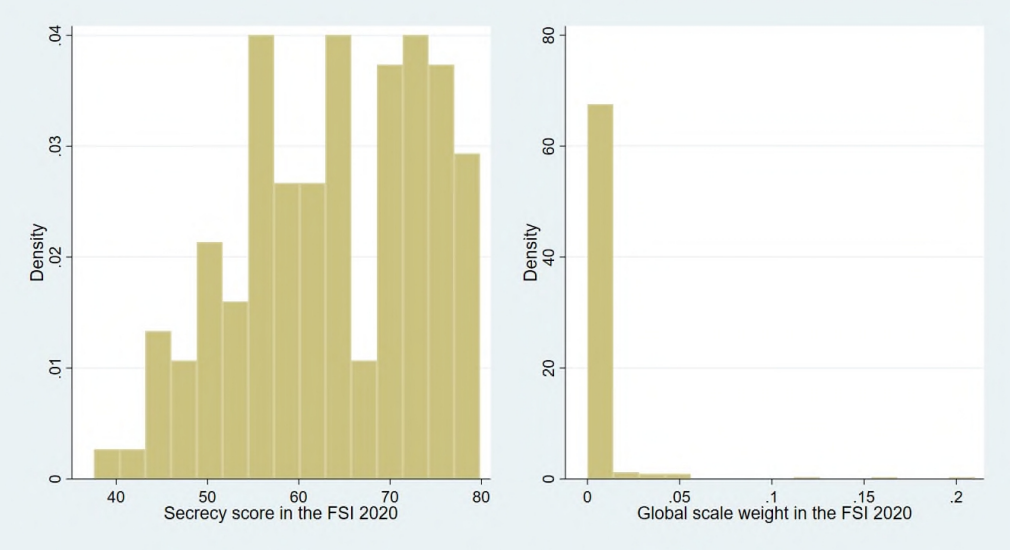
I have identified some problematic aspects about the procedure: First, the two parts of the index are combined in a non-linear and highly opaque way. For instance, I suppose the ranking of the index across country pairs is quite sensitive to the methodological choice to raise FSI to the power of 3 while raising the bilateral component to the power of 1/3. It is unclear why this particular functional form is chosen.

Author: I agree that the construction of the index, as with any such measure, involves a number of methodological choices that are difficult to objectively justify. The reason behind the choice of this particular formula for the BFSI goes back to the methodology of the FSI, as it was one of our objectives to maintain consistency with the already established measure of financial secrecy.

That said, the choice of the formula in the original FSI was discussed thoroughly within our internal team at the Tax Justice Network; we discussed the other options in great detail prior to the publication of FSI 2018, in whose development I was heavily involved and which was guided by best practices in risk index creation (Becker et al. 2016). Here is how we argued in the FSI 2018's methodology (Tax Justice Network 2018, Chapter 5): "In the choice of how to combine secrecy scores with global scale weights we are led by the FSI's core objective: the FSI measures a jurisdiction's contribution to global financial secrecy in a way that highlights harmful secrecy regulations. [...] relative to a simple multiplicative combination of the two entities, by cubing the secrecy score and taking a cube root of the global scale weight, we highlight the importance of harmful secrecy regulations in contributing to global financial secrecy."

The reason why a simple multiplicative combination does not work is that the distributions of the two variables (SS and GSW/BSW) are very different, with the GSWs being strongly skewed to the left due to their construction as a share of a global total (see Figure C.2). Therefore, a simple multiplicative formula produces Index values that are almost perfectly correlated with global scale weights, and secrecy scores do not play a strong role. Cubing the secrecy scores and taking a cube root of the global scale weights thus makes the secrecy score a relatively more important determinant of the final Index value.

Figure C.2: Histograms of GSW and SS of the FSI 2020



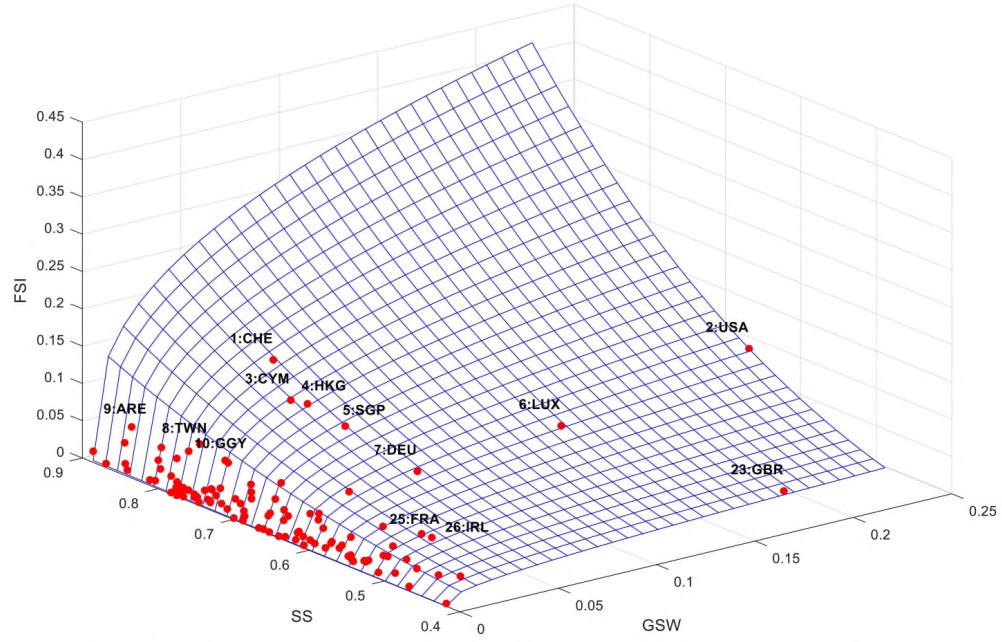
Source: Tax Justice Network (2020a)

Ultimately, we find that the the cube/cube-root formula produces a suitable outcome in terms of what influences the Index value. Figure C.3 shows the surface created by all the possible combinations of the secrecy scores and global scale weights. As noted by Becker and Saisana (2018): "... the gradient is quite high in corner of high SS and low GSW, meaning that in this area, a small increase in GSW results in a very sharp increase in the FSI. The implication is that countries that have a similar SS can have markedly different FSIs as a result of a relatively small difference in GSW [...] countries with low SS and low GSW will only experience a small increase in FSI if the GSW were to be increased. Overall, for countries with small GSW, their FSI is driven much more by their GSWs than by their SSs. The opposite is true for countries with large GSW: here countries are differentiated mainly on their secrecy scores."

To summarize, I agree that the choice of the formula is necessarily arbitrary to a degree, and I believe that the cube/cube-root formula is among the ones that produce a desirable outcome, which is why it seems an appropriate choice for the BFSI.

In response to this comment, in Section 4.3.1, we now more clearly refer to this reasoning as it was published in Tax Justice Network (2018).

Figure C.3: Surface plot of FSI against GSW and SS, 2018 edition



Note: Countries labelled with highest 10 FSI scores, and highest 10 GSW scores. Numbers indicate FSI ranks.

Source: European Commission, Joint Research Centre (JRC), 2017

Source: Becker and Saisana (2018)

Second, given the way the bilateral component is constructed it is unavoidable that major economies such as the United States and Japan will emerge as large «providers of secrecy». As a minimum, one would expect that the bilateral component would account for the «size» of the counterpart. Perhaps, one could estimate a gravity model and use the residuals, the part of bilateral positions that cannot be explained by country size and geography, instead of the bilateral positions themselves.

Author: Thank you, I believe that this is an important discussion. The FSI (as well as the BFSI) are constructed to be measures of risk. While it is clear that not all of the cross-border activity that is captured by the GSW/BSW happens because there is secrecy in the recipient jurisdictions, the Indices are designed to measure the risk associated with high activity *combined with* high secrecy. And because the formula that combines secrecy scores with the weights is multiplicative, only if a country provides *some* financial secrecy can it score high on the Index.

Using residuals from a gravity model would be an alternative approach, which would attempt to measure not risk but rather the actual scale of an

issue, and thus would be akin to what we do in Chapter 3. Also, we are applying this approach in a stream of related research that I am currently involved in. Admittedly, both approaches have their obvious strenghts and drawbacks, and I believe both are useful for different purposes.

In response to this comment, we now include a discussion of how our approach differs from actual estimates of secrecy-related activity (such as those provided by Zucman (2013)) in Section 4.3.1.

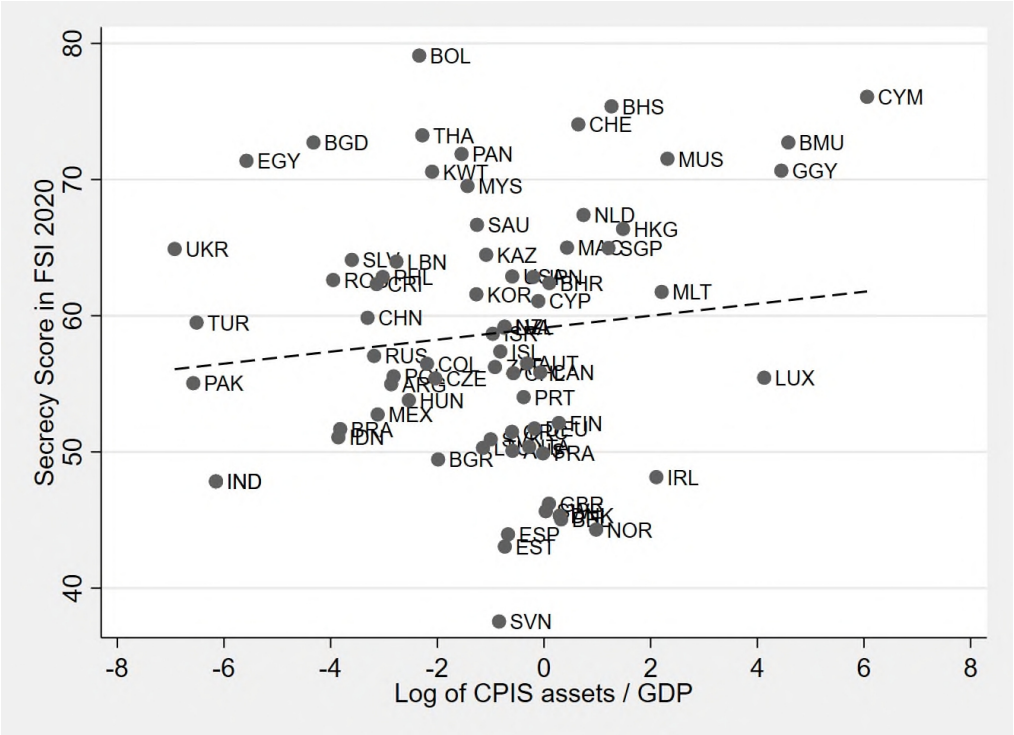
Third, the paper relies on assets observable in international portfolio statistics as an indicator of the «secrecy services» whereas the influential paper by Gabriel Zucman (2013, «The missing wealth of nations») shows that hidden wealth gives rise to positions that are observable from the liability side but not from the asset side. So, in a sense the secrecy index is based on the non-secret flows (e.g. to the U.S.) whereas it ignores the secret flows (e.g. to Bahamas).

Author: I agree that this is an issue of the used data. What Zucman (2013) observes is a discrepancy between globally reported assets and liabilities, with globally reported liabilities being consistently larger due to the incapability of the home governments of secrecy-using households to record their assets in secrecy jurisdictions. In his example, "Take the typical investment revealed by the Swiss data: French residents who own Luxembourg fund shares through their Swiss accounts. [...] These fund shares should be recorded as portfolio assets for France and liabilities for Luxembourg. In practice, France has no way to record assets. Luxembourg statisticians duly record portfolio liabilities—they are aware that foreigners own shares of domestic funds." What this means for the BFSI is that some of the assets invested by households from countries such as France in secrecy jurisdictions such as Luxembourg will in fact not show up in the data in case there is an intermediate secrecy jurisdiction (such as Switzerland) in the chain. Therefore, the BSW of these relationships (such as the one between France and Luxembourg) will be lower than it actually should be, making the calculated BFSI value biased downwards. In such cases, the BSW can be viewed as a lower bound of what is happening in reality.

Evidence on the size of this bias is difficult to provide with available data. One piece of evidence suggesting that the bias is not completely distorting the data is that, even in the available data, we do observe a positive relationship between secrecy scores and reported assets (as shares

of GDP). Figure C.4 shows this relationship. Most of the jurisdictions that have a high ratio of reported portfolio assets on GDP have, at the same time, a high secrecy score (with the exception of the notorious outliers of Luxembourg and Ireland, which can be easily explained by the fact that these are two of the most important jurisdictions in the world for hosting mutual funds, which inflates the statistics).

Figure C.4: Relationship between secrecy scores (from FSI 2020) and reported portfolio assets as shares of GDP



Source: Author.

In addition, there are other sources of data which are less likely to suffer from this bias, such as the Location Banking Statistics from the Bank for International Settlements. In response to this comment, we now calculate several versions of the Index using other data sources and we find that the results do not change significantly. We discuss these arguments in Section 4.3.1 and report on the basic comparison of the results from these different versions of the BFSI in Table A.24.

Like the other chapters, this chapter is well motivated and well written. However, I find that important elements of the methodology are questionable. In its current form, my guess is that the paper would be rejected at serious

economics journals.

Overall, I find that the dissertation consists of two chapters (2,3) – both with good prospects for publication in international journals and clearly above the bar for a successful PhD - and one chapter (4) that I do not think is publishable in a good economics journal in its current form and also, in my assessment, falls short of the usual standard in a PhD dissertation. I would recommend that chapter 4 is revised to somehow address the three comments made above before it is defended. As a minimum, I would like to see the points discussed verbally, but it would be great if some of them gave rise to improvements in the methodology or robustness tests.

Author: Thank you very much for all your comments and suggestions. Since the pre-defense, especially Chapter 4 has undergone a major revision, which incorporates these useful comments from you as well as other reviewers of this dissertation and four reviewers at the journal *Regulation and Governance*, where the paper is now close to publication and I believe the paper is now substantially improved thanks to the feedback we received.

C.3 Gabriel Zucman

Miroslav Palanský has produced a well-founded, rigorous, and innovative dissertation thesis focused on corruption and tax evasion. Studying illegal activity is by definition fraught with difficulties. Miroslav provides new quantifications of these phenomena by applying novel empirical techniques and creating new data series. This dissertation pushes forward scientific knowledge in three field of economic research: international economics, political economy, and public economics. This dissertation provides an original contribution to the scientific literature in these fields. It would be defendable in any respected institution, such as UC Berkeley where I teach or the London School of Economics. Indeed, two of the three main chapters of the dissertation have already been published or accepted for publication in well-regarded, peer-reviewed academic journals (Public Choice and International Tax and Public Finance). This is a notable achievement for a PhD student and proof that Miroslav is capable of conducting research at a high international standard. Therefore, I recommend this thesis for defense without substantial change.

In the rest of this report I will summarize Miroslav's work and make a few comments on some of the chapters most connected to my area of expertise.

The first chapter of the dissertation aptly motivates the work and summarizes the main results of the dissertation.

C.3.1 Chapter 2: Value of Political Connections

The second chapter is based on a sole-authored paper titled "The value of political connections during the post-transition period: Evidence from Czechia" which is forthcoming in the peer-reviewed journal Public Choice. This paper demonstrates that Miroslav is able to conduct original work on his own at a high international standard and indeed publishable in well-respected peer-reviewed outlets. Since the paper has already been accepted for publication and falls somewhat outside my area of expertise, I will not provide additional comments and suggestions.

Author: Thank you very much for the kind words.

C.3.2 Chapter 4: Secrecy Jurisdictions and the Countries They Harm

The third chapter is based on a paper titled "Estimating the scale of profit shifting and tax revenue losses related to foreign direct investment," jointly written with Petr Janský. This paper has been published in International Tax and Public Finance, a well-regarded peer-reviewed academic journal in the field of public economics. This paper is close to my area of expertise, and I could check that the Miroslav appropriately refers to all the relevant papers in this sub-field. Miroslav has an excellent command of the literature and this chapter usefully contributes to pushing forward knowledge about the size of corporate profit shifting, a subject which is highly relevant to global policy debates. The basic idea of the paper, to compare rates of returns on foreign direct investment to detect traces of profit shifting, is sound. This chapter builds on the methodology pioneered by UNCTAD (2015), and extends it in a number of ways. The results suggest that around $420 billion USD is shifted from the 79 countries in the authors' sample, resulting in $125 billion USD foregone in tax revenue. (Note that the introduction on page 6 mistakenly writes "millions" instead of billions"). The chapter discusses the distributional

effects of international corporate profit shifting and compares the results with other existing estimates of its scale. Strikingly, the findings are very consistent to those in Torslov, Wier and Zucman (2018), lending support to the idea that the corporate income tax revenue losses due to profit shifting are indeed of around $125 billion annually. For future work, I can only encourage the authors to incorporate the new Foreign Affiliates Statistics recently released by many countries to refine their estimates.

Author: Thank you, I have already explored the possibility to use the new Foreign Affiliates Statistics in a preliminary phase of an ongoing project. I have corrected the typo in the Introduction.

C.3.3 Chapter 4: Secrecy Jurisdictions and the Countries They Harm

The last chapter, based on a working paper co-authored with Petr Janský and Markus Meinzer, constructs a ranking of the most important secrecy jurisdictions for each country individually, what the authors call the Bilateral Financial Secrecy Index (BFSI). The chapter then evaluates two major recent policy efforts aimed at fighting tax evasion: the blacklisting process of the European Commission and the bilateral automatic information exchange of bank information. The results suggest that tax havens continue to supply secrecy to some of their key partners. Of course, the creation of Secrecy Indexes (and hence bilateral indexes) involves several judgement calls, and so I expect that this paper might be harder to publish than the first 2 chapters. However it nonetheless constitutes in my view a valuable—if somewhat more qualitative—addition to the literature on tax havens.

Author: Thank you, this chapter has been thoroughly revised since the pre-defense, taking into account comments from not only you and other reviewers of this disseration, but also from four reviewers at the journal *Regulation and Governance*, from which we have now received a request for further minor revisions and the paper is close to being accepted for publication. In particular, we have positioned the paper much closer to the international political economy literature, updated the data to its newest version, and amended the analysis in a number of ways. In order to justify some of the methodological choices we made, we newly ran several ro-

bustness checks, including using other data sources to construct the BFSI, and compared the various versions of the index which have shown to yield similar results.

Overall, this is a rigorous dissertation, that I can recommend for defense without substantial changes.