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First the stick, then the carrot? A cross-country evaluation of the OECD's initiative against harmful tax competition

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Es wird gebeten, sich mit Anregungen und Kritik direkt an den Autor zu wenden.

First the stick, then the carrot? A cross-country evaluation of the OECD's initiative against harmful tax competition^{*}

Markus Grottke[†] / Maximilian Kittl[‡]

Abstract:

In 1998 the OECD launched one of the most comprehensive initiatives ever to combat harmful tax competition. The following paper analyzes how companies' investment activities in tax havens are associated with the three most salient elements of the OECD initiative. Our analysis is based on a U.S. and on a German panel data set. The results suggest that the investment activities of both countries' companies in tax havens are negatively associated with the first element, i.e., the tax havens' public status as being blacklisted. We additionally find that the two subsequent elements of the initiative, that is, the tax havens' commitments to the OECD standards of transparency and the existence of bilateral information exchange agreements, are positively associated with U.S. companies' investment activities in these countries. Unlike the U.S. case, tax havens' commitments are negatively associated with German companies' investment activities, while the evidence is mixed for the case of a bilateral information exchange agreement with Germany. We interpret our findings in the light of reputational effects.

Keywords: Tax havens, OECD blacklisting, TIEA, Companies' investment activities

JEL classification: H2, H26, H87

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1 Introduction

In 1998 the Organisation for Economic Co-operation and Development (OECD) launched one of the most comprehensive initiatives ever to combat harmful tax competition.¹ The initiative essentially consisted of two parts. On the one hand, a stick: tax havens were featured on the prominent OECD blacklist² and hence potentially faced reputational losses and, in consequence, reduced corporate investments from abroad. On the other hand, a carrot: affected tax havens were offered an opportunity to potentially restore their reputation by participating in the initiative's downstream process and subsequently be withdrawn from the list. This involved a commitment to the OECD's standards of transparency and the signing of at least 12 bilateral agreements to exchange tax-relevant information with other countries (e.g., Nicodème 2009, Hanlon, Maydew and Thornock 2015).

The ongoing concerns of policy makers and governmental agencies about companies' increased use of tax havens seem to suggest that the initiative did not have the desired effect on companies' investment activities (e.g., Neuerer 2013, Hallman 2014). Yet, there is still a paucity of in-depth empirical evidence to document the real effects the three elements of the initiative have had on companies' activities in tax havens so far (Devereux 2002). Kudrle (2008) uses aggregated data from the Bank of International Settlements to provide initial evidence on the impact of the OECD initiative on individual liabilities in a subset of tax havens. He did not find significant effects. Avi-Yonah (2009) concludes from the fact that tax revenues declined in non-OECD member states but not in OECD member states that the initiative has been a success. Rather as a side effect, Hanlon, Maydew and Thornock (2015) find that – thanks to increased OECD scrutiny of tax havens – inbound foreign portfolio investment to the U.S. from tax havens decreased after 1998 and 2001, respectively. However, an ample empirical analysis has yet to be undertaken of the impact of this approach and its individual elements.

Our study provides such empirical evidence. We have chosen two countries for this purpose, namely the United States and Germany. These countries are characterized by certain differences which allow us to draw more generalized conclusions on the initiative's impact. For one, they epitomize the two major (in part opposing) camps that

¹ Another major initiative is the EU Savings Directive, which focuses on the taxation of foreign interest income earned by EU individuals. For details see, for example, Hemmelgarn and Nicodème (2009) and Johannesen (2014).

² Throughout this article we refer to the list initially published by the OECD in 2000 as the blacklist (OECD 2000).

shaped the initiative, namely the Anglophone and the European Continental camp (Sharman 2006). Furthermore, Gray (1988) highlights that several cultural dimensions differ between the U.S. and Germany. For instance, transparency is highly appreciated in the U.S while Germany has a high tolerance for secrecy. There are further differences in the legal system (common law vs. code law) and the underlying tax system (tax credit system vs. tax exemption system; see for example Gumpert, Hines and Schnitzer 2012). Empirical evidence from these two jurisdictions' perspectives hence produces particularly revealing insights. In the following, we focus on two research questions:

1. Which effect does the OECD's initial blacklisting have on companies' material activities in tax havens?
2. How are those material activities associated with the existence of a tax haven's formal commitment to the OECD's standards of transparency and the existence of a bilateral information exchange agreement between a tax haven and the companies' home country?

We examine the first research question using a difference-in-difference approach that is designed to measure the adjustments in companies' material activities in blacklisted tax havens (treatment group) in comparison with a control group of tax havens that were not blacklisted. The second research question is addressed by testing based on the previous treatment group whether an effect on companies' investment activities in blacklisted tax havens emerged from the initiatives' subsequent steps.

Companies' activities are measured based on two panel data sets generated by automatic text analysis. The first panel data set comprises information from Exhibit 21 forms of U.S. companies registered in the SEC's Edgar system. The second panel data set is based on a unique collection of information contained in the annual reports (based on consolidated accounts) of German Prime Standard companies.

The following findings emerge. As for the effect of blacklisting, we find that in both samples companies' activities in tax havens seem to be indeed negatively impacted. We interpret this finding as induced by reputational costs. With respect to the two steps following the blacklisting, our findings are as follows. First, we find a positive association between investment activities and the existence of tax information exchange agreements (TIEA) in the U.S. and to some extent also in the German sample. The results suggest that such agreements essentially allow tax havens to regain their reputation. Second, with respect to the previously blacklisted tax havens' formal commitments to transpar-

ency, we find a significant positive association with companies' investment activities in the U.S. sample, but a significant negative association in the German sample.

Our research results contribute to academic literature in several ways. We are among the first to explicitly address companies' tax haven investment activities from the perspective of tax havens' reputation. To our knowledge we are also the first to trace the impact of various industrialized countries' actions against tax havens, namely, multilateral blacklisting and multilateral agreements stipulating a commitment to the OECD's standards of transparency on the one hand and bilateral TIEAs on the other. Beyond the initiative itself, we add evidence concerning how counteractions targeted at the reputation of suppliers of tax planning opportunities could affect the demand for these opportunities. Finally, we contribute with evidence from a unique, manually verified large panel data set of tax haven activities from German companies. We are not aware of any such data having been generated outside the U.S. before.

Our research is relevant to policy makers, the OECD, and tax havens themselves for a variety of reasons. For the OECD and for policy makers, it provides insights into how their actions influence the material activities undertaken by companies. In this context, our study serves as a kind of follow-up evaluation of the alleged impact of the most salient features of the OECD's initiative against harmful tax competition. This is most relevant for similar future initiatives. The OECD is currently considering in Action 5 of the BEPS Action Plan how to "revamp the work on harmful tax practices with a priority on improving transparency" (OECD 2013, p. 18). One underlying assumption in this report mirrors that statement: corporate reputation can play a major role in companies' tax risk assessments and in turn also in their decisions on tax planning (OECD 2013). In this respect, our research sheds light on how – due to reputational concerns (EY 2014) – future corporate tax planning could potentially change in the light of the OECD's BEPS initiative (Donohoe, McGill and Outslay 2014).

However, we believe the results to be also relevant for tax havens themselves. Sharman (2006, 2009) points out that tax havens have reacted differently to the OECD's blacklisting activities, depending on their attitude towards the potential reputational costs. We provide evidence on whether a fear of these costs has been justified and, in addition, whether the further elements of the OECD's strategy, i.e., commitments to transparency standards and the signing of bilateral information exchange agreements, have represented opportunities to regain reputation.

The remainder of this paper unfolds as follows. In the second section we provide a brief overview of the key facts of the OECD initiative and related literature that elucidate the background of both the most salient elements of the initiative and the role played by reputation therein. Based on this, we develop our research hypotheses. In the third section we characterize our data. In the fourth section we explain the research methodology which we designed to answer our research questions. In the fifth section we present our results. In the final section we provide concluding remarks and point to the limitations of our study.

2 Institutional environment and hypothesis development

2.1 The OECD initiative against harmful tax competition

The OECD initiative³ was effectively launched in 1998 when the OECD published its “Report on harmful tax competition” (OECD 1998). In this report the OECD identified tax havens as a major source of harmful tax competition and commissioned the Forum on Harmful Tax Practices to identify such tax havens. The Forum’s examination finally led to the initial OECD tax haven list which was published in 2000 and comprised 35 countries (OECD 2000).⁴ To rid themselves of their status as a tax haven, the countries had to pledge compliance with the OECD’s standards of transparency and cooperation and sign at least 12 TIEAs. Progress reports have been regularly published since 2002. By 2012, none of the countries on the initial blacklist were listed any longer (OECD 2012).⁵

In the following sections we review the literature relevant to the development of the hypotheses designed to answer our research questions.⁶

³ For a more detailed illustration of the OECD initiative see Webb (2004), Sharman (2006) and Nicodème (2009).

⁴ In fact, six additional countries were found to be tax havens as defined by the OECD. However, because they signed so-called “advance commitments” they did not appear on the list (OECD 2001).

⁵ However, Nauru and Niue remain on the so-called grey list as they have yet to fully meet the TIEA criterion (OECD 2012).

⁶ There is a vast amount of literature on tax havens. Dharmapala (2008), Hines (2010) and Hebous (2014) provide excellent surveys on this literature. One major strand focuses on the company characteristics that encourage tax haven engagement (e.g., Desai, Foley and Hines 2006, Gumpert, Hines and Schnitzer 2012, Buettner, Holzmann, Overesch and Schreiber 2013). In contrast, Dharmapala and Hines (2009) investigate the attributes of tax havens. A further strand of literature focuses on the effects of using tax havens for tax avoidance purposes (e.g., Dyreng and Lindsey 2009, Markle and Shackelford 2012).

2.2 Blacklisted tax havens and reputational effects on corporate investment behavior

A crucial asset of low-tax jurisdictions consists in their reputation as countries that offer not only tax-efficient but also reliable investment opportunities for international investors in a stable legal environment (Sharman 2006). As a result, tax havens go to great lengths to cultivate an image of themselves as trustworthy jurisdictions (Kudrle 2008). The OECD initiative ended up with putting significant pressure on exactly this weak point.⁷ By publishing a list of tax havens – whose visibility was additionally amplified thanks to widespread coverage in the public press (Hebous 2014) – the OECD put a negative stamp on these countries, associating them with the stigma of facilitating tax evasion.⁸

In this context, Sharman (2009) conducts a case study of seven different tax havens. He finds that blacklisting is highly effective in nudging tax havens towards compliance with the OECD's demands even in the absence of legal sanctions. The study points to reputational concerns that potentially translate into real economic losses, for example because foreign investors prefer not to be associated with tax haven engagements. Depending on the type of investor (institutional or individual) that is most relevant to a jurisdiction and its sensitivity to reputational aspects, tax havens may comply either in anticipation of the economic costs or after their actual occurrence. The study concludes that – due to the role of reputation – blacklisting is not just a bark but the bite itself (Sharman 2009).

Basically, these results are corroborated by the literature on the relationship between reputation and tax avoidance that with few exceptions has been pointed out practically unanimously across archival studies (Cloyd, Mills and Weaver 2003, Hanlon and Slemrod 2009, deviating: Gallemore, Maydew and Thornock 2014), surveys (EY 2014, Graham, Hanlon, Shevlin and Shroff 2014) and experimental studies (Hardeck and Hertl 2014), that multinational companies are and should be sensitive to reputational concerns when considering their tax planning strategies. Besides being perceived as unpatriotic and not paying their fair share, the expectation that a company's tax aggres-

⁷ In 2000 the OECD also considered material sanctions for listed tax havens (OECD 2000) yet started to abandon the approach that same year (Webb 2004, Sharman 2006). In the literature serious doubts have been raised as to whether sanctions could have materialized at any point in time (for example, Devereux 2002, Gilmore 2002). By contrast, the reputational aspect was never in doubt.

⁸ Some U.S. states' tax authorities pursue a similar strategy by publishing lists of noncompliant taxpayers (e.g., <http://revenue.wi.gov/html/delqlist.html>).

siveness potentially mirrors its aggressiveness in investor affairs also plays an important role in this context (e.g., Desai, Dyck and Zingales 2007, Hanlon and Slemrod 2009).

The OECD's blacklisting essentially has the power to induce such relevant corporate reputational costs by establishing an association between companies' activities in affected tax havens and the impression of aggressive and therefore unfair tax avoidance or even illegal tax evasion. These costs may even outweigh any realizable tax advantages. Foreign investors, in particular in publicly listed companies, could therefore attempt to avoid being associated with such an engagement, even though it may be completely legitimate (Webb 2004, Sharman 2006). This causes tax havens to fear the loss of corporate (and of individual) activities since this means the loss of tax revenue. The observable willingness of blacklisted tax havens to co-operate with the OECD was an expression of this fear. We hence phrase our corresponding hypothesis as follows:

H1: Companies' investment activities are negatively associated with a tax haven's blacklisting status due to reputational concerns.

By investigating explicitly whether companies adapted their investment activities once tax havens were put on the OECD's blacklist, we contribute not only to the literature on tax induced reputational concerns but add also evidence with respect to the relationship between the tax havens' and the companies' reputational costs. Such results could be relevant beyond the concrete initiative at hand since they may be transferable to the more general relationship between the reputation of the suppliers of tax planning opportunities on the one hand and that of the demanding parties on the other, which ultimately translates into real demand effects.

2.3 Commitments to transparency, bilateral information exchange and reputational recovery

With respect to our second research question two strands of literature are of relevance: research on commitments to transparency and the literature on bilateral information exchange.

With respect to the first strand, literature is scarce. Of crucial relevance is the introduction of the "Isle of Man" clause, whose scope of application was extended by the OECD's Progress Report 2001 (OECD 2001) to all tax havens that commit to the OECD's transparency standards. According to this clause, no jurisdiction is obliged to make adjustments until every OECD member country agrees to make the same changes according to a shared timetable. Since Switzerland and Luxembourg, both OECD mem-

bers, refused to participate in the initiative at all, the required commitments to the OECD's standards of transparency essentially involve an obligation to do nothing (Sharman 2006). As a consequence, Nicodème (2009) suspects that many jurisdictions have yet to put in practice their commitments to transparency standards. Hence, there are no material reasons why companies' engagement in tax havens should be affected by the existence of such formal commitments.

Similarly, the effectiveness of bilateral treaties on information exchange, in particular in the context of TIEAs, has been challenged by prior literature (e.g., Neslund 2009, Sheppard 2009, Tax Justice Network 2009, Shaxson and Christensen 2011). First, an information exchange requires the requesting jurisdiction to already have detailed knowledge about the person or company under examination as well as of the alleged offense. A mere suspicion is not enough to trigger this process. As Huizinga and Nicodème (2004) note, an automatic exchange of information represents "the only viable way to enforce taxation" (p. 1104). TIEAs are customarily subject to a much more restricted approach (Keen and Ligthart 2006), i.e., information exchange on request, which may explain the low number of bilateral information exchange requests (Ligthart and Voget 2008, U.S. Government Accountability Office 2011). Second, tax havens tend to collect very little information, which restricts the effectiveness of information sharing per definition (Sullivan 2009). Ligthart and Voget (2008) analyze data on information sharing requests by the Dutch tax authorities. They find, among other things, that countries with low domestic tax rates and those that are net importers of capital – both typical characteristics of tax havens – are less willing to engage in information sharing. Still, one should keep in mind that despite these criticisms, even minor increases in the risk of detection, maybe even only perceived increases, can in practice have a negative impact on investment activities. Finally, however, unlike individuals, companies are assumed to rather use tax havens for legal tax avoidance than for the purpose of illegal tax evasion (e.g., Webb 2004, Dharmapala 2008, Kudrle 2008, Gravelle 2009, Buettner, Holzmann, Overesch and Schreiber 2013). Hence, even if an information exchange based on TIEAs were effective, companies would not have to fear any tax-induced fines. On balance, therefore, TIEAs ought not to have a negative impact on companies' engagement in tax havens because they would not trigger any material consequences.

Unlike commitments to transparency, the bilateral exchange of information has already been subject to numerous empirical analyses. Davies (2004) points out that dou-

ble tax treaties can have two opposing effects on foreign direct investment (FDI). On the one hand, FDI is potentially fostered as both double taxation and existing legal uncertainty are reduced. On the other, the exchange of information is expected to affect FDI negatively due to the possibly increased transparency, which potentially facilitates the home countries' scrutiny. This refers especially to strategic transfer pricing, the fight against which was one of the OECD's initial objectives (OECD 1994). The literature has confused both effects, leading to mixed evidence of tax treaties on FDI (e.g., Blonigen and Davies 2004, Egger, Larch, Pfaffermayr and Winner 2006, Louie and Rousslang 2008). However, Blonigen, Oldenski and Sly (2011) succeed in isolating both expected effects and find that the negative impact of information exchange can even outweigh the positive impact. Both Davies (2004) and Blonigen, Oldenski and Sly (2011) emphasize that this enforcement effect crucially depends on the rigorous implementation and realization of the information exchange.

Huizinga and Nicodème (2004) analyze to which extent international banking flows react on tax policy, including bilateral information exchange. Based on data from the Bank for International Settlements, they find no significant effect and interpret their finding as symptomatic of ineffective enforcement.

Johannesen and Zucman (2014) scrutinize the effect of TIEAs on individuals' bank deposits in tax havens. Based on confidential data provided by the Bank for International Settlements, they show that private bank deposits in tax havens are only modestly affected by a tax haven having signed TIEAs with the resident state of the deposit owner. Moreover, rather than being repatriated to the resident state, the funds are shifted to tax havens that are not yet obliged to exchange information. Neither for Switzerland do they find robust evidence that tax compliance increases due to the signing of a TIEA. As a possible reason for the rather small impact, they mention the potential ineffectiveness of the information exchange, which hardly increases the risk of detection and thus fails to motivate the majority of deposit owners to move their deposits.

Hanlon, Maydew and Thornock (2015) find that inbound foreign portfolio investment from tax havens is negatively associated with TIEAs. They interpret this evidence as a reduction in individual tax evasion due to TIEAs.

Braun and Weichenrieder (2015) find evidence showing that, based on the MiDi database and different specifications of the econometric estimation approach, German multinational companies reduce their activities in tax havens after a TIEA is signed between Germany and the respective tax haven. These results are interpreted as showing

that a loss of secrecy renders it less attractive to invest in such a tax haven, despite low tax rates.

Keeping in mind that the partners involved in the signing of TIEAs in the context of the initiative are often a tax haven and an industrialized country (Bilicka and Fuest 2014), the evidence that suggests a negative influence of TIEAs on FDI makes tax havens' incentives to engage in such an agreement rather unclear. However, particularly in the context of the OECD initiative, commitments to the standards of transparency and TIEAs could serve an additional purpose that so far has been neglected in prior literature: reputation. The OECD emphasizes that both elements are appropriate opportunities to signal co-operation and the willingness to resolve the dispute with the OECD, so that a previously lost reputation may be potentially restored (Owens 2000, Webb 2004 with additional references, Owens 2007). For this reason, and in line with the findings of the majority of studies mentioned before, we state our second hypothesis as follows:

H2: For the group of tax havens that are subject to the OECD initiative, companies' investment activities are positively associated with a tax haven's commitment to the OECD's standards of transparency and its signing of a TIEA with the companies' home country.

Taken together, the findings with respect to both hypotheses will allow comprehensive insights into the effects of the OECD initiative.

3 Data

A distinctive feature of our study lies in our samples, as we make use of two different yet comparable data sets, i.e., a U.S. and a German sample. This section describes in detail the data sets.

3.1 U.S. sample

With respect to the U.S. sample, we rely on a freely accessible data set of Scott Dyreng that has already been used in several publications, such as Dyreng and Lindsey (2009) and Dyreng, Lindsey and Thornock (2013).⁹ The sample contains U.S. incorporated multinational companies for which a CIK number is available in Compustat and whose total assets exceed 10 million USD. Data is available for the period 1995 to 2009.

⁹ The data set is freely available at <https://sites.google.com/site/scottdyreng/>.

Based on the companies' SEC filings, the authors analyze Exhibit 21 of Form 10-K for various years. According to SEC Regulation S-K § 229.601 (21), Exhibit 21 appendices list all significant subsidiaries of a registrant. Using a text scan program, the authors extracted the locations of companies' material operations around the world. Due to the automatic text analysis of the filings, the data carries some noise, e.g., non-English spelling of country names (for more information see <https://sites.google.com/site/scottdyren/ Home/data-and-code/EX21-Dataset>).

For our analysis, we adjust the data in the following four dimensions. First, we focus on those tax havens which are either subject to the OECD initiative (OECD 2000, 2001) or on the tax haven list of Dyreng and Lindsey (2009). Their list is commonly deemed one of the most comprehensive tax haven lists. Second, we exclude years prior to 1998 for reasons of compatibility with our German sample and to ensure the availability of control variables. Third, we exclude companies which are either from the financial services industry (Standard Industrial Classification (SIC) code 6000–6999) or for which a SIC code was not available in Thomson One Banker. Finally, we include only companies and tax havens for which at least one activity is recorded.

3.2 German sample

Our German sample contains companies that belong to the German Prime Standard (DAX, MDAX, TecDAX, SDAX). Companies listed here are subject to both public attention and increased transparency standards. The former indicates that our analysis could produce particular insightful evidence with respect to reputational effects. Publicly listed companies are known to be much more sensitive to negative reputational effects than private unlisted companies (Aerts 1994, Webb 2004, Graham, Hanlon, Shevlin and Shroff 2014). The latter enhances the reliability of our data set. To avert any form of survivorship bias and look-back bias, we follow the indices exactly (Wallmeier 2007), based upon the historical composition of the indices according to Deutsche Börse (2012). We focus on the period 1998 to 2012 since data availability decreases disproportionally before this period.¹⁰

To identify material tax haven activities undertaken by the companies of interest, we did not rely on databases but collected information on our own, based on the companies' annual reports (based on consolidated accounts) which usually also contain a list

¹⁰ 2012 represents the last year for which full information was available at the beginning of the data collection.

of affiliates.¹¹ Our sample specification leads to a total number of 2,736 relevant company-years. The annual reports were collected in three ways. First, we screened the companies' websites. Second, we examined databases of annual reports. Third, we contacted the remaining companies by E-mail asking for the annual reports. Most of the latter were reports from the early years of the identified sample. This enabled us to collect information on 2,634 company-years, which represents coverage of 96.27%.¹² Based on this sample, we extracted the information on tax haven activities using the same list as in the U.S. case. Our operationalization of the companies' investments in tax havens relied on text passages drawn from their annual reports that revealed whether they were active in those tax havens.

To analyze the enormous number of annual reports for references to tax haven activities, we used a rule-based text scan program that is similar to that used to generate the data for the U.S. sample. The software was developed by the Data Center for Qualitative Accounting Research in Passau, Germany. It uses a rule-based framework to identify relevant text passages within an annual report. Unlike a simple key word search, this enables researchers to take the immediate context into account. Using the program, we scanned the entire annual report for the noun and adjective forms of tax haven names plus their inflections. Furthermore, we set some negative conditions to avoid misidentification for the most prominent and at the same time clearly erroneous hits (for example, traditional dishes from a certain area). To ensure the validity and reliability of the final data set, we conducted a manual content analysis of the more than 70,000 text passages which the program identified as relevant. This led us to exclude text passages containing the locations of institutional shareholders and the nationalities of supervisory board members.¹³ For 44 annual reports it proved technically impossible to perform a software-based analysis. Table 1 summarizes the procedure of data generation for the German sample.

The sample adjustments follow those already outlined in Section 3.1. Analogously to the U.S. case, we also excluded companies whose registered headquarter is not in Germany.

¹¹ In some cases we had to rely on annual accounts as annual reports based on consolidated accounts were not available.

¹² This does not necessarily imply that we were unable to obtain the missing annual reports. In many cases it was not evident whether a further annual report exists, for example because the company had filed for bankruptcy in the year in question. Thus, the stated coverage represents a lower bound.

¹³ A complete list of all types of erroneous and excluded text passages is available from the authors on demand.

Table 1 Generation of the German sample

	Number	Coverage
Relevant company-years acc. to Deutsche Börse (2012)	2,736	
Relevant companies	399	
./. Company-years for which information is not obtainable	102	
Available company-years	2,634	96.27%
./. Company-years for which the analysis is technically not possible	44	
Analyzed company-years	2,590	94.66%
Analyzed companies	384	96.24%

The individual steps of the data preparation, in particular the manual review of the individual text passages, were designed to produce high quality data. However, it is worth noting that there remain some caveats with respect to the external validity of our data. This is the case, for example, when the annual report provides only the city of domicile but not the country name. In addition, in a small number of cases we found that the text scanning program failed to recognize some tables that were embedded as images in the electronic version of the annual report.¹⁴ After all, a software-based method is the only way to analyze such a large number of annual reports (for a similar reasoning see also Dyreng and Lindsey 2009). For instance, Dinkel, Keller and Schanz (2014) rely on the DAX 30 companies potentially also due to the effort associated with manual data collection. So from our point of view the data, even if imperfect, represents the best possible approximation we have come across so far. As we rely on consolidated accounts, we also consider material activities in tax havens that may be undertaken by sub-subsidiaries and not directly by the parent. The commonly used Midi database that provides anonymous evidence on micro data of foreign direct investments by German companies (e.g., Gumpert, Hines and Schnitzer 2012, Braun and Weichenrieder 2015), for instance, only records such activities under even stricter premises (Lipponer 2011). In terms of precision, then, the quality of the data therefore clearly exceeds that of this and other commonly known databases (for a discussion of these databases' shortcomings, see Dinkel, Keller and Schanz 2014).

¹⁴ In those cases in which we verified the country names mentioned in such tables, the names were also mentioned in another section of the annual report, so that no information was lost.

4 Methodology

As outlined in Sections 2.2 and 2.3, our hypotheses refer to the reputational effects of the consecutive steps of the OECD initiative. In this respect Hypothesis 1 focuses on the OECD's name-and-shame-strategy that potentially induces reputational costs. To assess this effect, we use the following estimation approach:

$$\begin{aligned} Investment_{i,j,t} = & \alpha + \beta \cdot Blacklist_j \cdot PostPublication_t \\ & + \gamma \cdot Controls_{j,t} + \delta_{Country} + \delta_{Year} + \varepsilon_{i,j,t} \end{aligned} \quad (1)$$

From the samples described in Section 3, we extract the information to construct $Investment_{i,j,t}$, which represents a binary variable, taking the value one if company i undertakes material activities in tax haven j in year t and zero otherwise. This neglects more detailed information on the number of investment activities carried out by a company per tax haven and year. Our results therefore represent rather conservatively estimated lower bound effects. In the same vein, it should be noted that further underestimation of real investments occurs because we neglect further adjustments to the investment activities of local suppliers of the companies in question. In a reputational context, this binary variable seems particularly appropriate since it reflects a company's general attitude towards investing in a specific host country.

The coefficient of main interest is β which is attributable to the interacted term $Blacklist_j \cdot PostPublication_t$. $Blacklist_j$ is a binary variable, which indicates the appearance of tax haven j on the OECD's initial tax haven list (OECD 2000). We refer to this set of tax havens as the treatment group. As a consequence, all remaining tax havens that were not on the OECD's blacklist form our control group. Table 2 details the composition of both the treatment and the control group.

The second element of the interacted variable is $PostPublication_t$, which marks the years after the first publication of this list, i.e., after 2000.¹⁵ Hence, the chosen approach

¹⁵ As the blacklist was published in 2000, we alternatively define $PostPublication_t$ in untabulated robustness checks to indicate the years after 1999 in order to also capture a potential impact in the year of publication. The results remain qualitatively unchanged.

Table 2 Tax havens within the scope of this study and their appearance on the initial OECD tax haven list

Tax haven	Blacklist (Treatment)	Non- Blacklist (Control)	Tax haven	Blacklist (Treatment)	Non- Blacklist (Control)
Andorra	✓		Liechtenstein	✓	
Anguilla	✓		Luxembourg		✓
Antigua & Barbuda	✓		Macau		✓
Aruba	✓		Maldives	✓	
Bahamas	✓		Malta*		✓
Bahrain	✓		Marshall Islands	✓	
Barbados	✓		Mauritius*		✓
Belize	✓		Monaco	✓	
Bermuda*		✓	Montserrat	✓	
Botswana		✓	Nauru	✓	
British Virgin Islands	✓		Netherlands Antilles	✓	
Brunei Darussalam		✓	Niue	✓	
Cape Verde		✓	Palau		✓
Cayman Islands*		✓	Panama	✓	
Cook Islands	✓		Samoa	✓	
Costa Rica		✓	San Marino*		✓
Cyprus*		✓	Seychelles	✓	
Dominica	✓		Singapore		✓
Gibraltar	✓		St. Christopher & Nevis	✓	
Grenada	✓		St. Lucia	✓	
Guernsey	✓		St. Vincent & the Grenadines	✓	
Ireland		✓	Switzerland		✓
Isle of Man	✓		Tonga	✓	
Jersey	✓		Turks & Caicos Islands	✓	
Latvia		✓	Uruguay		✓
Lebanon		✓	U.S. Virgin Islands	✓	
Liberia	✓		Vanuatu	✓	

Tax havens within the scope of this study according to OECD (2000, 2001) and Dyreng and Lindsey (2009), broken down by their appearance or non-appearance on the initial OECD tax haven list (OECD 2000). * indicates tax havens that were not on the initial OECD tax haven list as they signed an advance commitment, although they were subject to the OECD initiative.

basically represents a typical difference-in-difference regression framework (Angrist and Pischke 2009).¹⁶

To shed further light on the blacklisting effect, we extend this baseline approach by dividing $PostPublication_t$ in three (in the German case four) dummy variables, each focusing on a period of three consecutive years (2001–2003, 2004–2006, 2007–2009, 2010–2012). Letting each of these dummy variables interact with $Blacklist_j$ allows us to draw further conclusions with respect to the temporal development of the overall blacklisting effect.

Based on related prior literature concerned with investment decisions (e.g., Brainard 1997, Carr, Markusen and Maskus 2001, Buch, Kleinert, Lipponer and Toubal 2005, Overesch and Wamser 2009, 2010, Barrios, Huizinga, Laeven and Nicodème 2012, Hanlon, Maydew and Thornock 2015) we include a set of commonly used control variables. Detailed definitions as well as data sources can be found in Appendix 1.

First, in order to control for differences in tax havens' market size, we introduce $LN\ GDP_{j,t}$, which reflects the natural logarithm of tax haven j 's gross domestic product in year t measured in constant U.S. dollars of the year 2005.

Second, we control for differences in the host countries' political situation and hence the environment faced by foreign investments in a broader sense. Based on the Worldwide Governance Indicators of the World Bank (Kaufmann, Kraay and Mastruzzi 2010) and similar to Dharmapala and Hines (2009), we include $Governance_{j,t}$, which reflects the perception of host country j 's governance quality in year t . Higher values indicate better governance quality.

Third, despite our rather homogeneous sample of host countries, which are all assumed to be tax havens and therefore all offer a tax-related investment-friendly envi-

¹⁶ In the accounting literature the examination whether the underlying sample fulfills the common trend assumption does not play a major role (e.g., Diller and Theelen 2014, Alberternst and Sureth 2015, Hanlon, Maydew and Thornock 2015). A detailed examination, however, is often carried out in the economics literature. Unfortunately this is not possible in a meaningful way for both of our samples due to the unavailability of data prior to 1998 for the German sample. For the two years of the pre-treatment period, we compare the treatment and control group with respect to country-specific characteristics. We find that both groups are statistically different only with respect to determinants of country size. As we introduce a comprehensive set of control variables, we should be able to at least mitigate such concerns. A more detailed analysis of the raw data for the U.S. sample for the period 1995–1999 indicates, furthermore, that the common trend assumption can be approved at least for a subset of tax havens that excludes the biggest countries in the control group, e.g., Switzerland, Luxembourg and Ireland (which are OECD member states). As the results for this subset based on raw data are in line with those of the entire sample, the validity of our analysis is further strengthened. However, still the results should be interpreted with caution.

ronment, we also consider $Tax\ Rate_{j,t}$, which represents the top statutory corporate income tax rate of host country j in year t .¹⁷

Fourth, we control for differences in the availability of labor skills and human capital in the respective host countries. Based on the assumption that higher productivity is reflected in higher GDP per capita, we use $Similarity_{j,t}$ as a proxy for the similarity of the host country j 's and the companies' home country's endowment with labor skills in year t (Buch, Kleinert, Lipponer and Toubal 2005). The variable can take values between zero and one and is calculated based upon the respective countries' GDP per capita in constant U.S. dollars of the year 2005. The closer the value is to one, the higher the similarity.

Fifth, we use $LN\ Phone_{j,t}$, the natural logarithm of tax haven j 's number of fixed-telephone subscriptions per 100 inhabitants in year t as proxy to control for differences in technological development.

Finally, we also consider $LN\ Population_{j,t}$, defined as the natural logarithm of host country j 's population in year t .

Besides the tax haven-specific time-variant factors, we additionally implement year-fixed effects (δ_{Year}) and country-fixed effects ($\delta_{Country}$).¹⁸ In our case year-fixed effects are particularly important in the light of the financial crisis as well as of tax-related legal amendments¹⁹ and regulatory changes²⁰ in the companies' home country. Country-fixed effects on the other hand control explicitly for time-invariant tax haven-specific factors, such as historical background, resource endowment and distance from the home

¹⁷ We are aware that the statutory tax rate is generally not sufficient to control for country-specific tax incentives (e.g., Devereux and Griffith 1998). However, as tax-specific data is generally extremely difficult to obtain for our set of tax havens, this single variable is the best proxy available (see also Hanlon, Maydew and Thornock 2015).

¹⁸ In untabulated robustness checks, we additionally introduce industry-fixed effects and alternatively company-fixed effects. The results remain qualitatively unchanged.

¹⁹ Relevant examples are, among others, Germany's 2009 Act to Combat Tax Evasion (*Steuerhinterziehungsbekämpfungsgesetz*), the U.S. tax holiday under the 2004 American Jobs Creation Act, and the Stop Tax Haven Abuse Act in 2009.

²⁰ Relevant examples of regulatory changes in the accounting system are the introduction of the more detailed German Accounting Standards (DRS) 5 and 15 on management commentary in 2004, the change from German GAAP to IFRS in 2005, and the replacement of IAS 14 by IFRS 8 in 2009. Moreover, the enforcement system changed when the German Financial Reporting Enforcement Panel was introduced in 2004 and the balance sheet oath came into force in 2007. Noteworthy changes in the U.S. include legislation passed to ensure more transparency and disclosure, such as the Sarbanes-Oxley Act of 2002, as well as subsequent numerous changes designed to increase disclosure with relevance to tax issues (Donohoe, McGill and Outslay 2014).

country.²¹ These fixed effects also capture the main effects of the individual variables of which our interaction terms are composed (e.g., Hanlon, Maydew and Thornock 2015).

Despite the binary nature of our dependent variable, we have deliberately chosen not to rely on logistic or probit regression models, as non-linear binary response models potentially suffer from the incidental parameter problem, that is, inconsistent estimators within the context of panel data and fixed effects (Neyman and Scott 1948, Lancaster 2000). Although conditional fixed-effects logistic regression models (e.g., Andersen 1970, Chamberlain 1980, Charbonneau 2014) are a potential solution in this respect, our complex panel data structure does not allow for an appropriate application. We avoid the incidental parameter problem by adopting a linear probability model with heteroscedasticity-robust standard errors (Greene 2004), which is a commonly used approach (e.g., Williams 2013, Hanlon and Hoopes 2014). As a side effect, the linear probability model – unlike logistic regression models – allows for a straightforward interpretation of the estimated coefficients (Angrist and Pischke 2009), a feature that is especially valuable when interpreting interacted variables (Ai and Norton 2003). A problem commonly associated with linear probability models is the danger of resulting estimates outside the reasonable range of probabilities, i.e., below 0 or above 1. However, as the fitted values of our various models fall within [-0.0259, 0.7769], this objection is not a major concern in our case. To take residual correlation into account (Bertrand, Duflo and Mullainathan 2004), we follow the suggestion of Cameron, Gelbach and Miller (2008) to cluster standard errors at not too fine a level and therefore cluster only at the company level.²²

Hypothesis 2 focuses on the initiative's elements following the initial blacklisting. The reputational effects induced by a tax haven's commitment to the OECD's standards of transparency as well as by its signing of an agreement to exchange tax-relevant information are examined based on the following estimation approach:

$$\begin{aligned} Investment_{i,j,t} = & \alpha + \beta_1 \cdot Commitment_{j,t} + \beta_2 \cdot TIEA_j \cdot PostEffective_t \\ & + \gamma \cdot Controls_{j,t} + \delta_{Country} + \delta_{Year} + \varepsilon_{i,j,t} \end{aligned} \quad (2)$$

We apply this model exclusively to those tax havens that were subject to the OECD initiative. First, as only these countries were obliged to formally commit to transparency

²¹ We refrain from taking into account an indicator variable for EU membership, since its inclusion is not meaningful to analyze the U.S. sample. Moreover, as EU membership is to a large extent time-invariant due to the selection of tax havens, potential effects should be captured by the country-fixed effects.

²² In untabulated robustness checks, we alternatively cluster standard errors at the company and host country level (two-way clustering). The presented significances remain qualitatively unchanged.

standards, it would not be appropriate to include the additional non-OECD tax havens in the estimation sample. Second, signing a TIEA can have several reasons. As Hypothesis 2 focuses explicitly on the potential recovery of reputation lost in the course of the dispute with the OECD, this sample restriction mitigates confusing effects due to the otherwise potentially increased variance of reasons – that were laid out in the literature review in Section 2.3 – that could have induced the signing of TIEAs.

In this setting the main variables of interest are $Commitment_{j,t}$ and $TIEA_j \cdot PostEffective_t$.²³ $Commitment_{j,t}$ and $TIEA_j$ represent binary variables, which indicate either that a formal commitment to the standards of transparency exists for tax haven j in year t or that a TIEA exists between tax haven j and the respective home country. Agreements are neglected for those cases in which the OECD's review process finds that they do not meet the required standards. As the exchange of information can also be conducted based on double tax treaties (DTT), we also consider those agreements provided there is no corresponding TIEA. $PostEffective_t$ encompasses two alternative operationalizations: first, the baseline approach considers $PostSigning_t$, which indicates the years after the signing of such an agreement. Second, $PostInForce_t$ indicates the years after the agreement came into force. Table 3 presents the various years relevant for the construction of the respective variables.

In analogy to the setting referred to in Hypothesis 1, we use the same estimation strategy, including the linear probability model with heteroscedasticity-robust standard errors clustered at the company level and the same set of control variables, including also country- and year-fixed effects.

²³ In contrast to $TIEA \cdot PostEffective$, $Commitment$ does not require an operationalization as interacted variable, as any tax haven in this sample has signed such a commitment.

Table 3 Tax haven-specific dates associated with the OECD initiative

Tax haven	Commitment	TIEA/DTT signed		TIEA/DTT in force	
		U.S.	Germany	U.S.	Germany
Andorra	2009		2010		2012
Anguilla	2002		2010		2011
Antigua & Barbuda	2002	2001	2010	2003	2012
Aruba	2002	2003		2004	
Bahamas	2002	2002	2010	2006	2011
Bahrain	2001				
Barbados		1984		1984	
Belize	2002				
Bermuda	2000	1988	2009	1988	2012
Botswana					
British Virgin Islands	2002	2002	2010	2006	2011
Brunei Darussalam					
Cape Verde					
Cayman Islands	2000	2001	2010	2006	2011
Cook Islands	2002		2012		
Costa Rica		1989		1991	
Cyprus	2000	1984	2011	1985	2011
Dominica	2002				
Gibraltar	2002	2009	2009	2009	2010
Grenada	2002	1986	2011	1987	
Guernsey	2002	2002	2009	2006	2010
Ireland		1997	2011	1998	2012
Isle of Man	2000	2002	2009	2006	2010
Jersey	2002	2002	2008	2006	2009
Latvia		1998	1997	2000	1998
Lebanon					
Liberia	2007				
Liechtenstein	2009	2008	2009	2009	2010
Luxembourg			2012		
Macau					
Maldives					
Malta	2000	2008	2001		2001
Marshall Islands	2007	1991		1991	
Mauritius	2000		2011		2012
Monaco	2009	2009	2010		2011
Montserrat	2002		2011		2012
Nauru	2003				
Netherlands Antilles	2000	2002		2007	
Niue	2002				
Palau					
Panama	2002				
Samoa	2002				
San Marino	2000		2010		2011

Table 3 cont'd Tax haven-specific dates associated with the OECD initiative

Tax haven	Commitment	TIEA/DTT signed		TIEA/DTT in force	
		U.S.	Germany	U.S.	Germany
Seychelles	2001				
Singapore					
St. Christopher & Nevis	2002		2010		
St. Lucia	2002	1987	2010		
St. Vincent & the Grenadines	2002		2010		2011
Switzerland			1971		1972
Tonga					
Turks & Caicos Islands	2002		2010		2011
Uruguay			2010		2012
U.S. Virgin Islands	2002				
Vanuatu	2003				

Commitment refers to the year a tax haven formally committed to the OECD's transparency standards. TIEA/DTT signed (in force) refers to the year a tax information exchange agreement (TIEA) or a double tax treaty (DTT) with the United States or Germany, respectively, was signed (came into force). TIEAs are only considered if fulfilling the international standards and if relevant to the analysis, i.e., only those prior to 2010 for the U.S. and prior to 2013 for Germany. DTTs are only considered in the absence of a corresponding TIEA. The information stems from www.oecd.org and www.eoi-tax.org.

5 Results

5.1 Descriptive statistics

Table 4 provides descriptive statistics for the two data sets underlying the following analysis.²⁴

With respect to the dependent variable *Investment*, both samples generally exhibit a certain similarity. Whereas the U.S. sample shows tax haven activities in 5.3% of all observations, the German sample has a slightly lower value of 4.8%. Figure 1 sheds additional light on this aspect, as the top ten relevant tax havens are presented for both samples. Figure 1 is based on the tax havens' share of activities, which is calculated as the tax haven-specific number of activities across all years divided by the total sum of activities. In addition to this host country-specific evaluation, we present the analogously calculated cumulated share of activities in tax havens subject to the OECD initiative.

²⁴ Table 9 in Appendix 2 also presents a correlation matrix. Naturally, the correlation between *LN Population* and *LN GDP* is slightly increased. However, this should not raise any concerns with respect to multicollinearity.

Table 4 Descriptive statistics for the U.S. sample (Panel A) and the German sample (Panel B)

Panel A: U.S. sample

	Obs.	Mean	Median	Std. Dev.	Min.	Max.
<i>Investment</i>	754,137	0.053	0.000	0.225	0.000	1.000
<i>GDP</i>	734,698	19,001.400	2,856.256	60,566.780	44.707	423,512.400
<i>Governance</i>	754,137	-0.003	0.018	1.002	-3.793	1.701
<i>Tax Rate</i>	698,274	0.218	0.250	0.132	0.000	0.449
<i>Similarity</i>	732,417	0.378	0.334	0.277	0.004	0.999
<i>Population</i>	745,928	901.075	109.045	1,578.756	4.377	7,742.765
<i>Phone</i>	705,392	40.419	38.403	24.173	0.054	118.905

Panel B: German sample

	Obs.	Mean	Median	Std. Dev.	Min.	Max.
<i>Investment</i>	97,800	0.048	0.000	0.215	0.000	1.000
<i>GDP</i>	95,330	20,160.690	3,041.763	63,011.420	21.096	439,795.800
<i>Governance</i>	97,800	-0.031	-0.057	1.002	-3.793	1.701
<i>Tax Rate</i>	90,695	0.207	0.250	0.130	0.000	0.449
<i>Similarity</i>	95,135	0.394	0.323	0.273	0.006	0.990
<i>Population</i>	96,976	944.439	176.280	1,623.167	4.377	7,997.399
<i>Phone</i>	89,563	39.748	36.514	24.480	0.000	121.725

The variables largely follow their definitions as stated in Section 4 and in Appendix 1. *GDP* is presented in millions of U.S. dollars, *Population* in thousands.

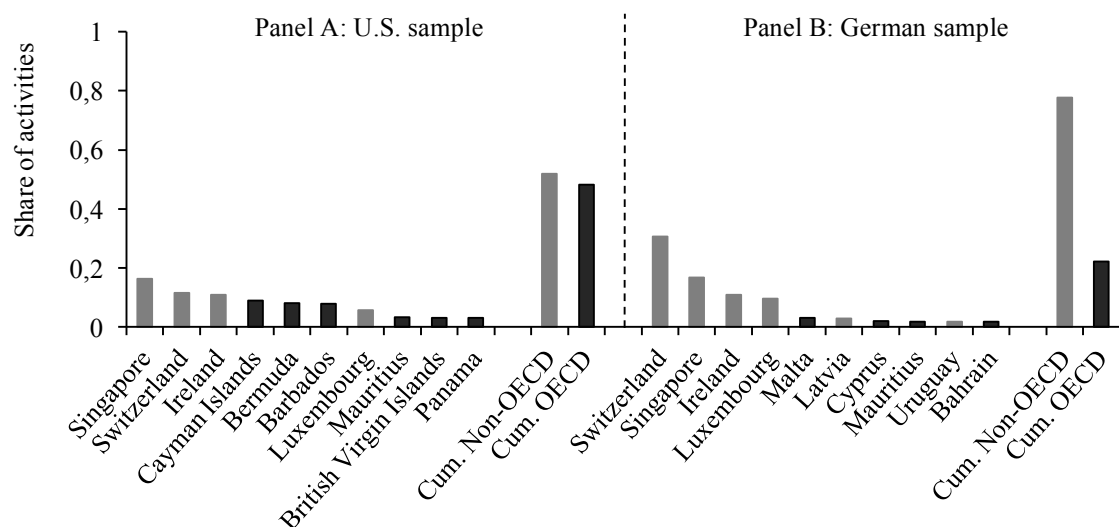


Figure 1 Top ten relevant tax havens for companies in the U.S. sample (Panel A) and the German sample (Panel B). The share of activities is calculated as the number of activities in a certain tax haven over all years divided by the sum of activities over all tax havens and all years. Cum. OECD and Cum. Non-OECD represent the cumulated values for tax havens subject or not subject to the OECD initiative, respectively (see Table 2). Black (grey) bars identify tax havens that are (not) subject to the OECD initiative. Blacklisted tax havens are considered as being subject to the OECD initiative.

Figure 1 indicates that of the ten most relevant tax havens, in the U.S. case six and in the German case four were subject to the OECD initiative. This impression is corroborated by the cumulative share of activities in OECD tax havens. Whereas in the U.S. sample 48% of tax haven activities occur in OECD tax havens, in the German sample they only account for 22%. This suggests that the OECD initiative has generally been more relevant for U.S. companies than for their German counterparts. This could be due to the geographical proximity of several OECD tax havens to the United States.

5.2 Results for Hypothesis 1: The effect of the OECD's blacklisting strategy on companies' investment activities

Before we focus on the results of the multivariate analysis, Figure 2 graphically depicts the development of the proportion of companies' investment activities in blacklisted tax havens. This provides us with first descriptive evidence of an impact of the OECD's blacklisting.

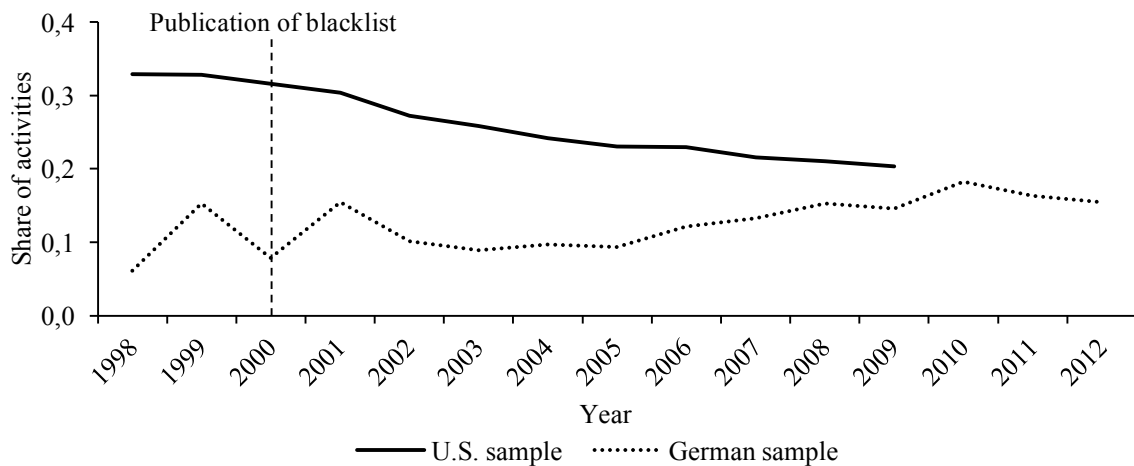


Figure 2 Temporal development of the relevance of blacklisted tax havens (see Table 2). The share of activities is calculated as the number of activities in blacklisted tax havens in a certain year divided by the sum of activities over all tax havens in that year. The solid line refers to the U.S. sample, the dotted line refers to the German sample.

Figure 2 tells us that in the U.S. – after a similar level of activities in 1998 and 1999 – the importance of blacklisted tax havens based on the share of activities clearly declined after the blacklisting date, which is consistent with our Hypothesis 1. In Germany, by contrast, this seems less evident. To draw reasonable conclusions, we continue by applying our estimation approach as presented in Section 4. Table 5 reports the corresponding results for the U.S. sample.

Selected measures showing the overall model fit for our various estimations are presented at the bottom of Table 5. Keeping in mind that R^2 tends to be misleading in our case owing to the binary nature of our dependent variable, we complement this measure

by the ratio of correctly classified observations, which is calculated based on the fitted values for the estimated models and a threshold of 0.5. The calculated hit ratio always exceeds the value of the proportional chance criterion, which expresses the expected hit ratio by random group assignment under consideration of the dependent variable's group proportions (Morrison 1969, Hosmer and Lemeshow 2000). The indicated model fit therefore generally seems good.

Consistent with Hypothesis 1 we find a highly significant negative effect of a tax haven's blacklisting status on a company's propensity to be active in a specific tax haven. This finding is robust to the inclusion of various sets of control variables as indicated by Models 1 to 3. We interpret this finding based on the mechanism described in Section 2.2 as a result of induced reputational concerns. Companies try to avoid being associated with activities in countries officially labeled as tax havens and therefore tend to refrain from such activities. Hence, the reputational costs of companies are passed to tax havens and translate into economic costs. As for the magnitude of this effect, we find that blacklisting reduces the propensity of a company to invest in a tax haven by approximately 3.5 percentage points (Model 3). Comparing this amount with the mean value of *Investment*, which is 5.3% in the U.S. sample according to Table 4, blacklisting is also economically highly significant.

Model 4 sheds more light on this result by focusing on the temporal development of the effect. The division into several subperiods indicates that the effect's magnitude grows over time. Whereas the initial impact of blacklisting was only 2 percentage points during the period 2001–2003, it went up to 4.9 percentage points during the period 2007–2009. This result potentially reflects that the relocation of operations requires a certain amount of time. An alternative (or additional) reason could be that the inclusion of reputational concerns in companies' tax risk management has grown over time following the increasing significance of companies' public visibility in the press and media (EY 2014). Keeping in mind that many affected tax havens co-operated relatively fast, our results confirm that the reputational effects of the OECD initiative have been remarkably sustainable. This finding is largely in line with prior literature on reputation building and destruction, which finds that it is relatively easy to destroy a company's reputation, whereas to recover its reputation later is a challenging and long-winded affair (Fombrun, Gardberg and Barnett 2000, Schwalbach 2001).

Table 5 Regression results for Hypothesis 1 and the U.S. sample

	Pred. Sign	Model 1		Model 2		Model 3		Model 4	
<i>Blacklist · PostPublication</i>	-	-0.03198	***	-0.03294	***	-0.03503	***		
		(0.00209)		(0.00211)		(0.00214)			
<i>Blacklist · Years01–03</i>	-							-0.01984	***
								(0.00191)	
<i>Blacklist · Years04–06</i>	-							-0.03804	***
								(0.00252)	
<i>Blacklist · Years07–09</i>	-							-0.04907	***
								(0.00293)	
<i>TIEA · PostSigning</i>						0.01580	***	0.01832	***
						(0.00162)		(0.00162)	
<i>LN GDP</i>		0.02293	***	0.01509	**	0.02625	***	0.02665	***
		(0.00345)		(0.00657)		(0.00641)		(0.00640)	
<i>Governance</i>		0.00014		0.00107		0.00268	***	0.00302	***
		(0.00075)		(0.00074)		(0.00072)		(0.00072)	
<i>Tax Rate</i>		0.02765	***	0.01618		0.02434	**	0.03019	***
		(0.00922)		(0.01121)		(0.01134)		(0.01129)	
<i>Similarity</i>				0.00968		0.00140		-0.01093	
				(0.01537)		(0.01526)		(0.01528)	
<i>LN Population</i>				0.04238	***	0.03780	***	0.04467	***
				(0.00850)		(0.00834)		(0.00841)	
<i>LN Phone</i>				-0.00044		-0.00117		-0.00200	
				(0.00168)		(0.00167)		(0.00167)	
<i>Constant</i>		-0.49257	***	-0.80200	***	-0.98015	***	-1.05077	***
		(0.07415)		(0.10913)		(0.11151)		(0.11162)	
Country-fixed effects		Yes		Yes		Yes		Yes	
Year-fixed effects		Yes		Yes		Yes		Yes	
Observations		681,116		649,711		649,711		649,711	
R ²		0.1781		0.1792		0.1793		0.1797	
Correctly classified		0.9413		0.9393		0.9393		0.9393	
Prop. chance criterion		0.8895		0.8860		0.8860		0.8860	

We estimate a linear probability model with *Investment* as the dependent variable, which takes the value one if company *i* undertakes material activities in tax haven *j* in year *t* and zero otherwise. Independent variables follow their definitions as stated in Section 4 and Appendix 1. Country- and year-fixed effects are included. Heteroscedasticity-robust standard errors clustered at the company level are presented in parentheses. The ratio of correctly classified observations is calculated based on a threshold of 0.5. The proportional chance criterion is calculated following Morrison (1969). * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

Table 6 Regression results for Hypothesis 1 and the German sample

	Pred. Sign	Model 1		Model 2		Model 3		Model 4	
<i>Blacklist · PostPublication</i>	-	-0.01668	***	-0.01529	***	-0.01542	***		
		(0.00498)		(0.00491)		(0.00493)			
<i>Blacklist · Years01–03</i>	-							0.00076	
								(0.00398)	
<i>Blacklist · Years04–06</i>	-							-0.01290	*
								(0.00696)	
<i>Blacklist · Years07–09</i>	-							-0.02241	***
								(0.00705)	
<i>Blacklist · Years10–12</i>	-							-0.03918	***
								(0.00897)	
<i>TIEA · PostSigning</i>						-0.00475		-0.00186	
						(0.00322)		(0.00315)	
<i>LN GDP</i>		0.02051	***	0.01856	**	0.01601	**	0.01492	*
		(0.00814)		(0.00812)		(0.00793)		(0.00794)	
<i>Governance</i>		0.00450		0.00505	***	0.00537	***	0.00523	***
		(0.00173)	***	(0.00164)		(0.00168)		(0.00166)	
<i>Tax Rate</i>		-0.04217	***	-0.04615	**	-0.04766	**	-0.04959	**
		(0.02079)		(0.01998)		(0.01993)		(0.01976)	
<i>Similarity</i>				0.02990		0.03243		-0.01305	
				(0.02073)		(0.02074)		(0.02175)	
<i>LN Population</i>				0.02065		0.02021		0.03195	
				(0.02448)		(0.02450)		(0.02468)	
<i>LN Phone</i>				0.00311	*	0.00327	*	0.00238	
				(0.00172)		(0.00173)		(0.00170)	
<i>Constant</i>		-0.44321	***	-0.67165	**	-0.61462	**	-0.67615	**
		(0.17570)		(0.30991)		(0.30913)		(0.31085)	
Country-fixed effects		Yes		Yes		Yes		Yes	
Year-fixed effects		Yes		Yes		Yes		Yes	
Observations		88,420		82,524		82,524		82,524	
R ²		0.3282		0.3335		0.3335		0.3342	
Correctly classified		0.9575		0.9556		0.9556		0.9556	
Prop. chance criterion		0.8991		0.8943		0.8943		0.8943	

We estimate a linear probability model with *Investment* as the dependent variable, which takes the value one if company *i* undertakes material activities in tax haven *j* in year *t* and zero otherwise. Independent variables follow their definitions as stated in Section 4 and Appendix 1. Country- and year-fixed effects are included. Heteroscedasticity-robust standard errors clustered at the company level are presented in parentheses. The ratio of correctly classified observations is calculated based on a threshold of 0.5. The proportional chance criterion is calculated following Morrison (1969). * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

We apply essentially the same analysis to our sample of German companies. Table 6 shows the corresponding findings. The model fit measures presented at the bottom of Table 6 are largely in line with Table 5 and again indicate an overall good model fit.

Our results for the German sample as presented in Table 6 are also found to be generally consistent with Hypothesis 1 and therefore essentially confirm our findings for the U.S. sample. The observable magnitude of the negative impact of blacklisting on a company's propensity to invest in a tax haven is found to be 1.5 percentage points. In contrast to the findings in the U.S. sample, we observe no significant impact immediately after the publication of the blacklist based on Model 4. However, we subsequently find a growing significant effect over the long term which peaks at 3.9 percentage points in the period 2010–2012, which is – given the mean value of *Investment* of 4.8% according to Table 4 – again highly significant also in economic terms.

All in all, our analysis indicates that Hypothesis 1 can be confirmed for both samples. The evidence suggests that the perceived reputational loss induced by the OECD's blacklisting is not just a national, but also a transnational phenomenon.

5.3 Results for Hypothesis 2: The effect of tax havens' signing of formal commitments and TIEAs on companies' investment activities

This section focuses on the initiative's elements that potentially offered tax havens that have been subject to the OECD initiative an opportunity to restore their reputational loss, namely by pledging formally to comply with the standards of transparency and cooperation and signing bilateral TIEAs. Starting with the analysis of the U.S. sample, Table 7 presents the corresponding results.

Again, an overall good model fit is realized for all estimated models. As indicated in Table 7, Hypothesis 2 is fully confirmed based on the U.S. sample. Both subsequent steps provided by the OECD initiative, i.e., formal commitments and bilateral TIEAs, are found to have a highly significant positive effect on companies' propensity to be active in a certain tax haven. Hence, both steps to leave the blacklist – the carrots, so to speak – have allowed tax havens to recover at least some of their reputation and to regain their attractiveness for foreign investors. Consequently, even if TIEAs offer increased opportunities for scrutiny by home countries' tax authorities – which has been heavily challenged in the literature – the findings indicate that the recovery of reputation overcompensates for this negative impact.

Table 7 Regression results for Hypothesis 2 and the U.S. sample

	Pred. Sign	Model 1		Model 2		Model 3		Model 4	
<i>Commitment</i>	+	0.00981	***	0.01232	***	0.01118	***	0.01373	***
		(0.00117)		(0.00128)		(0.00121)		(0.00132)	
<i>TIEA · PostSigning</i>	+	0.01384	***	0.01386	***				
		(0.00148)		(0.00146)					
<i>TIEA · PostInForce</i>	+					0.01456	***	0.01515	***
						(0.00175)		(0.00177)	
<i>LN GDP</i>		0.02676	***	-0.00495		0.02465	***	-0.00801	
		(0.00346)		(0.00712)		(0.00349)		(0.00715)	
<i>Governance</i>		-0.00075		0.00028		-0.00060		0.00054	
		(0.00071)		(0.00073)		(0.00069)		(0.00071)	
<i>Tax Rate</i>		0.05654	***	0.05976	***	0.06495	***	0.07027	***
		(0.00876)		(0.01045)		(0.00915)		(0.01068)	
<i>Similarity</i>				0.08748	***			0.09287	***
				(0.01742)				(0.01754)	
<i>LN Population</i>				0.06321	***			0.06089	***
				(0.01022)				(0.01004)	
<i>LN Phone</i>				0.01269	***			0.01562	***
				(0.00153)				(0.00162)	
<i>Constant</i>		-0.57109	***	-0.71651	***	-0.52622	***	-0.64128	***
		(0.07436)		(0.09712)		(0.07491)		(0.09657)	
Country-fixed effects		Yes		Yes		Yes		Yes	
Year-fixed effects		Yes		Yes		Yes		Yes	
Observations		517,143		485,738		517,143		485,738	
R ²		0.1135		0.1161		0.1136		0.1161	
Correctly classified		0.9631		0.9617		0.9631		0.9617	
Prop. chance criterion		0.9289		0.9263		0.9289		0.9263	

We estimate a linear probability model for the sample of tax havens subject to the OECD initiative (see Table 2) with *Investment* as the dependent variable, which takes the value one if company *i* undertakes material activities in tax haven *j* in year *t* and zero otherwise. Independent variables follow their definitions as stated in Section 4 and Appendix 1. Country- and year-fixed effects are included. Heteroscedasticity-robust standard errors clustered at the company level are presented in parentheses. The ratio of correctly classified observations is calculated based on a threshold of 0.5. The proportional chance criterion is calculated following Morrison (1969). * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

Table 8 Regression results for Hypothesis 2 and the German sample

	Pred. Sign	Model 1		Model 2		Model 3		Model 4	
<i>Commitment</i>	+	-0.00701	**	-0.00580	**	-0.00695	**	-0.00589	**
		(0.00270)		(0.00274)		(0.00268)		(0.00275)	
<i>TIEA · PostSigning</i>	+	0.00727	***	0.00268					
		(0.00266)		(0.00259)					
<i>TIEA · PostInForce</i>	+					0.00914	***	0.00588	
						(0.00344)		(0.00357)	
<i>LN GDP</i>		0.02090	**	0.02487	***	0.02034	**	0.02519	***
		(0.00908)		(0.00881)		(0.00873)		(0.00876)	
<i>Governance</i>		0.00020		0.00108		0.00075		0.00125	
		(0.00133)		(0.00122)		(0.00134)		(0.00122)	
<i>Tax Rate</i>		-0.01830		-0.01012		-0.01557		-0.00892	
		(0.01804)		(0.01678)		(0.01766)		(0.01678)	
<i>Similarity</i>				0.02122				0.02085	
				(0.02040)				(0.02037)	
<i>LN Population</i>				0.01532				0.01469	
				(0.02343)				(0.02340)	
<i>LN Phone</i>				0.00361	**			0.00345	**
				(0.00169)				(0.00170)	
<i>Constant</i>		-0.45922	**	-0.75005	**	-0.44715	**	-0.74886	**
		(0.19628)		(0.29778)		(0.18886)		(0.29656)	
Country-fixed effects		Yes		Yes		Yes		Yes	
Year-fixed effects		Yes		Yes		Yes		Yes	
Observations		64,825		58,929		64,825		58,929	
R ²		0.0244		0.0254		0.0245		0.0255	
Correctly classified		0.9840		0.9839		0.9840		0.9839	
Prop. chance criterion		0.9685		0.9683		0.9685		0.9683	

We estimate a linear probability model for the sample of tax havens subject to the OECD initiative (see Table 2) with *Investment* as the dependent variable, which takes the value one if company *i* undertakes material activities in tax haven *j* in year *t* and zero otherwise. Independent variables follow their definitions as stated in Section 4 and Appendix 1. Country- and year-fixed effects are included. Heteroscedasticity-robust standard errors clustered at the company level are presented in parentheses. The ratio of correctly classified observations is calculated based on a threshold of 0.5. The proportional chance criterion is calculated following Morrison (1969). * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

The findings are robust to the inclusion of various sets of control variables and, in addition, to alternative operationalizations of *PostEffective*.²⁵ Taking into account the date at which TIEAs actually came into force instead of the signing date, the effect is found to increase slightly in magnitude. In Model 4 we find that a tax haven's formal commitment to transparency increases companies' propensity to be active by 1.4 percentage points. The existence of an effective TIEA with the companies' home country leads to an additional increase of 1.5 percentage points. As the mean value of *Investment* is found to be 3.4% within this subsample of tax havens that are subject to the OECD initiative, both effects can be considered economically highly significant. Focusing finally on the German sample, Table 8 presents the corresponding results for Hypothesis 2.

With respect to the impact of a tax haven's formal commitment to the standards of transparency, we are not able to confirm Hypothesis 2 based on our German sample. As shown in Table 8, commitments are found to exert a significant but negative impact on companies' propensity to be active in a specific tax haven. One potential reason for this finding is Germany's tradition of secrecy (Gray 1988) so that even the mere fact that tax havens commit to transparency may have further stigmatized those countries as tax havens. This indicates that the incentive for tax havens to commit to transparency standards is potentially not a clear-cut one, but requires a detailed, home country-specific examination.

Focusing on the effect of TIEAs, we find mixed evidence. The respective coefficient is found to be of the predicted sign in all models and operationalizations of *PostEffective*, however statistical significance is only given in Models 1 and 3. More detailed examinations show that the loss of statistical significance can be traced back to the inclusion of *LN Phone*. Although *LN Phone* has been used by Hanlon, Maydew and Thornock (2015) as a proxy to capture technological development, its effectiveness is potentially limited in the context of this subsample of rather small tax havens. The loss of significance in Models 2 and 4 should therefore be considered with caution.²⁶ For this reason, we interpret the results for TIEAs overall as being in line with Hypothesis 2.

²⁵ In untabulated robustness checks we use *PostSigning · PostBlacklist* as an alternative operationalization for *PostEffective*, indicating the years after the signing of an information exchange agreement for the cases where the agreement was signed after the publication of the blacklist. The assumption here is that reputational reasons to sign a TIEA should be relevant especially after the blacklist has been published. The results remain unchanged compared to the baseline operationalization.

²⁶ The p-value corresponding to the coefficient of *TIEA · PostInForce* in Model 4 is found to be exactly 0.1.

5.4 Material effects vs. disclosure effects

To generate the dependent variable *Investment*, we rely either on companies' own publicly available Exhibit 21 forms (U.S. sample) or on their annual reports (German sample). It is therefore conceivable that the variation in the dependent variable does not reflect real, material changes but only changes in companies' reporting behavior. Then our results would be due to a change in disclosure strategies only. While these results would rather point to the need for better enforcement so as to induce material effects they would, however, still underline the role played by reputation. However, for both countries several aspects suggest material effects do prevail.

For the U.S., there are very clear legal prescriptions in this case. According to SEC Regulation S-K § 229.601 (21) and the definition of "significant subsidiary" (SEC Regulation S-X § 210.1-02 (w)), subsidiaries whose consolidated statements alone or together account for more than 10% of the registrant's total assets have to be reported. These rules are straightforward and therefore not easily circumvented. Moreover, reporting needs to be guaranteed as true by CEOs. Under similar conditions, Dyreng, Hoopes and Wilde (2014) find that public pressure induced UK companies to materially abstain from tax avoidance activities when they were forced to disclose their subsidiaries although they did abstain from reporting as long as reporting was not obligatory. Still, it should be noted that lately several studies have identified changes in the disclosure pattern of U.S. companies over time (for example, Gramlich and Whiteaker-Poe 2013, Holzer 2013). For instance, whereas Oracle reported 428 significant subsidiaries for the financial year 2009, this number has decreased to only six in 2010 (Gramlich and Whiteaker-Poe 2013). Donohoe, McGill and Outslay (2012) argue that it is exactly the increased public and academic scrutiny that has led to a reduction of the reported number of foreign subsidiaries in such locations. However, those adjustments took place after the period represented in the U.S. sample.

Under German GAAP there has been a long legal tradition of consistency over time in financial accounting (Sect. 252 (1) no. 6 German Commercial Code). This legal requirement demands that – in the absence of external reasons – companies have to apply their reporting pattern consistently over time. This principle has remained in place also after the introduction of IFRS (IAS 8). Moreover, for German management commentaries German Accounting Standard (DRS) 20.26 prescribes that reporting has to be consistent over time, meaning changes having to be explicitly justified. While a company is not obliged to follow such requirements in the free part of the annual report, pre-

parers still have to justify potential inconsistencies to auditors when they change their reporting pattern in the free part but not in the legal document composed of financial statements, notes and management commentary. At least, this is what the German auditing standard (IDW PS) 202 requires auditors to verify.

Despite these legal and professional requirements, however, preparers may have too much market power so that auditors may be forced to accept that preparers change the reporting pattern over time even though the underlying economic reality has not changed (for some evidence of this kind in the German financial marketplace see, e.g., Grottko and Plüschke 2014). To account for this we conduct an additional robustness check for the German sample: we eliminate from the sample all company-years of those companies that have been identified at least once in the period of our analysis by the German Financial Reporting Enforcement Panel as having reported inadequately with respect to disclosure (59 companies, 481 company-years).²⁷ This is because these companies have been identified as possibly deliberately distorting their reporting behavior to a certain extent. Re-estimating our models as presented in Section 4 based on this subsample, the (untabulated) results remain qualitatively unchanged.

Although we cannot completely rule out that our findings are to some extent driven by disclosure effects, we conclude for both countries that it is very likely that material effects prevail.

6 Concluding remarks and limitations

In this study we take a detailed look on the impact of the OECD initiative against harmful tax competition and observe effects that are both potentially desired and expected by the OECD. The way in which companies' investment activities have changed suggest that the OECD initiative works as a kind of stick-and-carrot-policy. Focusing in the first instance on the effect of blacklisting, we find that due to the resulting reputational costs, companies' activities in tax havens have indeed been negatively affected by the OECD's name-and-shame strategy. With respect to the formal commitment, the expected positive effect on companies' engagement in tax havens was only found in the U.S. sample, not in the German sample. While the stick has worked as desired, this is not necessarily true for the carrot, which may be spoiled rather than sweet at times. This

²⁷ We extracted the respective information from www.elektronischer-bundesanzeiger.de. The list of companies is available from the authors on demand.

suggests that particular care is necessary to ensure that multilateral actions have the desired effect in the end. For TIEAs we find a positive effect in both cases, although the evidence is rather mixed in the German case.

In addition to the already mentioned limitations with respect to data generation, this study is subject to two more major limitations. First, we use a binary measure for investment activities of companies in tax havens. This neglects more detailed information on the question of the number of investment activities carried out by a company per host country and year. Although such information is potentially promising, we are aware that we cannot guarantee the absence of measurement errors in this case due to the characteristics of the process of data generation for both samples (for the U.S. sample, see for example <https://sites.google.com/site/scottdyren/Home/data-and-code/EX21-Dataset>). Focusing on a binary operationalization, by contrast, enables us to avoid such a measurement error.

Second, due to data restrictions we are not able to control for potentially implemented tax-relevant legal amendments in the various tax havens during the period underlying the analysis. Analogously we cannot control for potential influences of relevant changes in other countries' tax laws directed against the tax havens in question.

Although tax havens have already been subject to intensive research, our study points at several interesting avenues for future research. These concern, first, the heterogeneity with respect to the relevance of certain tax havens across both home countries. We suspect that there may be further reasons for this that relate to the proximity of legal traditions between Germany or the U.S. and the tax havens. Whether this is the case, however, would need further scrutiny of the legal environment. Additionally, this study exclusively focuses on publicly listed companies; the differences to and similarities with private unlisted companies also offer space for further research. Furthermore, it seems promising to consider the impact of reputation explicitly on the extent of tax avoidance behavior, which could be captured based on detailed information on, e.g., transfer pricing, royalties and finance activities.

Appendix 1 Variable definitions & data sources

Investment_{i,j,t}

Binary variable; One if company i carries out material activities in tax haven j in year t , zero otherwise. Data stems from own analysis and Scott Dyreng (<https://sites.google.com/site/scottdyreng/>; downloaded in September 2013).

Blacklist_j

Binary variable; One if tax haven j was part of the initial list of tax havens as published by the OECD (2000), zero otherwise.

Commitment_{j,t}

Binary variable; One if a commitment to the OECD's standards of transparency exists for tax haven j in year t , zero otherwise. Information stems from www.oecd.org.

TIEA_j

Binary variable; One if a tax information exchange agreement (TIEA) exists between tax haven j and the respective home country, i.e., Germany or the U.S., zero otherwise. TIEAs are considered if they are not found to be opposed to the international standards based on the OECD's review process. Double tax treaties are considered if no corresponding TIEA exists. Information stems from the OECD's Exchange of Tax Information Portal (www.eoi-tax.org).

LN GDP_{j,t}

The natural logarithm of tax haven j 's gross domestic product of year t measured in constant U.S. dollars of the year 2005. The data stems from the United Nations Statistics Division (<http://unstats.un.org/unsd/snaama/dnllist.asp>; downloaded in January 2014). Information for Gibraltar, Guernsey, Jersey and the U.S. Virgin Islands is drawn from official government websites.

Governance_{j,t}

This variable describes tax haven j 's perceived quality of governance in year t . It is based on the World Bank's Worldwide Governance Indicators (<http://info.worldbank.org/governance/wgi/>; downloaded in December 2013), which consists of six country-

level dimensions associated with governance quality, with higher values indicating higher quality. We construct a summarizing variable based on an approach similar to Dharmapala and Hines (2009). We calculate the mean value of five governance dimensions for each tax haven (voice and accountability, rule of law, political stability, government effectiveness and control of corruption), which is then normalized to have a mean value of zero and a standard deviation of one across all tax havens. Missing values for dependencies of the British Crown or the Kingdom of the Netherlands are substituted by the values of the United Kingdom or the Netherlands, respectively. Missing values for Aruba and the Cook Islands are substituted by values of New Zealand. Missing values of the U.S. Virgin Islands are substituted by values of the United States. As no values are available for 1998 and 2001, we use the values of the preceding year.

Tax Rate_{j,t}

Tax haven j 's top statutory corporate income tax rate effective in year t . Data stems from various years' versions of Deloitte Corporate Tax Rates Survey, EY Worldwide Corporate Tax Guide, KPMG Corporate Tax Rates Survey/Corporate & Indirect Tax Survey, PricewaterhouseCoopers Worldwide Tax Summary, IBFD European Tax Handbook and IBFD Global Corporate Tax Handbook. Additionally, we use the OECD Tax Database, the World Tax Database of the Office of Tax Policy Research at the University of Michigan, Mintz and Weichenrieder (2010) as well as official government documents. We obtained any remaining information by E-mail from various offices of Deloitte, EY, KPMG and PricewaterhouseCoopers located in or nearby the countries in question.

Similarity_{j,t}

This variable reflects the similarity of tax haven j 's and the companies' home country's endowment with labor skills in year t . It is based on the assumption that higher productivity is reflected in a higher GDP per capita. The calculation follows Buch, Kleinert, Lipponer and Toubal (2005) and is based on the following procedure:

$$Similarity_{j,t} = 1 - \frac{abs\{GDP\ per\ capita_{j,t} - GDP\ per\ capita_{home\ country,t}\}}{max\{GDP\ per\ capita_{j,t}; GDP\ per\ capita_{home\ country,t}\}}$$

Gross domestic product per capita is measured in constant U.S. dollars of the year 2005. The data stems from the United Nations Statistics Division (<http://unstats.un.org/>)

unsd/snaama/dnllist.asp; downloaded in January 2014). Information for Gibraltar, Guernsey, Jersey and the U.S. Virgin Islands is drawn from official government websites.

*LN Population_{*j,t*}*

Natural logarithm of tax haven *j*'s population in year *t*. Data stems from the United Nations Statistics Division (<http://unstats.un.org/unsd/snaama/dnllist.asp>; downloaded in December 2014) as well as the World Bank database (<http://data.worldbank.org>; downloaded in January 2014).

*LN Phone_{*j,t*}*

Number of fixed-telephone subscriptions in tax haven *j* and year *t* per 100 inhabitants. Data stems from the United Nations database (<http://data.un.org>; downloaded in December 2014).

Appendix 2 Matrix of correlation coefficients

Table 9 Correlation coefficients between all baseline variables for the U.S. sample (Panel A) and the German sample (Panel B)

Panel A: U.S. sample

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>Investment</i>	(1)		-0.1520	-0.0712	0.0824	0.2154	0.1985	-0.0648	0.1725	0.1547	0.1573
<i>Blacklist · PostPublication</i>	(2)	-0.1520		0.4862	0.0473	-0.4789	-0.0860	0.1198	-0.1200	-0.3930	-0.0958
<i>Commitment</i>	(3)	-0.0712	0.4862		0.1955	-0.2622	0.0368	0.0469	0.0694	-0.2966	0.0804
<i>TIEA · PostSigning</i>	(4)	0.0824	0.0473	0.1955		0.0890	0.1881	-0.1034	0.1278	0.0021	0.2412
<i>LN GDP</i>	(5)	0.2666	-0.4758	-0.2814	0.0726		0.2338	-0.1147	0.3465	0.7438	0.2274
<i>Governance</i>	(6)	0.1850	-0.0818	0.0544	0.2031	0.2974		-0.0440	0.6957	-0.1348	0.6338
<i>Tax Rate</i>	(7)	-0.0500	0.0653	0.0042	-0.1040	-0.0413	-0.0040		-0.2095	0.0297	0.0235
<i>Similarity</i>	(8)	0.1762	-0.0949	0.0470	0.1068	0.3936	0.6639	-0.2307		-0.1965	0.7564
<i>LN Population</i>	(9)	0.1719	-0.4208	-0.3207	-0.0057	0.7593	-0.1773	0.0678	-0.1610		-0.3313
<i>LN Phone</i>	(10)	0.1207	-0.1003	0.0651	0.2368	0.2852	0.6506	0.0705	0.5951	-0.2877	

Table 9 cont'd Correlation coefficients between all baseline variables for the U.S. sample (Panel A) and the German sample (Panel B)

Panel B: German sample

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>Investment</i>	(1)		-0.1813	-0.1465	0.2057	0.3120	0.2437	-0.0304	0.1673	0.2768	0.1507
<i>Blacklist · PostPublication</i>	(2)	-0.1813		0.4678	-0.1223	-0.4975	-0.1290	0.0669	-0.0896	-0.4276	-0.0792
<i>Commitment</i>	(3)	-0.1465	0.4678		0.1140	-0.2682	0.0267	-0.0101	0.1044	-0.3387	0.1309
<i>TIEA · PostSigning</i>	(4)	0.2057	-0.1223	0.1140		0.1974	0.2405	-0.0776	0.1390	0.1010	0.2326
<i>LN GDP</i>	(5)	0.3852	-0.4808	-0.2758	0.2316		0.2925	-0.0721	0.3781	0.7427	0.2172
<i>Governance</i>	(6)	0.2309	-0.1242	0.0351	0.2445	0.3335		0.0022	0.6522	-0.0845	0.7059
<i>Tax Rate</i>	(7)	0.0082	0.0172	-0.0486	-0.0530	0.0305	0.0403		-0.1466	0.0711	0.0057
<i>Similarity</i>	(8)	0.1696	-0.0774	0.0787	0.1067	0.3955	0.6183	-0.1672		-0.1091	0.6998
<i>LN Population</i>	(9)	0.2895	-0.4346	-0.3450	0.1202	0.7581	-0.1376	0.1169	-0.1022		-0.3521
<i>LN Phone</i>	(10)	0.1060	-0.1117	0.0594	0.1871	0.2642	0.6519	0.0309	0.5389	-0.2977	

Pearson correlations are displayed below the diagonal, Spearman correlations above the diagonal. Variables follow their definitions as stated in Section 4 and Appendix 1.

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