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Lukas Menkhoff and Jakob Miethe

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DIW Berlin
German Institute for Economic Research
Mohrenstr. 58
10117 Berlin

Tel. +49 (30) 897 89-0
Fax +49 (30) 897 89-200
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Abstract

We use newly released bilateral locational banking statistics of the Bank for International Settlements to show the full circle of international tax evasion via tax havens. Surprisingly, white-washed money from tax havens is also withdrawn from banks in non-havens if an information treaty is signed between both countries. There are time lags and other economically plausible structures in these reactions. Interestingly, the effect of additional information-upon-request treaties seems to fade out over time. By contrast, new treaties based on automatic information exchange again show bite; this puzzling evidence is best explained by dirty money changing its packaging.

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Keywords: Tax evasion, international capital flows; international information exchange treaties; bank deposits

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Lukas Menkhoff, Humboldt-University Berlin and DIW Berlin (German Institute for Economic Research), 10108 Berlin, Germany; e-mail: lmenkhoff@diw.de; tel. ++49 (0)30 89 789 435.

Jakob Miethe, Humboldt-University Berlin and DIW Berlin (German Institute for Economic Research), 10108 Berlin, Germany; e-mail: jmiethe@diw.de; tel. ++49 (0)30 89 789 439.

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1 Introduction

Tax havens have a long-standing history. Since open borders allow for the international transfer of capital, there is an incentive to shift capital and earnings on this capital to places where taxes are relatively low and secrecy is high. While these shifts are, to some extent, legal, to some extent, they are not. Illegal shifts of capital are of great concern to policy makers for at least three reasons: first, they reduce tax income; second, they reduce effective taxation of the rich; and, third, the majority may lose trust in the tax system and in state institutions in general.

Therefore, the fight against tax evasion is high on the international agenda. Whistleblowers and leaks of large datasets document that international tax evasion via tax havens remains a relevant problem. Capital is shifted into such tax havens where ownership of funds is hidden. In a sense, the formerly evaded ‘dirty’ money is white-washed. Funds are initially transferred to tax havens, they need to be invested in non-havens at some point, because appropriate investment opportunities in tax havens are typically limited. This creates outbound capital flows from non-havens into tax havens and inbound flows back again. While we know that outbound flows react to information exchange agreements that are exclusively aimed at tax evaders (Johannesen, 2014a; Johannesen and Zucman, 2014), do the inbound flows, i.e. those from tax havens into non-havens, react in the same way? If so, have such agreements succeeded over time, leading to ‘clean’ capital flows into and out of tax havens or is ‘dirty’ money still flowing?

While a few studies inform about some of these capital flows (which we report later), this research is the first to show the full circle of outbound and inbound flows, here based on bank deposits data. In October 2016, the Bank for International Settlements released a significant portion of its bilateral locational banking statistics. This newly released dataset provides bilateral deposit data with which we can analyze – for the first time in the literature in the same dataset – outbound tax evasion with capital flowing from non-havens to tax havens as well as inbound reactions where funds flow back into the non-haven. We show that deposits in both directions react to information exchange agreements between non-havens and tax havens, concluding that these flows include relevant shares of evaded capital. Due to newly available

data on international information exchange initiatives, we are able to base this analysis on both older tax and information exchange agreements (TIEAs) as well as newer implementations of the OECD's common reporting standard (CRS). While the former treaties allow for information exchange upon request, the latter codifies automatic exchange of bank information.

The main focus of our analysis addresses inbound banking flows, i.e. the change in banking deposits that counterparties from tax havens hold in non-havens. This is interesting for two reasons. First, there is a gap in the literature documenting the existence of tax evasion effects in such international flows and their sensitivity to tax agreements. Only Hanlon et al. (2015) provide a glimpse of the effect. In a sample ending with 2008, they show that four information exchange agreements between the US and tax havens negatively affected inward foreign portfolio investment from these tax havens. The sample we use includes several hundred agreements along with a large bilateral countrypair dimension that includes both tax havens and non-haven countries up to the second quarter of 2017.

The second motivation to study inbound flows stems from the fact that these flows provide a missing link to show the full circle of tax evasion. The reaction of inbound capital flows from tax havens to non-havens may seem surprising, given the fact that the money is white-washed in tax havens such that the "true" owners in non-havens are hidden. Still, available reports from leaked data indicate that the organizers of such tax evasion flows are highly concerned about backwards flows. The vital piece of information that evaders try to hide is their ownership of evaded capital and, therefore, the ownership structure of their sham corporations, private foundations, or trusts as well as the connected bank accounts. Thus, tax authorities in home countries have a second angle to tackle tax evasion. They can take the occurrence of such backwards flows to investigate this ownership and look for evidence about earlier outbound flows (which could be illegal). Our analysis strongly supports this reasoning.

In order to show this circle of capital flows, we also analyze outbound flows from non-havens into tax havens, thus extending the analysis in Johannesen and Zucman (2014). Their study is the first to analyze bilateral flows into tax havens by also considering the impact of tax and information exchange agreements. They had private access to a subset of the BIS data in the outbound direction until mid-2011, which are now largely public with a longer time dimension. We confirm for a similar group of countries that, despite twice as many tax agreements existing today, effects are qualitatively the same. However, we do find a gradual decline of this effect for TIEAs over time, as we also cover the more recent years. That means

capital flows into tax havens no longer react to newly signed TIEAs, raising the question of whether TIEAs have successfully eliminated tax evasion. Alternatively, there is the possibility that tax evasion changes its forms, thus only formally complying with the information exchange agreements.

In order to distinguish between these alternative explanations, we also assess newer forms of information exchanges. In this respect, we analyze the OECD's mutual common authority agreements that allow for automatic information exchange under a Common Reporting Standard (CRS) after a bilateral matching as well as two US-Switzerland agreements and the US Foreign Account Tax Compliance Act (FATCA). If the principle goal of regulators is realized, then former tax evaders are compliant and the new agreements do not reduce capital flows into tax havens. Our data show, however, the opposite: new agreements produce the effects that are hauntingly similar to those of the older TIEAs. This evidence speaks against the long-term success of TIEAs but is consistent with the hypothesis that tax evaders change the packaging of their funds and with other results in the literature that find no decline in aggregate offshore wealth (Zucman, 2013; Alstadsæter et al., 2017b).

This research proceeds in six more sections: Section 2 sets out the identification via tax agreements and documents the institutional background. Section 3 introduces the empirical approach, including data. Results on TIEAs are presented in Section 4, while Section 5 provides analogous results on more recent information exchange agreements. Robustness checks are presented in Section 6 and Section 7 provides conclusions.

2 Identification: The long journey of dirty money

In this section, we describe our identification of international tax evasion via tax havens, in line with conventional procedures. Section 2.1 lays out the principal flows of this 'dirty' money, Section 2.2 discusses the definition of the first target of such flows, i.e. tax havens. Finally, Section 2.3 introduces into the international information exchange agreements that serve as our tool to identify flows of evaded capital.

2.1 The return journey of dirty money

Transferring capital from a non-haven to a tax haven, called the 'outbound' journey here, is a relatively simple matter for evaders. Setting up a sham corporation with a bank account

offshore can be done online for a small fee (see Sharman, 2010, for some real-life examples) and since offshore wealth is to a large proportion held by very wealthy individuals, such fees are negligible (Alstadsæter et al., 2017a). The most common way to transfer funds to that account is an invoice for ‘consulting services’ carried out by the evader (see Zucman, 2013, 2014, for examples). Qualitative evidence shows how simple such arrangements are in practice: the evader simply sells his imaging rights to such a sham corporation he owns via some intermediaries and never declares the income on those rights. Moving the money abroad does not seem to be a great concern for evaders.

What is more problematic is the ownership structure of those funds that might be exposed when the sham corporation re-invests funds in non-haven countries. Indeed, Sharman (2010) shows that it is increasingly difficult, although possible, to establish an evasion setup without providing identification at some point to the service provider in the tax haven. When tax evaders are convicted, it happens by tying ownership of bank accounts or sham corporations to them or close relatives. Therefore, layers of secrecy are added between the outbound and inbound investments. Further, it is possible that the funds are used for investment in capital markets, the funds are shifted and invested in other tax havens, and some funds are used for luxury consumption. However, at some point, at least part of the funds, or the capital gains thereof, will move back from some tax-haven to some non-haven. We call this the ‘inbound’ journey.

Two considerations motivate this return move: First, the most relevant investment opportunities for tax evaders, such as luxury items or real estate in OECD capitals, are found in non-havens. There is no point in accumulating funds via tax evasion if some part of it does not raise the standard of living of the evader or their family. Second, the established home bias of international capital flows (see Coeurdacier and Rey, 2012, for an overview) is also visible for tax evaders, as established by Hanlon et al. (2015). If home bias is present in tax evasion, funds even end up in the original non-haven from where the evaded capital originated. Again, not all investment from tax havens are connected to evasion: in some cases, it is legitimate tax planning by foreign investors (Dharmapala and Hines, 2009). However, evasion is part of the story, as we show.

Repatriating funds does not need to take place via illegal means. An offshore trust holding evaded capital could open an entirely legal bank account in a non-haven in order to finance consumption, with this trust held by people close to the evader, potentially family members. Alternatively, an offshore sham corporation could open bank accounts in non-havens with funds

available to finance legitimate real estate investment, for example.¹ If the final investment of an evader takes place in the original home country, however, it can be an evasion move in itself. The sham corporation hides the original identity of the evader and foreign investors are exempt from capital gains taxes in many countries (Hanlon et al., 2015). We provide the first study showing results consistent with such round-tripping behavior in bank deposits.

In all of these cases, however, the funds are repatriated and registered in a bank account of a non-haven bank office as a liability against a tax haven counterparty that holds the account. This bank office has to report to the respective central bank, which in turn reports to the Bank for International Settlements, our data source.

Figure 1 provides a schematic visualization of the circle of tax evasion between non-havens and tax havens. First, funds are moved into the tax haven where ownership is disguised via sham corporations (outbound). Transfers between tax havens can take place, layers of secrecy can be added with a network of sham corporations and the funds can generate capital gains. Since it is unlikely that tax havens will themselves chase down tax evaders, we use positions between tax havens as a falsification sample where no threat of detection is expected (tax haven falsification). At some point, at least a fraction of the funds will be repatriated to a non-haven if they are to be used for consumption or investment in non-haven countries (inbound). This return journey is our main focus. It can take place via very different funding schemes and from more than one tax haven, not necessarily the original one.

--- Figure 1 about here ---

Once the funds are back in a non-haven, the evasion scheme is complete. However, and this is crucial for our identification, at some point in the journey evasion takes place. The original funds could be evaded capital, the funds in the sham corporation could produce undeclared income, or investment in the evaders home country could take place under false identity. In all of these cases, a threat of detection remains. It is the ownership structure of the sham corporation as well as the ultimate beneficiary of capital gains from the funds it holds that can – and, as convictions show, does – expose the evader. The consequences range from relatively benign repayment obligations to severe criminal charges. It has been shown that tax evaders react to such ‘extrinsic motivation’ in much more benign circumstances already

¹ The US Internal Revenue Service provides yearly examples of abusive tax schemes as well as the harsh penalties incurred when these are discovered. The list includes examples in line with sham corporations in tax havens with bank accounts in non-havens, like those we use for identification: <https://www.irs.gov/uac/examples-of-abusive-tax-schemes-fiscal-year-2015>

(Dwenger et al., 2016). Positions between non-haven countries on the other hand should not be connected to international tax evasion of the same form we are analyzing. We therefore use capital positions between non-havens as a second falsification group (non-haven falsification). Before we test for these effects empirically, we provide information on the tax havens included in the analysis as well as the information exchange agreements that we use to identify evasion.

2.2 Tax havens

Although there is consensus defining a tax haven as a jurisdiction with low or zero tax rates on some income types, most definitions go further and restrict ‘tax havens’ to countries with high bank secrecy rules and low transparency regulations. Tax havens also score high on governance indicators, have relatively sophisticated communication infrastructure, and few natural resources (Dharmapala, 2008; Dharmapala and Hines, 2009). Being classified as a tax haven, however, is fiercely disputed by jurisdictions thus labeled and there are cases of political intervention to influence official tax haven lists (Gravelle, 2015).

The empirical literature commonly employs relatively unrestricted lists. For most research questions, including a de facto non-haven into the tax haven list leads to a more conservative estimation. We therefore follow this convention and use, as our baseline, a large list of tax havens by combining the list of tax havens by Gravelle (2015), which already collects different sources, with the one by Johannesen and Zucman (2014). This tax haven classification is altered in robustness checks, but results are unaffected. Table 1 summarizes key information on those tax havens for which comprehensive BIS data are available. A complete list of the 58 tax havens is provided in [Appendix A1](#).

--- Table 1 about here ---

As the first three columns document, the stereotypical small pacific island is still pervasive and colonial ties remain important. Column 4 shows the number of tax and information exchange treaties signed by the tax haven country. Column 5 documents the deposit data availability reported by the tax haven itself. The deposits of non-havens are used in our outbound analysis. Most importantly, column 6 shows the number of countries that report deposit data on that specific tax haven, revealing the availability of a large cross-sectional dimension. We use this data reported by non-havens for the inbound analysis. As an example: Guernsey, a British Crown Dependency with less than 70,000 inhabitants has signed 34 treaties

with non-haven countries during the 58 quarter sample period (2003:I – 2017:II) that meet our requirements outlined in the next section. It reports bilateral data to the BIS since the fourth quarter of 2001 and twenty-three other reporting countries report data against Guernsey. To create a balanced panel, we drop all but three non-haven reporting countries (part of the inbound sample) and six tax haven reporting countries (part of the tax haven falsification sample).

2.3 Information exchange agreements

Since tax evasion is, by definition, the illegal withholding of tax liabilities, international regulation attempts are mainly focused on detecting the delinquent by exchanging information between tax authorities. The most comprehensive regulatory attempt, aimed at increasing the likelihood of detection, was launched by the OECD. In 1998 the OECD explicitly addressed harmful tax practices, in 2000 it published a list identifying countries it considers to be tax havens and suggested the signature of bilateral information exchange treaties to facilitate detection of tax evasion (OECD 1998; OECD 2000). Such treaties became popular after April 2009, when the G20 decided to sanction tax havens if they did not sign at least 12 treaties (G20, 2009). We discuss these treaties here before introducing more recent measures.

Since the provision of services to facilitate tax evasion has benefits, at least in the form of service fees, tax havens have an incentive to maintain secrecy regimes. Cooperation is enforced by threat of economic sanctions, which presents the dilemma of choosing between compliance and secrecy. Konrad and Stolper (2016) model the dilemma of tax havens between collecting fees on the one hand and facing international pressure on the other as a coordination game between investors and tax havens. The tax haven abolishes a secrecy regime only if political costs are high enough or if coordination with investors fails. They also predict a negative signaling effect if a tax haven shows compliance of some sort, a point we return to later. With political pressure, as of 2017 more than 3,000 bilateral tax and information exchange treaties (TIEAs) have been signed. Bilicka and Fuest (2014) document that fears that tax havens would simply sign with 12 other tax havens or economically meaningless countries did not materialize: on average treaties are signed between tax havens and non-havens with strong economic ties.

The provisions in these short treaties establish a procedure of bilateral information exchange upon request. There are several caveats: information has to be “forseeably relevant,” (changed from the stronger ‘necessary’ prior to 2005) which implies knowledge of the identity

of the evader; there can be refusal for public policy reasons (such as a request being ‘at variance’ with laws of the counterparty); and requests cannot be aimed at information that the requesting country can obtain itself (Christensen II and Tirard, 2016). TIEAs therefore do not have very serious implications for evaders in reality: information requests are rare in practice and necessitate a level of detail of information in the non-haven about the evader that would most likely imply trouble for her even without information exchange. Nevertheless, a threat of detection creates an incentive for evaders to react.

The fact that reactions to TIEAs are observable is due to their bilateral nature. Roughly 230 self-governing jurisdictions on the planet would require more than 26,000 bilateral treaties for a complete network, so loopholes are abundant. Assuming that tax evaders – or their financial service providers – are well informed and well-behaved, they will react even to marginal threats by shifting capital elsewhere. Since not all economic activity in tax havens is illegal, the fact that a genuine investor should be entirely unaffected by such treaties makes them an ideal strategy to identify tax evasion.

As our data source, we use the OECD list of TIEAs and double taxation conventions (DTCs), also used by Bilicka and Fuest (2014), as it provides a publicly accessible, reasonably frequently updated, and comparable standard of what constitutes a tax and information exchange treaty. Figure 2 confirms the earlier notion that most of the signed TIEAs (top panel) are indeed treaties between tax haven and non-haven countrypairs, while DTCs (middle panel) that nest information exchange are more common between non-havens. The peak of new signatures was in 2010, but new treaties continued to be signed. By 2015, new signatures levelled out, which makes us confident that we capture the bulk of the OECD initiative.

--- Figure 2 about here ---

The fact that these treaties only allow exchange of information upon request resulted in criticism and to the development of agreements that target automatic exchange of information. In 2014, 44 “early adopters” committed to automatic exchange of information in the framework of the OECD’s Common Reporting Standard Multilateral Competent Authority Agreement (CRS MCAA). While the reporting standard is agreed to multilaterally, automatic exchange relationships of bank data must be agreed upon through a bilateral matching process. We use these matches to analyze shifts in bilateral capital flows. These relationships start in 2017 and are depicted in the bottom panel of Figure 2. As of November 2017, a total of 1,539 bilateral commitments have been made, with 1,064 already taking effect during our sample period

(OECD, 2016). In a further step, we evaluate attempts by the US to unilaterally target tax evasions by constructing a net of bilateral automatic reporting requirements centered on the US via the Foreign Account Tax Compliance Act (FATCA), on which the CRS was modelled, and two treaties aimed specifically at Switzerland. These US measures are introduced in more detail in Section 6.2.

3 Capital flow data

Information about tax havens is rather rare in general, in particular regarding disaggregated capital flow statistics. Data on hidden positions are unavailable by definition. In the capital flow literature common practice is to drop tax havens from the sample (as in Broner et al., 2013) or to control for them with a designated dummy (as in Lane and Milesi-Feretti, 2008). With the important role that tax havens play in the international financial system, this approach is not satisfactory. Identifying or measuring tax evasion empirically is challenging, but there is progress, as Slemrod (2015) summarizes. There are two approaches to fill the data gap and study tax evasion in the empirical literature.

The first strand of research aims at filling it directly with data reported by financial institutions in tax havens as well as leaked data. These contributions use direct bank claims data reported by tax havens via the Bank for International Settlements (see Johannesen and Zucman, 2014; Johannesen, 2014a) or leaked datasets (Caruana-Galizia and Caruana-Galizia, 2016; O'Donovan et al., 2016). The second line of research targets the problem by using indirect identification strategies and data reported by non-havens. Hanlon et al. (2015) use bilateral foreign portfolio investment data of the US, which are compiled vis-à-vis a long list of counterparties, including tax havens. The advantage of such an approach is that it does not rely on tax havens as a source of data. For the first time in the literature, we are able to combine these two approaches. Our study provides results based on both data from havens (outbound) and non-havens (inbound) taken from the same dataset. The large international dimension also supports our identification strategy.

A total of 46 national central banks report the claims and liabilities² of their banks in the BIS locational banking statistics (LBS), including 19 jurisdictions on our tax haven list. A

² Reporting institutions to BIS statistics include “commercial banks, savings banks, savings and loan associations, credit unions, cooperative credit banks, building societies, post office giro institutions, other

significant subset of these bilateral locational banking statistics (LBS) has recently been released. The release includes both the reports from tax-havens as well as from non-havens, thus allowing us to analyze deposits of non-banks both to and from tax havens using the same data source. Johannesen and Zucman (2014), as well as Johannesen (2014a), are the only two sources in the tax evasion literature that use this data (precisely: an earlier private access to these “outbound” data).

These LBS ‘by residence of counterparty’ are compiled according to balance of payments conventions³ via the immediate counterparty, not the ultimate guarantor or beneficiary of deposits (see IMF, 2013; BIS, 2013, 2014). In our context this implies that we can observe aggregated deposits in tax havens, even when they are later transferred (back) to a non-haven. A complete disaggregation of deposits with respect to the depositor is not available but we can distinguish non-banks from bank deposits. This data is ideal for the exercise at hand for two reasons.

First, we do not need to know the type of the depositor in the non-haven: the broad non-bank category fits our purposes perfectly as funds can be deposited by a firm or by an individual. We do not need to know which structure is most prevalent. The reaction of the aggregate to information exchange agreements captures all of the financing arrangements discussed so far. Another reason for including firm deposits into the analysis is that research suggests a much weaker delineation between tax evasion and tax avoidance than previously thought (see O’Donovan et al., 2016; Hebous and Johannesen, 2016; or Crista and Nguyen, 2016). Thus, the broad ‘non-banks’ category for deposits is exactly what is needed to carry out our analysis.

On top of our variable of interest, which we expect to include deposits related to tax evasion, the BIS provides several other variables at the bilateral level with almost identical coverage. These allow us to control for systematic shifts in the international financial system, themselves unrelated to tax evasion along several dimensions. We use total liabilities against all counterparties to build a number of weight measures. First, we create a variable $x_{ijt}^{Finweight}$ measuring the relative weight of the bilateral relationship $i - j$ compared to all liabilities reported by other countries, i , against counterparty, j . This is a measure of the importance of

government controlled savings banks and other institutions if they take deposits or issue close substitutes for deposits” (BIS, 2013, p. 8).

³ Some discrepancies remain; liabilities on account of shares of money market funds for example would be included as deposits in BPM6 but not in the BIS data. However, the geographic breakdown of counterparties is consistent.

the bilateral relationship compared to the total international financial integration of the counterparty in question:

$$x_{ijt}^{financial\ weight} = bankliabs_{ijt} / \sum_{i=1}^I bankliabs_{ijt}; \quad i, j \in havens \cup nonhavens \quad (1)$$

Second, we limit the sample to data reported by tax havens and create $x_{ijt}^{offshore\ weight}$ as a measure of the importance of tax haven i for counterparty j relative to the entire offshore world. This measure will be high for a tax haven – non-haven countrypair where the reporting tax haven is relatively important compared to other tax havens that report against this counterparty and is constructed as:

$$x_{ijt}^{offshore\ weight} = bankliabs_{ijt} / \sum_{i=1}^I bankliabs_{ijt}; \quad i \in havens; \quad j \in havens \cup nonhavens \quad (2)$$

Finally, we create that same measure from the non-haven perspective, thus measuring the relative importance of non-haven i bank liabilities for counterparty j relative to all other non-havens that report against this counterparty:

$$x_{ijt}^{onshore\ weight} = bankliabs_{ijt} / \sum_{i=1}^I bankliabs_j; \quad i \in nonhavens; \quad j \in havens \cup nonhavens \quad (3)$$

These three measures allow us to control for financial integration of the banking sector along all four samples (inbound, outbound, tax haven falsification, non-haven falsification) and are included as explanatory variables where appropriate.

Transparent avoidance strategies, as discussed in Johannesen (2014b), Gumbert et al (2016) or OECD (2014), will not influence the results, because they should not react to the threat of information exchange. We reduce our data to a balanced panel of 58 quarters ranging from 2003:I to 2017:II and split the available data into four samples. The inbound sample consists of 170 countrypairs reported by eleven reporting non-havens against a combined 44 tax havens. Not every reporting country reports data on every counterparty as Table 1 shows. The outbound sample consists of 558 countrypairs with deposit data reported by eight tax haven countries on deposits held by a combined 147 non-haven counterparties. Additionally, we can construct two falsification samples where we do not expect effects as outlined above. The non-haven falsification sample consists of 575 countrypairs reported by the same eleven non-havens against a combined 146 non-havens. Finally, the tax haven falsification group is made up of reports by the same eight tax havens on deposits of a combined 46 tax havens resulting in 197 unique countrypairs. These four samples are used in the analysis presented below.

4 Results on TIEAs

We present results in three steps. First, we show results on the inbound journey of capital from tax havens (Section 4.1). We run several tests controlling for bilateral integration, a placebo analysis and differentiated treaty effects to interpret the results. To complete the picture, outbound capital flows into tax havens are evaluated along the same lines (Section 4.2). Moreover, our long sample allows us to test for time-varying effects from TIEAs and DTCs (Section 4.3).

4.1 The reaction of non-bank deposits in non-haven countries (inbound)

In this section, we fully exploit the bilateral dimension of the newly released locational banking statistics to test several hypotheses about the behavior of non-bank deposits from tax havens in non-haven countries.

Empirical approach. Since we rely on data reported by non-havens, we can analyze reactions in deposits from 46 different tax-haven jurisdictions that are reported to the BIS. In order to test for signs of tax evasion in the journey of capital from tax havens, we analyze the reaction of bank deposits by tax haven counterparties in non-haven countries to new information exchange treaties. We thus estimate the following model:

$$\log(\text{deposits}_{ijt}) = \alpha_{ij} + \gamma_t + \sum_{k=0}^K \beta_k (\text{signed}_{ijt-k}) + \delta X_{ijt} + \epsilon_{ijt} \quad (4)$$

Here subscripts ij denote the countrypair, t the respective quarter, k the lag variations of the signature timing we introduce below. Bank liabilities against nonbank counterparties are used to approximate deposits; of which we take logs for interpretation purposes. As outlined above, we expect significant negative results to treaties only in deposits from tax-havens. Control variables in X_{ijt} are introduced on the bilateral level ($x_{ijt}^{\text{financial weight}}, x_{ijt}^{\text{offshore weight}}, x_{ijt}^{\text{onshore weight}}$), as introduced in the data section. Available macroeconomic data is too rough compared to this quarterly bilateral data to contribute meaningfully, which is why we relegate such tests to the robustness section. This empirical setup is employed in the regressions we present.

Baseline result. Our first and main hypothesis is that there is a reaction of inbound deposits to treaties. Baseline results are presented in Table 2. Column 1 shows that a treaty reduces deposits by tax haven counterparties in non-haven countries by 36%, quantitatively in

line with the effect reported by Hanlon et al. (2015) for US portfolio liabilities. At first glance, this result looks as if residents in tax havens use non-haven countries to evade taxes and are afraid that their government will use the availability of information exchange to detect just that. Of course, there is no reason to evade taxes when they are at 0% and since we have defined tax havens as countries with strong secrecy rules, their governments are unlikely to ask for information themselves to expose evaders. The result is meaningful as soon as we see it in the context of the findings by Johannesen and Zucman (2014) and Hanlon et al. (2015). The capital deposited by tax haven counterparties does not constitute capital from citizens of these jurisdictions but rather that of foreign depositors who are originally residents of non-havens.

--- Table 2 about here ---

Lagged reactions. There is reason to believe that reactions in the return journey happen with some lag. As funds are already invested, re-arranging its structure takes time. Indeed, we find no consistently significant results before the fourth lag (column 2). After one year, deposits remain lower, which is in line with an interpretation of a treaty as a signal effect on the side of the tax haven; something commonly assumed in the literature. Once a tax haven is branded as compliant with non-haven authorities, its reputation as a ‘good tax haven’ is tarnished (Konrad and Stolper, 2016).

Bilateral controls. In columns 3 and 4 of Table 2, we introduce two control variables outlined in the data section that exploit the richness of the BIS data and act on the bilateral dimension. The first variable captures the importance of a reporting non-haven compared to other reporting non-havens for each tax haven in our sample over time (onshore weight). The second captures the importance of a non-haven in the financial system: its bilateral weight when compared to all liabilities reported against that tax haven by tax havens and non-havens combined (financial weight). Both of these variables are significant and strongly positive, substantially improving model fit. The advantage of these control variables is that they operate at the same frequency and have the same bilateral dimension and data availability as the deposit variable of interest. This is not true for commonly used macroeconomic control variables, which are typically only available annually and unreliable or not available for the tax havens under study. In the robustness section, we consider such variables: they do not change the results and have little explanatory power. While our main results are unchanged, this change does seem to increase precision as all effects decline marginally, with the borderline significant result on the first three lags now insignificant. We employ these variables in all further specifications.

Placebo study. A concern connected to our research design is the non-randomness of signature dates. The OECD initiative had its own internal logic and momentum. As shown in Figure 2, while there is time variation in treaty signatures over the whole sample, the bulk of new information exchange treaties was signed between 2009 and 2012. This period includes significant balance sheet reduction in the international banking system. Our results could thus be driven by changes in the control group over that period. In order to alleviate this concern, we carry out a placebo analysis where all 1,077 countrypairs in our four samples that have *not* signed an information exchange treaty are assigned a placebo-treaty. In order to mirror the signature momentum, we calculate a cubic spline over the quarters in which treaties are signed based on the number of signature in each quarter, for all countrypair groups and for both treaty types. Based on these (normalized) splines, we draw placebo treaties for each countrypair in the respective group and build a placebo treatment variable based on these. Figure 3 plots, as an example, TIEAs and placebo-treaties for non-haven – tax haven countrypairs with non-normalized splines. With these placebos thus carry the same ‘signature’ momentum into the control group that we observe in treaty signatures meaning that the placebo also starts taking effect during the deleveraging period of 2009 – 2012.

--- Figure 3 about here ---

Column 5 of Table 2 uses this placebo as a control variable. The coefficient is insignificant and economically almost zero with the results largely unchanged, thus making us confident that we do indeed capture a negative reaction to treaties and not a positive development in the control group. A further analysis of time effects is found in the robustness section.

To reiterate, not all of this capital is connected to tax evasion. However, legitimate flows should not react at all to information exchange agreements. A genuine investor or capital holder has nothing to fear from exchange of information. Inversely, we argue that any act of illegal evasion at any stage of the long journey of capital will lead to reactions to information exchange. Since relocation of the evasion scheme to a tax haven that has not signed is simple, the threat of detection operates almost marginally. The original funds placed in the tax haven could be evaded capital, the profits made with legitimate capital in the tax haven might not have been declared as income in a country where foreign earnings or wealth would be taxed, or the return journey might be part of an evasion scheme. If tax evaders are rational, they will react to the threat of detection in all of these cases. This is where the abovementioned advantage of

aggregate positions of firms and households comes in. We do not need to know the exact configuration of the evasion scheme to detect evasion here.

TIEAs vs. DTCs. So far, we lump tax and information exchange agreements together with double taxation conventions since both entail information exchange clauses. Table 3 shows results for differentiated treaties. Column 1 repeats the result of treaties between non-havens and tax havens. Column 2 now differentiates the two treaty types. Interestingly, the significance of TIEAs drive the effect, not DTCs, which are insignificant. This is in line with the aims of these treaty types. While TIEAs explicitly aim at information exchange to curb tax evasion, DTCs are more complicated treaties that cover a range of double taxation issues, with information exchange being one of many clauses. Further, as we showed in Figure 2, non-haven – tax haven countrypairs mostly signed TIEAs.

--- Table 3 about here ---

Interestingly, this result is reversed entirely when we focus on treaties between non-havens where we do not expect tax evasion. Column 3 shows results for deposits in the non-haven falsification sample that confirm this expectation. Column 4 shows in more detail that information exchange between non-havens is associated with an *increase* of bilateral deposits and that this effect is only significant and economically meaningful for DTCs. Clearly, countries that are increasingly integrated will have both increasing deposits and a greater incentive to sign double taxation conventions. While we do not seek to establish causality here, there is evidence that bilateral tax treaties have a positive effect on capital inflows (Bloningen et al., 2014). Thus, it is meaningful to interpret the findings in columns 4 and 5 as the ‘normal’ case in which tax evasion does not drive the results. Again, non-haven countries sign more DTCs than TIEAs amongst each other and it is reasonable to assume that such double taxation conventions increase bilateral confidence of investors and, thus, financial integration.

We take the results in this section as evidence that funds deposited by tax haven counterparties in non-haven countries include dirty money that has, at some point in its journey, been evaded. While we cannot determine at which point in the evasion process illegal action takes place, we expect that the dirty money is coming home. In order to fully establish this circle of evasion, we now turn to the outbound journey of tax evaded flows and analyze deposits in tax havens in the following section.

4.2 The reaction of non-bank deposits in tax havens (outbound)

This section serves two purposes: First we compare the results of the last section with reactions of deposits by non-haven counterparties in tax-havens. This allows us to analyze the whole journey of evaded capital based on a single data source with identical treatment variables in both directions. Second, we can investigate whether the findings of Johannesen and Zucman (2014) still hold in a much longer sample.

Baseline result. Only a limited number of tax havens report bilateral deposits to the BIS and that data are rarely released, which is why empirical studies of evasion are rare. However, the countries that do report to the BIS, both tax havens and non-havens, report against a large bilateral dimension of a combined 152 non-havens and 46 tax havens which we also exploit. The two samples employed here are the outbound sample, where we analyze deposits from non-haven countries in tax havens, and the tax haven falsification sample, where we analyze deposits of tax havens amongst each other. The BIS data is provided first by banks to their central banks and then by these central banks to the BIS. This quasi-administrative data alleviates the usual concerns of data quality of data provided by tax havens.

Table 4 shows the baseline results for reactions of non-bank deposits of non-haven counterparties in tax havens to the signature of a treaty including information exchange. The results in Johannesen and Zucman (2014) can be qualitatively confirmed and are quantitatively even larger in our sample. Positions in tax havens are reduced by almost 28% after an information exchange treaty (column 1).

--- Table 4 about here ---

Lagged reactions. We find anticipation effects (column 2), which is plausible considering that professionals who offer financial services needed for evasion strategies are well informed and react before a treaty goes into effect. By signing a treaty, however, a tax haven also signals some sort of compliance with international anti-evasion measures. As Konrad and Stolper (2016) predict, this reduces the long term credibility of the promise of secrecy, consequently making the tax haven less interesting for evasion in general, as highlighted by the still negative treatment variable.

Thus, the economic magnitude of this treaty effect is much larger than that reported by Johannesen and Zucman (2014), who find effects in the range of 11% to 16%, compared to our 28%. This difference in magnitude is due to treaty definitions, as we show in a robustness check.

Our results are more in line with the effect of the EU savings directive on Swiss deposits analyzed in Johannesen (2014a), who establish reactions of deposits in the range of 30% to 40% (15% to 30% for other tax havens). What should be noted, however, is that the economic size of outbound and inbound reactions is quite similar in our study. This cautiously indicates a connection between these two flows, which we explore in more detail below.

Columns 3 and 4 again introduce bilateral control variables (offshore weight and financial weight respectively) that capture the relative importance of the tax haven in question for each non-haven. The effect is quite similar, with both being important control variables but not changing our relationship of interest. Column 5 introduces the same placebo used in the last section. Beyond an improvement in the model fit, these changes do not affect our main results. Columns 6 and 7 finally change the sample to the tax haven falsification group where we do not expect reactions since these jurisdictions are unlikely to prosecute tax evasion. This is confirmed by the data: both the parsimonious (column 6) as well as the full specification (column 7) show insignificant results of information exchange amongst tax havens.

4.3 The effect of TIEAs over time

Our relatively long sample period allows us to compare different reactions over time. Interestingly, we find that the effect of treaties on capital flows weakens. Figure 4 shows this development for both outbound and inbound reactions of tax evaders. In both cases, we estimate our baseline results with a rolling window, limiting the sample to eight quarters before and after the time indicated on the horizontal axis. We then plot the coefficient on treaty signatures over time and indicate statistical significance with the shaded areas, light grey indicating 10% and dark grey 5% significance.

--- Figure 4 about here ---

The decreasing effect of information exchange treaties is unexpected given the results of Johannesen and Zucman (2014), who argue that treaties do not reduce the amount of tax evasion but induce a shift of capital into non-complying tax havens. If such a shift would continuously occur, new treaties should have larger rather than smaller effects as a decreasing number of tax havens accumulates more and more capital. As a caveat, it should be mentioned that Johannesen and Zucman (2014) themselves report mixed results concerning robustness checks and

Johannesen (2014a) finds mixed results on deposit shifting concerning the EU savings directive. Nevertheless, the fact that we find a decreasing effect is striking.

There are two interpretations for this finding. Either treaties are indeed successful in decreasing tax evasion or evaders increasingly relying on other options to evade taxes. Since calculations of aggregate evaded capital, as provided by Zucman (2013) and Alstadsæter et al., (2017b) point, if anything, to increasing evasion over time, the first interpretation is not plausible. Instead, it seems likely that tax evaders channel their capital flows via non-complying tax havens or create a new packaging for their funds (Johannesen and Zucman, 2014). In the following, we test an implication of the latter hypothesis.

5 Results on recent information exchange agreements

The recent declining effect of TIEAs on capital flows in both directions, i.e. outbound into tax havens as well as inbound into non-havens, may be caused by adjustment of tax evaders regarding the form by which they channel their funds. If this is the case, we should again expect effects of recent far-reaching agreements based on an automatic exchange of information (Section 5.1). We test this first for the CRS (Common Reporting Standard) in Section 5.2 and then for a set of further agreements specific to the US in Section 5.3.

5.1 The motivation for examining recent information exchange agreements

Criticism concerning information exchange upon request was pervasive, coming to light even as TIEAs were being introduced (Kurdle, 2008). Information upon request presupposes that most of the information necessary to convict a tax evader is already known to authorities, most importantly, her identity. There are two international attempts to introduce automatic information exchange on foreign nationals: FATCA and the CRS.

FATCA is bilateral in nature, with the US asking foreign countries to enter bilateral treaties starting in 2010. With the threat of effectively being excluded from US capital markets by a 30% withholding tax, foreign financial institutions must report the identity of American account holders that they serve (Johannesen et al., 2017). These requirements are so restrictive that FATCA is criticized for overreaching US competences in foreign countries (Michel and Rosenbloom, 2011). However, it should be pointed out that FATCA's implementation regulations have significantly reduced these requirements. The US Senate's Permanent

Subcommittee on Investigations published a report outlining several feasible evasion strategies circumventing FATCA (United States Senate, 2014). As of November 2017, there are 127 signed FATCA agreements, 63 of which are included in our balanced panel.⁴

Inspired by the US model of automatic information exchange, the OECD, with the support of the G20, initiated the Common Reporting Standard (CRS) and the multilateral competent authority agreement (MCAA) under which automatic exchange of information was introduced. Information exchange is based on a bilateral matching process, which again leaves ample loopholes for evaders as many countrypairs have not agreed on such a matching. As in the case of TIEAs, 26,000 bilateral agreements would be needed for a complete network, while only 1,539 have been committed to as of November 2017, of which 1,064 were activated during our sample period and 225 are included in the balanced panel. Under these agreements, comprehensive information is automatically exchanged annually on investment income, account balances, and proceeds from the sale of financial assets. This data is reported by banks, custodians, brokers, investment vehicles and insurance firms regarding accounts held by individuals and entities, including trusts and foundations (OECD, 2016). Thus, the CRS is significantly more comprehensive in scope and its automatic nature introduces a real threat of detection that might induce additional evaders to react. Additionally, only about two-thirds of the countrypairs activating the CRS had previously signed TIEAs or DTCs that met the OECD standard. Thus, additional reactions could be triggered by CRS relationships where no prior TIEA has been signed.

Conceptually, these measures again aim for introducing an information exchange between tax havens and other countries, thus making flows and their owners potentially known to relevant tax authorities. The fact that new agreements exist can be seen as indication that authorities are not fully satisfied with their earlier initiatives. If TIEAs and DTCs had fully succeeded, then policy makers would have the best world – from their perspective – as tax evasion would be a marginal phenomenon. In the case of new packaging, however, the problem is even demanding because then the regulation of tax havens had largely failed and they would have to come up with new rules.

In order to shed light on the latter mechanism, we significantly extend our database of information exchange treaties by the activated bilateral CRS relationships, signed FATCA

⁴ These are recorded at: <https://www.treasury.gov/resource-center/tax-policy/treaties/Pages/FATCA.aspx> Last accessed December 5, 2017.

agreements, and two dummies for Swiss-US agreements. This extended database is analyzed in the same way TIEAs are analyzed. If these new agreements reduce capital flows into tax havens, while recent TIEAs do not, this indicates that the form of tax evasion may have changed.

5.2 The reaction of non-bank deposits in tax havens to CRS

Common Reporting Standards (CRSs) contain an automatic information exchange and, thus, are much stricter, seen from the perspective of a tax evader. Interestingly, their introduction leads to a decline in capital flows from non-havens to tax havens as was seen during the early years of TIEAs, again measured by the change in respective bank deposits in tax havens. Table 5 column (1) shows that the respective coefficient shows a 37% reduction in reaction to CRS activation.

--- Table 5 about here ---

In further regressions, we check the sensitivity of this result. In column (2) of Table 5 we add two control variables to the main regression in order to see whether the result may be specific to country size. The variables are, first, the relative importance of the respective tax haven to all reporting countries for the country of outflowing capital (financial weight) and, second, the relative importance of the respective tax haven to all reporting tax havens for the country of outflows (offshore weight). The two significantly positive coefficients show that larger countries are related to larger capital flows but the coefficient of interest, i.e. the one on CRS activation, remains unaffected. In another step, we also add the earlier signed treaties, i.e. TIEAs and DTCs, finding that this does not change the CRS coefficient (column 3). Column 4 investigates the difference between countrypairs that have only signed upon request treaties, only activated CRS relationships, or completed both. Surprisingly, the reaction is highest for those countrypairs that sign both, meaning a CRS on top of an existing TIEA/DTC. This is again evidence that these shifts are qualitatively different from those analyzed in Section 4 and that tax evasion has not disappeared.

Finally, we run tests in our two falsification groups, i.e. we test whether CRS activation has an effect on capital flows either between tax havens or between non-havens. The expectation is that the CRS coefficients should be insignificant, as information exchange is largely irrelevant for capital flows that are not aiming to be secret. Indeed, columns (5) to (8)

in Table 5 show the expected insignificant CRS coefficients. By contrast, the coefficients on the relative country weights are significantly positive and the coefficients on earlier treaties between non-havens remain significantly positive, just as they were for the isolated analysis of TIEAs and DTCs.

Overall, we do find the exact same pattern in empirical results as we did for TIEAs. Capital flows from non-haven countries to tax havens go down significantly if relationships between these countries are activated in the form of CRS. At the same time, the falsification samples are unaffected. While we see a very clear effect for the outbound flows, it is currently impossible to analyze the inbound flows, because CRSs were only activated at the end of our sample, as Figure 2 shows. It seems reasonable to assume that the inbound flows will react with some delay relative to outbound flows, as in the case of TIEAs.

5.3 The reaction of non-bank deposits in tax havens to US agreements

While CRSs have a very broad cross-country coverage, two agreements were set up just between the US and Switzerland. These are featuring prominently in current research since the US is the world's largest economy and Switzerland the largest provider of personal banking services to foreign customers (Johannesen and Stolper, 2017; Johannesen et al., 2017). In this section, we cover country-specific agreements plus the Foreign Account Tax Compliance Act (FATCA), a US-initiative signed bilaterally with 127 countries, 63 of which are included in our balanced panel.

The US led several initiatives targeting Swiss bank secrecy, completing two intergovernmental agreements as a result. While Johannesen and Stolper (2017) as well as Johannesen et al. (2017) analyze these and related measures in detail, we limit our analysis to the two intergovernmental treaties that we expect to be important enough to generate reactions in aggregate bilateral deposit data. The expectation about the respective coefficient signs are straightforward in two cases: the first is an agreement between both governments that the largest Swiss bank, UBS, which was at that time involved in a criminal case of tax evasion in the US, would reveal the identity of 4,450 customers to US authorities. This was the first official undermining of the Swiss banking secrecy in history and, as it was detrimental for tax evaders, we expect a negative coefficient sign. The case is reversed for the US-Swiss bank program that allows Swiss banks to apply for an amnesty by reporting previously hidden deposits of US

nationals. As of November 2017, 78 banks⁵ have signed up for this program, which will potentially report deposits that were previously hidden positions. Indeed, empirical results confirm these expectations, as Table 6, column (1) shows. However, since these treaties only act on one bilateral connection in the sample (the Switzerland – United States countrypairs constitutes 58 observations in each sample), we do not want to overinterpret these results.

--- Table 6 about here ---

Of the 63 countries in our balanced panel that have signed FATCA, eight report liabilities against the US and, thus, results should be more reliable. The coefficient sign is negative and highly significant for the outbound direction (see columns 3 and 4), indicating that capital flows from the US to tax havens are reduced due this agreement.

In summary, for these US agreements, we conclude that we find the same kind of reactions of outbound capital flows as for the earlier TIEAs. This also holds when we control for TIEAs (columns 2 and 4) and the pattern is partly also seen in inbound flows. The latter is most obvious for the two US-Swiss agreements. Regarding FATCA, the inbound coefficient is insignificant. These unclear effects are largely expected because the inbound flows are diluted due to the US-focused nature of these treaties. While in the case of CRS activations and TIEAs we can analyze a world-wide inbound sample looking at positions from all tax havens to all non-havens, FATCA only allows to look for direct flows back to the US. However, evaded capital does not need to flow back to the country where it came from, here the US. For example, the capital may flow from the US to a tax haven and from there to another non-haven, other than the US. Accordingly, we do not measure an inbound flow or any reaction to the agreement. The same logic applies to direct round-tripping flows at the countrypair level where the matching is also weak for TIEAs as we demonstrate in Appendix 1.

Overall, results regarding the US-specific agreements are consistent with the interpretation that tax evasion is still topical. New tax agreements between tax havens and non-havens again reduce capital flows, indicating – in combination with the loss of effect for TIEAs and DTCs – that the form of tax evasion may change, i.e. there is a change in packaging these funds but reactions are still visible.

⁵ See <https://www.justice.gov/tax/swiss-bank-program> for a list of participating banks. Last accessed December 5, 2017.

Another effect going into this same direction, but not referring to tax havens, is indicated by the mounting evidence that some of the activities usually attributed to tax havens are increasingly carried out directly in non-haven countries. Dyreng et al. (2013) show intra US competition along such lines focusing on the role of Delaware as a domestic tax haven. Sharman (2010) provides qualitative evidence showing that financial service providers in OECD countries make it possible to set up sham corporations with bank accounts without any personal identification. Representatives of the Cayman Islands, Bermuda and the Ilse of Man pointed out this ‘hypocrisy’ during the 2016 London corruption summit, arguing that the focus on small jurisdictions is outdated (European Parliament, 2016). This allegation is unsubstantiated: our research shows robust and strong evasion effects in tax havens. The sham corporations set up by Sharman (2010) via firms in OECD countries are also mostly incorporated in small, affluent, and well-governed islands, as characterized by Dharmapala and Hines (2009) and Hines (2010). Still, this new outside option for evaders is a promising venue for future research.

6 Robustness

This section summarizes main analyses and results, demonstrating the robustness of our main findings, while details are presented in [Appendix 2](#). These checks are organized along three themes. First, we analyze our TIEA/DTC results regarding the inbound flows, and second regarding the outbound flows. Third, we check the potential impact of various tax haven lists on our results.

With respect to inbound flows we replicate our main Table 2 for various sub-sample periods: reducing it to the Johannesen and Zucman (2014) sample or excluding the financial crisis. Next, we use various macro controls in the spirit of Hanlon et al. (2015) which leave our results unchanged. Then, we check the identification strategy by testing for effects of information exchange in that parts of bank balance sheets where we do not expect tax evasion to have a significant impact, i.e. bank claims. There is indeed neither an effect for overall claims nor for loan claims. Finally, we broaden our very restrictive definition of treaties in different ways, but results are broadly consistent.

Regarding the outbound flows, we test different treaty definitions and sample lengths and re-create the results of Johannesen and Zucman (2014), the outbound reference study. The results qualitatively confirm our main findings. The use of specific tax haven lists is potentially relevant for the inbound journey, since we use data reported against 46 tax haven counterparties.

Even when reducing this list down to the smallest consensus in the literature our results hold. Only when we start to include countries such as Ireland or Macao as non-havens do we observe a loss of significance.

7 Conclusions

In this paper, we present the first comprehensive evidence on outbound capital flows to tax havens and its inbound return. Our database for capital flows is based on non-bank deposits that the Bank for International Settlements made public in October 2016. For the first time in the literature, we establish a tax evasion effect in inbound bank deposits, mirroring that of outbound flows found by Johannesen and Zucman (2014). The signature of information exchange treaties between a non-haven and a tax haven reduces the amount of bank deposits in both directions significantly. While this is quite plausible for outbound flows since dirty money in tax havens is conventional wisdom, it is surprising for inbound flows, as these funds are already white-washed. It is, however, in line with narrative evidence indicating concerns of tax evaders that inbound flows may stimulate tax authorities to search for earlier illegal behavior.

We complement this main finding by examining flows in more detail: we find that inbound flows also react to treaties with a lag. Moreover, disaggregating treaties into TIEAs and DTCs shows that conventional double taxation treaties do not affect capital flows between non-havens and tax havens in either direction. They rather tend to increase capital flows between non-havens.

Regarding outbound flows, we confirm the Johannesen and Zucman (2014) result for a somewhat different sample. Interestingly, as we cover a longer and newer period, we find that the effect of treaties based on information exchange upon request on capital flows weakens over time. This is unexpected given the earlier result that such treaties generate a shift of capital to non-complying tax havens (Johannesen and Zucman, 2014). If such a shift would continuously occur, new treaties should have larger effects as the remaining tax havens accumulate more and more capital. While evidence does not support the view that existing treaties would have been able to reduce the overall amount of international tax evasion (Zucman, 2013, Alstadsæter et al., 2017b), our results are consistent with the interpretation that tax evaders prefer new kinds of tax evasion. This is corroborated by the finding that new information exchange agreements based on the automatic exchange of information create the same pattern we know from early TIEAs. These new agreements, such as the CRSs and the US-specific FATCA agreements,

significantly reduce the outbound capital flows from non-havens to tax havens, and in some cases – where data are available – we also find this pattern for inbound flows.

In summary, it is an open issue whether, and if so to what degree, existing information exchange agreements are able to substantially fight international tax evasion. Existing evidence shows the success of respective agreements, but at the same time tax evaders seem to find new paths, such as diverting funds into non-complying tax havens, looking for regulatory loopholes in non-havens, and – what we examine here – packaging their funds into new forms that are formally consistent with regulatory requirements. Better understanding such reactions is a crucial issue for policy and, thus, for future research.

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Figure 1: The clean homecoming of dirty money

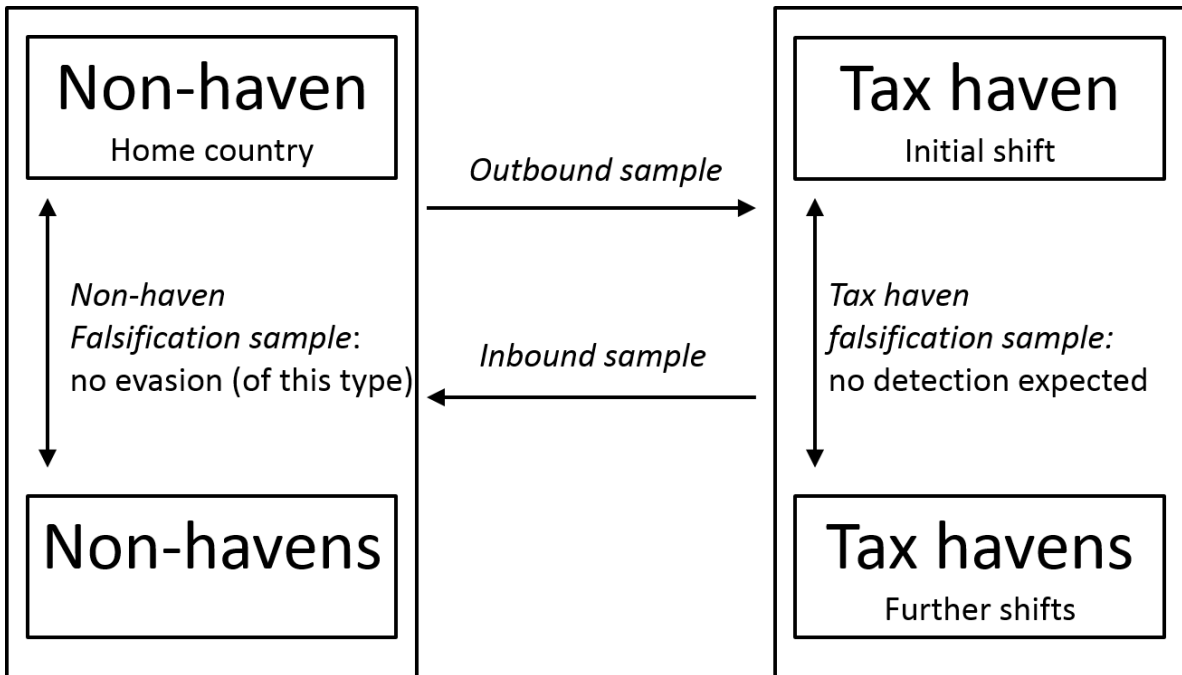
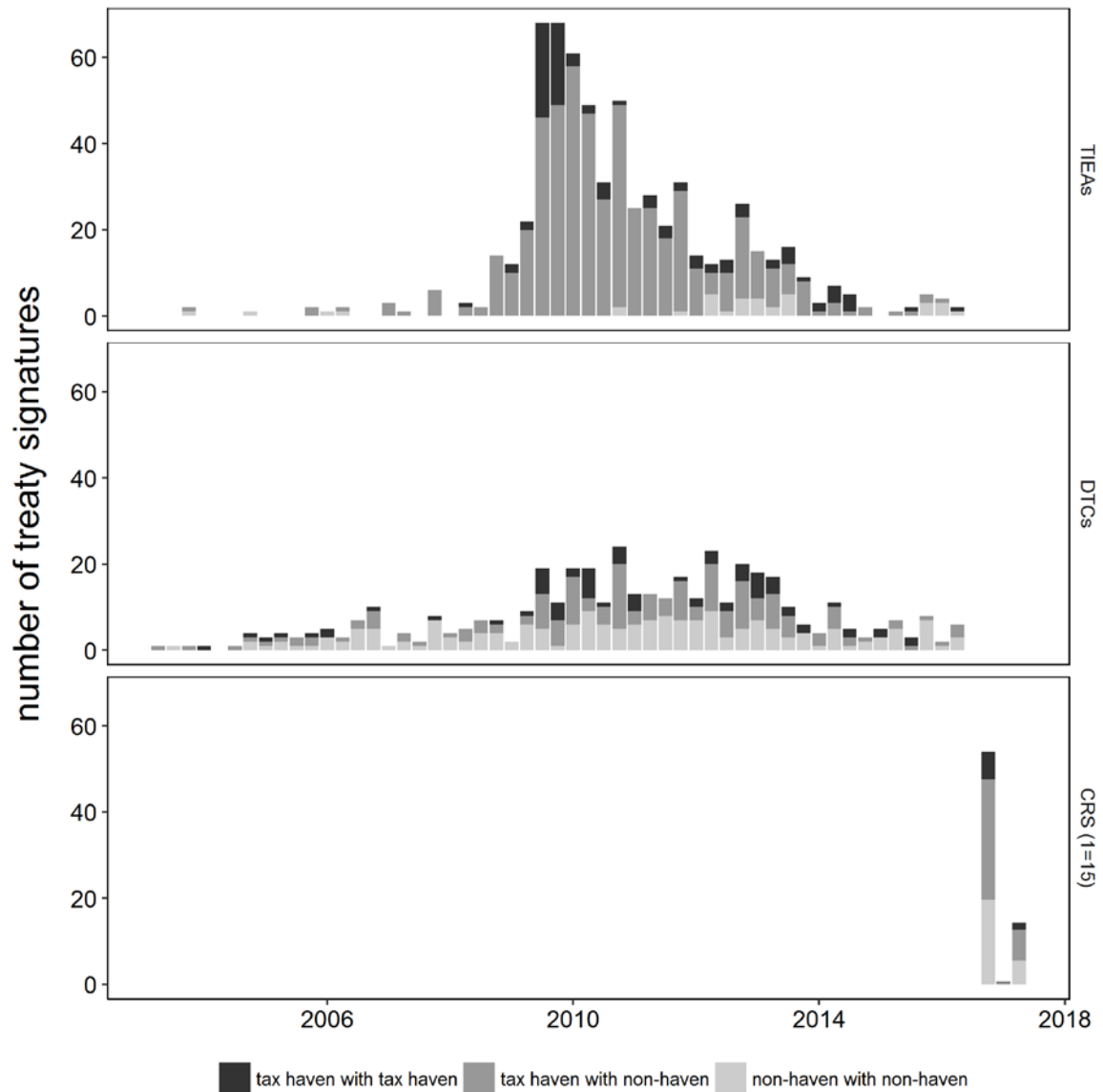


Table 1: Tax havens with significant available deposit data

Jurisdiction	Population (2016)	Affiliation	Treaties with non- havens	BIS bilateral LBS availability as reporting country	As counterparty (balanced panel: non-havens; tax havens)
Austria	8,711,770	EU member state	14	2007q3 – 2017q2	26 (8; 7)
Bahamas	327,316	Commonwealth of Nations	21	-	26 (6; 7)
Bahrain	1,378,904	-	18	-	24 (3; 7)
Belgium	11,409,077	EU member state	11	1977q4 – 2017q2	27 (7; 6)
Bermuda	70,537	British Overseas Territory	26	-	26 (6; 5)
Cayman Islands	57,268	British Overseas Territories	25	-	27 (6; 7)
Chile	17,650,114	-	3	2002q4 – 2017q2	25 (5; 4)
Cyprus	1,205,575	EU member state	15	-	25 (4; 5)
Guernsey	66,297	British Crown Dependency	34	2001q4 – 2017q2	23 (3; 6)
Hong Kong	7,167,403	S.A.R. of the P.R. of China	14	2014q4 – 2017q2	25 (8; 7)
Ireland	4,952,473	EU member state	9	1977q4 – 2017q2	26 (8; 6)
Isle of Man	88,195	British Crown Dependency	28	2001q4 – 2017q2	21 (2; 6)
Jersey	98,069	British Crown Dependency	32	2001q4 – 2017q2	23 (4; 6)
Luxembourg	582,291	EU member state	26	1977q4 – 2017q2	27 (8; 6)
Macao	597,425	S.A.R. of the P.R. of China	9	2013q4 – 2017q2	20 (3; 3)
Panama	3,705,246	-	17	-	27 (6; 5)
Singapore	5,781,728	-	25	-	26 (7; 7)
Switzerland	8,179,294	-	18	1977q4 – 2017q2	27 (8; 6)

Notes: Shows tax havens that report BIS statistics or appear as counterparties of at least 10 reporting countries in the balanced panel (2003:I – 2017:II). Population in column 2 and affiliations in column 3 are taken from the CIA World Factbook. The number of information exchange treaties signed with non-havens in column 4 is based on the OECD Exchange of Tax Information Portal; narrowed down to treaties signed in the balanced panel, meeting OECD standards and including the updated less stringent requirements for information exchange. LBS data availability in column 5 shows the dimension of publicly available BIS data reported by the tax haven. These data include time series of deposits by non-haven counterparties (inbound sample) as well as tax haven counterparties (tax haven falsification sample). The first number in column 6 shows the frequency of each tax haven in the time series reported by all other reporting countries. The first number in parentheses shows the number of non-havens reporting data in the balanced panel (inbound sample), the second indicates the number of other tax havens reporting against this specific tax haven (tax haven falsification sample). A comprehensive table including all tax havens on the list is included as [Appendix A1](#).

Figure 2: Unique information exchange relationships over time



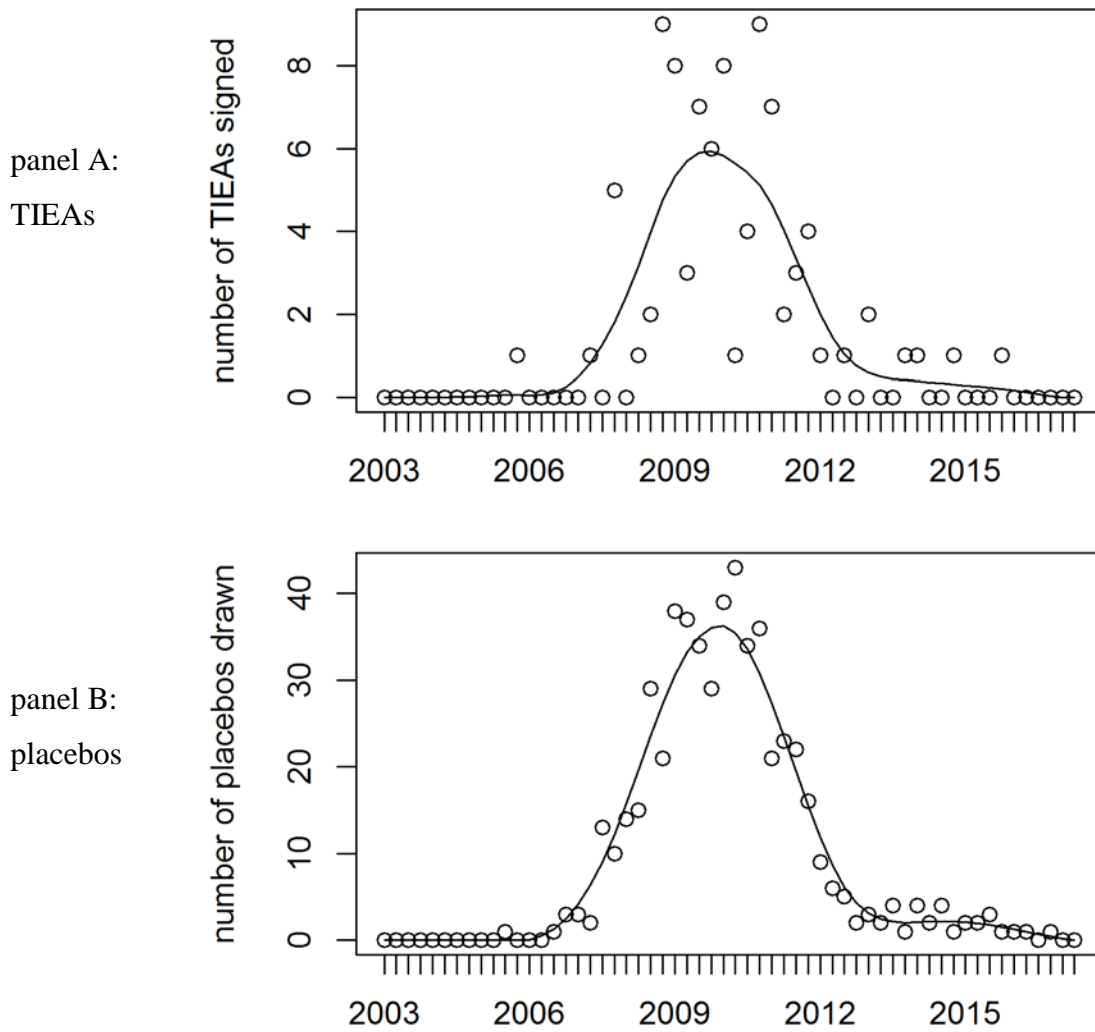
Notes: First depicts the number of signatures of Tax and Information Exchange Agreements (TIEAs, top panel) and Double Taxation Conventions (DTCs, middle panel). Both treaty types are based on data from the OECD Exchange of Tax Information Portal; narrowed down to treaties signed in the balanced panel (2003:I – 2017:II), meeting OECD standards and including the updated less stringent requirements for information exchange. The number of activations of the Common Reporting Standard (CRS, bottom panel) is based on data provided in the OECD Automatic Exchange Portal and divided by 15 for consistent exposition. Shading is based on the countrypair type with treaties between tax haven shown in black, treaties between non-havens shown in light grey and treaties signed between tax havens and non-havens shown in dark grey. The underlying tax haven list combines those of Gravelle (2015) and Johannesen and Zucman (2014) as detailed in Table 1.

Table 2: Reaction of deposits in non-havens (inbound) to information exchange

	<i>Dependent variable: log(deposits)</i>				
	inbound (1)	inbound (2)	inbound (3)	inbound (4)	inbound (5)
Signed (t : T)	-0.364*** (0.121)				
Signed (t : t + 3)		-0.166* (0.095)	-0.142 (0.089)	-0.140 (0.086)	-0.134 (0.096)
Signed (t + 4 : t + 6)		-0.329*** (0.106)	-0.302*** (0.105)	-0.304*** (0.102)	-0.296** (0.118)
Signed (t + 7 : T)		-0.417*** (0.141)	-0.397*** (0.136)	-0.397*** (0.133)	-0.386** (0.156)
Onshore weight			2.477*** (0.559)	1.238* (0.750)	1.238* (0.749)
Financial weight				2.081*** (0.738)	2.082*** (0.737)
Placebo (t : T)					0.016 (0.111)
countrypair f.e.	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes
Observations	9,860	9,860	9,850	9,850	9,850
R ²	0.197	0.199	0.263	0.275	0.275
Adjusted R ²	0.179	0.180	0.245	0.258	0.258

Notes: Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables ‘Signed’ take value 1 if a bilateral treaty specifying information exchange has been signed. Lag structures are shown in parentheses with T referring to the last available observation. Column 1 thus shows the effect of a dummy variable starting to take value 1 with the signature date, zero otherwise. Column 2 changes the lag structure. Column 3 adds the bilateral weight of the non-haven relative to all other non-havens (Onshore weight). Column 4 adds the relative bilateral weight of the non-haven relative to the entire sample (Financial weight) and is used as the inbound baseline henceforth. Column 5 adds the placebo constructed to control for the momentum of the OECD initiative. The dependent variable are data on time series of deposits by tax haven counterparties in non-havens banks (inbound sample). The sample consists of 170 countrypairs reported by 11 reporting non-havens against a combined 44 tax havens over 58 quarters (2003:I – 2017:II). * denotes 10% significance, ** 5% significance, and *** 1% significance.

Figure 3: TIEAs and placebos non-haven – tax havens



Notes: Panel A plots the number of TIEAs signed between tax havens and non-havens over the sample period in the circles. Each circle denotes the number of treaties signed during the respective quarter on the horizontal axis. The solid line depicts a cubic spline calculated over these treaties. This spline is normalized and used to draw a number of placebo treaties which mirror the time momentum of the TIEAs. These placebo treaties are depicted in Panel B. Again, the circles represent the number of placebo treaties per quarter on the horizontal axis. The solid line in Panel B is a spline based on the placebos and only shown for comparability. Since the group of countrypairs that have not signed treaties is much larger than those which have, the vertical axes of both panels differ. The placebo treaties in panel B are then randomly assigned to the countrypairs which have not signed a bilateral treaty.

Table 3: Reactions of deposits in non-havens (inbound) differentiated by treaty type

	<i>Dependent variable: log(deposits)</i>			
	inbound (1)	inbound (2)	non-haven falsific. sample (3)	non-haven falsific. sample (4)
Signed (t : T)	-0.342*** (0.114)		0.231* (0.123)	
DTC signed (t : T)		-0.080 (0.177)		0.232* (0.137)
TIEA signed (t : T)		-0.437*** (0.130)		0.219 (0.173)
Onshore weight	1.237 (0.754)	1.272* (0.750)	0.506** (0.253)	0.506** (0.253)
Financial weight	2.078*** (0.744)	2.063*** (0.730)	1.216*** (0.281)	1.216*** (0.281)
countrypair f.e.	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes
Observations	9,850	9,850	27,432	27,432
R ²	0.273	0.276	0.186	0.186
Adjusted R ²	0.256	0.259	0.170	0.170

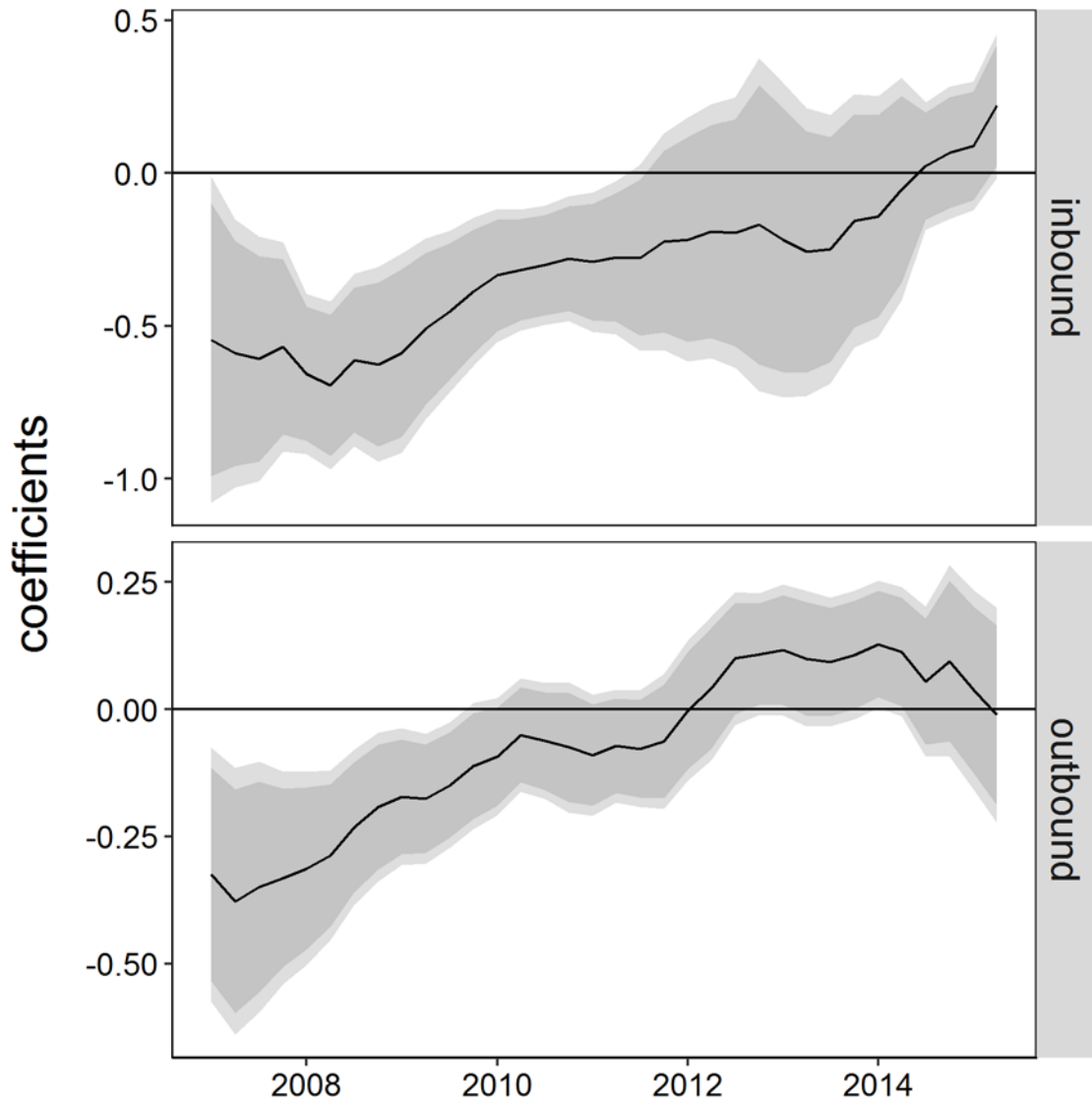
Notes: Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables ‘Signed’ take value 1 starting with the signature of the respective treaty and are zero before that. Column 1 repeats the baseline without detailing the lag structures. Column 2 separates the treatment variable into Double Taxation Conventions (DTC) and Tax and Information Exchange Agreements (TIEA). Columns 3 and 4 repeat the same exercise for the non-haven falsification sample where no tax evasion of our type is expected. The dependent variable in columns 1 and 2 are data on time series of deposits by non-haven counterparties in tax haven banks (outbound sample). The dependent variable in columns 3 and 4 are data on time series of deposits by non-haven counterparties in non-havens banks (inbound sample). The sample consists of 170 countrypairs reported by 11 reporting non-havens against a combined 44 tax havens over 58 quarters (2003:I – 2017:II). The dependent variable in columns 3 and 4 are data on time series of deposits by non-haven counterparties in non-havens banks (non-haven falsification sample). This sample consists of 475 countrypairs reported by the same 11 reporting non-havens but against a combined 146 non-havens. * denotes 10% significance, ** 5% significance, and *** 1% significance.

Table 4: Reaction of deposits in tax havens (outbound) to information exchange

<i>Dependent variable: log(deposits)</i>							
	outbound (1)	outbound (2)	outbound (3)	outbound (4)	outbound (5)	tax haven falsific. sample (6)	tax haven falsific. sample (7)
Signed (t : T)	-0.278*** (0.077)	-0.291*** (0.081)	-0.282*** (0.081)	-0.285*** (0.080)	-0.278*** (0.080)	-0.045 (0.119)	-0.015 (0.117)
Signed (t – 1)		-0.167** (0.074)	-0.157** (0.071)	-0.158** (0.071)	-0.154** (0.070)		-0.002 (0.092)
Signed (t – 2)		-0.152** (0.065)	-0.156** (0.063)	-0.156** (0.063)	-0.152** (0.062)		-0.029 (0.086)
Offshore weight			1.664*** (0.152)	1.340*** (0.168)	1.340*** (0.168)		2.515*** (0.493)
Financial weight				0.972*** (0.209)	0.972*** (0.209)		1.215* (0.716)
Placebo (t : T)					0.010 (0.037)		
countrypair f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,364	32,364	32,364	32,364	32,364	11,426	11,426
R ²	0.080	0.081	0.146	0.152	0.152	0.082	0.186
Adjusted R ²	0.063	0.063	0.130	0.136	0.136	0.061	0.167

Notes: Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables ‘Signed’ take value 1 if a bilateral treaty specifying information exchange has been signed. Lag structures are shown in parentheses with T referring to the last available observation. Column 1 thus shows the effect of a dummy variable starting to take value 1 with the signature date, zero otherwise. Column 2 changes the lag structure. Column 3 adds the bilateral weight of the tax haven relative to all other tax havens (Offshore weight). Column 4 adds the relative bilateral weight of the tax haven relative to the entire sample (Financial weight) and is used as the outbound baseline henceforth. In column 5 we add the placebo constructed to control for the momentum of the OECD initiative. Column 6 and 7 repeat two specifications in the tax haven falsification sample where we do not expect a threat of detection and therefore no reactions. The dependent variable in columns 1-5 are data on time series of deposits by non-haven counterparties in tax haven banks (outbound sample). The sample consists of 558 countrypairs reported by 8 reporting tax havens against a combined 147 non-havens over 58 quarters (2003:I – 2017:II). The dependent variable in columns 6 and 7 are data on time series of deposits by tax haven counterparties in tax haven banks (tax haven falsification sample). This sample consists of 197 countrypairs reported by the same 8 reporting tax havens against a combined 46 tax havens. * denotes 10% significance, ** 5% significance, and *** 1% significance.

Figure 4: Changes in treaty effects over time



Notes: Both panels show estimates in a rolling window of +/- 8 quarters around the quarter plotted on the horizontal axis. Estimations in the top panel are equivalent to Table 2, showing the reaction of deposits in non-havens by tax haven counterparties (inbound). Estimations the bottom panel are equivalent to Table 4 showing the reaction of deposits in tax havens by non-haven counterparties (outbound). The dark grey area denotes 5% significance while the light grey area denotes 10% significance in pointwise confidence bands. Estimated coefficients are plotted on the vertical axis and the horizontal line denotes a 0 effect.

Table 5: Reactions to Common Reporting Standard

	<i>Dependent variable: log(deposits)</i>							
	outbound (1)	outbound (2)	outbound (3)	outbound (4)	tax haven falsific. sample (5)	tax haven falsific. sample (6)	non-haven falsific. sample (7)	non-haven falsific. sample (8)
CRS activated (t : T)	-0.367*** (0.085)	-0.368*** (0.083)	-0.314*** (0.080)		-0.023 (0.143)	0.028 (0.127)	-0.019 (0.110)	-0.041 (0.112)
Financial weight		0.963*** (0.209)	0.970*** (0.209)	0.970*** (0.209)		1.215* (0.717)		1.216*** (0.281)
Offshore weight		1.347*** (0.167)	1.341*** (0.167)	1.342*** (0.167)		2.516*** (0.493)		
Onshore weight								0.506** (0.253)
TIEA/DTC (t : T)			-0.258*** (0.075)			-0.015 (0.112)		0.231* (0.123)
Only CRS				-0.383*** (0.131)				
Only TIEA/DTC				-0.261*** (0.075)				
CRS + TIEA/DTC				-0.530*** (0.130)				
countrypair f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,364	32,364	32,364	32,364	11,426	11,426	27,550	27,432
R ²	0.077	0.149	0.153	0.153	0.082	0.186	0.146	0.186
Adjusted R ²	0.059	0.132	0.137	0.137	0.061	0.167	0.129	0.170

Notes: Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables ‘CRS activated’ take value 1 if automatic exchange of information on the basis of the OECDs Common Reporting Standard has been activated on the bilateral node. Column 1 shows the effect of this dummy variable. In column 2, we add the same bilateral controls used in the other tables. In column 3, we add the treatment variable used in the previous section with TIEA/DTC denoting signatures of information exchange upon request (previously ‘Signed (t : T)’). Column 4 compares countrypairs having only signed the CRS, having only signed TIEA/DTC type treaties and those having signed both. Columns 5 and 6 repeat the main specifications in the tax haven falsification sample. Columns 7 and 8 repeat the same exercise in the non-haven falsification sample with the weight variable therefore changed to Onshore weight: the relative weight of the non-haven compared to other non-havens. The dependent variable in columns 1-4 are data on time series of deposits by non-haven counterparties in tax haven banks (outbound sample). The sample consists of 558 countrypairs reported by 8 reporting tax havens against a combined 147 non-havens over 58 quarters (2003:I – 2017:II). The dependent variable in columns 5 and 6 are data on time series of deposits by tax haven counterparties in tax haven banks (tax haven falsification sample). This sample consists of 197 countrypairs reported by the same 8 reporting tax havens against a combined 46 tax havens. The dependent variable in columns 7 and 8 are data on time series of deposits by non-haven counterparties in non-havens banks (non-haven falsification sample). This sample consists of 475 countrypairs reported by the same 11 reporting non-havens but against a combined 146 non-havens. * denotes 10% significance, ** 5% significance, and *** 1% significance.

Table 6: Reactions to US specific measures

	<i>Dependent variable: log(deposits)</i>			
	inbound (1)	inbound (2)	outbound (3)	outbound (4)
2009 US-CH Agreement	-0.167*** (0.056)	-0.252*** (0.070)	-0.097*** (0.034)	-0.122*** (0.034)
US – Swiss Bank Program	0.324*** (0.097)	0.336*** (0.097)	0.610*** (0.133)	0.589*** (0.121)
FATCA signed (t : T)	0.142 (0.126)	0.104 (0.126)	-0.489*** (0.155)	-0.483*** (0.141)
TIEA/DTC signed (t : T)		-0.361*** (0.122)		-0.277*** (0.077)
countrypair f.e.	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes
Observations	9,860	9,860	32,364	32,364
R ²	0.186	0.198	0.076	0.082
Adjusted R ²	0.166	0.179	0.058	0.064

Notes: Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables value 1 if a bilateral treaty has been signed. Column 1 shows the effect of US specific regulation attempts: two bilateral treaties with Switzerland (the US-CH Agreement and the US – Swiss Bank Programm) that only act on this bilateral dimension as well as FATCA which was signed with 127 countries. In column 2 we control for the international TIEA/DTC treaties used before. Columns 3 and 4 replicate this analysis in the outbound sample. The dependent variable in columns 1 and 2 are data on time series of deposits by tax haven counterparties in non-havens banks (inbound sample). The sample consists of 170 countrypairs reported by 11 reporting non-havens against a combined 44 tax havens over 58 quarters (2003:I – 2017:II). The dependent variable in columns 3 and 4 are data on time series of deposits by non-haven counterparties in tax haven banks (outbound sample). This sample consists of 558 countrypairs reported by 8 reporting tax havens against a combined 147 non-havens* denotes 10% significance, ** 5% significance, and *** 1% significance.

APPENDIX

to accompany

“Dirty money coming home: Capital flows into and out of tax havens”

Table A1: Tax havens full list

Table A2: Disagreements in tax haven lists

Appendix 1: Linkages between outbound and inbound flows (incl. Figure A1)

Appendix 2: Robustness checks (incl. Tables A3 – A7)

Table A1: Tax havens full list

Jurisdiction	Population (2016 CIAfb estimate)	Affiliation	treaties with non- havens	BIS bilateral LBS availability as reporting country	as counterparty (balanced panel: non-havens; tax havens)
Andorra	85,660	-	14	-	21 (2; 4)
Anguilla	16,752	British Overseas Territory	13	-	-
Antigua and Barbuda	93,581	Commonwealth of Nations	11	-	-
Aruba	113,648	Self-governed within the Kingdom of the Netherlands	12	-	19 (1; 2)
Austria	8,711,770	EU member state	14	2007q3 – 2017q2	26 (8; 7)
Bahamas	327,316	Commonwealth of Nations	21	-	26 (6; 7)
Bahrain	1,378,904	-	18	-	24 (3; 7)
Barbados	291,495	Commonwealth of Nations	15	-	22 (2; 4)
Belgium	11,409,077	EU member state	11	1977q4 – 2017q2	27 (7; 6)
Belize	353,858	Commonwealth of Nations	13	-	22 (3; 3)
Bermuda	70,537	British Overseas Territory	26	-	26 (6; 5)
Cayman Islands	57,268 (July 2013 est.)	British Overseas Territories	25	-	27 (6; 7)
Chile	17,650,114	-	3	2002q4 – 2017q2	25 (5; 4)
Cook Islands	9,556	Self-governing in free association with New Zealand	14	-	-
Costa Rica	4,872,543	-	15	-	26 (3; 4)
Curacao	149,035	Self-governed within the Kingdom of the Netherlands	Added with Sint Maarten (Dutch part) to construct synthetic Netherlands Antilles		
Cyprus	1,205,575	EU member state	15	-	25 (4; 5)
Dominica	73,757	Commonwealth of Nations	15	-	22 (1; 1)
Gibraltar	29,328	British Overseas Territory	20	-	23 (2; 4)
Grenada	111,219	Commonwealth of Nations	11	-	20 (1; 2)
Guernsey	66,297	British Crown Dependency	34	2001q4 – 2017q2	23 (3; 6)
Holy See (Vatican City State)	1,000 (2015 est.)	-	-	-	-
Hong Kong	7,167,403	Special Administrative Region of the P.R. of China	14	2014q4 – 2017q2	25 (8; 7)
Ireland	4,952,473	EU member state	9	1977q4 – 2017q2	26 (8; 6)

Isle of Man	88,195	British Crown Dependency	28	2001q4 – 2017q2	21 (2; 6)
Jersey	98,069	British Crown Dependency	32	2001q4 – 2017q2	23 (4; 6)
Jordan	8,185,384	-	-	-	24 (2; 4)
Lebanon	6,237,738	-	-	-	23 (3; 4)
Liberia	4,299,944	-	14	-	24 (5; 4)
Liechtenstein	37,937	-	15	-	22 (3; 4)
Luxembourg	582,291	EU member state	26	1977q4 – 2017q2	27 (8; 6)
Macao	597,425	Special Administrative Region of the P.R. of China	9	2013q4 – 2017q2	20 (3; 3)
Malaysia	30,949,962	Commonwealth of Nations	12	-	28 (7; 5)
Maldives	392,960	-	-	-	18 (1; 1)
Malta	415,196	EU member state, Commonwealth of Nations	17	-	24 (4; 5)
Marshall Islands	73,376	-	10	-	20 (2; 4)
Mauritius	1,348,242	Commonwealth of Nations	13	-	23 (3; 4)
Monaco	30,581	-	14	-	-
Montserrat	5,267	British Overseas Territory	9	-	-
Nauru	9,591	Commonwealth of Nations	-	-	14 (-; 1)
Netherlands Antilles	Synthetic	Added Sint Maarten (Dutch part) and Curacao after Separation	16	-	23 (3; 5)
Niue	1,190	Self-governing in free association with New Zealand	6	-	-
Palau	21,347	-	-	-	-
Panama	3,705,246	-	17	-	27 (6; 5)
Saint Kitts and Nevis	52,329	Commonwealth of Nations	13	-	-
Saint Lucia	164,464	Commonwealth of Nations	13	-	19 (-, 2)
Saint Vincent and the Grenadines	102,350	Commonwealth of Nations	12	-	19 (3, 3)
Samoa	198,926	-	9	-	18 (1; 3)
San Marino	33,285	-	25	-	16 (-, 2)
Seychelles	93,186	-	11	-	23 (3; 4)
Singapore	5,781,728	-	25	-	26 (7; 7)
Sint Maarten (Dutch part)	41,486	Self-governed within the Kingdom of the Netherlands		Added with Curacao to construct synthetic Netherlands Antilles	
Switzerland	8,179,294	-	18	1977q4 – 2017q2	27 (8; 6)
Tonga	106,513	Commonwealth of Nations	-	-	15 (-; -)

Trinidad and Tobago	1,220,479	Commonwealth of Nations	-	-	23 (3; 2)
Turks and Caicos Islands	51,430	British Overseas Territory	13	-	20 (3; 3)
Uruguay	3,351,016	-	18	-	26 (3; 4)
Vanuatu	277,554	Commonwealth of Nations	-	-	18 (3; 3)
Virgin Islands, British	34,232	British Overseas Territory	18	-	-
Virgin Islands, US	102,951	Organized, unincorporated territory of the USA	-	-	-

Notes: Combined tax haven lists of Gravelle (2015) and Johannesen and Zucman (2014). Population statistics in column 2 and affiliations in column 3 are taken from the CIA World Factbook. The number of information exchange treaties signed with non-havens in column 4 is based on the OECD Exchange of Information Portal; narrowed down to treaties that are signed in the sample period, passed the OECD peer review process and include the updated less stringent requirements for information exchange. LBS data availability in column 5 shows since when the tax haven reports now publicly available deposit data to the Bank for International Settlements. Those reports include time series of deposits of non-haven counterparties (the inbound sample) as well as deposits of tax haven counterparties (part of the tax haven falsification sample). The last column, 6, shows the appearance of each tax haven in the counterparty dimension of time series reported by other reporting countries. The first number indicates the total number of time series available. The numbers in parentheses show availability in a balanced panel (2003:I – 2017:II) first indicating the number of non-havens reporting data on the tax haven (forming the inbound sample) and the second and the numbers in parentheses indicates the number of other tax havens reporting against this specific tax haven (part of the tax haven falsification sample).

Table A2: Disagreements in tax haven lists

	Glautier and Bassinger (1987)	Hines and Rice (1994)	OECD (2000)	Dharmapala (2008)	Johannesen and Zucman (2014)	Gravelle (2015)
Andorra		1	1	1	1	1
Anguilla		1	1	1	1	1
Aruba			1	1	1	1
Austria	1				1	
Belgium					1	
Chile					1	
Costa Rica	1				1	1
Dominica		1	1	1	1	1
Ireland	1	1		1		1
Jordan		1		1		1
Lebanon		1		1		1
Macao		1		1	1	1
Malaysia					1	
Maldives		1	1	1		1
Malta		1	1	1	1	1
Marshall Islands		1	1	1	1	1
Mauritius			1	1		1
Monaco		1	1	1	1	1
Nauru			1	1	1	1
Netherlands	1					
Niue			1	1	1	1
Saint Lucia		1	1	1	1	1
Samoa			1	1	1	1
San Marino			1	1	1	1
Seychelles			1	1	1	1
Tonga			1	1		1
Trinidad and Tobago					1	
Uruguay					1	
Virgin Islands, US			1	1	1	1

Notes: All of the surveys shown in Table A2 include the following list of countries as tax havens: Antigua and Barbuda, Bahamas, Bahrain, Barbados, Belize, Bermuda, Cayman Islands, Cook Islands, Curacao*, Cyprus, Gibraltar, Grenada, Guernsey**, Hong Kong, Isle of Man, Jersey**. Liberia, Liechtenstein, Luxembourg, Montserrat, Panama, Saint Kitts and Nevis, Saint Vincent and the Grenadines, Singapore, Sint Maarten (Dutch part)*, Switzerland, Turks and Caicos Islands, Vanuatu, Virgin Islands, British

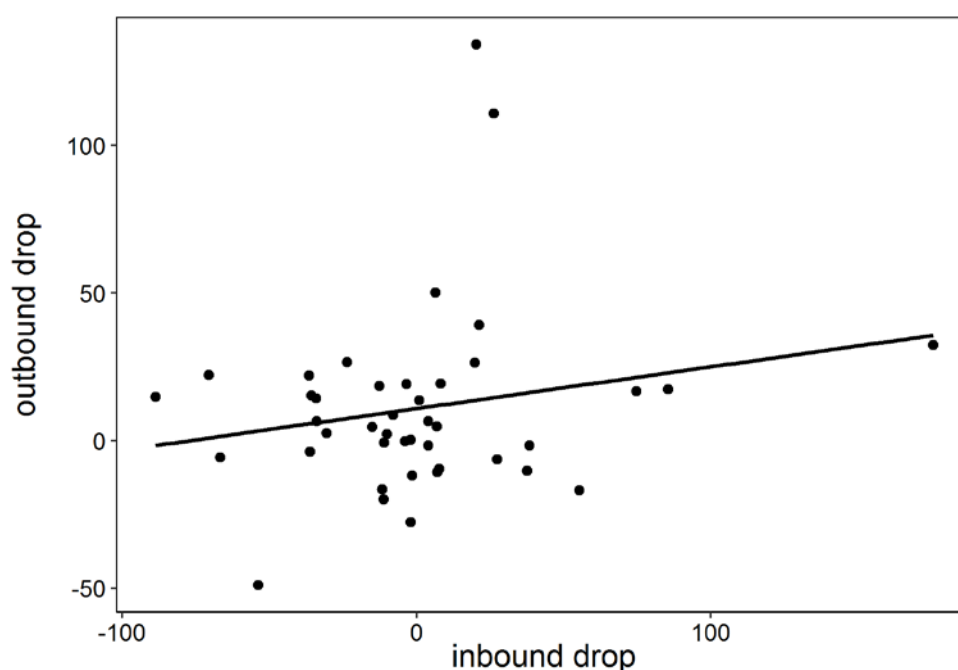
* Curacao and Sint Maarten (Dutch part) are included as the Netherlands Antilles in some publications, they separated on the 10th of October 2010.

** Guernsey and Jersey are included as The Channel Islands in some publications.

Appendix 1: Linkages between outbound and inbound flows

Having examined outbound and inbound flows above, we now analyze relations between these two. In a world of frictionless capital markets, there does not need to be any relation between outbound and inbound effects as the capital that was transferred to a tax haven may be reinvested anywhere in the world again. In fact, however, Hanlon et al. (2015) have demonstrated for their small sample of 4 TIEAs and ending in 2008 that there is a home bias in portfolio investment to the US, so that typically flows return to the same country where they started their journey of tax evasion. While we cannot track specific capital flows we can provide some arguments supporting the claim that there is home bias also in international tax evasion. The rational reason behind this is that many tax evading persons mainly live in one country where they aim for enjoying the benefits from their evaded capital.

Figure A1: Deposit drops in reaction to tax haven – non-haven treaties



Notes: Calculated based on the baseline results. The graph shows changes in deposits in relation to deposit levels around treaties for tax haven – non-haven countrypairs which signed a treaty and for which BIS data is available in both directions. This limits the representation to countrypairs that include havens for which deposit data has been released (see Table 1). To calculate the outbound drop, we take into account the anticipation effects we find and deduct deposits in havens by non-havens in $t+1$ from those in $t-2$ with t being the quarter of signature. For the inbound drop, we deduct deposits in non-havens by haven counterparties in $t+6$ from those in $t+3$ as our baseline results suggest. We show both values as a ratio of mean deposits in the same window ($t:t+3$) starting with the signature date. The solid line is the line of best fit. In line with the main text, treaties are taken from the OECD Exchange of Information Tax Portal as used in the main analysis and the tax haven - non-haven distinction is based on the Gravelle (2015) and Johannesen and Zucman (2014).

To support this idea we plot, for all available countrypairs that signed a treaty and for which BIS data is available in both directions, the relative changes in outbound and inbound bank deposits around an information exchange treaty. In the case of home bias, i.e. a relation between these flows, the changes should be lined up close to a 45-degree line in Figure A1 which has changes in outbound flows on the x-axis and respective changes on inbound flows on the y-axis. This expected relation is indeed evident in the data. Since this graphical representation again includes positions which have nothing to do with evasion, the fact that a positive correlation is visible is additionally striking.

Appendix 2: Robustness checks

This robustness analyses are organized along three themes. To test the robustness of our TIEA/DTC results, we split the analysis into first inbound flows, second outbound flows. Third, we run an extensive robustness check over different tax haven lists since the rough binary classification into tax haven and non-haven could affect all results reported here.

Sample period. To test the robustness of our main results, the inbound journey, we first test whether the results we find are an artifact of the sample period or hold over sub-samples. Table A3 reports the results with column 1 repeating the baseline from Table 2 for easy comparison. Column 2 shows results for a sample limited to 2003:IV - 2011:II, which is the time period used by Johannesen and Zucman (2014). Our main result holds as seen by the significant and economically meaningful reaction in the fourth to the sixth lags. If anything, reactions are stronger. Column 3 shows results with the financial crisis excluded, which we start with 2007:II and the Bear Stearns fund failures, running though 2008:IV, when the US started emerging from the crisis. Again, the results are virtually unchanged, which shows that they do not depend on the sample period.

--- Table A3 about here ---

Macro controls. Due to the rich bilateral nature of our dataset and limited macroeconomic data availability for tax havens, macroeconomic control variable have proven mostly meaningless for our analysis. Quarterly bilateral data starting in 2003 is usually not available for the type of jurisdiction introduced in Table 1: even basic macroeconomic data is hard to find for the typical tax haven. To show this, column 4 includes a number of such control variables in the spirit of Hanlon et al. (2015): namely the log of the number of telephone landlines per 100 persons as well as the population in the counterparty tax haven and the growth rate of GDP per capita as well as the population in the reporting non-haven. This data is taken from the yearly World Development Indicators dataset and linearly interpolated to match our quarterly frequency. As it is neither bilateral, nor available for all tax havens, the sample is reduced. As expected, results are unchanged and coefficients are mostly insignificant.

Bank claims. Next, we check the identification strategy by testing for effects of information exchange in the parts of bank balance sheets where we do not expect tax evasion to have a significant impact. So far, we have used bank liabilities against international nonbank counterparties to proxy deposits of firms and households. Indeed, if such deposits are used for tax evasion and if we have identified tax evasion correctly, we should find no discernible effects

for bank claims. In non-haven countries, from which this data are reported, we do not expect bank lending to react significantly to lower evaded deposits and banks themselves have not been convicted in the kind of personal tax evasion cases we identify. We neither find results for overall claims (column 5) nor for loan claims (column 6). The fact that claims are lower after more than three years (lag 13 and later) is consistent with our interpretation of a general negative signaling effect of treaties on the attractiveness of the tax haven that signs them.

More treaties. In order to establish the robustness of our results to this choice of treaties, we broaden our very restrictive definition of which treaties constitute a credible threat of detection. Table A4 compares the baseline (column 1) to the inclusion of the 31 treaties signed within the balanced panel dimension that do not include paragraphs 4 and 5¹ but were reviewed by the OECD and met the standard (column 2). Then, we include another 61 treaties that were not reviewed at the analysis (column 3). Finally, we also include those 22 treaties that were reviewed, but failed to meet the OECD standard (column 4). Results are broadly consistent.

--- Table A4 about here ---

Replication of the outbound flow analysis. Since the outbound journey is inspired by Johannesen and Zucman (2014), we continue the robustness analysis by replicating their results with our data. Thus, Table A5 uses their preferred lag order specification in order to compare effects directly. Column 1 shows the results using our sample, tax haven list, and treaty specification. Column 2 again reduces the sample length to 2003:IV-2011:II, which, if anything, makes results stronger. While it is not our intention to evaluate the quantitative effect of tax and information exchange agreements, it seems worthwhile to determine what gives rise to the lower results.

--- Table A5 about here ---

Therefore, we first change the tax haven list to that used by Johannesen and Zucman (2014, column 3) before also employing their treaty variable which they made available (column 4). The treatment variable now includes a number of treaties that were not reviewed or did not meet the OECD peer review standard as well as domestic law changes triggering information exchange. The results in column 4 are quantitatively very close to those of Johannesen and Zucman (2014). Thus, it is not the sample length or the tax haven list that

¹ As described before, these paragraphs relax the requirements of the requesting party from demanding 'necessary' to 'foreseeably relevant' information.

differentiates our quantitatively larger results, but the differing list of information exchange treaties. Since the goal of this study is not to evaluate the OECD initiative quantitatively but to employ information exchange treaties to identify tax evasion, this again confirms our choice of a very restrictive treaty definition as a lens to detect tax evasion.

Tax haven list. The controversies surrounding tax haven lists are very relevant for the inbound journey, since we use data reported against 46 tax haven counterparties. This data can change considerably when changing the tax haven list. Table A6 reports results for different lists that are commonly used in the literature (see the appendix for an overview of different lists). Column 1 repeats the baseline results before we change the tax haven dimension in the counterparties to that of the OECD (2000) list (column 2) and the list provided in Gravelle (2015) (column 3). Results only change in magnitude, staying significant throughout.

--- Table A6 about here ---

A tougher test is the reduction of the tax haven list to the absolute minimum on which recent studies agree. Therefore, we compile a tax haven list with the consensus candidates of Hines and Rice (1994), the OECD (2000), Dharmapala (2008), Dharmapala and Hines (2009), Johannesen and Zucman (2014), Bilick and Fuest (2014), Hanlon et al. (2015), and Gravelle (2015). This means that we remove 22 countries² from the tax haven list used so far, including, most notably, Belgium, Ireland, and Macao. Column 4 shows that our results hold despite losing almost half of our observations. To go even further, we now include the tax havens thus dropped from the tax haven list as non-haven countries. This means that we use their reported data against tax havens as if they were non-havens. Ireland and Macao are now in the same category as France and Sweden when it comes to facilitating tax evasion. Column 5 shows that this finally does reduce both the magnitude and the significance of our results. Signs remain consistent but the effect is blurred. Since tax haven lists necessarily carry a certain judgement, we report the results, even though including Macao as a non-haven, for example, seems quite unreasonable.

For completeness, Table A7 repeats the same exercise for the outbound journey. Since the tax haven dimension in this direction is the reporting country level, we rely on a much smaller number of tax havens. Indeed, reducing the list from our baseline results to the

² These include Aruba, Austria, Belgium, Chile, Costa Rica, Ireland, Jordan, Lebanon, Macao, Malaysia, Maldives, Mauritius, Nauru, Niue, Samoa, San Marino, Seychelles, Tonga, Trinidad and Tobago, Uruguay and the US Virgin Islands.

consensus list drops only three tax havens that report data: Belgium, Chile, and Ireland. The OECD (2000) and Gravelle (2015) lists are identical when reduced to reporting havens, which is why we drop the former. We can see that results fluctuate somewhat in magnitude but remain unchanged qualitatively.

--- Table A7 about here ---

Table A3: Robustness of inbound results

	<i>Dependent variable:</i>					
	log(deposits)				log(claims)	log(loans)
	baseline inbound (1)	JZ14 sample inbound (2)	no crisis inbound (3)	inbound (4)	inbound (5)	inbound (6)
Signed (t : t + 3)	-0.140 (0.086)	-0.134 (0.095)	-0.166* (0.086)	-0.098 (0.096)	0.005 (0.108)	0.069 (0.105)
Signed (t + 4 : t + 6)	-0.304*** (0.102)	-0.383*** (0.116)	-0.300*** (0.106)	-0.254** (0.111)	-0.057 (0.114)	0.062 (0.111)
Signed (t + 7 : T)	-0.397*** (0.133)	-0.557*** (0.191)	-0.403*** (0.135)	-0.376*** (0.140)		
Onshore weight	1.238* (0.750)	1.955** (0.808)	1.255 (0.774)	0.186 (0.561)	-0.080 (0.663)	0.159 (0.656)
Financial weight	2.081*** (0.738)	1.791*** (0.693)	2.003*** (0.747)	2.584*** (0.755)	2.113*** (0.801)	1.562* (0.817)
log(landlines cparty)				0.079 (0.108)		
Growth of GDP/capita				0.032* (0.019)		
log(pop)				-4.655* (2.717)		
log(pop cparty)				-0.087 (0.716)		
Signed (t + 7 : t + 9)					-0.133 (0.122)	-0.019 (0.136)
Signed (t + 10 : t + 12)					-0.212 (0.135)	-0.073 (0.146)
Signed (t + 13 : T)					-0.386*** (0.147)	-0.246 (0.183)
countrypair f.e.	Yes	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,850	5,270	9,000	7,004	9,708	8,547
R ²	0.275	0.275	0.282	0.255	0.180	0.147
Adjusted R ²	0.258	0.246	0.263	0.234	0.160	0.125

Notes: Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables 'Signed' take value 1 with signature. Lag structures are shown in parentheses with T referring to the last available observation. Column 1 repeats the baseline, column 2 limits the sample time dimension to that used by Johannesen and Zucman (2014), 2003:IV-2011:II. Column 3 (no crisis) drops the period from 2007:II – 2008:IV. Column 4 adds macroeconomic controls to the baseline specification. Columns 5 and 6 change the dependent variables to bank claims and bank loan claims respectively. The dependent variable are data on time series of deposits by tax haven counterparties in non-havens banks (inbound sample). The sample consists of 170 countrypairs reported by 11 reporting non-havens against a combined 44 tax havens over 58 quarters (2003:I – 2017:II), reduced over the columns as mentioned above. * denotes 10% significance, ** 5% significance, and *** 1% significance.

Table A4: Robustness of inbound results to treaty definition

	<i>Dependent variable: log(deposits)</i>			
	baseline (1)	balanced signature (2)	+ no para 4/5 (3)	+ not reviewed (4)
Signed (t : t + 3)	-0.166* (0.095)	-0.009 (0.126)	-0.037 (0.095)	-0.159* (0.082)
Signed (t + 4 : t + 6)	-0.329*** (0.106)	-0.196** (0.092)	-0.171** (0.082)	-0.302*** (0.093)
Signed (t + 7 : T)	-0.417*** (0.141)	-0.392*** (0.128)	-0.387*** (0.118)	-0.462*** (0.132)
countrypair f.e.	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes
Observations	9,860	9,860	9,860	9,860
R ²	0.199	0.199	0.200	0.204
Adjusted R ²	0.180	0.180	0.181	0.185

Notes: Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables ‘Signed’ take value 1 if a bilateral treaty specifying information exchange has been signed. Lag structures are shown in parentheses with T referring to the last available observation. Column 1 repeats the baseline, for simplicity without using the weight variables. Column 2 ignores treaties signed before the sample period thus adding countries which already had the possibility to request information to the control group. Column 3 includes treaties signed in the sample period that did not include the paragraphs relaxing the requirement to request information to ‘foreseeably relevant’ from ‘necessary’ information. Column 4 additionally includes treaties signed in the sample period that were not reviewed by the OECD and column 5 includes treaties that were reviewed but did not meet the standard. The dependent variable are data on time series of deposits by tax haven counterparties in non-havens banks (inbound sample). The sample consists of 170 countrypairs reported by 11 reporting non-havens against a combined 44 tax havens over 58 quarters (2003:I – 2017:II).. * denotes 10% significance, ** 5% significance, and *** 1% significance.

Table A5: Robustness of outbound results

	<i>Dependent variable: log(deposits)</i>			
	baseline outbound (1)	JZ14 sample outbound (2)	+ JZ14 tax haven list outbound (3)	+ JZ14 treaty definition outbound (4)
Signed (t)	-0.181** (0.072)	-0.271*** (0.087)	-0.258*** (0.077)	0.004 (0.049)
Signed (t + 1)	-0.134* (0.075)	-0.292*** (0.089)	-0.265*** (0.088)	-0.086 (0.063)
Signed (t + 2)	-0.161** (0.073)	-0.327*** (0.104)	-0.301*** (0.104)	-0.117* (0.065)
Signed (t + 3)	-0.229*** (0.073)	-0.451*** (0.111)	-0.378*** (0.101)	-0.190*** (0.073)
Signed (t + 4 : T)	-0.301*** (0.082)	-0.437*** (0.097)	-0.363*** (0.090)	-0.214*** (0.076)
countrypair f.e.	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes
Observations	32,364	17,298	16,554	16,554
R ²	0.081	0.107	0.127	0.123
Adjusted R ²	0.063	0.075	0.096	0.092

Notes: Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables ‘Signed’ take value 1 if a bilateral treaty specifying information exchange has been signed. Lag structures are shown in parentheses with T referring to the last available observation. Column 1 repeats the outbound baseline. Column 2 reduces the sample to the time dimension available to Johannesen and Zucman (2014): 2003:IV-2011:II. Column 3 additionally changes the tax haven list to the one used by those authors and column 4 uses less restrictive treaties employed by them. The dependent variable are data on time series of deposits by non-haven counterparties in tax haven banks (outbound sample). The baseline sample consists of 558 countrypairs reported by 8 reporting tax havens against a combined 147 non-havens and is reduced as the tax haven list is changed in column 3. The time dimension in the baseline is 58 quarters (2003:I – 2017:II) and reduced over columns as mentioned above. * denotes 10% significance, ** 5% significance, and *** 1% significance.

Table A6: Robustness of results to changes in tax haven list: inbound

	<i>Dependent variable: log(deposits)</i>				
	baseline inbound (1)	OECD 2000 inbound (2)	Gravelle 2015 inbound (3)	consensus tax havens inbound (4)	consensus both inbound (5)
Signed (t : t + 3)	-0.166* (0.095)	-0.302*** (0.108)	-0.283*** (0.103)	-0.217** (0.101)	-0.074 (0.093)
Signed (t + 4 : t + 6)	-0.329*** (0.106)	-0.484*** (0.128)	-0.454*** (0.120)	-0.371*** (0.117)	-0.161 (0.117)
Signed (t + 7 : T)	-0.417*** (0.141)	-0.569*** (0.169)	-0.546*** (0.159)	-0.409*** (0.157)	-0.214 (0.180)
countrypair f.e.	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes
Observations	9,860	6,670	7,772	6,322	8,526
R ²	0.199	0.239	0.235	0.213	0.097
Adjusted R ²	0.180	0.219	0.216	0.191	0.074

Notes: Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables ‘Signed’ take value 1 if a bilateral treaty specifying information exchange has been signed. Lag structures are shown in parentheses with T referring to the last available observation. Column 1 repeats the inbound baseline, column 2 limits the tax haven list to that proposed by the OECD (2000), column 3 to that used by Gravelle (20015). Column 4 (consensus tax havens) limits the tax havens to those agreed on by the authors cited in the text and detailed in Appendix A2. Column 5 (consensus both) adds the countries thus excluded from the tax-haven list to sample as non-haven countries. The dependent variable are data on time series of deposits by tax haven counterparties in non-havens banks (inbound sample). The baseline sample consists of 170 countrypairs reported by 11 reporting non-havens against a combined 44 tax havens and is changed over the columns as tax haven lists are changed. The time dimension in the baseline is 58 quarters (2003:I – 2017:II). * denotes 10% significance, ** 5% significance, and *** 1% significance.

Table A7: Robustness of results to changes in tax haven list: outbound

	<i>Dependent variable: log(deposits)</i>			
	baseline outbound (1)	Gravelle 2015 outbound (2)	consensus tax havens outbound (3)	consensus both outbound (4)
Signed (t : t+3)	-0.291*** (0.081)	-0.331*** (0.082)	-0.365*** (0.080)	-0.318*** (0.075)
Signed (t - 2)	-0.152** (0.065)	-0.169** (0.066)	-0.155*** (0.056)	-0.116** (0.053)
Signed (t - 1)	-0.167** (0.074)	-0.193*** (0.075)	-0.182*** (0.066)	-0.129** (0.063)
countrypair f.e.	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes
Observations	32,364	24,766	22,272	25,288
R ²	0.081	0.086	0.099	0.098
Adjusted R ²	0.063	0.068	0.081	0.080

Notes: Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables ‘Signed’ take value 1 if a bilateral treaty specifying information exchange has been signed. Lag structures are shown in parentheses with T referring to the last available observation. Column 1 repeats the outbound baseline. Column 2 limits the tax haven list to that proposed by Gravelle (20015). Column 3 (consensus tax havens) limits the tax havens to those agreed on by the authors cited in the text and detailed in Appendix A2. Column 4 (consensus both) adds the countries thus excluded from the tax-haven list to sample as non-haven countries. The dependent variable are data on time series of deposits by non-haven counterparties in tax haven banks (outbound sample). The baseline sample consists of 558 countrypairs reported by 8 reporting tax havens against a combined 147 non-havens and is changed as the tax haven list changes over the columns. The time dimension in the baseline is 58 quarters. * denotes 10% significance, ** 5% significance, and *** 1% significance.