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# **Tax evasion in new disguise?**

## **Examining tax havens' international bank deposits\***

Lukas Menkhoff<sup>†</sup> and Jakob Miethe<sup>‡</sup>

### **Abstract**

Recent efforts to reduce international tax evasion focus on information exchange with tax havens. Using bilateral bank data for 1,397 country pairs in a balanced quarterly panel from 2003:I – 2017:IV, we first show that information-on-request treaties with tax havens reduce bank deposits in tax havens by 27.5%. Second, also deposits from tax havens in high tax countries decline after such treaties are signed, giving authorities a second angle to detect tax evasion. Both reactions dissipate over time and treaties signed after 2010 trigger no further reactions. These results cannot be explained by deposit shifting alone and we find no evidence of transitioning into legality. Third, recent policy initiatives based on the automatic exchange of bank information lead to very similar initial reactions as earlier treaties, consistent with adjustments on the part of tax evaders. This suggests that tax evaders adapt to established information exchange treaties by using new disguises to hide their true income, and react again to new measures. These results cast doubt on the effectiveness of current forms of information exchange to tackle international tax evasion.

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# **Tax evasion in new disguise?**

## **Examining tax havens' international bank deposits**

### **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 capital in tax havens is not necessarily illegal, it can be, for example, if interest income is not reported in the country where the recipients pay their taxes. Such international tax evasion is of great concern to policy makers for at least three reasons: first, it reduces tax revenues; second, it reduces effective taxation of the rich; and, third, the majority may lose trust in both the tax system and state institutions.

Therefore, the fight against tax evasion is high on the international agenda. Whistleblowers and leaks of large datasets indicate that international tax evasion via tax havens remains a relevant problem. Bank deposits of non-banks are shifted into tax havens where the ownership of these deposits is hidden. While we know that such deposits initially reacted to information-exchange-on-request (IoR) agreements that are exclusively aimed at tax evaders (Hanlon et al., 2015; Johannesen and Zucman, 2014), important questions remain unanswered. Have these agreements eliminated tax evasion over time or is there evidence that a significant share of bank deposits in tax havens still evades taxation? If we find reactions of deposits in tax havens (outbound deposits); do we find the same pattern in bank deposits from tax havens in non-havens (inbound deposits)? Further, how do non-banks' bank deposits in tax havens react to new types of international agreements that are based on the automatic exchange of bank information?

These three questions are addressed by relying on a data set released in October 2016, when the Bank for International Settlements (BIS) made a significant portion of its bilateral locational banking statistics available, including bilateral deposit data. Using the bank deposits of non-banks reported in this data, the aforementioned three questions can be addressed for the first time, to the best of our knowledge. Results shed new light on international tax evasion: Interestingly, the 'success' of early IoR treaties does not show that they have realized their goal but rather that tax evaders seem to adapt to regulation by putting their bank deposits into new disguises. This major finding is supported by empirical examinations of the three questions.

First, we extend the analysis of bank deposits in tax havens (outbound) by relying on the

event study approach of Johannesen and Zucman (2014). Their study is the first to analyze changes in bank deposits in tax havens by considering the impact of bilateral tax information exchange agreements. They had private access to a subset of the BIS data, going from 2002 to mid-2011 in the outbound direction, which are now largely public but with a longer time dimension. For a similar group of countries, we confirm that, despite having twice as many tax agreements by 2017, the effects of IoR treaties on bank deposits in tax havens are qualitatively the same. Going beyond their sample period, we find a – so far unreported – gradual decline of this effect for new IoR treaties over time, starting around 2010. That means non-banks’ bank deposits in tax havens no longer react to newly signed treaties, raising the question of whether treaties have successfully eliminated tax evasion or whether tax evaders have adapted to the new regulation by structuring their evasion schemes in a way that circumvents IoR treaties.

Second, the BIS data allow us to address a gap in the literature by also considering the inbound direction, i.e. bank deposits of non-banks from tax havens in non-havens. ‘Inbound’ and ‘outbound’ are used from the perspective of the tax evader here who deposits outbound (in the tax haven) or inbound (from the tax haven to the non-haven). The reaction to IoR treaties in inbound deposits may seem surprising, given the fact that the money is already deposited in tax havens and the “true” owners are hidden. Still, the ownership structure of shell companies, private foundations, and trusts, as well as the connected bank accounts, is theoretically vulnerable to detection if, for example, the tax evader is documented as a beneficial owner. Thus, while single deposits cannot be followed through the tax haven cloud, the analysis of inbound deposits provides a second angle for tax authorities in home countries to tackle tax evasion. They can take the occurrence of such inbound deposits to investigate their ownership and look for evidence of illegal behavior.

Third, in order to investigate the longer-term effects of information exchange on bank deposits in and from tax havens, we also assess newer forms of information exchange. In this respect, we analyze the OECD’s Mutual Competent Authority Agreement, which allows for automatic exchange of information (AEI) under its 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 original goal of regulators was realized, former tax evaders are compliant and the new agreements do not reduce bank deposits in tax havens. Our data show, however, the opposite: new agreements produce effects that are hauntingly similar to those of the older IoR treaties. This evidence speaks against the long-term success of bilateral information exchange, but is consistent with the hypothesis that tax evaders hide their funds

using new disguises and with other results in the literature that find no decline in aggregate offshore wealth (Alstadsæter et al., 2018; Pellegrini et al., 2016; Zucman, 2013). We also take into account amnesties and voluntary disclosure programs, showing that the results are not driven by their introduction. Unfortunately, we cannot analyze the longer-term effects of the AEs due to their novelty and the resulting lack of observations.

**Literature.** Information about tax havens is generally rare, including bank balance sheet positions. Due to such incomplete data, it is common practice in the literature on international financial integration 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-Ferretti, 2008). Identifying or measuring tax evasion empirically is challenging, but there is progress, as Slemrod (2015) summarizes.

One of the rare studies with access to data about bank positions in tax havens is Johannesen and Zucman (2014). This benchmark study for our work applies an event-study type approach to changes in banks deposits of non-banks in tax havens in relation to IoR agreements. This study is part of a strand of research working with tax haven data that, on top of tax haven bank deposits (see also Johannesen, 2014b), works with surveys of institutions registered in tax havens (Heckemeyer and Hemmerich, 2018) or leaked datasets (Caruana-Galizia and Caruana-Galizia, 2016; O'Donovan et al., 2019). Johannesen and Zucman (2014) find sizable significant reductions in these bank deposits as treaties threaten bank secrecy. While relying on their method, we use more recent and broader data. Thus, we can replicate their results, but find that it no longer holds during more recent years. Interestingly, however, similar results show up again when CRS agreements (with first activations in the last quarter of 2016) are analyzed. The extended data also allow constructing another clean falsification sample (non-haven to non-haven deposits) and to analyze inbound deposits.

Relying on indirect identification strategies and data reported by non-havens, only Hanlon et al. (2015) provide a glimpse of the existence of tax evasion effects in inbound positions and their sensitivity to tax agreements. In a sample ending with 2008, they show that four IoR treaties between the US and tax havens negatively affected inward foreign portfolio investment from these 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 provide results based on both deposit data from tax havens (outbound) and non-havens (inbound) taken from the same dataset. The large international dimension also supports our identification strategy.

This paper proceeds in seven 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 the effect of IoR treaties on outbound deposits are presented in Section 4, while Section 5 provides analogous results for inbound deposits and Section 6 examines potential effects of more recent information exchange agreements. Robustness checks are presented in Section 7, while Section 8 provides conclusions.

## **2 Identification of international tax evasion**

In this section, we describe our identification of international tax evasion via tax havens, in line with typical procedures during our sample span. Section 2.1 lays out the dynamics of bank deposits in and from tax havens. Section 2.2 discusses the definition of the first target of such deposits, i.e. tax havens. Finally, Section 2.3 introduces into the international information exchange agreements that serve as our tool to identify evaded bank deposits.

### **2.1 Bank deposits in and from tax havens**

Transferring capital from a non-haven to a tax haven, called the ‘outbound deposit’ here, is a relatively simple matter for evaders. Setting up a bank account offshore can be done online for a small fee (see Sharman, 2010, for some real-life examples). As offshore wealth is typically held by very wealthy individuals, such fees are negligible (Alstadsæter et al., 2019). The most common way to transfer funds to that account is via an invoice for ‘consulting services’ carried out by the evader (see Zucman, 2013, 2014, for examples). As long as bank secrecy is upheld in the tax haven, this information is not exchanged with the home authorities of the depositing individual. Such deposits – protected by tax haven bank secrecy – can generate untaxed capital gains if the individual chooses to not self-report and evade taxes.

Figure 1 provides a schematic visualization of how funds are deposited in and from tax havens. First, funds are moved into the tax haven (‘outbound’ from the perspective of a tax evader). Transfers between tax havens can take place, layers of secrecy can be added with a network of shell companies, 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 for various purposes (‘inbound’ from the perspective of the tax evader). Finally, positions between non-haven countries should not be connected to international tax evasion of the same form we are

analyzing. Therefore, we use capital positions between non-havens as a second falsification group (non-haven falsification).

— Figure 1 about here —

As inbound deposits are less researched than outbound deposits, we want to clarify three issues in this regard: (i) Regardless of tax evasion motives or not, capital that is brought to tax havens will typically not be invested in the local economy of a tax haven but somewhere in the world economy. Inbound (bank) deposits are just one way to hold these assets and some of these bank deposits may be part of a tax evasion story. (ii) We do not expect a direct linkage between outbound and inbound deposits. Outbound deposits will also be used to buy assets other than inbound bank deposits and this can occur in different countries; however, there is some evidence of home bias in international capital flows (see Coeurdacier and Rey, 2013, for an overview).

(iii) When funds that are linked to illegal activity, such as not declaring capital gains and, thus, evading taxation, are transferred to a bank account in a non-haven, the individual in question can hide their identity behind the veil of secrecy erected by tax haven shell companies that opened the bank account. However, there is a risk of detection here as the ownership structure of these funds might be exposed. 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. The World Bank, cooperating with the United Nations Office on Drugs and Crime (UNODC), records hundreds of cases of grand corruption in their ‘Stolen Assets Recovery Initiative’ with numerous cases including tax evasion using such schemes. Thus, qualitative evidence on such arrangements is available, due in part to convictions.<sup>1</sup> The US Internal Revenue Service provides another list of exemplary cases including such setups.<sup>2</sup> In many of these cases, bank accounts with non-haven banks were opened by shell companies or trusts domiciled in tax havens. Such schemes show up in the inbound deposits data we use and are part of; for example, the 6.5 billion USD French

<sup>1</sup>For example, in July 2007, Diepre Alamieyeseigha was sentenced to two years in prison for charges of false declaration of assets and money laundering. In order to transfer funds from his native Nigeria, where he was state governor, he used corporate vehicles in the Seychelles, the British Virgin Islands, the Bahamas, and South Africa, via which he transferred funds all over the world, including into a U.S. dollar account with UBS in London (van der Does de Willebois et al., 2011). Such funds show up as ‘inbound deposits’ by tax haven counterparties in the data we employ.

<sup>2</sup>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 shell companies 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>

banks reported in deposits from the channel island of Jersey (98,069 inhabitants) in the middle of 2013 in our data.

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 in 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, 2009a; Hines, 2010).

The empirical literature commonly employs relatively unrestricted tax haven lists. For most research questions, including a de facto non-haven into the tax haven list leads to a more conservative estimation. Therefore, we follow this convention and use, as our baseline, a large list of tax havens obtained by combining the list of tax havens by Gravelle (2015), which already collects different sources (such as Hines and Rice, 1994), with that of Johannesen and Zucman (2014), which we also test separately. This tax haven classification is altered in extensive 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 Table A1](#) and the few cases of disagreement over tax havens in different studies are summarized in [Appendix Table A2](#).

— 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 information exchange treaties signed by tax haven countries. Column 5 shows the number of countries that report deposit data on that specific tax haven, revealing the availability of a large cross-sectional dimension. Column 6 shows end-of-sample total liabilities reported against non-bank counterparties in the entire world to give an idea of financial size. Finally, columns 7 and 8 indicate if the respective tax haven is used in our outbound and/or inbound analysis. As an example: Guernsey, a British Crown Dependency with fewer than 70,000 inhabitants but more than 31 billion USD in total external bank liabilities, has signed 34 treaties with non-haven



countries during the 60 quarter sample period (2003:I – 2017:IV) that meet our requirements, as outlined in the next section. It reports bilateral data to the BIS and is thus used in our outbound sample, and twenty-four other reporting countries report data against Guernsey. To create a balanced panel, we drop all but three non-haven reporting countries that report positions against Guernsey (part of the inbound sample) and all but 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 mainly focus on detecting the delinquent by agreeing to exchange information between tax authorities (OECD, 1998, 2000). Such treaties became popular after April 2009, when the G20 decided to sanction tax havens if they did not sign at least 12 such treaties (G20, 2009).

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 this dilemma of tax havens and predict a negative signaling effect (for tax evaders) 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 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 that have 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 ‘foreseeably 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 III and Tirard, 2016). Thus, 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 (see Dwenger et al., 2016, for a discussion of intrinsic versus extrinsic incentives).

Moreover, loopholes are abundant: with roughly 230 self-governing jurisdictions on the planet, a complete network would require more than 26,000 bilateral treaties. Assuming that tax evaders – or their financial service providers – are well informed and well-behaved, they will even react 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. Transparent tax avoidance strategies, as discussed in Johannesen (2014a), OECD (2014), Hebous and Johannesen (2015), and Gumpert et al. (2016), will not influence the results, because they should not react to the threat of information exchange.

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 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. We refer to both of these treaty types as IoR (information-exchange-on-request) treaties in the rest of the text. New signatures peaked 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 was criticized and resulted in the development of agreements that target automatic exchange of information (AEI). In 2014, 44 ‘early adopters’ committed to automatic exchange of information within 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 (OECD, 2016). We use these matches to analyze shifts in bilateral bank deposits. These relationships start in 2017 and are depicted in the bottom panel of Figure 2. As of July 2018, a total of 2,243 bilateral exchanges are in effect, of which 336 can be matched with bilateral BIS data in our balanced panel.

### 3 Data and empirical approach

This data section has two parts. First, we describe the bank deposit data (Section 3.1) and, second, we introduce our samples and the empirical approach (Section 3.2).

#### 3.1 Bank deposit data

The BIS publishes aggregated data on 47 jurisdictions whose national central banks report the claims and liabilities of their banks in the BIS locational banking statistics (LBS) (see BIS, 2013).<sup>3</sup> A significant subset of these LBS was first released as a bilateral version in October 2016. The release includes reports from both tax-havens and non-havens, thus allowing us to analyze deposits of non-banks both in and from tax havens using the same data source.<sup>4</sup> With privileged access, these data are used in Huizinga and Nicodème (2004); Johannesen (2014b); Johannesen and Zucman (2014) and more recently, the published version is employed in Langenmayr (2017). To our knowledge, we provide the first study that exploits this dataset in all four subsamples introduced above (outbound, inbound, tax haven falsification, and non-haven falsification).

These LBS ‘by residence of counterparty’ are compiled according to balance of payments conventions<sup>5</sup> via the immediate counterparty, not the ultimate guarantor or beneficiary of deposits (BIS, 2013, 2014; IMF, 2013). A complete disaggregation of deposits with respect to the depositor is not available, but we can distinguish non-banks from banks holding deposits. This data is ideal for the exercise at hand because 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 both by a shell company 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.

In addition to 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,

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<sup>3</sup>The list is maintained by the BIS here: [https://www.bis.org/statistics/rep\\_countries.htm](https://www.bis.org/statistics/rep_countries.htm), last accessed April 16, 2019.

<sup>4</sup>Analyzing outbound and inbound deposits in the same study is not affected by the global asset-liabilities discrepancies that Zucman (2013) uses for his estimate of total unrecorded offshore wealth. We employ data taken only from the international liability side of banks’ balance sheets and only use positions reported against non-bank counterparties.

<sup>5</sup>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.

themselves unrelated to tax evasion. We use total claims against all international counterparties to build a financial weight measure  $x_{ij,t}^{financialweight}$  measuring the relative weight of the bilateral relationship  $i - j$  compared to all claims reported by other countries,  $i$ , against counterparty,  $j$ . As we show in the robustness section, total bank claims show no connection to tax evasion. This is a measure of the importance of the bilateral relationship compared to the total international financial integration of the counterparty in question and varies over time and on the countrypair level:

$$(1) \quad x_{ij,t}^{financialweight} = int.claims_{ij,t} / \sum_{i=1}^I int.claims_{ij,t} \quad ; \quad i, j \in havens \cup nonhavens$$

### 3.2 Samples and empirical approach

Only a limited number of tax havens report bilateral deposits to the BIS. However, 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 fully exploit. We reduce our data to a balanced panel of 60 quarters, ranging from 2003:I to 2017:IV, and split the available data into four samples after creating the financial weight variable introduced above. The outbound sample consists of 557 countrypairs with deposit data reported by eight tax haven countries on deposits held by a combined 146 non-haven counterparties. Not every reporting country reports data on every counterparty, as Table 1 shows. The inbound sample consists of 170 countrypairs reported by eleven reporting non-havens against a combined 44 tax havens. Additionally, we construct two falsification samples where we do not expect effects, as outlined above. The tax haven falsification group is made up of reports by seven tax havens on deposits of a combined 46 tax havens resulting in 197 unique countrypairs. Finally, the non-haven falsification sample consists of 473 countrypairs that are reported by eleven non-havens against a combined 146 non-havens. These four samples are used in the analyses presented below. Three samples are employed in this Section 4, i.e. the outbound sample, the tax haven falsification sample, and the non-haven falsification sample. Later, Section 5 turns to data reported by non-haven countries against tax haven depositors, the inbound sample. These quasi-administrative data alleviate the usual concerns of data quality of data provided by tax havens.

In order to test for signs of tax evasion in tax haven deposits, we analyze the reaction of bank deposits by non-haven non-bank counterparties in tax haven countries to new information

exchange treaties. Thus, we estimate the following model:

$$(2) \quad \log(deposits)_{ij,t} = \alpha_{ij} + \gamma_t + \sum_{k=-2}^K \beta_k(IoRtreaty_{ij,t}^k) + \delta X_{ij,t} + \varepsilon_{ij,t}$$

Here subscripts  $ij$  denote the countrypair,  $t$  the respective year-quarter, and  $k$  the lag variations of the signature timing introduced below. Bank liabilities against non-bank 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 in tax-havens. We consider the financial weight variable in  $X_{ij,t}$ , as introduced above. This empirical setup is employed in all regressions we present, with specification changes outlined in the text. In the robustness section, we provide a dynamic differences-in-differences treatment analysis that confirms our panel results.

## 4 Results of information-exchange-upon-request treaties on outbound deposits

We present results in four steps. First, we show our baseline results on outbound deposits and validate the analysis of Johannesen and Zucman (2014) (Section 4.1). Then, we provide various kinds of falsification exercises (Section 4.2). Next, our long sample allows us to test for time-varying effects of information-upon-request treaties, the main contribution of this section (Section 4.3). Finally, we show results on deposit shifting in response to information-upon-request treaties and argue that this behavior alone cannot explain the reactions we find (Section 4.4).

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

We analyze the change in non-bank deposits in tax havens, i.e. the outbound sample, in three steps, considering different treaty, time, and tax haven dimensions.

**Reactions to IoR treaties.** We start our baseline results in Table 2 by showing the effect of information-upon-request (IoR) treaties using the sample, tax haven definition, balancing, and treaty restrictiveness specified above. Column 1 shows the associated effect, a 27.5% reduction that is highly statistically significant. Despite criticisms raised against IoR treaties at the time (Kudrle, 2008), they did lead to significant reactions on the bilateral connection on which they were signed.

— Table 2 about here —

**Lagged reactions and bilateral controls.** We find some 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 significantly negative treatment variable. We also introduce the financial weight variable capturing the relative importance of a tax haven for its non-haven counterparty in international bank claims. This control variable operates at the same frequency, 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, provide unreliable information, or are 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. In the following columns, we work our way to the original Johannesen and Zucman (2014) results to show that we can replicate them almost perfectly, albeit with a different data basis.

**Tax haven definitions and sample effects.** Some degree of judgement is involved with any tax haven list. Here we confirm that the effects we find are not due to particular countries but show a representative effect. In column 3, we reduce the sample of reporting tax havens to the list employed by Johannesen and Zucman (2014), thus dropping Ireland. Included in our outbound sample is Switzerland, itself responsible for around a third of household offshore wealth (Zucman, 2013). Thus, in column 4 we drop reports by Switzerland and show that results hold. In column 5, we show results based on our restrictive treaty definition, introduced in Section 2.3, and the full tax haven list and only limit the sample time to that of Johannesen and Zucman (2014). The resulting effect is larger than that in column 1, thus suggesting that the reaction to IoR treaties is weaker in the second part of our sample. We investigate this in detail in Section 4.3. Limiting our information set to that of Johannesen and Zucman (2014) entirely, we can verify that our slightly different outbound sample still captures the effects they found. Column 6 shows results where we use the tax haven definition, the sample length, as well as the tax treaty definition of these authors. Although we have access to a slightly different countrypair coverage,<sup>6</sup> results are virtually identical: we report a 13% drop in

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<sup>6</sup>Our reporting tax havens in the balanced panel are: Belgium, Chile, Guernsey, Ireland, the Isle of Man, Jersey, Luxembourg, and Switzerland. Johannesen and Zucman (2014) have access to six more tax havens in an unbalanced panel, part of which we employ further below, but did not employ non-haven reports.

deposits, against their 12% drop, after exchange upon request became possible. The difference in effect magnitude is due to treaty definitions, as we show in a robustness check. More restrictive treaty definitions show larger effects. Next, we carry out a comprehensive test for sample effects in all possible dimensions: the counterparty dimension, the reporting country dimension, and the time dimension.

Figure 3 shows, in four panels, the fluctuation of our estimates and the associated p-values around the baseline (the black square). In the top panel, we sequentially drop one (dark grey circles) and any combination of two (light grey crosses) counterparties from the sample and plot the re-estimated treatment effects and their p-values. The second panel does the same for the country dimension. Triangles indicate estimates based on samples where Switzerland was dropped. The third panel shows the effects of dropping one or two year-quarters at a time and the bottom panel finally shows dropping one or two entire years from the panel. Results are very robust to all of these changes. Not a single specification dropping only one case of the four characteristics leads to insignificant estimates. Only when we drop two reporting tax havens at the same time (a quarter of our outbound sample) do we find five combinations of reduced significance: the only two insignificant results amongst these more than 12,000 specifications arise from having dropped both Guernsey and a second British Overseas Territory (the Isle of Man or Jersey). The two estimates between 5% and 10% significance arise from having dropped Guernsey together with Switzerland or Luxembourg. Since all of these countries are included in all tax haven lists employed in the literature (see [Appendix Table A2](#)), the fact that our results are sensitive to their exclusion re-assures us that our results are indeed driven by classic tax haven jurisdictions.

— Figure 3 about here —

## 4.2 Falsification exercises

The analyses in this section extend results of Section 4.1. We make an econometric contribution by carrying out a placebo analysis, an economic contribution by distinguishing two kinds of international agreements both including IoR clauses, and verify our results by comparing the outbound sample to the two falsification samples.

**Placebo study.** A concern connected to our research design is potential non-randomness of signature dates. The OECD initiative has 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. In order to alleviate concerns that our results are driven by this momentum of signatures (and not the precise treaty quarters), we carry out a placebo analysis where all 1,074 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 total number of signatures in each year-quarter, for all countrypair groups and for both IoR 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 4 plots, as an example, TIEAs and placebo-treaties for non-haven – tax haven countrypairs with non-normalized splines. As can be seen, the placebos mirror the signature momentum of the OECD initiative. They start taking effect with that same concentration of treatments during the deleveraging period of 2009 – 2012.

— Figure 4 about here —

Column 1 of Table 3 uses this placebo treatment to test if it reduces the treatment effect we analyze. 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 the exact information treaty used.

— Table 3 about here —

**TIEAs vs. DTCs.** So far, we follow Johannesen and Zucman (2014) by employing all possibilities of IoR by using both tax information exchange agreements (TIEAs) and double taxation conventions (DTCs) as a treatment variable. In order to go into further detail, Table 3 also shows results for differentiated treaties. Column 2 differentiates the two treaty types introduced above. Interestingly, only TIEAs drive the effect, not DTCs, which are insignificant both statistically and economically. 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 among many clauses. Further, as we show in Figure 2, non-haven – tax haven countrypairs mostly signed TIEAs.

**Falsification samples.** Columns 3 and 4 change the sample to the tax haven falsification group where we do not expect reactions if these jurisdictions are less likely to prosecute tax



evasion. This is confirmed by the data: both the parsimonious (column 3) as well as the specification differentiating treaty types (column 4) show insignificant results of information exchange amongst tax havens. Using data reported by non-havens allows us to compare the case of tax evasion via tax havens to the ‘normal’ behavior of deposits between non-havens. Columns 5 and 6 change the sample to this non-haven falsification group. Column 5 shows that the effect of information exchange-upon-request treaties actually changes sign when employed in this sample and is marginally significant. Column 6 again differentiates treaties. We do not claim to establish the direction of causality here, but there is evidence that bilateral tax treaties have a positive effect on capital positions (Blonigen et al., 2014) and, in our case, these effects are indeed positive. Thus, we interpret the findings in columns 5 and 6 as the ‘normal’ case in which tax evasion does not drive the results.

These various falsification exercises make us confident that we have indeed identified a reaction of tax evaders to information exchange upon request in our main results. As we show in the following section, however, this effect fades over time.

### **4.3 The effect of treaties over time**

Our relatively long sample period allows us to compare reactions during different time periods. The initial results already hint at a qualitative change in the reaction to information-upon-request treaties over the course of our sample. Before we go into further detail, however, it is important to confirm random assignment, namely that early signatures are comparable to late ones. To the best of our knowledge, the only available study that investigates the characteristics of countrypairs signing IoR treaties is Bilicka and Fuest (2014). Replicating their data as closely as possible, we use the three yearly bilateral positions they analyze, namely: total export of goods and services from the IMF’s Direction of Trade statistics, the stock of foreign direct investment (fdi) taken from the OECD, and the stock of foreign portfolio investment (fpi) taken from the IMF’s Coordinated Portfolio Investment survey. Figure 5 shows these bilateral positions averaged for all countrypairs that sign a treaty at the time plotted on the horizontal axis. Before averaging, the data are transformed using the log equivalent inverse hyperbolic sine transformation to retain zero and negative values.

— Figure 5 about here —

The top panel shows that the tax haven – non-haven countrypairs (i.e. outbound) that sign new treaties toward the end of our sample exhibit similar, if not higher, bilateral positions

in trade and bilateral investment stocks compared to those countrypairs signing early in the sample. The middle panel shows that this holds for positions reported by non-havens against their tax haven signature counterparties as well (i.e. inbound). As the relevant point of comparison, the bottom panel shows the momentum of new signatures in the sample. It is not mirrored in the relative importance of the signing countrypairs, which we take this as evidence that, at least for observable characteristics, treaty signatures are not systematically non-random across our sample.

Having established that countrypairs signing IoR are comparable over time, we find that the effect of treaties on tax haven deposits starts to approach zero around the year 2010. Figure 6 shows this development for outbound reactions of tax evaders for a five year period. 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, as well as showing effects of new IoR treaties within that time frame. We then plot the coefficient on a treatment variable collecting the most significant treatment lags of the baseline specification ( $k = -2$  to  $k = 2$  around the treaty signature date) as well as their standard errors and indicate statistical significance with the shaded areas, light grey indicating 5% and dark grey 10% significance.

— Figure 6 about here —

#### **4.4 The short term effect of deposit shifting**

This effect of decreasing reactions to IoR treaties over time is not immediately intuitive. In light of increasing estimates of total international tax evasion (Pellegrini et al., 2016; Zucman, 2013) or offshore wealth (Alstadsæter et al., 2018), the effect of newer treaties should increase rather than decrease. This is especially true since Johannesen and Zucman (2014) show that one reaction to IoR treaties was to shift deposits to unaffected bilateral connections. As the counterparty non-haven, say France, signs more and more treaties with other tax havens, the tax haven that has not signed an IoR treaty with France experiences an increase in deposits from France. If that tax haven eventually signs and if tax evaders still use setups that are theoretically vulnerable to IoR treaties, the new treaty should trigger an even stronger reaction.

Taking the results of these studies as motivation that IoR treaties do not reduce tax evasion in aggregate but may rather create deposit shifting into less regulated tax havens, we analyze this mechanism in our sample. Using all available information, as in Johannesen and Zucman

(2014),<sup>7</sup> we find some degree of deposit shifting, as shown in Figure 7. This figure plots the coefficients of a variable counting treaties the counterparty non-haven has signed with other tax havens. It is evident that the effect is rather short-lived and took place in anticipation to the bulk of IoR treaties. In the 4-year rolling window we employ, as before, the last sample where we cannot reject the null hypothesis of no deposit shifting runs from 2004 Q4 through 2008 Q4 with the middle of the sample (in this case 2006 Q4) plotted on the vertical axis. This indicates that the results established in the literature are present but also that deposit shifting is probably not the whole story, as the mixed results on deposit shifting by Johannesen and Zucman (2014) and Johannesen (2014b) indicate already.

— Figure 7 about here —

Focusing on the early years of IoR treaties, it looks as if these treaties may in fact motivate tax evaders to shift their capital into those tax havens that do not sign IoR treaties. However, starting at about 2008 there is a combination of effects of new IoR treaties with no deposit shifting and still no evidence of decreasing aggregate tax evasion. This suggests that something else is taking place: if capital income is not legalized and if it is not invested in tax havens that do not comply with IoR treaties, then we argue that this capital is put in other forms, a new disguise.

An example of such a new disguise are shell companies with beneficial owners close to, but not identical to, the tax evader. Such structures alleviate the agency problem of financial services and allow the tax evader to retain an element of control but circumvent IoR treaties that require knowledge about the identity of the evader. The bank in the tax haven has no information about the actual tax evader that it could share with tax agencies.<sup>8</sup>

Already such simple setups successfully circumvent IoR treaties and would explain the

<sup>7</sup>This increases our bilateral countrypairs from the 557 in the balanced panel to 1,616 in an unbalanced panel. For completeness, we re-run the rolling outbound analysis of Figure 6 in this unbalanced sample and find the same petering out of IoR impacts. This is documented in Appendix 2, Figure A1.

<sup>8</sup>One example of such a setup are the daughters of German businessman Curt Engelhorn who were charged with tax evasion after the Paradise Paper leaks uncovered them as the beneficial owners of trusts set up for them years earlier (See <https://www.tagesschau.de/ausland/paradisepapers/paradisepapers-125.html>, last accessed April 16, 2019). Similarly, the father of the international football star Lionel Messi was the ultimate beneficiary of shell companies exposed in the Panama Papers that led to tax fraud charges (See <https://panamapapers.sueddeutsche.de/articles/57021852a1bb8d3c3495b438/>, last accessed April 16, 2019). Such structures were addressed to some extent in 2013, three years after the treatment effect we report fades out: The guidelines on politically exposed persons (PEPs) of the financial action task force (FATF) recommends as best practice that “if a customer or beneficial owner is identified as a family member or close associate of a PEP, then the requirements for PEPs should also apply accordingly” (FATF Guidance Politically Exposed Persons, Recommendations 12 and 22, section C.34, p. 10, Last accessed April 16, 2019 at <http://www.fatf-gafi.org/media/fatf/documents/recommendations/Guidance-PEP-Rec12-22.pdf>).

petering out of our treatment effect and the short lived deposit shifting effect. Unfortunately, we have no quantitative measure to evaluate such structures directly.<sup>9</sup> After adjustments have taken place, no further reactions to IoR treaties are to be expected and our treatment effect dies out even though treatment variation is still substantial after 2010. In effect, IoR treaties have lost their usefulness in making tax evasion visible. The next opportunity that allows a glimpse into the behavior of international tax evaders is the international introduction of automatic exchange of bank information. Before turning to this more recent initiative, we shortly complete the picture of international tax evasion by analyzing inbound deposits from tax havens in non-haven countries.

## 5 Results of information exchange-upon-request treaties on inbound deposits

In this section, we compare the results of the last section with reactions of deposits by tax haven counterparties in non-havens. This allows us to analyze inbound deposits based on the same data source as outbound deposits with identical treatment variables. We analyze the inbound deposits here in the same order as the outbound deposits in the preceding Section 4.

**Baseline result.** Our first hypothesis is that there is a reaction of inbound deposits to treaties that mirrors the outbound evasion effects shown above. Column 1 of Table 4 shows that a treaty reduces deposits by tax haven counterparties in non-haven countries by 37%, quantitatively in line with the inbound 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. Since we define tax havens as countries that uphold strong secrecy rules and relatively low tax rates, this is unlikely. The result is meaningful once viewed through the lens of the findings in the last section. The capital deposited by tax haven counterparties does not only constitute capital from citizens of these jurisdictions but includes that of foreign depositors who are originally residents of non-havens and deposit some of these funds in non-havens disguised as tax haven residents.

— Table 4 about here —

There is reason to believe that reactions in inbound deposits happen with some lag. As funds are already invested, re-arranging their structure takes time. Indeed, although the sign is

<sup>9</sup>The sham corporation variable in Johannesen and Zucman (2014) is not applicable here as we are not necessarily talking about positions from another tax haven. The new structure can well be in the same jurisdiction as the original deposit.

already negative, we do not find consistently significant results before the fourth lag (column 2). After one year, deposits remain lower, which is in line with a negative signaling effect of compliance (Konrad and Stolper, 2016). We confirm this lagged reaction in a dynamic differences-in-differences treatment analysis in the robustness section.

**Extended results.** In column 3, we introduce the bilateral control variable capturing the relative importance of the bilateral connection in international bank claims (financial weight). Again, this variable exhibits a significant and positive coefficient. Mirroring the reform momentum of our treatment variable, column 3 also introduces the placebo treatment. The coefficient is insignificant and economically close to zero. The differentiation between TIEAs and DTCs provided in column 4 confirms that the inbound effect is also driven by TIEAs. Addressing potential concerns regarding the tax haven list, column 5 reduces the counterparty tax havens to those on the Johannesen and Zucman (2014) list and column 6 additionally excludes Switzerland. As for outbound deposits, we carry out a comprehensive test of sample effects dropping any combination of one or two counterparties, countries, year-quarters, and years. Not one of the almost 3000 resulting specifications is insignificant or leads to qualitatively different coefficients (see robustness section).

**The effect over time.** The final step in showing some symmetry of outbound and inbound bank deposits via tax havens is to compare the developments of inbound reactions for new treaties each in rolling windows. Figure 8 shows this development for inbound reactions. Again, 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 and comparing new signatures within these sub-samples. The plotted treaty coefficient is based on the most relevant lags found in the baseline ( $k = 4 : k = 6$  around the treaty signature date) and its standard errors are shown in light grey (95% confidence interval) and dark grey (90% confidence interval). Except for a slight lag in the inbound reactions pointed out above, these results are virtually identical to those we find in a completely different sample for outbound deposits. Between 2011 and 2012, exchange-upon-request treaties lose significance and the treatment effect converges toward zero.

— Figure 8 about here —

We take the results in this section as evidence that funds deposited by tax haven counterparties in non-haven countries could be connected to tax evasion. Quantitatively, our results in both directions are in line with the effect of the EU savings directive on Swiss deposits

analyzed in Johannesen (2014b), who establishes reactions of deposits in the range of 30% to 40% (15% to 30% for other tax havens) and those of Hanlon et al. (2015) for inbound portfolio liabilities. It is especially striking how similarly these reactions evolve over time: this is not an automatic effect of the congruency of inbound and outbound bank deposits, as we only use liability data reported against non-bank counterparties. Instead it shows that not all funds deposited by tax evaders end up in tax haven sinks (though some will, as Garcia-Bernardo et al. (2017), show with the example of corporate ownership structures), rather some return to non-haven countries.

There are two interpretations for the finding of reduced reactions over time. Either treaties are indeed successful in decreasing tax evasion or evaders increasingly rely on other options to evade taxes. Since, if anything, calculations of aggregate evaded capital or offshore wealth, as provided by Zucman (2013), Pellegrini et al. (2016), and Alstadsæter et al. (2018), point to increasing evasion over time, the first interpretation is not plausible. Instead, it seems likely that tax evaders channel their wealth via non-complying tax havens or create new disguises for their funds. In the following, we test an implication of the latter hypothesis.

## **6 Results on recent information exchange agreements**

The recent declining effect of IoR treaties on bank deposits in both directions, i.e. outbound in tax havens as well as inbound in non-havens, may be caused by tax evaders adjusting how they set up their deposits. If this is the case, we should again expect effects of recent far-reaching agreements based on an automatic exchange of information (Section 6.1). We test this first for the CRS (Common Reporting Standard) in Section 6.2 and then for a set of further agreements specific to the US in Section 6.3.

### **6.1 The motivation for examining recent information exchange agreements**

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

The Foreign Account Tax and Compliance act (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 identities of American account holders who they serve (Johannesen et al., 2018). These requirements are so restrictive that FATCA is criticized for overreaching US competences in foreign countries (Michel and Rosenbloom, 2011), especially since intra-US competition has led to states like Delaware offering tax haven type services themselves (Dyrenge et al., 2013) and tax havens are already complaining about such ‘hypocrisy’ (European Parliament, 2016). However, it should be pointed out that FATCA’s implementation regulations 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 July 2018, there are 127 signed FATCA agreements, 63 of which are included in our balanced panel.<sup>10</sup>

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 IoR treaties, 26,000 bilateral agreements would be needed for a complete network, while only 2,243 were activated during our sample period; 336 can be matched to bilateral deposit data in our balanced panel. Under these agreements, comprehensive information is automatically exchanged annually on investment income, account balances, and proceeds from the sale of financial assets. These data are 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 evaders to react.

Conceptually, these measures again aim for introducing an information exchange between tax havens and other countries, thus making deposits 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 IoR treaties 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 disguises, however, the regulation of tax havens

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<sup>10</sup>These are recorded at: <https://www.treasury.gov/resource-center/tax-policy/treaties/Pages/FATCA.aspx>, last accessed April 16, 2019. In order to keep the results comparable to Sections 4 and 5 and to be able to test both results in the same setting, we remain in the balanced panel used before. A wider balanced panel starting at a later point in time than 2003 shows consistent results, however.

has largely failed and a new set of rules would be appropriate.

In order to shed light on the latter mechanism, we significantly extend our database of information exchange treaties with the activated bilateral CRS relationships, signed FATCA agreements, and two dummies for US-Switzerland agreements. This extended database is analyzed in the same way IoR treaties are analyzed. If these new agreements reduce non-haven bank deposits in tax havens, while recent IoR treaties do not, this indicates that the form of tax evasion may have changed.

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

The Common Reporting Standard (CRS) contains automatic information exchange and, thus, is much stricter, as seen from the perspective of a tax evader. Interestingly, bilateral activations of automatic information exchange relationships under the CRS lead to a decline in outbound deposits similar to the one we observed during the early years of IoR treaties. Table 5 column (1) shows a 43% reduction in reaction to CRS activation with the financial weight variable again being significantly positive.

— Table 5 about here —

It has to be considered that the introduction of the CRS and its automatic exchange of information coincided with a number of domestic amnesties and voluntary disclosure programs. As the setup varies from country to country, with only a subset of countries implementing these, we do not attempt to explain their occurrence (see Bayer et al., 2015, for such a discussion). Important in our context is their varying success. Argentina broke records with 116 billion USD flowing back due to the amnesty,<sup>11</sup> while the South African program has, essentially, been ignored at the time of writing. If successful, at least a part of the CRS effect we find could be driven by such amnesties (see Langenmayr, 2017, for a discussion). To investigate this, we collected data on 18 of such programs<sup>12</sup> and add them as a dummy variable in column 2 of Table 5. The resulting effect is insignificant and economically almost zero. A caveat applies here: Several of the programs under consideration are still ongoing at the time of writing. Past experience shows that self-reporting peaks just before such programs end. Therefore, it is likely that we do not capture their full effect in these results. Still, the effect of CRS activation does

<sup>11</sup>“Argentina rakes in \$116.8 billion from hidden assets amnesty” – Bloomberg April 4, 2017, <https://www.bloomberg.com/news/articles/2017-04-04/argentina-rakes-in-116-8-billion-from-hidden-assets-amnesty>, last accessed April 16, 2019.

<sup>12</sup>This list includes one or more amnesties in Argentina, Australia, Brazil, Brazil, India, Indonesia, Israel, Kenya, Mexico, Pakistan, Russia, South Africa, South Korea, Turkey, and the United Kingdom.



not seem to be driven by amnesties and Langenmayr (2017) shows, that voluntary disclosure programs can even increase tax evasion. This indicates that a significant portion of capital shifted in reaction to CRS activation is not legalized. We also add the earlier signed IoR treaties and the placebo used above, both only marginally change the CRS coefficient. In the robustness section, we test for anticipation effects and can show that it is indeed the activation of the bilateral CRS exchange and not the earlier signature of the multilateral agreement that drives these results. We also test for interaction effects with previous IoR treaties and find none.

Finally, we run tests in our two falsification groups, i.e. we test whether CRS activation affects bank deposits, either between tax havens or between non-havens. The expectation is that the CRS coefficients again should be insignificant. Indeed, columns (3) to (6) in Table 5 show the expected insignificant and economically small CRS coefficients. By contrast, the coefficients on the financial weight are significantly positive and the coefficients on earlier treaties between non-havens remain marginally significantly positive, just as they were for the isolated analysis of IoR treaties.

Overall, we find the exact same pattern in empirical results as we did for information exchange based on IoR treaties. Bank deposits from non-haven countries to tax havens go down significantly if AEI relationships between these countries are activated in the form of the CRS. At the same time, the falsification samples are unaffected. While we see a very clear effect for the outbound deposits, it is currently not reasonable (keeping in mind the lagged effect of IoR inbound deposits) to analyze the inbound deposits, because CRSs were only activated at the end of our sample, as Figure 2 shows. These results point to a worrying tendency: Reactions to the CRS mirror those to IoR treaties, thus suggesting the CRS is not the game changer it is often touted to be. The still bilateral exchange of information leaves loopholes open but the CRS can also be circumvented directly.

The “new disguise” that we described above and that protects against IoR treaties by naming a close relative as the beneficial owner will react to CRS activations. Suddenly, large sums would appear held in the name of an owner who was not previously suspected of tax evasion and therefore not targeted by information requests. However, the CRS can be circumvented by acquiring dual citizenship and tax residence in a tax haven. If the evader moves their funds to a bank account in that tax haven with local citizenship and does not disclose their original citizenship to the tax haven bank, such an account is not reported in the

OECD system as it is not an international position. Again, new disguises are at work and, for example, the United Arab Emirates have already made this sort of setup possible.<sup>13</sup> It is also possible that tax evaders increasingly use countries not usually considered as tax havens for such activities as argued by Cusi et al. (2018). Additionally, it will not be reported to the BIS as an international liability and the bilateral deposits as used in this analysis will fall when the funds are moved into this account, consistent with the negative reaction we find.

### **6.3 The reaction of non-bank deposits in tax havens to US agreements**

While CRSs have a very broad cross-country coverage, there are two additional agreements between the US and Switzerland. These feature prominently in current research since the US is the world's largest economy and Switzerland is the largest provider of personal banking services to foreign customers (Johannesen et al., 2018; Johannesen and Stolper, 2018). 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. Results confirm the CRS results (details in [Appendix 1](#)) and are consistent with the interpretation that tax evasion is still topical.

## **7 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 four themes. First, we analyze our results on IoR treaties regarding outbound deposits and, second, regarding inbound deposits. Third, we check the potential impact of various tax haven lists on our results, and, fourth, we check the robustness of our CRS activation results and their relation with IoR agreements.

(i) Regarding the outbound deposits, we test different treaty definitions and sample lengths in order to re-create the results of Johannesen and Zucman (2014), the outbound reference study. The results qualitatively confirm our main findings. We exclude year-quarters after G20 and the OECD initiative introduced in the main text but find no change in effects. (ii) With respect to inbound deposits, we replicate our main Table 4 for various sub-sample periods: reducing it to the Johannesen and Zucman (2014) sample or excluding the financial crisis. Next,

<sup>13</sup>It is possible to obtain residence and a company with tax residence in the United Arab Emirates. This company can open a bank account and the bank will have to check for tax residence that is domestic. Thus, no international position has to be reported. See Economist, September 27, 2018, last accessed April 16, 2019 at <https://www.economist.com/international/2018/09/27/how-the-united-arab-emirates-became-an-oasis-for-tax-evaders>

in the spirit of Hanlon et al. (2015), we use macro controls, which leave our results unchanged. 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, i.e. bank claims. Indeed, there is no effect, neither for overall claims nor for loan claims. This also confirms that bank claims are a valid basis for the financial weight variable that is used to control for non-tax evasion financial integration. Thereafter, we repeat the extended robustness check regarding sample specifications (see Figure 3) for inbound deposits. We also broaden our very restrictive definition of treaties in different ways, but results are broadly consistent. Finally, we show results of dynamic differences-in-differences treatment analyses, confirming effects of the regression results as well as their timing both in the outbound and the inbound samples. A direct bilateral matching between outbound and inbound reactions is weak as we demonstrate in [Appendix 3](#).

(iii) The use of specific tax haven lists is potentially relevant for inbound deposits, since we use data reported against 44 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 Macao as non-havens do we observe a loss of significance. (iv) Finally, we show robustness to sample changes of the CRS results test whether the decreasing effect of TIEAs since about 2012 may be due to the later CRS agreements that were foreseeable at least since 2014; there does not seem to be any anticipation or interaction effects here.

## 8 Conclusion

In this paper, we present the first comprehensive evidence on the reaction of non-banks' 'outbound' bank deposits in tax havens and non-banks' 'inbound' bank deposits (i.e. from tax havens in non-havens) to information exchange agreements. We use non-bank deposits that the Bank for International Settlements made public in October 2016. As a first step to link our study to the literature, we confirm the Johannesen and Zucman (2014) result on outbound deposits for somewhat different samples: one running exactly until the end of their same sample-period (2011:II) and another one running over our full sample period, i.e. until 2017:IV. Beyond confirming the main pattern, we find that the effect of information-exchange-on-request (IoR) treaties 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). We show that deposit shifting is part of the story but only happened for a brief time and anticipated most treaty signatures. If such a shift were to occur continuously, new treaties should have rather

larger effects as the remaining tax havens accumulate more and more capital. While evidence does not support the view that existing treaties have been able to reduce the overall amount of international tax evasion (Alstadsæter et al., 2018; Pellegrini et al., 2016; Zucman, 2013), our results are consistent with the interpretation that tax evaders prefer new kinds of tax evasion.

In order to complete these results, we test, for the first time in the literature, whether reactions of tax evaders in inbound bank deposits mirror those of outbound deposits. This reaction is found in the data and may be surprising, as inbound deposits stem from a tax haven with bank secrecy. It is, however, in line with narrative evidence indicating concerns of tax evaders that inbound deposits may encourage tax authorities to search for earlier illegal behavior. We also provide more detailed analyses: we find that inbound deposits also react to treaties with a lag. Moreover, disaggregating IoR treaties into TIEAs and DTCs shows that conventional double taxation treaties do not affect the size of bank deposits between non-havens and tax havens in either direction.

Finally, we extend the former type of analysis to the automatic exchange of information. These commitments create the same pattern we know from early IoR treaties. These new agreements, such as activations of the CRS, significantly reduce the outbound deposits from non-havens in tax havens and, in some cases, where data are available (i.e. not for CRS activations), we also find this pattern for inbound deposits. 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 deposits into non-complying tax havens, looking for regulatory loopholes in non-havens, and – what we seem to uncover here – putting their funds into new disguises that circumvent regulatory requirements.

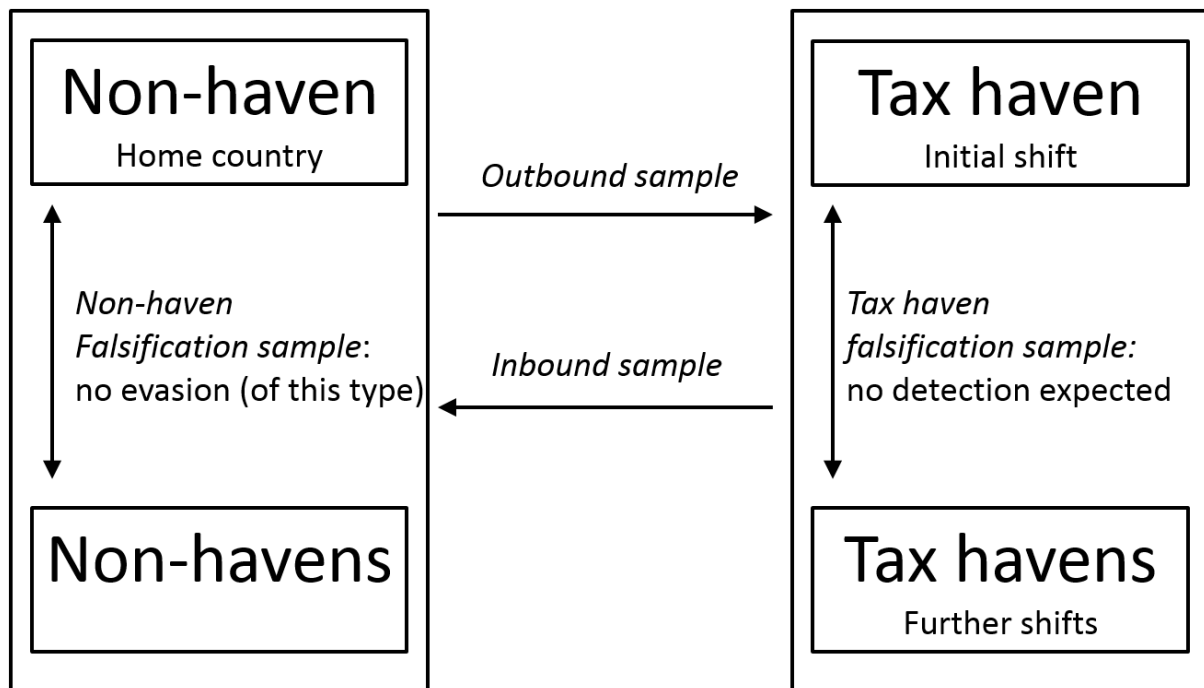
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Figure 1: **International bank deposits of tax evaders**



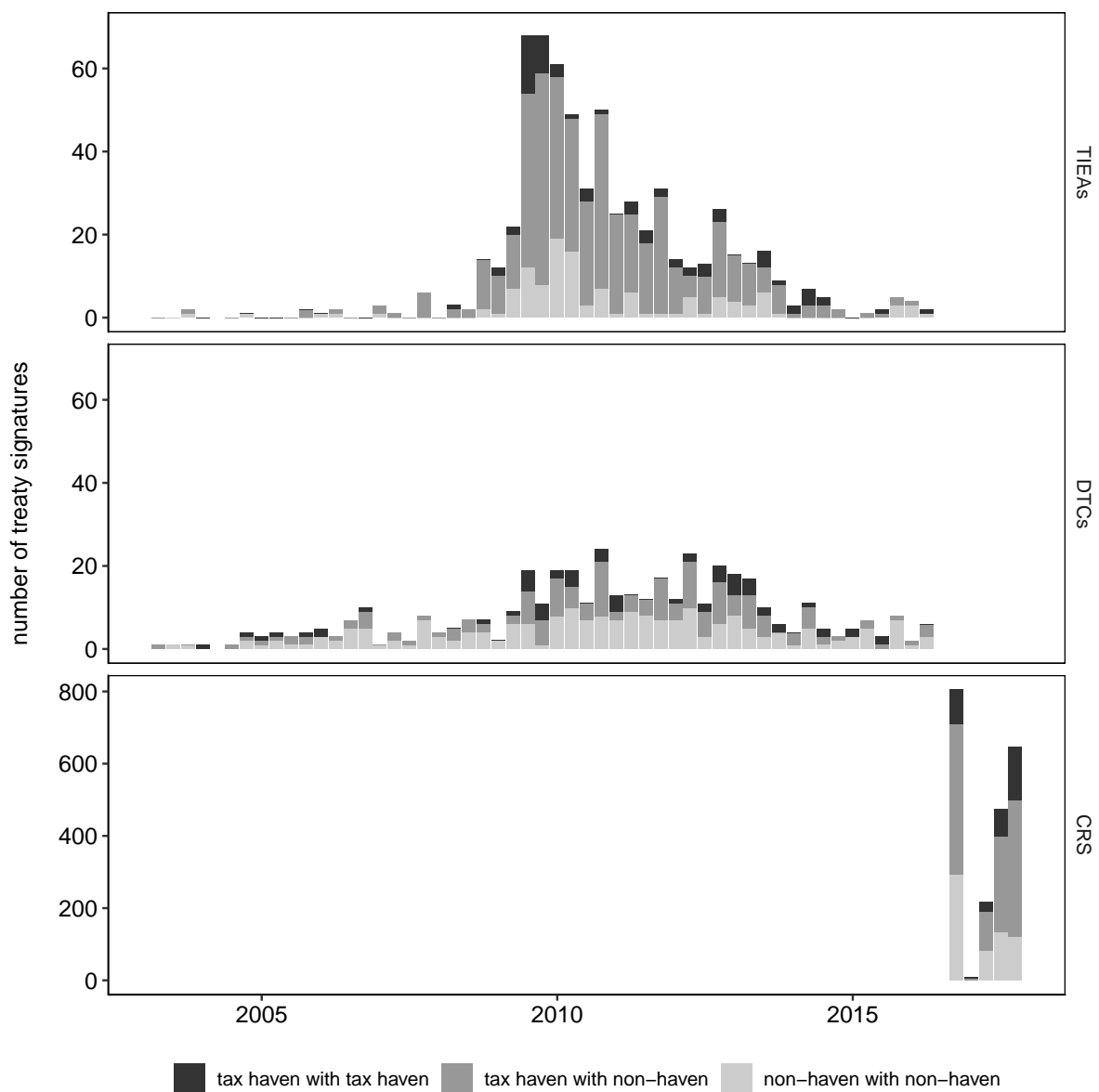


**Table 1: Tax havens with significant available deposit data**

Jurisdiction	population	Affiliation	Treaties with non- havens	Deposit time series (balanced panel: non-havens; tax havens)	total lia- bilities (m. USD 2017:IV)	out- bound	in- bound
Austria	8,711,770	EU	6	28 (8; 7)	72196	-	1
Bahamas	327,316	C.o.N	21	28 (6; 7)	35356	-	1
Bahrain	1,378,904	-	18	26 (3; 7)	54294	-	1
Belgium	11,409,077	EU	3	29 (7; 6)	103478	1	1
Bermuda	70,537	British o.T.	27	28 (6; 5)	1986	-	1
Cayman Islands	57,268	British o.T.	25	29 (6; 7)	268102	-	1
Chile	17,650,114	-	2	27 (5; 4)	2715	1	1
Guernsey	66,297	British C.D.	34	24 (3; 6)	31805	1	1
Hong Kong	7,167,403	China S.A.R.	14	27 (8; 7)	458182	-	1
Ireland	4,952,473	EU	12	28 (8; 6)	66194	1	1
Isle of Man	88,195	British C.D.	28	23 (2; 6)	25430	1	1
Jersey	98,069	British C.D.	32	25 (4; 6)	59786	1	1
Luxem- bourg	582,291	EU	10	29 (8; 6)	148886	1	1
Malaysia	30,949,962	C.o.N	6	27 (7; 5)	28186	-	1
Panama	3,705,246	-	17	29 (6; 5)	22344	-	1
Singapore	5,781,728	-	11	28 (7; 7)	254465	-	1
Switzerland	8,179,294	-	12	29 (8; 6)	454075	1	1

*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:IV). Population in column 2 and affiliations in column 3 are taken from the CIA World Factbook. Affiliations are abbreviated as follows: ‘British C.D.’s are the British Crown Dependencies, ‘British o.T.’s the British overseas Territories, ‘C.o.N.’ is the Commonwealth of Nations, and ‘China S.A.R.’s are special administrative zones of China. 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 after 2003:I, meeting OECD standards and including the updated less stringent requirements for information exchange. The first number in column 5 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). Column 6 shows the total liabilities reported by that tax haven against the rest of the world (not bilaterally) and Columns 7 and 8 show if the tax haven is included in the inbound and/or outbound sample. A comprehensive table including all tax havens listed in recent studies is available in [Appendix Table A1](#).

Figure 2: **Unique information exchange relationships over time**



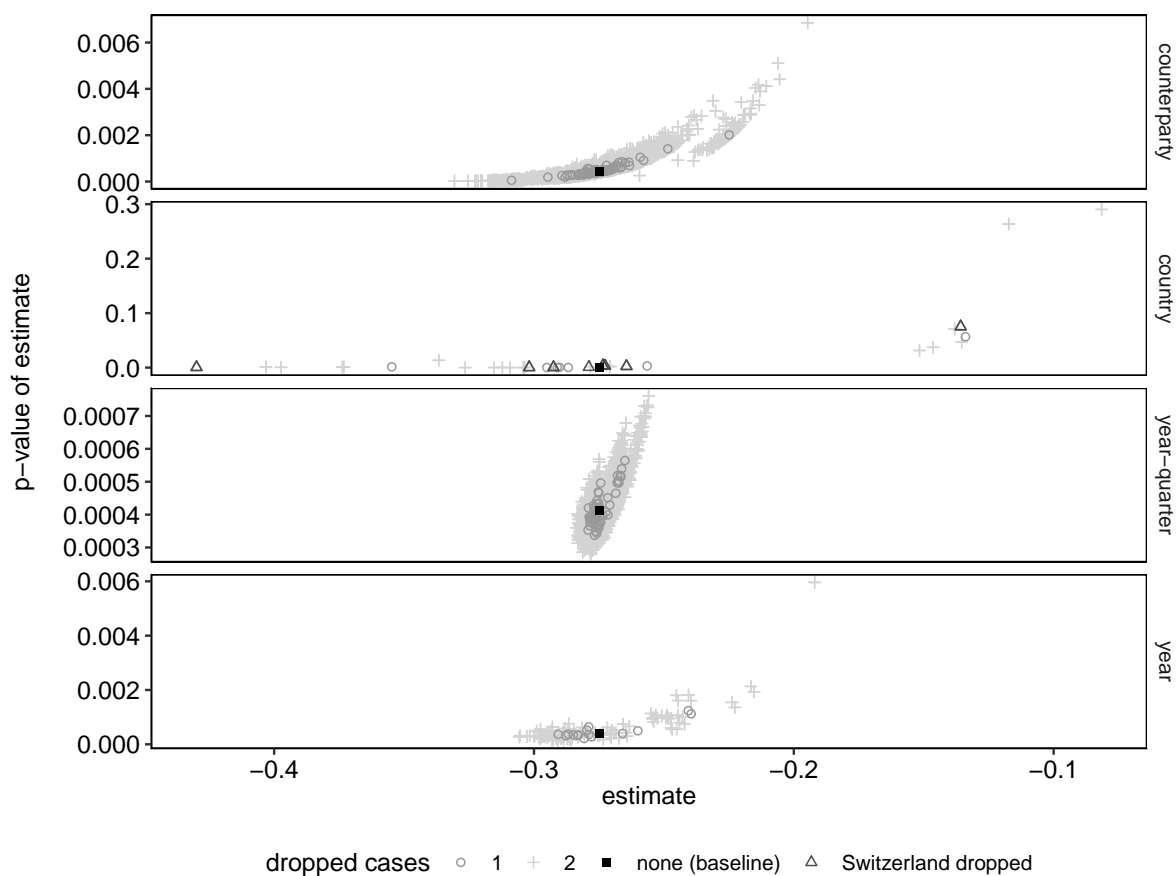
*Notes:* First depicts the number of signatures of tax information exchange agreements (TIEAs, top panel) and double taxation conventions (DTCs, middle panel). These treaties specify information-exchange-on-request (IoR). Both treaty types are based on data from the OECD Exchange of Tax Information Portal; narrowed down to treaties signed in the balanced panel (with the last treaty available signed in 2016: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. CRS activations are shown till the end of the sample: 2017:IV. 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 tax havens (outbound) to IoR treaties

<i>Dependent variable: log(deposits)</i>						
	outbound baseline	outbound	outbound JZ(2014) tax haven list	outbound JZ(2014) list without Switzerland	outbound JZ(2014) sample time only	outbound JZ(2014) close replica
	(1)	(2)	(3)	(4)	(5)	(6)
IoR treaty signed	−0.275*** (0.078)	−0.303*** (0.087)	−0.327*** (0.087)	−0.359*** (0.095)	−0.384*** (0.090)	−0.133** (0.062)
IoR treaty <sub>k=−1</sub>		−0.146* (0.075)	−0.138** (0.070)	−0.178** (0.076)		
IoR treaty <sub>k=−2</sub>		−0.130* (0.066)	−0.117** (0.059)	−0.146** (0.065)		
Financial weight		0.554*** (0.199)	0.493** (0.194)	0.593** (0.258)		
countrypair f.e.	Yes	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	33,420	28,682	26,184	18,364	17,267	16,523
R <sup>2</sup>	0.080	0.081	0.084	0.085	0.105	0.120
Adjusted R <sup>2</sup>	0.063	0.061	0.064	0.064	0.074	0.089

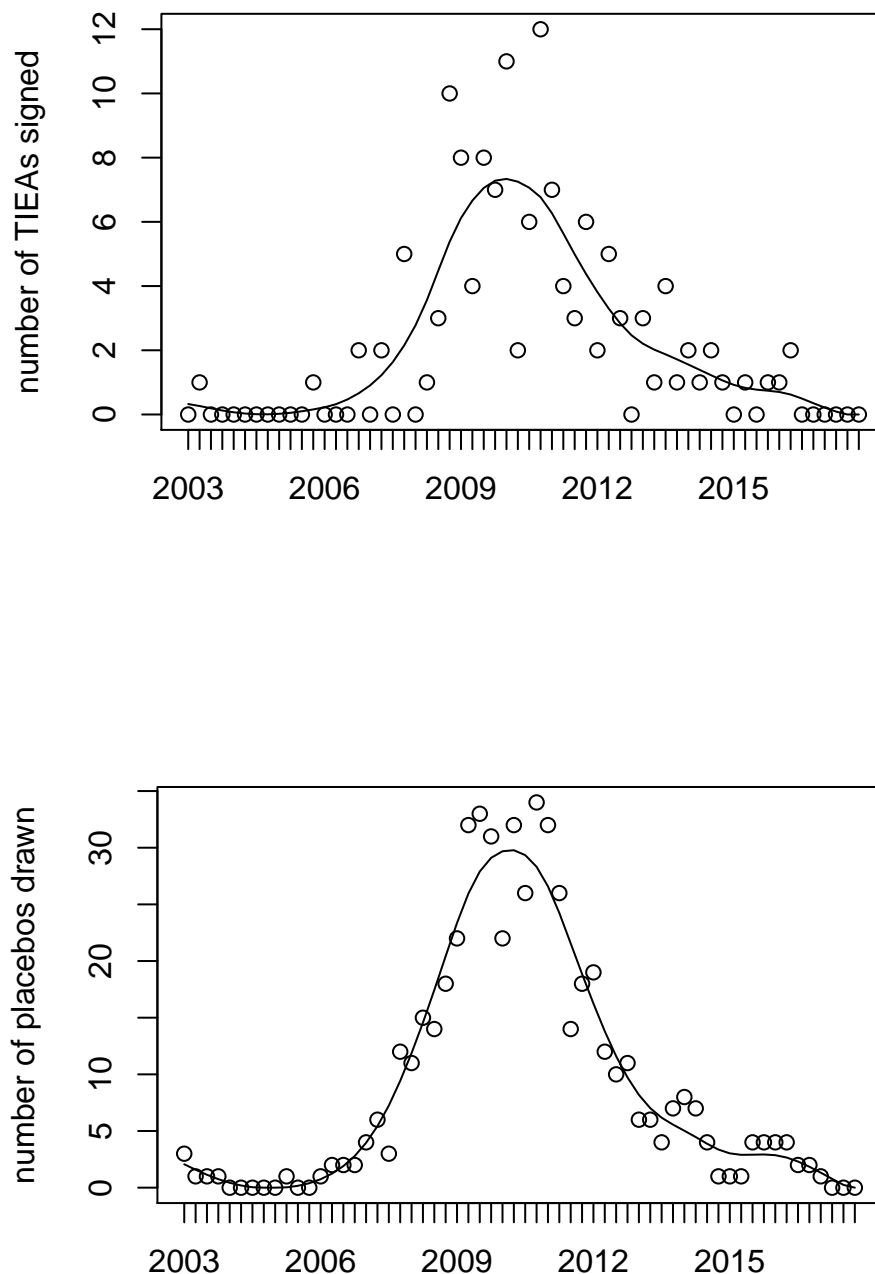
*Notes:* Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variable ‘IoR treaty signed’ takes value 1 if a bilateral treaty specifying information exchange on request has been signed. Column 1 shows the baseline effect of IoR using the entire balanced panel, the restrictive treaty definition, and the full tax haven list as discussed in the main text. Column 2 introduces two leads and the weight of the bilateral integration in bank claims relative to all other countries that report data against the counterparty non-haven (Financial weight) to show that our results are not driven by omitted developments in the banking sector. In column 3, we reduce the sample to reports by jurisdictions categorized as tax havens in Johannesen and Zucman (2014) – “JZ(2014)” for brevity – thus dropping reports by Ireland. In column 4 we additionally drop the reports by Switzerland. Column 5 changes the treaty and tax haven definitions to our baseline but only uses the sample time available to Johannesen and Zucman (2014). Column 6 shows the effect in a sample limited to the length, treaty restrictiveness, and tax haven definition used by Johannesen and Zucman (2014). The dependent variable in all columns are data on time series of deposits by non-haven counterparties in tax haven banks (outbound sample). The balanced sample consists of 557 countrypairs reported by 8 reporting tax havens against a combined 146 non-havens over 60 quarters (2003:I - 2017:IV). \* denotes 10% significance, \*\* 5% significance, and \*\*\* 1% significance.

Figure 3: **Sample robustness outbound**



*Notes:* The four panels plot the robustness of our results to changes in the sample. In all panels, the black rectangle shows the baseline results with the estimate plotted on the horizontal axis and its p-value plotted on the vertical axis. The top panel shows results from estimations dropping one (dark grey circle) or any combination of two (light grey cross) counterparties at a time. The second panel drops one or all combinations of two reporting countries at a time with the triangles indicate having dropped Switzerland. The third panel drops one or any combination of two year-quarters and the final panel drops one or any combination two entire years from the sample.

Figure 4: **TIEAs and placebos non-haven – tax havens**



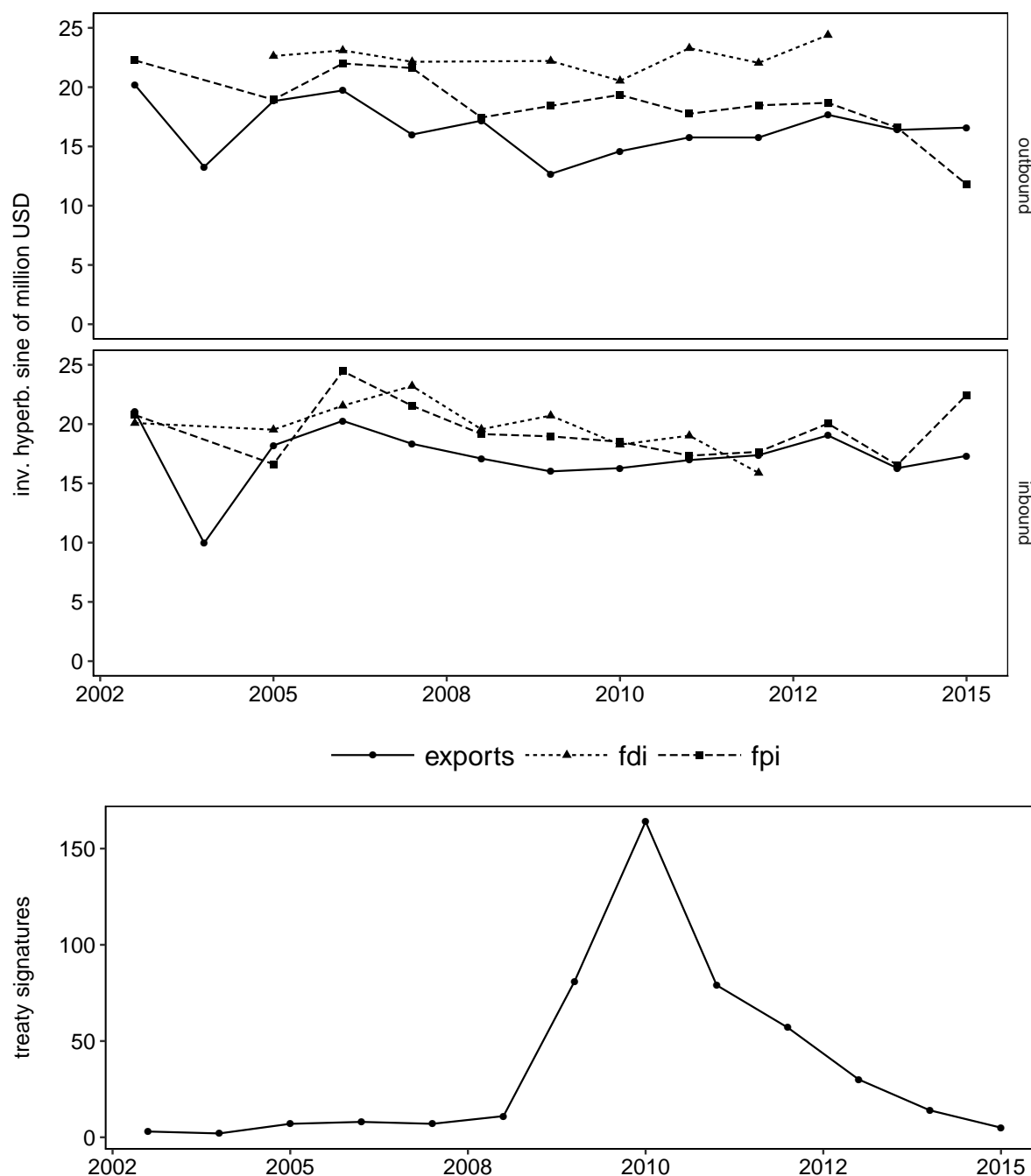
*Notes:* The top panel plots the number of TIEAs signed between tax havens and non-havens over the sample period in the circles. Each circle denotes the number of treaties (vertical axis) signed during the respective quarter (horizontal axis). The solid line depicts a cubic spline calculated over these 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 the bottom panel. Again, the circles represent the number of placebo treaties per quarter. The solid line in the bottom panel 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 are then randomly assigned to the countrypairs which have not signed a bilateral treaty.

Table 3: Treaty differentiation and falsification results

	<i>Dependent variable: log(deposits)</i>					
			tax haven		non-haven	
			falsification		falsification	
	outbound	outbound	sample	sample	sample	sample
	(1)	(2)	(3)	(4)	(5)	(6)
IoR treaty signed	−0.294*** (0.082)		−0.064 (0.123)		0.205* (0.122)	
Placebo assigned	−0.003 (0.045)					
TIEA signed		−0.505*** (0.094)		−0.253 (0.200)		0.294 (0.196)
DTC signed		0.100 (0.122)		0.067 (0.132)		0.194 (0.135)
Financial weight	0.553*** (0.199)	0.568*** (0.200)	0.525*** (0.196)	0.508*** (0.195)	0.385*** (0.131)	0.384*** (0.131)
countrypair f.e.	Yes	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28,682	28,682	11,133	11,133	27,431	27,431
R <sup>2</sup>	0.080	0.088	0.083	0.086	0.152	0.152
Adjusted R <sup>2</sup>	0.061	0.068	0.061	0.064	0.135	0.135

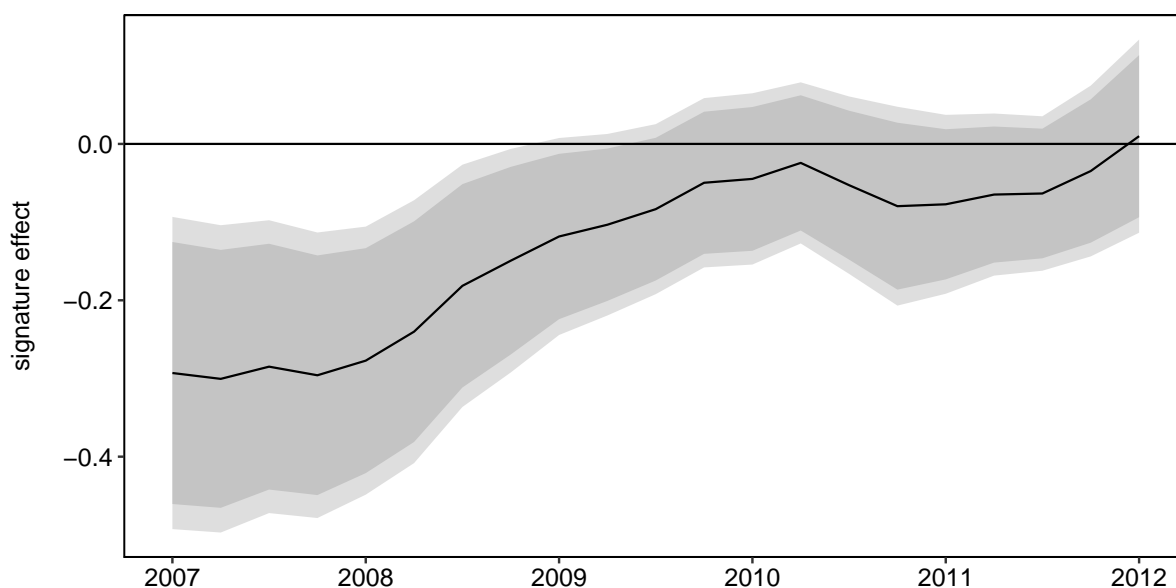
*Notes:* Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables takes value 1 if a bilateral treaty specifying information exchange on request has been signed. Column 1 adds the placebo treatment to the IoR specification. Column 2 separates the treatment variable into double taxation conventions (DTC) and tax information exchange agreements (TIEA). Columns 3 and 4 change the sample to the tax haven falsification group and repeat columns 1 and 2 in this sample. Columns 5 and 6 change the sample to the non-haven falsification group and repeat the same exercise. The sample in columns 1 and 2 are data on time series of deposits by non-haven counterparties in tax haven banks (outbound sample) resulting in 557 countrypairs reported by 8 reporting tax havens against a combined 146 non-havens. The dependent variable in columns 3 and 4 are data on time series of deposits by tax haven counterparties in tax haven banks (tax haven falsification sample). This sample consists of 197 tax haven - tax haven countrypairs. The dependent variable in columns 5 and 6 are data on time series of deposits by non-haven counterparties in non-havens banks (non-haven falsification sample). This sample consists of 473 countrypairs. All series run over 60 quarters (2003:I - 2017:IV). \* denotes 10% significance, \*\* 5% significance, and \*\*\* 1% significance.

Figure 5: Average positions of countrypairs at time of IoR agreement



*Notes:* The three panels compare the yearly development of bilateral positions at the time of IoR agreements in the outbound (top panel) and inbound (middle panel) directions with signature momentum over time shown in the bottom panel. The top two panels show the development of the same variables used by Bilicka and Fuest (2014) namely exports of goods and services, the stock of foreign direct investment (fdi), and the stock of foreign portfolio investment (fpi) from the reporting country to the counterparty with which a treaty is signed. All variables are in million USD and transformed using the log-equivalent inverse hyperbolic sine transformation to retain zeros and negative stocks. The momentum visible in the bottom panel is not mirrored in the top and middle panel, suggesting that newly signed treaties are signed between countrypairs of similar bilateral importance as those that signed treaties earlier.

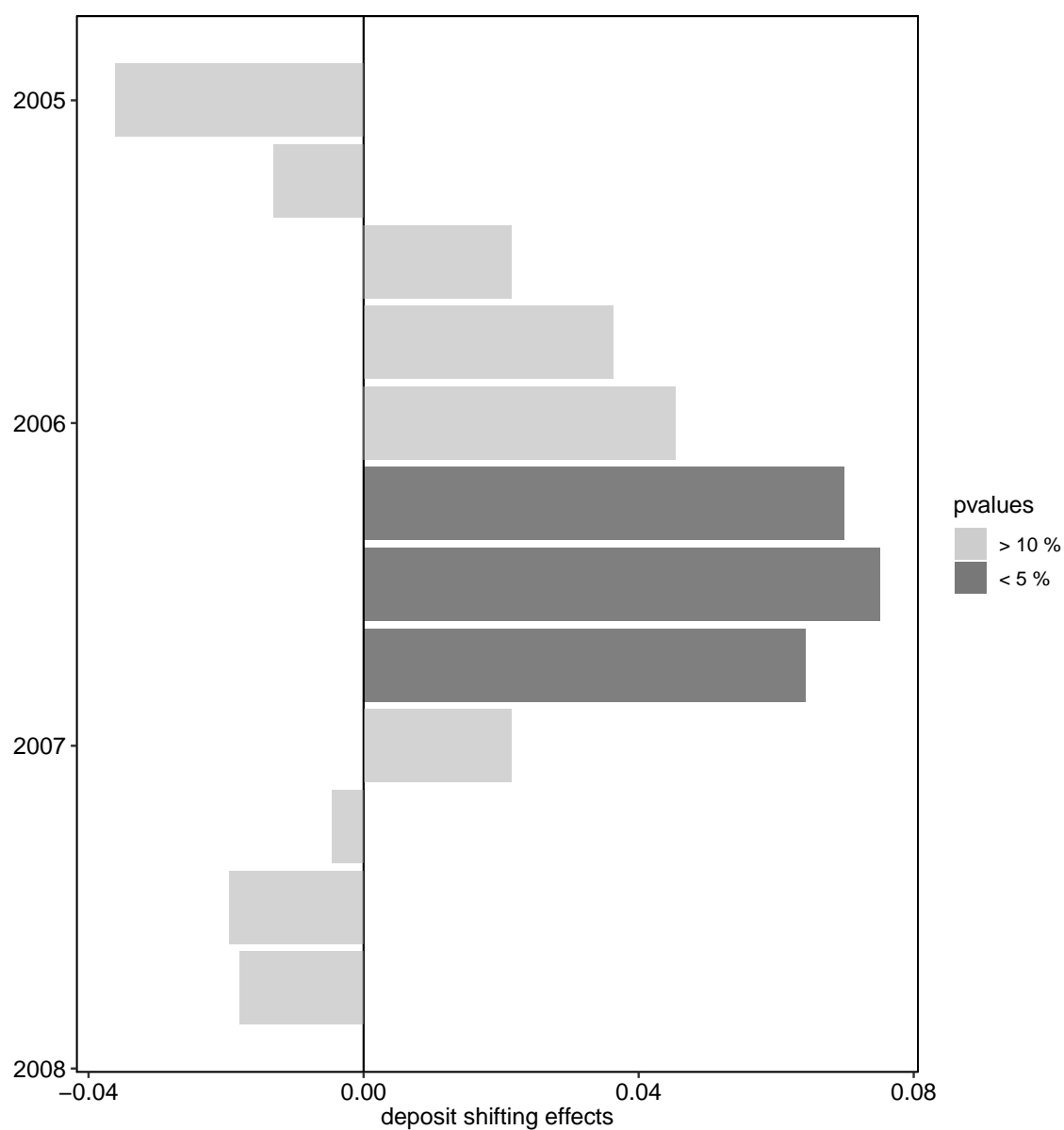
Figure 6: **Rolling estimation window: Outbound**



*Notes:* This figure shows the reaction of deposits in tax havens by non-haven counterparties (outbound) over time. The solid black line shows coefficients of regressions in a rolling window of  $\pm 8$  quarters around the year-quarter plotted on the horizontal axis. The coefficient captures the effect of the most significant lags of IoR signatures ( $k = -2 : k = 2$ ). The dark grey area denotes 10% significance while the light grey area denotes 5% significance in pointwise confidence bands. Estimated coefficients are plotted on the vertical axis and the horizontal line denotes a 0 effect.



Figure 7: **Deposit shifting reactions**



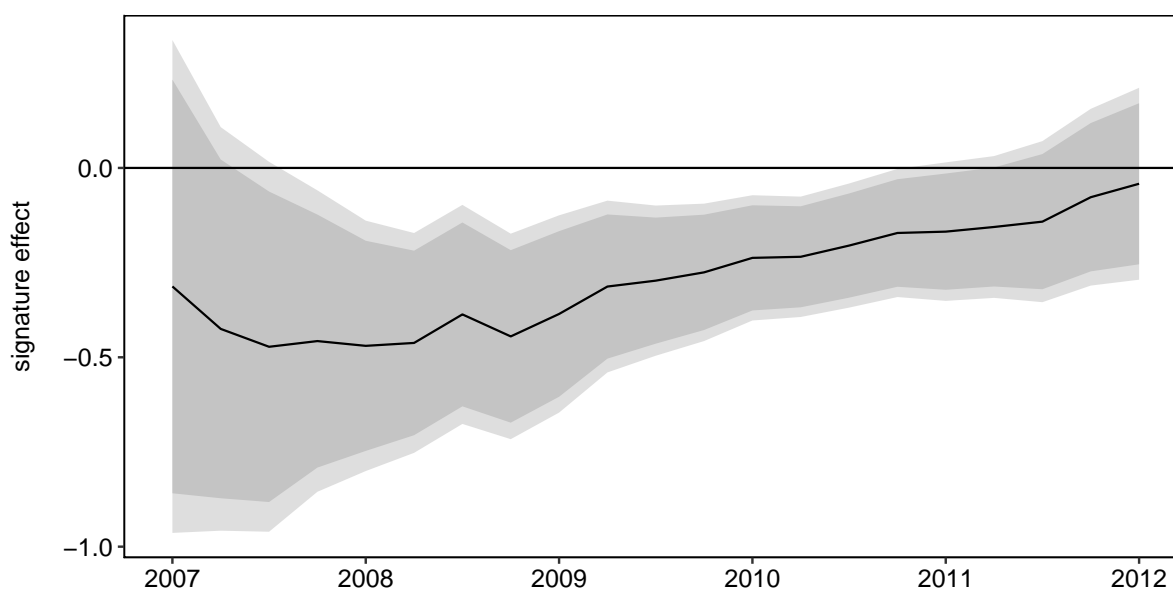
*Notes:* This figure shows the reaction of deposits in tax havens by non-haven counterparties (outbound) to treaties the counterparty non-haven signs with other tax havens in an unbalanced panel. Bar lengths show coefficient sizes of regressions in a rolling window of +/- 8 quarters around the quarter plotted on the horizontal axis. The deposit shifting variable is constructed by adding the IoR treaties a counterparty non-haven has signed with other tax havens. Shading denotes significance and no estimate has a p-value between 0.1 and 0.05.

Table 4: Reaction of deposits in non-havens (inbound) to IoR treaties

	<i>Dependent variable: log(deposits)</i>					
	inbound	inbound	inbound	inbound	inbound JZ(2014) tax haven list	inbound JZ(2014) list without Switzerland
	(1)	(2)	(3)	(4)	(5)	(6)
IoR treaty signed	−0.373*** (0.124)				−0.330** (0.135)	−0.339** (0.138)
IoR treaty <sub>k=1:k=3</sub>		−0.167* (0.095)	−0.158 (0.100)			
IoR treaty <sub>k=4:k=6</sub>		−0.331*** (0.107)	−0.303*** (0.115)			
IoR treaty <sub>k&gt;6</sub>		−0.425*** (0.143)	−0.385** (0.152)			
Placebo assigned			0.066 (0.111)		0.067 (0.122)	0.085 (0.128)
TIEA signed				−0.463*** (0.140)		
DTC signed				−0.160 (0.183)		
Financial weight			0.921*** (0.322)	0.932*** (0.328)	0.840** (0.336)	0.839** (0.337)
countrypair f.e.	Yes	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,200	10,200	10,057	10,057	9,046	8,566
R <sup>2</sup>	0.195	0.197	0.214	0.214	0.202	0.197
Adjusted R <sup>2</sup>	0.177	0.179	0.195	0.196	0.183	0.178

*Notes:* Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables take value 1 if a bilateral treaty specifying IoR has been signed with Column 1 showing the effect of the treaty signature alone. Column 2 changes the lag structure. Column 3 adds the weight of the bilateral integration in bank claims relative to all other countries the non-haven reports bank claims on (Financial weight) and the placebo constructed to take into account the momentum of the OECD initiative. Column 4 separates the treatment variable into double taxation conventions (DTC) and tax information exchange agreements (TIEA). Column 5 reduces the tax havens used in the counterparty dimension to the tax haven list by Johannesen and Zucman (2014) – “JZ(2014)” for brevity – and Column 6 additionally excludes Switzerland from the sample. The dependent variable are data on time series of deposits by tax haven counterparties in non-havens banks (inbound sample). This sample consists of 170 countrypairs reported by 11 non-havens against a combined 44 tax havens over 60 quarters (2003:I - 2017:IV). \* denotes 10% significance, \*\* 5% significance, and \*\*\* 1% significance.

Figure 8: **Rolling estimation window: Inbound**



*Notes:* This figure shows the reaction of deposits in non-havens by tax haven counterparties (inbound) over time. The solid black line shows coefficients of regressions in a rolling window of  $\pm 8$  quarters around the year-quarter plotted on the horizontal axis. The coefficient captures the effect of the most significant lags of IoR signatures ( $k = 4 : k = 6$ ). The dark grey area denotes 10% significance while the light grey area denotes 5% 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 CRS activations

	<i>Dependent variable: log(deposits)</i>					
			tax haven		non-haven	
			falsification		falsification	
	outbound	outbound	sample	sample	sample	sample
	(1)	(2)	(3)	(4)	(5)	(6)
CRS activated	−0.429*** (0.080)	−0.383*** (0.076)	−0.106 (0.138)	−0.074 (0.133)	0.030 (0.091)	−0.002 (0.094)
IoR treaty signed		−0.275*** (0.080)		−0.061 (0.123)		0.205* (0.123)
Amnesty / VDP		0.016 (0.074)				
Placebo assigned		−0.011 (0.044)				
Financial weight	0.557*** (0.200)	0.558*** (0.198)		0.521*** (0.196)		0.385*** (0.131)
countrypair f.e.	Yes	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28,682	28,682	11,820	11,133	28,380	27,431
R <sup>2</sup>	0.079	0.084	0.085	0.083	0.144	0.152
Adjusted R <sup>2</sup>	0.059	0.064	0.064	0.061	0.127	0.135

*Notes:* Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variable ‘CRS activated’ takes 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 with the bilateral control used in the other tables. In column 2, ‘IoR treaty signed’ denotes signatures of information exchange upon request and we start falsification exercises in the outbound sample with amnesties and voluntary disclosure programs and the previously used placebo treaty. Columns 3 and 4 repeat the main treatments in the tax haven falsification sample and Columns 5 and 6 in the non-haven falsification sample. The dependent variable columns 1 and 2 are data on time series of deposits by non-haven counterparties in tax haven banks (outbound sample) resulting in 557 countrypairs reported by 8 reporting tax havens against a combined 146 non-havens. The dependent variable in columns 3 and 4 are data on time series of deposits by tax haven counterparties in tax haven banks (tax haven falsification sample). This sample consists of 197 tax haven - tax haven countrypairs. The dependent variable in columns 5 and 6 are data on time series of deposits by non-haven counterparties in non-havens banks (non-haven falsification sample). This sample consists of 473 countrypairs. All series run over 60 quarters (2003:I - 2017:IV). \* denotes 10% significance, \*\* 5% significance, and \*\*\* 1% significance.

## **APPENDIX**

to accompany

### **“Tax evasion in new disguise?”**

#### **Examining tax havens’ international bank deposits”**

Table A1: Tax havens full list

Table A2: Disagreements in tax haven lists

Appendix 1: Reactions to US policy measures (incl. Table A3)

Appendix 2: Robustness checks (incl. Tables A4 - A11 and Figures A1 - A4)

Appendix 3: Linkages between outbound and inbound deposits (incl. Figure A5)

Table A1: Tax havens full list

Jurisdiction	population	Affiliation	Treaties with non- havens	Deposit time series - balanced panel (non-havens; tax havens)	total liabili- ties 2017:IV	out- bound	in- bound
Andorra	85,660	-	14	23 (2; 4)	-	-	1
Anguilla	16,752	British o.T.	13	- (-; -)	-	-	-
Antigua & Barbuda	93,581	C.o.N	12	- (-; -)	-	-	-
Aruba	113,648	Netherlands	12	19 (1; 2)	-	-	1
Austria	8,711,770	EU	6	28 (8; 7)	72196	-	1
Bahamas	327,316	C.o.N	21	28 (6; 7)	35356	-	1
Bahrain	1,378,904	-	18	26 (3; 7)	54294	-	1
Barbados	291,495	C.o.N	11	23 (2; 4)	-	-	1
Belgium	11,409,077	EU	3	29 (7; 6)	103478	1	1
Belize	353,858	C.o.N	13	24 (3; 3)	-	-	1
Bermuda	70,537	British o.T.	27	28 (6; 5)	1986	-	1
British Virgin Islands	34,232	British o.T.	18	- (-; -)	-	-	-
Cayman Islands	57,268	British o.T.	25	29 (6; 7)	268102	-	1
Chile	17,650,114	-	2	27 (5; 4)	2715	1	1
Cook Islands	9,556	New Zealand	13	- (-; -)	-	-	-
Costa Rica	4,872,543	-	17	28 (3; 4)	-	-	1
Curaçao	149,035	Netherlands	-	- (-; -)	4566	-	-
Cyprus	1,205,575	-	13	27 (4; 5)	14396	-	1
Dominica	73,757	C.o.N	15	23 (1; 1)	-	-	1
Gibraltar	29,328	British o.T.	19	24 (2; 4)	-	-	1
Grenada	111,219	C.o.N	11	20 (1; 2)	-	-	1
Guernsey	66,297	British C.D.	34	24 (3; 6)	31805	1	1
Hong Kong	7,167,403	China S.A.R.	14	27 (8; 7)	458182	-	1
Ireland	4,952,473	EU	12	28 (8; 6)	66194	1	1
Isle of Man	88,195	British C.D.	28	23 (2; 6)	25430	1	1

Jersey	98,069	British C.D.	32	25 (4; 6)	59786	1	1
Jordan	8,185,384	-	-	26 (2; 4)	-	-	1
Lebanon	6,237,738	-	-	25 (3; 4)	-	-	1
Liberia	4,299,944	-	14	25 (5; 4)	-	-	1
Liechten- stein	37,937	-	16	23 (3; 4)	-	-	1
Luxem- bourg	582,291	EU	10	29 (8; 6)	148886	1	1
Macau	597,425	China S.A.R.	9	22 (3; 3)	30898	-	1
Malaysia	30,949,962	C.o.N	6	27 (7; 5)	28186	-	1
Maldives	392,960	-	-	20 (1; 1)	-	-	1
Malta	415,196	EU; C.o.N	14	26 (4; 5)	-	-	1
Marshall Islands	73,376	-	11	21 (2; 4)	-	-	1
Mauritius	1,348,242	C.o.N	10	24 (3; 4)	-	-	1
Monaco	30,581	-	13	- (-; -)	-	-	-
Montserrat	5,267	British o.T.	8	- (-; -)	-	-	-
Nauru	9,591	C.o.N	-	14 (-; 1)	-	-	1
Netherlands Antilles		synthetic	-	24 (3; 5)	-	-	1
Niue	1,190	New Zealand	6	- (-; -)	-	-	-
Palau	21,347	-	-	- (-; -)	-	-	-
Panama	3,705,246	-	17	29 (6; 5)	22344	-	1
Samoa	198,926	-	9	18 (1; 3)	-	-	1
San Marino	33,285	-	25	19 (-; 2)	-	-	1
Seychelles	93,186	-	11	25 (3; 4)	-	-	1
Singapore	5,781,728	-	11	28 (7; 7)	254465	-	1
Sint Maarten	41,486	Netherlands	-	- (-; -)	-	-	-
St. Kitts & Nevis	52,329	C.o.N	14	- (-; -)	-	-	-
St. Lucia	164,464	C.o.N	14	20 (1; 2)	-	-	1
St. Vincent & Grenadines	102,350	C.o.N	13	20 (3; 3)	-	-	1
Switzerland	8,179,294	-	12	29 (8; 6)	454075	1	1
Tonga	106,513	C.o.N	-	15 (-; -)	-	-	-

Trinidad & Tobago	1,220,479	C.o.N	-	25 (3; 2)	-	-	1
Turks & Caicos Islands	51,430	British o.T.	13	20 (3; 3)	-	-	1
U.S. Virgin Islands	102,951	USA	-	- (-; -)	-	-	-
Uruguay	3,351,016	-	18	28 (3; 4)	-	-	1
Vanuatu	277,554	C.o.N	8	19 (3; 3)	-	-	1
Vatican City	1,000	-	-	- (-; -)	-	-	-

*Notes:* Shows all countries that show up on tax haven lists of recent studies. Population in column 2 and affiliations in column 3 are taken from the CIA World Factbook. Affiliations are abbreviated as follows: British C.D.s are the British Crown Dependencies, British o.T.s the British overseas Territories, C.o.N. is the Commonwealth of Nations, and China S.A.R.s are special administrative zones of China. 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 after 2003:I, meeting OECD standards and including the updated less stringent requirements for information exchange. The first number in column 5 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). Column 6 shows the total liabilities reported by that tax haven against the rest of the world (not bilaterally) and Columns 7 and 8 show if the tax haven is included in the inbound and/or outbound sample.



**Table A2: Disagreements in tax haven lists**

Tax.Haven	Glautier & Bassinger (1987)	Hines & Rice (1994)	OECD (2000)	Dharmapala (2008)	Johannesen & 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
Macau		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
Samoa			1	1	1	1
San Marino			1	1	1	1
Seychelles			1	1	1	1
St. Lucia		1	1	1	1	1
Tonga			1	1		1
Trinidad & Tobago					1	
U.S. Virgin Islands			1	1	1	1
Uruguay					1	

*Notes:* All of the surveys shown in this table include the following list of countries as tax havens: Antigua and Barbuda, Bahamas, Bahrain, Barbados, Belize, Bermuda, British Virgin Islands, 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, and Vanuatu.

\* 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 Reactions to US policy measures**

The US led several initiatives targeting Swiss bank secrecy, completing two intergovernmental agreements as a result. While Johannesen and Stolper (2018) as well as Johannesen et al. (2018) 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 April 2019, 81 banks<sup>A1</sup> have signed up for this program. They will potentially report deposits that were previously hidden positions. Indeed, empirical results confirm these expectations, as Table A3, column 1 shows. However, since these treaties only act on one bilateral connection in the sample (the Switzerland – United States countrypairs constitutes 60 observations in each sample), we do not want to overinterpret the results. Of the 50 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 deposits 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 deposits as for the earlier IoR treaties. This also holds when we control for the latter (columns 2 and 4) and the pattern is partly also seen in inbound deposits. The latter is most obvious for the two US-Swiss agreements. Regarding FATCA, the inbound coefficient is insignificant. This effect is largely expected because the inbound deposits are diluted due to the US-focused nature of these treaties. While, in the case of CRS activations and IoR treaties, we can analyze a world-wide inbound sample looking at positions from all tax havens to all non-havens, FATCA only allows looking for direct deposits back in the US. However, such deposits do not need to be deposited in the country where they came from; here the US. Accordingly, we do not measure an inbound reaction to the agreement. The same logic applies to direct round-

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<sup>A1</sup> See <https://www.justice.gov/tax/swiss-bank-program> for a list of participating banks. Last accessed April 16, 2019

tripping deposits at the countrypair level, where the matching is also weak for IoR treaties, as we demonstrate in Appendix 3.

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 shell companies with bank accounts without any personal identification. Representatives of Bermuda, the Cayman Islands, and the Isle 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 shell companies 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 (2009b) and Hines (2010). Still, this outside option for evaders is a potential avenue for future research.

Table A3: Reactions to US specific measures

	<i>Dependent variable: log(deposits)</i>			
	inbound (1)	inbound (2)	outbound (3)	outbound (4)
2009 US-CH Agreement	−0.170*** (0.057)	−0.257*** (0.070)	−0.094*** (0.034)	−0.119*** (0.034)
US-Swiss Bank Program	0.322*** (0.097)	0.332*** (0.098)	0.596*** (0.134)	0.576*** (0.121)
FATCA signed	0.162 (0.126)	0.124 (0.127)	−0.503*** (0.156)	−0.496*** (0.141)
IoR treaty signed		−0.370*** (0.124)		−0.274*** (0.078)
countrypair f.e.	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes
Observations	10,200	10,200	33,420	33,420
R <sup>2</sup>	0.183	0.196	0.076	0.082
Adjusted R <sup>2</sup>	0.164	0.177	0.059	0.064

*Notes:* Autocorrelation and heteroscedasticity robust standard errors in parentheses. Column 1 shows the effect of US specific regulation attempts: two bilateral treaties with Switzerland (the US-CH Agreement and the US-Swiss Bank Program) 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 IoR treaties previously used. 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 60 quarters (2003:I - 2017:IV). 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 557 countrypairs reported by 8 reporting tax havens against a combined 146 non-havens. \* denotes 10% significance, \*\* 5% significance, and \*\*\* 1% significance

## Appendix 2 Robustness checks

These robustness analyses are organized along four themes. To test the robustness of our results on IoR treaties, we split the analysis into, first, outbound deposits, second, inbound deposits. We also provide treatment graphs of a dynamic differences-in-differences exercise which confirms our results in both directions. 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. Finally, we investigate the relationship between the CRS and IoR treaties and show that the declining effect of IoR treaties over time is not driven by later CRS agreements. We also show robustness of the CRS results and confirm that the CRS reactions are close to identical for countrypairs that have or have not participated in previous policy initiatives.

**Replication of the outbound analysis.** Since the outbound analysis is inspired by Johannesen and Zucman (2014), we continue the robustness analysis by replicating their results with our data. Thus, Table A4 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, and shows stronger impacts, consistent with the fading treatment effect we discuss in the main text.

— Table A4 about here —

In order to replicate the Johannesen and Zucman (2014) results, we first change the tax haven list to that used by the authors (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 much closer to those of Johannesen and Zucman (2014). Thus, it is not the sample length or the tax haven list that differentiates our quantitatively larger results, but rather 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, these results confirm our choice of a very restrictive treaty definition as a lens to detect tax evasion.

**OECD and G20 meetings.** As we point out in the main text, IoR treaties were signed following active pressure by the OECD and the G20. In Table A5 we show results testing the hypothesis that these meetings might have a signaling effect dominating treaty signatures.

Column 1 shows the outbound baseline while the following columns alter the underlying sample used as shown by the decreasing number of observations. In column 2, we exclude the time frame during which the OECD initiative on harmful tax practices was actively endorsed and promoted by G20 meetings (2008:III-2010:I). In line with other robustness checks, this sample change does not affect our results much. Tax evasion, however, has stayed on the political agenda of the G20 until today. In Column 3, we therefore exclude all quarters following a G20 meeting in our sample period. We also exclude all IoR treaties signed during these quarters entirely from the analysis thus reducing the total number of IoR treaties in our balanced panel from 251 to 185. Still, results hold qualitatively. Columns 4-6 repeat these exercises for inbound deposits. Together with the treatment analysis reported further below, we take this as evidence that our effects are not driven by events such as G20 meetings but rather by the treaty signatures themselves.

— Table A5 about here —

**Unbalanced panel.** In order to verify the validity of using an unbalanced panel to show deposit shifting effects, we replicate the main outbound effect of a decreasing effect of IoR treaties. This is a direct copy of Figure 6 in the main text with the only difference being that the entire available locational banking statistics as reported by tax havens is used. Figure A1 shows that both the significant negative effect of IoR treaties as well as the decreasing effect over time show up here as well.

— Figure A1 about here —

**Sample period.** To test the robustness of results concerning inbound deposits, we first test whether the results we find are an artifact of the sample period or hold over sub-samples. Table A6 reports the results with column 1 repeating the baseline from Table 4 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 through 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 A6 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 some coefficients are 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. These claims include, for example, loans to other banks and do not include deposits (BIS, 2013). In non-haven countries, from which this data are reported, we do not expect bank lending to react significantly to decreased tax evasion and banks themselves have not been convicted in the kind of personal tax evasion cases we identify. We find results neither 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.

**Inbound sample effects.** Replicating the results of Figure 3 in the main text for inbound deposits, Figure A2 shows the same four panels, namely the fluctuation of estimates and the associated p-values around the baseline (the black square). In the top panel, we sequentially drop one (dark grey circles) and any combination of two (light grey crosses) counterparties from the sample and plot the re-estimated treatment effects and their p-values. Having dropped Switzerland as a counterparty is again highlighted with triangles. The second panel does the same for the country dimension. The third panel shows the effects of dropping one or two year-quarters at a time and the bottom panel finally shows dropping one or two entire years from the panel. Inbound results are even more robust than outbound results with no specification turning insignificant and estimates remaining stable in economic size. Neither particular time

periods nor countries drive our inbound results. The inbound results are quite robust to dropping particular tax havens since we draw on reports against a total of 44 tax havens.

— Figure A2 about here —

**More treaties.** In order to establish the robustness of our results to our choice of treaties, we broaden our very restrictive definition of which treaties constitute a credible threat of detection. Table A7 compares the baseline (column 1) to the inclusion of the 56 treaties signed within the balanced panel dimension that do not include paragraphs 4 and 5 but were reviewed by the OECD and met the standard (column 2). Then, we include another 19 treaties that were not reviewed at the time of analysis (column 3). Finally, we also include those 25 treaties that were reviewed, but failed to meet the OECD standard (column 4). Results are broadly consistent.

— Table A7 about here —

**Treatment analysis.** Here, we verify that the anticipation effects in outbound deposits and the lagged effects in the inbound deposits are no artifact of the strong fluctuations in international financial data. The treatment analysis used here allows us to visually compare developments in the treatment groups to aggregate developments while taking into account distance to IoR treaties explicitly. For outbound deposits, we run a dynamic differences-in-differences analysis of the following form:

$$(3)\log(deposits_{ijkt}) = \alpha_{ij} + \gamma_t + \theta_k + \sum_{k=-20}^{K=-4} \beta_k(IoRtreaty_{ij,k}^k) + \sum_{k=-2}^{K=20} \tau_k(IoRtreaty_{ij,k}^k) + \varepsilon_{ijkt}$$

The notation follows the main text: subscripts  $ij$  denote the countrypair,  $t$  the respective year-quarter, and  $k$  the distance from a treaty. The treatment fixed effects  $\theta_k$  are based on this distance and the coefficients we plot are interpretable relative to the omitted treatment distance period, in the outbound case  $k = -3$ . We use the placebos introduced in section 4.2 as the  $k = 0$  distance to ‘treaty’ for the control group countrypairs that have not signed IoR treaties. We focus on 20 quarters before and after treaty signatures to see if the pre-treatment trend is reasonably stable. The same setup is run for inbound deposits, this time taking  $k = 0$  as the point of comparison, as no anticipation was visible there.

Figure A3 shows the resulting treatment coefficients and associated standard errors plotted over the distance to treatment on the horizontal axis. Both outbound (top panel) and inbound



(bottom panel) graphs show a clear drop after IoR treaties. Pre-treatment trends aren't flat but reasonably stable for macroeconomic international financial data aggregated for the bilateral connection. As our panel results indicate, inbound deposits mirror the reaction but not the anticipation effect that we observe in outbound deposits. Both results, however, are clearly not driven by single lags of the treatment. These results show average reactions for all treaties in the sample and are thus not to be confused with the reactions over time we show in the main text where we analyze treaties sequentially.

— Figure A3 about here —

**Tax haven list.** The controversies surrounding tax haven lists are very relevant for the study at hand, it determines the assignment of our inbound and outbound samples. These data can change considerably when changing the tax haven list. For outbound deposits, table A8 reports results for different lists that are commonly used in the literature (see Table A2 for an overview of different lists). Column 1 repeats the baseline results before we change the tax haven dimension in the counterparties to the list provided in Gravelle (2015) (column 2). Results remain qualitatively the same.

— Table A8 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 Bilicka and Fuest (2014); Dharmapala (2008); Dharmapala and Hines (2009b); Gravelle (2015); Hanlon et al. (2015); Hines and Rice (1994); Johannesen and Zucman (2014); OECD (2000). This means that we remove 21 countries<sup>A2</sup> from the tax haven list used so far. Column 3 shows that our results hold despite losing almost half of our observations. To go even further, we now include the tax havens dropped from the tax haven list as non-haven countries. Macao is now in the same category as France and Sweden when it comes to facilitating tax evasion. Column 4 that in the outbound direction, results are not affected by this change.

Table A9 repeats the same exercise for inbound deposits. Since the tax haven dimension in this direction is the reporting country level, we rely on a much smaller number of tax havens. Here, we have a larger number of counterparty tax havens (44) and can thus differentiate the OECD (2000) list (column 2) from the Gravelle (2015) list (column 3). Moving to the

<sup>A2</sup>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 in the counterparty dimension (column 4) we still find no substantial change in effects. However, including countries that are not considered tax havens in the consensus list as non-havens finally does reduce our effect. 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 unreasonable.

— Table A9 about here —

**Robustness of CRS results.** In order to verify that the CRS results we report in the main text are as robust as the results on IoR treaties, we repeat the comprehensive robustness test for sample effects for this treatment variable. Figure A4 shows that the results are entirely robust to dropping one (black circle) or any combination of two (grey cross) counterparties (top panel), reporting countries (second panel, dropping Switzerland again denoted in red triangles), or year-quarters (third panel). No specification leads to insignificant results and only changes in reporting countries leads to some movement in the size of the effect. Even here, however, it remains firmly in negative territory. Since the earliest CRS activations take place in the fourth quarter of 2016, dropping 2017 from the sample is not sensible. The bottom panel of Figure A4 therefore excludes 2017 as a drop option and simply tests if other years drive the effect in the control group. As before, the baseline effect is shown as a black square.

— Figure A4 about here —

**Anticipation of the CRS.** It is possible, that the declining effect of IoR signatures we report is itself an anticipation of the introduction of automatic information exchange via the CRS. In order to investigate this, we analyze which kind of countrypairs drive the effects we report. Table A10 shows a baseline with just the CRS dummy in column 1 and in column 2 we have a dummy denoting countrypairs that have agreed in principle to share information under the Multilateral Competent Authority Agreement (MCAA) but have not activated bilateral CRS exchange relationships. The results show that the negative effect we observe for CRS activation does not hide a reaction to the MCAA but is mainly driven by countrypairs that actually activate their relationships.

— Table A10 about here —

Investigating the relation between the effect of IoR treaties on outbound deposits and the CRS, column 3 first separates the IoR treaties into early and late treaties. We use 2013:I as a

cutoff here but results are robust to similar cutoff points. Consistent with our results, late IoR treaties do not exhibit effects. If this non-effect is due to correct CRS anticipations, we should observe differentiated results for IoR treaties between countrypairs that later implement CRS measures and those that do not. Column 4 shows results for an interaction term of late IoR treaties with a dummy variable indicating if the countrypair in question joined the MCAA with no significant results. Column 5 shows an interaction of late IoR treaties with countrypairs that later activated bilateral CRS relationships, again the coefficient remains insignificant. Finally in column 6 we split the variable capturing late IoR treaties. The first variable takes value 1 only for the time during which if a late IoR was signed but CRS was not activated bilaterally yet and is set to 0 again after CRS activation. No significant effects emerge here. The second variable takes value 1 for late IoR treaties that are not followed by CRS relationships and thus repeats the results of late IoR treaties in column 5. Despite these detailed investigations, we do not find any evidence that there is a difference in late treaties amongst countrypairs that turn out to engage in automatic information exchange relationships and countrypairs that do not.

**Relations between CRS activations and other policy measures.** To support our main hypothesis, namely that reactions to the CRS are deposits that have been disguised differently than those that reacted to IoR treaties before, we here compare the relationship between CRS activations and earlier information exchange relationships. Table A11 first repeats the baseline (column 1) and then interacts CRS activations with IoR treaty signatures (column 2). In Column 3, we split the sample into countrypairs that had signed IoR relationships in the course of our sample and those that have not (column 4). The coefficients on CRS activation is virtually unaffected by these exercises. This is in line with our interpretation the CRS activations show a new reaction of tax evasion setups using new disguises that were not vulnerable to IoR treaties but have to be reshaped in reaction to the CRS. In column 5, we go back to the complete sample and show results on all policy initiatives considered in this text in one column. The EU Savings Tax Directive (or EU STD, see Johannesen, 2014b, for a detailed analysis), the US FATCA initiatives (see [Appendix 1](#) for details), IoR, and the CRS activation all have the expected signs, effect sizes, and are statistically significant. Finally, we lump together all AEI initiatives and create a dummy that takes value one if either the CRS is activated, the EU STD is agreed on, or FATCA has been signed and zero otherwise. Comparing the resulting coefficients in column 6 shows that their combined effect size is very close to that of IoR treaties.

— Table A11 about here —

Table A4: **Robustness of outbound results**

	<i>Dependent variable: log(deposits)</i>			
	baseline	+ JZ14 sample	+ JZ14	+ JZ14
	outbound	outbound	tax havens	treaty definition
	(1)	(2)	(3)	(4)
IoR treaty <sub>k=0</sub>	−0.177** (0.072)	−0.269*** (0.087)	−0.255*** (0.077)	0.006 (0.049)
IoR treaty <sub>k=1</sub>	−0.130* (0.075)	−0.290*** (0.089)	−0.262*** (0.088)	−0.083 (0.063)
IoR treaty <sub>k=2</sub>	−0.156** (0.073)	−0.325*** (0.104)	−0.299*** (0.104)	−0.114* (0.065)
IoR treaty <sub>k=3</sub>	−0.224*** (0.073)	−0.448*** (0.111)	−0.375*** (0.101)	−0.187** (0.073)
IoR treaty <sub>k&gt;1</sub>	−0.296*** (0.083)	−0.434*** (0.097)	−0.360*** (0.090)	−0.210*** (0.076)
countrypair f.e.	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes
Observations	33,420	17,267	16,523	16,523
R <sup>2</sup>	0.081	0.106	0.126	0.122
Adjusted R <sup>2</sup>	0.063	0.074	0.095	0.091

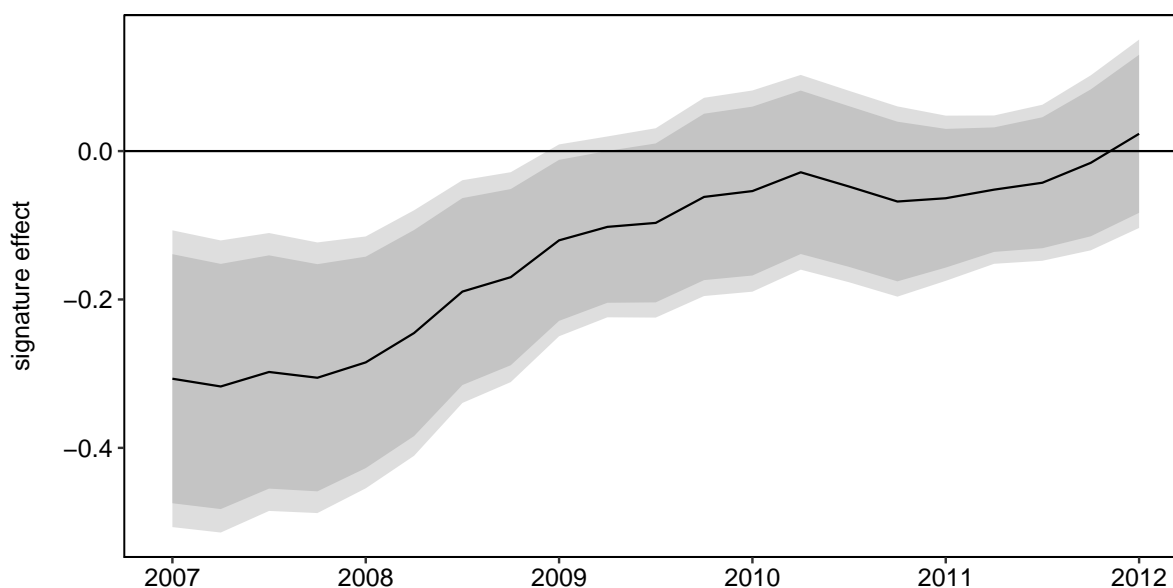
*Notes:* Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables ‘IoR treaty’ take value 1  $k$  quarters from the IoR treaty,  $k = 3$  for example denoting the third quarter after a treaty. Column 1 repeats the outbound baseline. Column 2 reduces the sample 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 (both in the country and counterparty dimension) 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 557 countrypairs reported by 8 reporting tax havens against a combined 146 non-havens over 60 quarters (2003:I - 2017:IV) and is reduced over columns as mentioned above. \* denotes 10% significance, \*\* 5% significance, and \*\*\* 1% significance.

Table A5: **Robustness to G20 meetings and OECD initiative**

<i>Dependent variable: log(deposits)</i>						
	baseline outbound	no OECD initiative outbound	no G20 last quarter outbound	baseline inbound	no OECD initiative inbound	no G20 last quarter inbound
	(1)	(2)	(3)	(4)	(5)	(6)
IoR treaty signed	−0.292*** (0.082)	−0.308*** (0.091)		−0.379*** (0.122)	−0.422*** (0.137)	
IoR treaty signed excl. those after G20 meetings			−0.260*** (0.095)			−0.388*** (0.138)
Financial weight	0.553*** (0.199)	0.608*** (0.209)	0.569*** (0.205)	0.931*** (0.325)	0.929*** (0.353)	0.880** (0.345)
countrypair f. e.	Yes	Yes	Yes	Yes	Yes	Yes
year-qtr f. e.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28,682	25,430	22,865	10,057	8,882	8,049
R <sup>2</sup>	0.080	0.076	0.008	0.212	0.226	0.031
Adjusted R <sup>2</sup>	0.061	0.054	−0.019	0.194	0.206	0.004

*Notes:* Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables ‘IoR treaty’ take value 1 after signature. Column 1 repeats the outbound baseline, column 2 shows results for a sample where the years after the OECD initiative and the G20 London summit of 2009 that explicitly targeted tax evasion are dropped. Column 3 instead drops all year-quarters after G20 meetings from our sample and additionally drops all treaties signed during such quarters. Column 4 to 5 repeat the same specifications for inbound deposits. The baseline sample in column 1-3 consists of 557 countrypairs reported by 8 reporting tax havens against a combined 146 non-havens over 60 quarters (2003:I - 2017:IV) and is reduced over columns as mentioned above. The baseline sample in columns 4-6 consists of 170 countrypairs reported by 11 reporting non-havens against a combined 44 tax havens over the same time span. \* denotes 10% significance, \*\* 5% significance, and \*\*\* 1% significance.

Figure A1: **Rolling estimation window: Outbound, unbalanced panel**



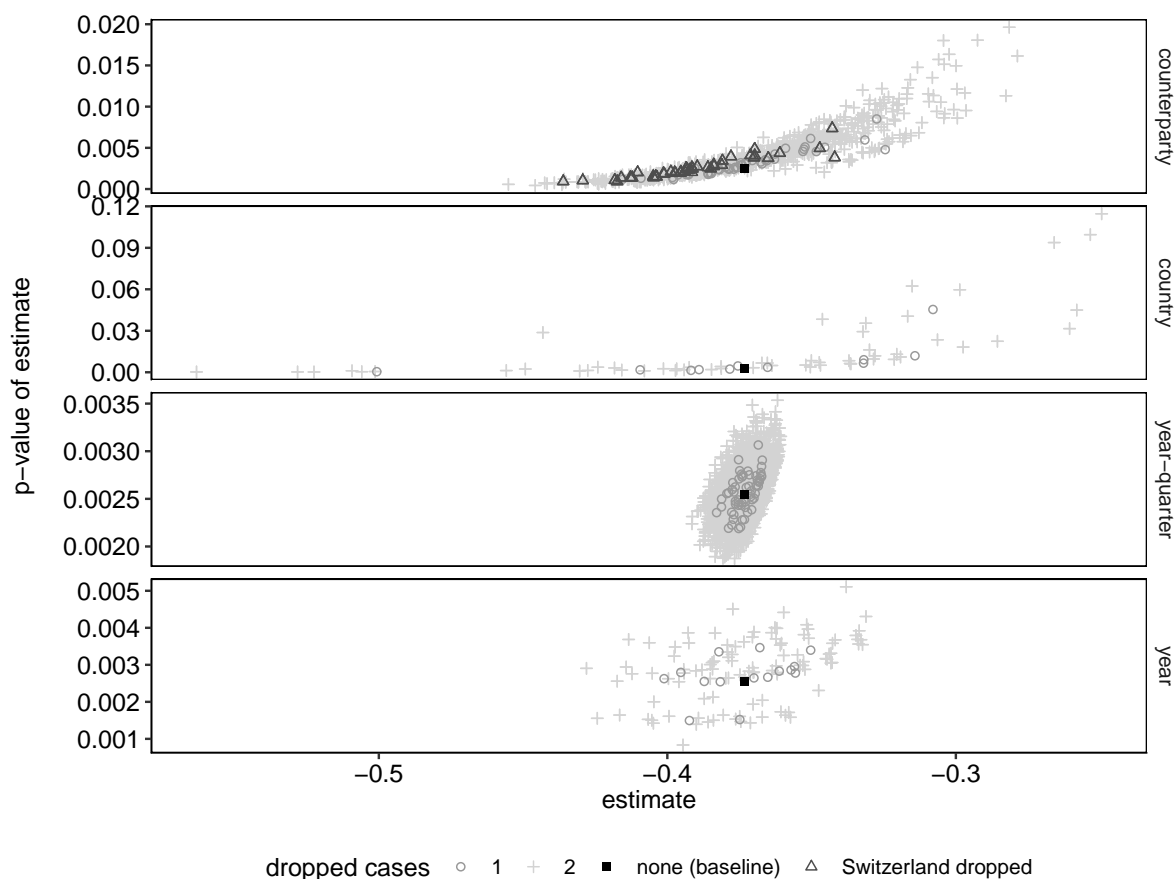
*Notes:* This figure shows the reaction of deposits in tax havens by non-haven counterparties (outbound) over time in a sample not limited to countrypairs that report during the entire sample length. The solid black line shows coefficients of regressions in a rolling window of  $\pm 8$  quarters around the quarter plotted on the horizontal axis. The coefficient captures the effect of the most significant lags of IoR signatures ( $k = -2 : k = 2$ ). The dark grey area denotes 10% significance while the light grey area denotes 5% significance in pointwise confidence bands. Estimated coefficients are plotted on the vertical axis and the horizontal line denotes a 0 effect.

Table A6: **Robustness of inbound results**

	<i>Dependent variable:</i>					
	log(deposits)				log(claims)	log(loans)
	baseline	JZ14 sample	no crisis			
	inbound	inbound	inbound	inbound	inbound	inbound
	(1)	(2)	(3)	(4)	(5)	(6)
IoR treaty <sub>k=1:k=3</sub>	−0.181*	−0.144	−0.217**	−0.175*	−0.008	0.057
	(0.094)	(0.100)	(0.095)	(0.099)	(0.114)	(0.110)
IoR treaty <sub>k=3:k=6</sub>	−0.334***	−0.340***	−0.331***	−0.317***	−0.066	0.053
	(0.105)	(0.118)	(0.112)	(0.109)	(0.120)	(0.114)
IoR treaty <sub>k&gt;6</sub>	−0.431***	−0.435**	−0.439***	−0.436***		
	(0.141)	(0.211)	(0.145)	(0.145)		
Financial weight	0.923***	0.358*	0.918***	0.867***		
	(0.323)	(0.199)	(0.333)	(0.324)		
log(landlines cparty)				0.152		
				(0.105)		
Growth of GDP/capita				0.219***		
				(0.008)		
log(Pop.)				−6.279**		
				(2.885)		
log(Pop. cparty)				−0.573		
				(0.662)		
IoR treaty <sub>k=7:k=12</sub>					−0.182	−0.058
					(0.129)	(0.140)
IoR treaty <sub>k&gt;12</sub>					−0.424***	−0.283
					(0.154)	(0.190)
countrypair f.e.	Yes	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,057	5,210	9,217	8,085	10,049	8,845
R <sup>2</sup>	0.214	0.193	0.224	0.248	0.153	0.126
Adjusted R <sup>2</sup>	0.195	0.160	0.204	0.227	0.133	0.104

Notes: Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables ‘IoR treaty’ take value 1  $k$  quarters from the IoR treaty,  $k = 1 : k = 3$  for example takes value one for the first three quarters after a treaty. 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 60 quarters (2003:I - 2017:IV). \* denotes 10% significance, \*\* 5% significance, and \*\*\* 1% significance.

Figure A2: Sample robustness inbound



*Notes:* The four panels plot the robustness of our inbound results to changes in the sample. In all panels, the black rectangle shows the baseline results with the estimate plotted on the horizontal axis and its p-value plotted on the vertical axis. The top panel shows results from estimations dropping one (dark grey circle) or any combination of two (light grey cross) counterparties at a time with the triangles indicate having dropped Switzerland. The second panel drops one or all combinations of two reporting countries at a time. The third panel drops one or any combination of two year-quarters and the final panel drops one or any combination two entire years from the sample.

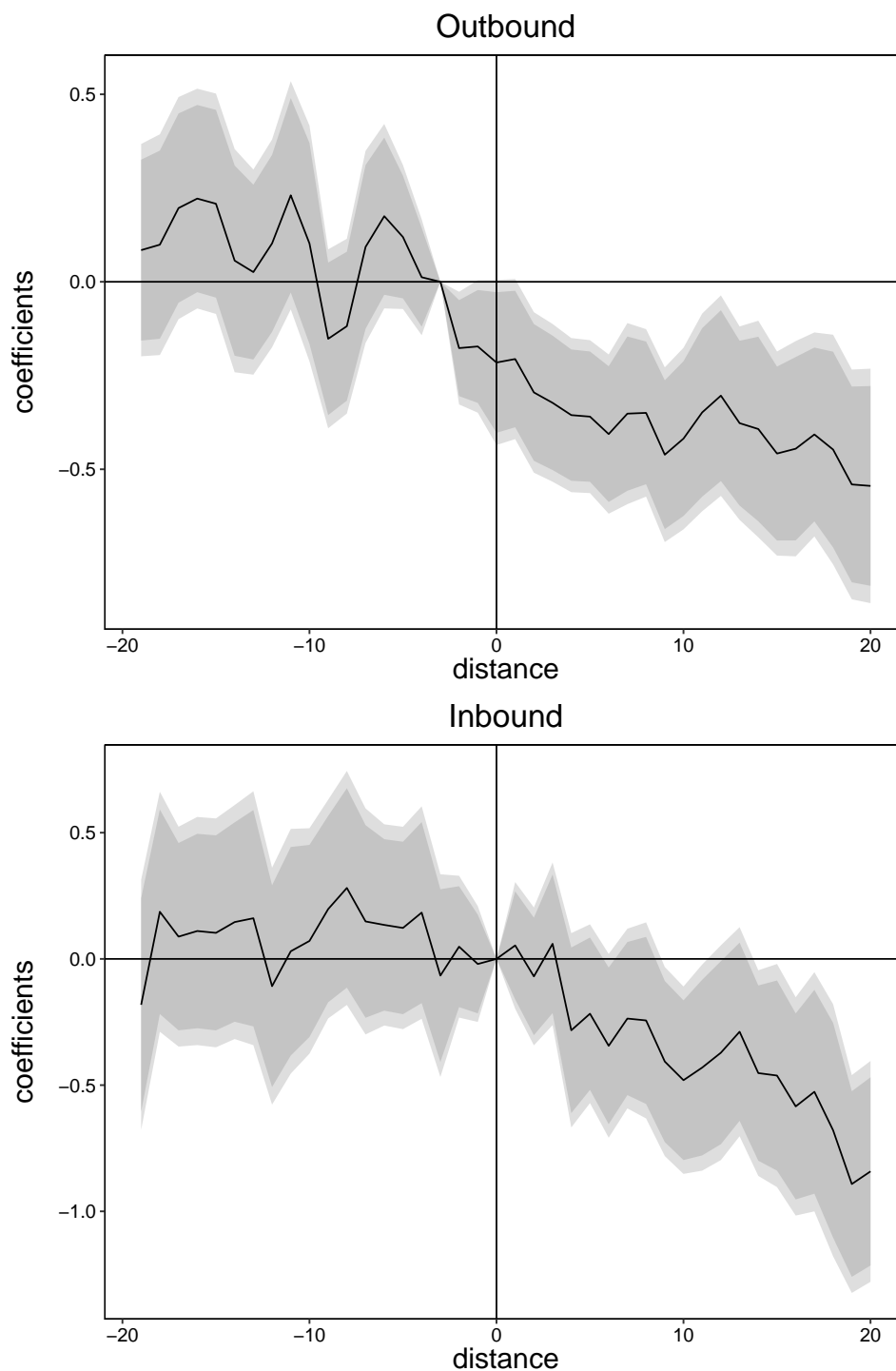


**Table A7: Robustness of inbound results to treaty definition**

	<i>Dependent variable: log(deposits)</i>			
	baseline inbound	balanced signature inbound	+ no para 4/5 inbound	+ not reviewed inbound
	(1)	(2)	(3)	(4)
IoR treaty <sub>k=1:k=3</sub>	−0.167* (0.095)	−0.356*** (0.123)	−0.371*** (0.125)	−0.387*** (0.124)
IoR treaty <sub>k=4:k=6</sub>	−0.331*** (0.107)	−0.331*** (0.113)	−0.325*** (0.112)	−0.341*** (0.111)
IoR treaty <sub>k&gt;6</sub>	−0.425*** (0.143)	−0.383*** (0.136)	−0.377*** (0.135)	−0.393*** (0.134)
countrypair fixed effects	Yes	Yes	Yes	Yes
year-qtr fixed effects	Yes	Yes	Yes	Yes
Observations	10,200	10,200	10,200	10,200
R <sup>2</sup>	0.197	0.195	0.195	0.197
Adjusted R <sup>2</sup>	0.179	0.177	0.177	0.178

*Notes:* Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables ‘IoR treaty’ take value 1 k quarters from the IoR treaty,  $k = 1 : k = 3$  for example takes value one for the first three quarters after a treaty. Column 1 repeats the baseline. 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 60 quarters (2003:I - 2017:IV). \* denotes 10% significance, \*\* 5% significance, and \*\*\* 1% significance.

Figure A3: **Dynamic Differences-in-Differences**



*Notes:* The two panels test the robustness of our main results outbound (Tables 2) and inbound (Table 4) in a dynamic differences-in-differences specification. The top panel shows outbound deposits around IoR treaty signatures (vertical line) against the baseline  $k = -3$  as explained in the text. The bottom panel shows inbound deposits against the baseline  $k = -1$ . All series are standardized at the countrypair level. The dark grey area denotes 10% significance while the light grey area denotes 5% significance of the associated coefficients. Estimated coefficients are plotted on the vertical axis and the horizontal line denotes a 0 effect.

Table A8: **Robustness of results to changes in tax haven list: outbound**

	<i>Dependent variable: log(deposits)</i>			
	baseline outbound	Gravelle 2015 outbound	consensus tax havens outbound	consensus both outbound
	(1)	(2)	(3)	(4)
IoR treaty signed	−0.287*** (0.082)	−0.324*** (0.083)	−0.356*** (0.082)	−0.350*** (0.077)
IoR treaty <sub>k=−2</sub>	−0.147** (0.066)	−0.164** (0.067)	−0.148*** (0.056)	−0.143*** (0.053)
IoR treaty <sub>k=−1</sub>	−0.162** (0.074)	−0.187** (0.075)	−0.175*** (0.066)	−0.160** (0.063)
countrypair f.e.	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes
Observations	33,420	25,560	22,980	25,500
R <sup>2</sup>	0.081	0.086	0.097	0.101
Adjusted R <sup>2</sup>	0.063	0.068	0.079	0.084

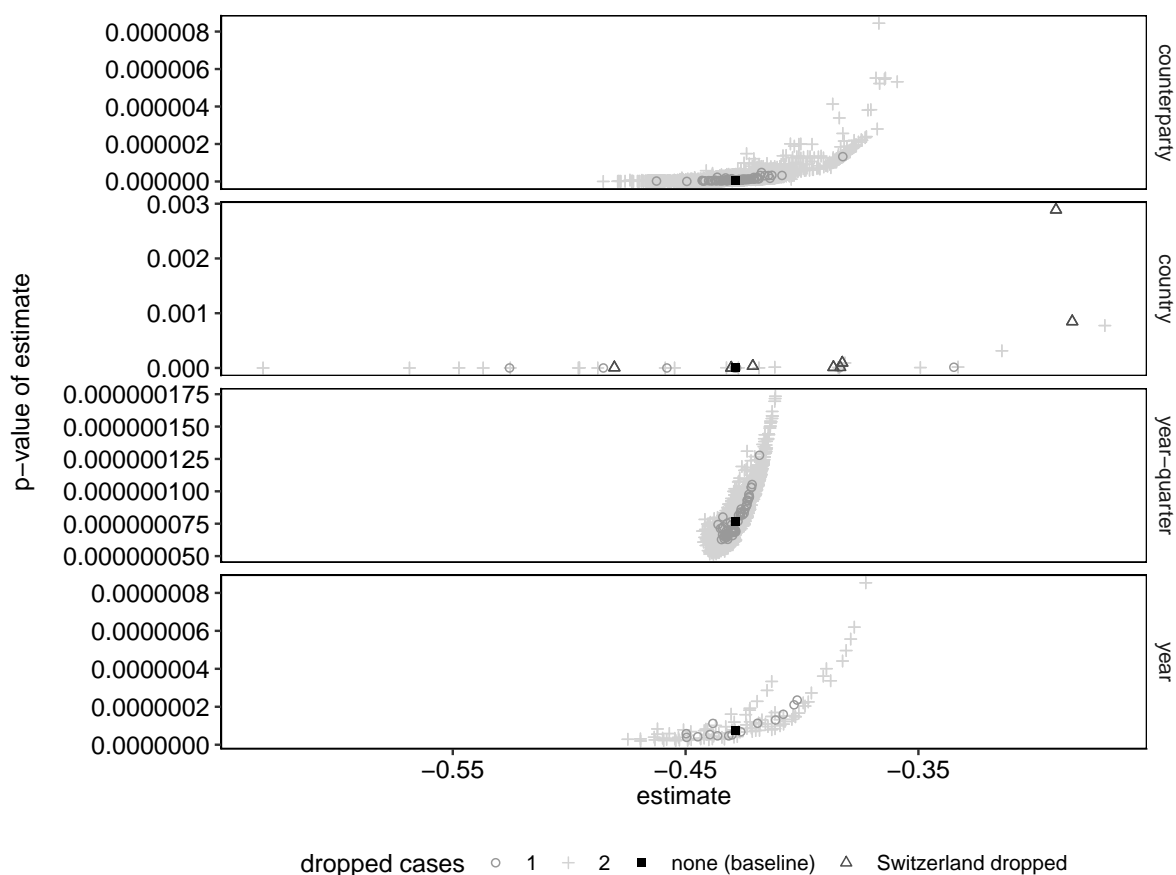
*Notes:* Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables ‘IoR treaty’ take value 1  $k$  quarters from the IoR treaty,  $k = -1$  for example denoting the quarter before a treaty. Column 1 repeats the outbound baseline, column 2 limits the tax haven 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 557 countrypairs reported by 8 reporting tax havens against a combined 146 non-havens over 60 quarters (2003:I - 2017:IV) and is changed as the tax haven list changes over the columns. . \* denotes 10% significance, \*\* 5% significance, and \*\*\* 1% significance.

**Table A9: Robustness of results to changes in tax haven list: inbound**

	<i>Dependent variable: log(deposits)</i>				
	baseline	OECD	Gravelle	consensus	consensus
	inbound	2000	2015	tax havens	both
	(1)	(2)	(3)	(4)	(5)
IoR treaty <sub>k=1:k=3</sub>	−0.167* (0.095)	−0.281*** (0.107)	−0.267*** (0.102)	−0.300*** (0.108)	−0.146 (0.097)
IoR treaty <sub>k=4:k=6</sub>	−0.331*** (0.107)	−0.459*** (0.128)	−0.436*** (0.120)	−0.484*** (0.129)	−0.262** (0.124)
IoR treaty <sub>k&gt;6</sub>	−0.425*** (0.143)	−0.523*** (0.170)	−0.514*** (0.160)	−0.557*** (0.173)	−0.340* (0.188)
countrypair f.e.	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes
Observations	10,200	7,080	8,220	6,840	9,240
R <sup>2</sup>	0.197	0.214	0.215	0.219	0.103
Adjusted R <sup>2</sup>	0.179	0.194	0.195	0.198	0.081

*Notes:* Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables ‘IoR treaty’ take value 1  $k$  quarters from the IoR treaty,  $k = 1 : k = 3$  for example takes value one for the first three quarters after a treaty. 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 Table 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 over 60 quarters (2003:I - 2017:IV) and is changed over the columns as tax haven lists are changed. \* denotes 10% significance, \*\* 5% significance, and \*\*\* 1% significance.

Figure A4: Sample robustness of CRS results



*Notes:* The four panels plot the robustness of the CRS results to changes in the sample. In all panels, the black rectangle shows the baseline results with the estimate plotted on the horizontal axis and its and p-value plotted on the vertical axis. The top panel shows results from estimations dropping one (dark grey circle) or any combination of two (light grey cross) counterparties at a time with the triangles indicate having dropped Switzerland. The second panel drops one or all combinations of two reporting countries at a time. The third panel drops one or any combination of two year-quarters and the final panel drops one or any combination two entire years from the sample. The year 2017 is omitted from the bottom panel since CRS activations only started in the fourth quarter of 2016.

Table A10: CRS/MCAA anticipation in late treaty signatures

	<i>Dependent variable: log(deposits)</i>					
	outbound (1)	outbound (2)	outbound (3)	outbound (4)	outbound (5)	outbound (6)
CRS activation	−0.356*** (0.078)	−0.303*** (0.078)		−0.303*** (0.074)	−0.302*** (0.074)	−0.289*** (0.074)
MCAA		−0.088 (0.069)				
IoR signed before 2013:I			−0.366*** (0.085)	−0.344*** (0.083)	−0.344*** (0.083)	−0.346*** (0.083)
IoR signed after 2013:I			0.188 (0.133)	0.127 (0.770)	0.195 (0.199)	
IoR signed after 2013:I *				0.091 (0.778)		
MCAA signature cpair						
IoR signed after 2013:I *					0.024	
CRS activation cpair					(0.257)	
IoR after 2013:I followed by but set to 0 after CRS						0.141 (0.153)
IoR signed after 2013:I, not followed by CRS						0.195 (0.199)
countrypair f.e.	Yes	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	33,420	33,420	33,420	33,420	33,420	33,420
R <sup>2</sup>	0.078	0.079	0.084	0.086	0.086	0.086
Adjusted R <sup>2</sup>	0.061	0.061	0.066	0.069	0.069	0.068

*Notes:* Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables take value 1 after signature or activation. Column 1 shows the effect of CRS activation without controls, column 2 adds a dummy indicating if the countrypair has signed up to the multilateral competent authority agreement (MCAA) after 2014:III within which the CRS can be activated. Column 3 shows differentiated effects for IoR treaties signed before and after 2013:I with the latter interacted with MCAA signature countrypairs (columns 4) and bilateral CRS activation countrypairs (column 5). Column 6 finally splits the resulting interaction into two variables. The first captures treaties signed after 2013:I followed by CRS activation but with the the CRS quarters set to zero. The second takes value 1 after IoR treaties signed after 2013:I for countrypairs that did not activate the CRS during the sample period. 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 557 countrypairs reported by 8 reporting tax havens against a combined 146 non-havens over 60 quarters (2003:I - 2017:IV) and is changed as the tax haven list changes over the columns. \* denotes 10% significance, \*\* 5% significance, and \*\*\* 1% significance.

Table A11: CRS activations and other policy initiatives

	<i>Dependent variable: log(deposits)</i>					
	outbound	outbound	outbound	outbound	outbound	outbound
			IoR signed	no IoR signed		
	(1)	(2)	(3)	(4)	(5)	(6)
CRS activation	−0.356*** (0.078)	−0.338*** (0.116)	−0.351*** (0.128)	−0.329*** (0.117)	−0.308*** (0.075)	
IoR treaty signed		−0.255*** (0.075)			−0.233*** (0.075)	−0.243*** (0.077)
CRS * IoR		0.059 (0.136)				
EU STD					−0.178** (0.074)	
FATCA signed					−0.462*** (0.144)	
all AEI						−0.235*** (0.057)
countrypair f.e.	Yes	Yes	Yes	Yes	Yes	
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes	
Observations	33,420	33,420	7,260	26,160	33,420	33,420
R <sup>2</sup>	0.078	0.083	0.140	0.072	0.086	0.084
Adjusted R <sup>2</sup>	0.061	0.065	0.118	0.054	0.068	0.067

*Notes:* Autocorrelation and heteroscedasticity robust standard errors in parentheses. The treatment variables take value 1 after signature or activation. Column 1 shows the effect of CRS activation without controls, column 2 interacts this variable with IoR treaty signatures. Column 3 shows CRS effects in a sample reduced to such countrypairs that have signed IoR treaties during our sample time and column 4 shows the sample of countrypairs that have not. Column 5, shows results on all policy initiatives considered in this text and in column 6, all initiatives that include automatic information exchange (AEI) are lumped into one dummy variable that takes value one if either the EU savings tax directive, FATCA, or the CRS are signed or activated (CRS) respectively. The baseline sample consists of 557 countrypairs reported by 8 reporting tax havens against a combined 146 non-havens over 60 quarters (2003:I - 2017:IV) and is changed in columns 3 and 4 as pointed out above. \* denotes 10% significance, \*\* 5% significance, and \*\*\* 1% significance.

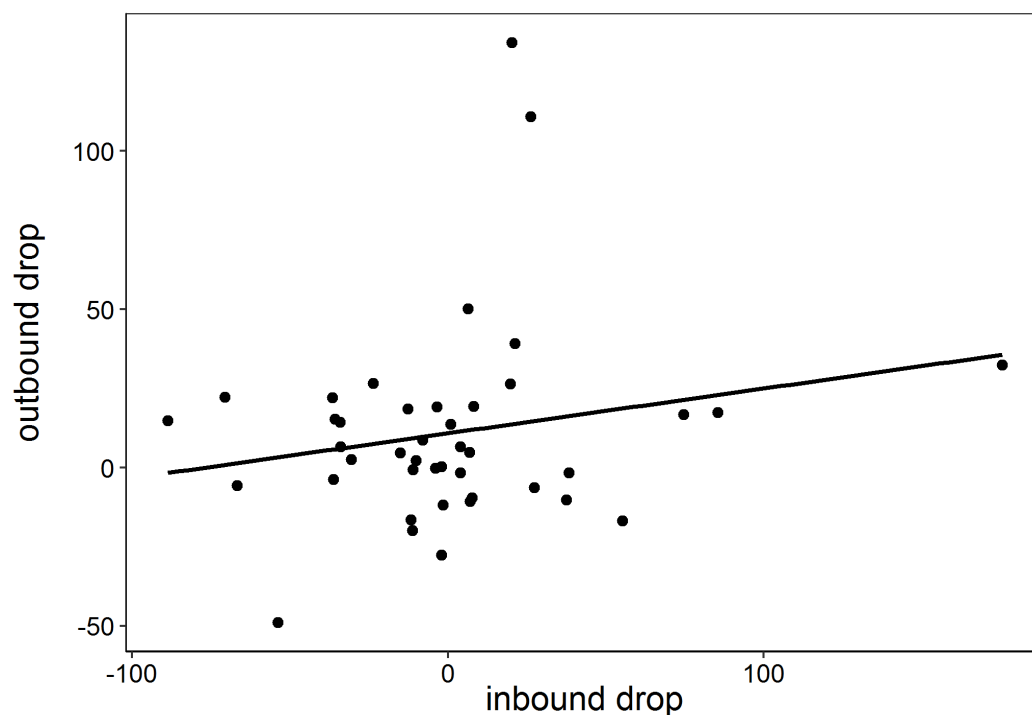
### **Appendix 3 Linkages between outbound and inbound deposits**

Having examined outbound and inbound deposits 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) demonstrate, for their small sample of 4 IoR treaties and ending in 2008, that there is a home bias in portfolio investment to the US, where funds typically move back to the same country where they started. While we cannot track specific capital positions, we can provide some arguments supporting the claim that there is also home bias in international tax evasion. The rational reason behind this may be that many tax evaders mainly live in one country where they can enjoy the benefits of their capital.

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 deposits, the changes should be lined up close to a 45-degree line in Figure A5, which has outbound changes on the x-axis and respective inbound changes on the y-axis. This expected relation is indeed evident in the data. Since this graphical representation again includes positions that have nothing to do with evasion, the fact that a positive correlation is visible is additionally striking.



Figure A5: Deposit drops in reaction to tax haven - non-haven IoR 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  $k + 1$  from those in  $k - 2$  with  $t$  being the quarter of signature. For the inbound drop, we deduct deposits in non-havens by haven counterparties in  $k + 6$  from those in  $k + 3$  as our baseline results suggest. We show both values as a ratio of mean deposits in the same window ( $k : k + 3$ ) starting with the signature date. The solid line is the line of best fit.