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// CHRISTOPH SPENGEL, ELISA CASI,
AND BARBARA STAGE

Cross-Border Tax Evasion after the Common Reporting Standard: Game Over?

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Cross-Border Tax Evasion After the Common Reporting Standard: Game Over?*

Elisa Casi

University of Mannheim

Christoph Spengel

University of Mannheim, ZEW Mannheim

Barbara M. B. Stage[1]

University of Mannheim, ZEW Mannheim

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Abstract

We study the short term effect of the first global multilateral standard for the automatic exchange of information (AEOI), the so called Common Reporting Standard (CRS), on cross border tax evasion. Our sample ranges from the fourth quarter of 2014 to the third quarter of 2017. Employing newly available bilateral data on cross-border deposits, we find that the CRS induced a reduction of 11.9% in cross-border deposits parked in tax havens. Moreover, regardless of the 4,000 bilateral exchange relations created under the CRS, relocation is still a desirable option. More specifically, upon the beginning of the automatic collection of information under the CRS, the United States, which so far did not commit to the CRS, seems to emerge as a potentially attractive country for the relocation of cross-border deposits

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[1]ZEW – Leibniz Centre for European Economic Research, L 7, 1, 68161 Mannheim, Email: stage@zew.de, Phone: +49 621 181-1701.

seeking secrecy.

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1. Introduction

In the last decades, capital mobility increased substantially thanks to globalization and rapid technological development. This provides individuals incentives to transfer their wealth and related income to countries offering very attractive tax systems together with a sound level of bank secrecy, i.e., the so-called tax havens. Recent estimates by Zucman (2013a) suggest that at least 8% of global household financial wealth is located in tax havens, translating into around 10% of the world GDP (Zucman, 2013a; Alstadsæter et al., 2018). While financial flows to tax havens may have legitimate motives, e.g., seeking business opportunities, they might also represent an important channel to hide wealth and related income to avoid tax obligations in the residence country. Although the exact size of the tax revenue loss is hard to quantify, it is generally agreed to be quite large. For example, according to a 2008 U.S. Senate staff report, at least USD 100 billion of tax revenue is lost every year due to "offshore tax abuses" (U.S. Senate Permanent Subcommittee on Investigations, 2008).

It is the general consensus at OECD level that cross-border tax evasion can be fought effectively by further increasing the information exchange between countries,[2] but empirically this remains an open question. This paper provides evidence on the effectiveness of the most powerful multilateral agreement on the Automatic Exchange of Information (AEOI) so far, the Common Reporting Standard (CRS).

Back in 2010, the United States was the first to strongly react to whistle blowing events and international data leaks, which had highlighted how pervasive cross-border tax evasion from its citizens was. This resulted in the implementation of the Foreign Account Tax Compliance Act (FATCA), a system forcing foreign financial institutions to collect and transfer financial account information on U.S. citizens to the IRS. OECD member states started being interested in requesting similar financial information on their residents. In this way, the introduction of FATCA pushed an international discussion at the OECD level on developing a global standard for the AEOI (Christensen III and Tirard, 2016). The debate culminated in early 2013 with a G20 formal request to the OECD to design a prototype for a universal system for the AEOI. On 21 July 2014, the OECD published the final

[2]For more details, see http://www.oecd.org/tax/transparency/about-the-global-forum/, accessed on 14.01.2020

version of the CRS (OECD, 2018). Thanks to its multilateral approach, broad scope, and extensive country coverage the CRS is substantially different from any initiative in the field of information exchange launched so far, including its role model FATCA. It could, thus, induce a revolution in the level of scrutiny on wealth and related income parked in tax havens and change the dynamics of cross-border tax evasion. We contribute to the literature by providing a detailed evaluation of the success of the CRS.

In the related literature, it is unanimously reported that the implementation of previous information exchange agreements, such as bilateral treaties, does not reduce tax evasion overall but instead induces a relocation of wealth from collaborative tax havens, i.e., those who signed such an agreement, to non-collaborative ones (Johannesen and Zucman, 2014; Hanlon et al., 2015; Caruana-Galizia and Caruana-Galizia, 2016; Omartian, 2017; De Simone et al., 2020). However, compared to earlier initiatives, the CRS achieves an impressive country coverage. At present, more than 100 countries worldwide have committed to the CRS.[3] In particular, the list of participating countries includes most of the so-called tax havens implying a substantial change in bank secrecy. Recent estimates by Deutsche Bank & Oliver Wyman (2017) suggest USD 1.1 trillion in outflows from offshore accounts by the end of 2017 as a reaction to the CRS.

In this study, we initially test whether the passing of the CRS law and the beginning of the automatic collection of information under the CRS, i.e. when the CRS becomes effective, both in the deposit country, induced a short term drop in cross-border deposits held through traditional tax havens. Upon the passing of the CRS laws in the deposit countries, we document a USD 46 billion decrease of cross-border deposits held in the tax havens in our sample and owned by residents of EU and OECD countries. Next to these out movements, we investigate relocation of cross-border deposits towards an unexpected location.

Anecdotal evidence suggests that the United States may be an attractive destination for placing wealth and related income owned by individuals seeking secrecy and tax evasion opportunities. This claim may seem surprising at first because it is not generally perceived as a tax haven. Nevertheless, the United States offers a high degree of bank secrecy (Cotorceanu, 2015) together with advantageous tax-free facilities for non-resident individuals (Brunson, 2014). Additionally, the United States is the only major financial center that remains not committed to the CRS. This represents a key advantage compared to traditional tax havens, which now automatically exchange information on foreign accounts under the CRS. Thus, we proceed by investigating whether after the CRS becomes effective, non-U.S. residents seeking secrecy and tax evasion opportunities relocate their deposits to the United

[3]For a complete list, see OECD (2020).

States.[4] We document a USD 60 billion increase of cross-border deposits held in United States and owned by residents of EU and OECD countries around the time when the CRS becomes effective in the majority of countries.

Following the related literature (Huizinga and Nicodème, 2004; Zucman, 2013a; Johannesen and Zucman, 2014; Menkhoff and Miethe, 2019; Alstadsæter et al., 2018), we consider the outstanding volume of cross-border deposits placed in tax havens as our measure of cross-border tax evasion.[5] The data we use originates from the Bank for International Settlements (BIS), which provides comprehensive disaggregated quarterly data on deposits held by individuals and entities that are not residents of the country where the reporting bank is located (i.e., cross-border deposits). We consider the period from the fourth quarter of 2014 to the third quarter of 2017. We supplement this dataset by hand collecting the exact dates when the CRS law is passed and when the CRS becomes effective in all countries in our sample.

We estimate tax evaders' reaction to this global initiative for the AEOI by using a difference-in-difference design. To test whether the CRS led to a decline in cross-border deposits held in tax havens, we compare the change in cross-border deposits held in tax havens (treated group) to the change in cross-border deposits held in non-tax havens (control group) after the passing of the CRS law and after the CRS becomes effective in the deposit country. By employing the country-level passing of the CRS law as exogenous shocks, our model absorbs all time-invariant factors that shift cross-border deposits across countries. Second, we test whether the relocation of cross-border deposits to the United States occurred, by estimating the change in cross-border deposits in the United States (treated group) as compared to the change in cross-border deposits in other non-tax havens (control group) after the CRS becomes effective. In both analyses, we control for between country-pair differences, by adding ordered country-pair fixed effects, and for (demand) shocks in the residence country, by adding residence-country quarter-year fixed effects. Thus, we investigate the CRS's effect on a within residence country-time and country-pair level.

We find that upon the passing of the CRS law in the deposit country, cross-border deposits held in tax havens decrease on average by 11.9% compared to non-tax havens. In event studies, we show that this is due to a statistically significant immediate decline of cross-border deposits held in

[4] We are aware that other relocation channels, which are not studied here, might be used by individuals for tax evasion or secrecy reasons in response to the CRS. For example, De Simone et al. (2020) attempt to measure whether in the context of FATCA investments in real estate and artwork might present attractive alternatives for cross-border tax evasion.

[5] Other related papers measuring cross-border tax evasion of U.S. citizens make use of a database of portfolio investments in the United States (Hanlon et al., 2015; De Simone et al., 2020).

tax havens in reaction to the CRS. If we exclude EU member states, which were already affected by the European Savings Directive,[6] the decrease is even higher, i.e., 27.9%. In our tests on relocation behavior, we find that after the CRS becomes effective cross-border deposits held in the United States are on average 10.9% higher, compared to those in other non-tax havens. What is more, in an event study we show that the increase of cross-border deposits in the United States after the CRS becomes effective is both immediate and persistent over the whole post-treatment period.

Our results are of great relevance to governments of CRS participating countries. Deposit holders still seem to deem reallocation a convenient option, but a new destination appears as very attractive for deposit holders, namely the United States.[7]

Our study highlights one critical aspect that could increase the ability to identify owners of foreign bank accounts, namely the U.S. participation in the CRS project. Nevertheless, we are aware that other aspects might hinder the CRS's effectiveness in tracking down ownership of wealth and related income located outside the residence country. For example, at present, the usability of the information collected under the CRS is far from certain (Finèr and Tokola, 2017) and the possibility to exploit the category "non-reportable financial institutions" represents a way to circumnavigate CRS reporting requirements (e.g., as in the case of the Occupational Retirement Scheme in Hong Kong). Still, the currently locally implemented CRS model is under revision by the OECD to address potentially existing loopholes.

The rest of the paper is organized as follows. In Section 2, we offer an overview of the related literature and we place the CRS in the context of previous related policies. In Section 3, we describe our research design. Section 4 contains the core of our paper, where we provide key results of our study in detail. Section 5 offers additional tests on the effect of the CRS on indirect channels of tax evasion. In Section 6, we summarize our findings.

2. Tax Evasion and the CRS as Countermeasure

2.1. Related Literature

Tax evasion represents a pervasive phenomenon. Estimates cited by the European Commission (2012) suggest an annual loss of around EUR 1 trillion due to tax evasion and avoidance within the EU alone, whereas the Internal Revenue Service (2016) provides estimates of an annual average tax revenue loss of USD 458 billion in the United States due to non-compliant tax behavior. While partially caused by unreported income held locally, a

[6]For more details see European Council (2003/48/EU).

[7]Next to relocation, another option for individuals is to repatriate their deposits after the passing of the CRS law. Due to a lack of high-quality data, however, we do not study directly to what extent repatriation occurred.

substantial portion is caused by unreported income held abroad. Zucman (2013a) estimates that around 8% of global household wealth is located in tax havens. More recently, Alstadsæter et al. (2018) show that this estimate varies significantly across the world. 60% of the wealth in tax havens is held by residents of the Gulf and certain Latin American countries, while only 15% by residents of continental Europe and even less by Scandinavians. Regardless of the geographical dispersion, the ownership of this hidden wealth strongly concentrates in the top 0.01% of the wealth distribution (Alstadsæter et al., 2019). Moreover, Hanlon et al. (2015) estimate a tax revenue loss of around USD 8 to 27 billion caused by U.S. investors' round-tripping activities.

As early as 1972, Allingham and Sandmo (1972) demonstrated that the individual level of evasion is a function of incentivizing and deterring factors, one deterrent being the probability of facing increased tax audits. Slemrod (2018) provides an overview of tax enforcement tools. The prevailing policy tool to increase the threat of detection in the context of cross-border tax evasion is the information exchange across countries (Dharmapala, 2016; Bott et al., 2019). Since more than a century, countries cooperate on tax matters using information exchange agreements. 1998 is one of the most crucial years on the route towards international tax transparency. In that year, the OECD issued its well-known report on harmful tax competition, which led a few years later to the development of a comprehensive model for tax information exchange agreements (TIEA) (Christensen III and Tirard, 2016). There is a vast empirical literature on the impact of early initiatives in the field of information exchange.

To begin with, Huizinga and Nicodème (2004) focus on the effect of bilateral tax information exchange agreements (TIEAs) among OECD member states from 1999 and find that the existence of exchange relationships across countries does not seem to diminish external liability flows. They attribute the result to the inefficiency of the TIEA network, in particular, the limited country coverage and the insufficient quality of the exchanged data. The network of TIEAs extended considerably between 2009 and 2011, when, thanks to international pressure, several tax havens signed agreements with non-tax havens (Bilicka and Fuest, 2014). Johannesen and Zucman (2014) consider this first wave of TIEA introductions and analyze its effectiveness in fighting cross-border tax evasion. They find that the introduction of TIEAs reduces the level of wealth and related income parked in tax havens, but they also document relocation behavior to non-collaborative tax havens. When considering the long-term impact of TIEAs, Menkhoff and Miethe (2019) find a diminishing effect starting from 2010.[8] Despite the detected relocation behaviour and diminishing effectiveness over time, Johannesen et al. (2020)

[8]This result is confirmed also by O'Reilly et al. (2019) and Beer et al. (2019).

provide evidence that enhanced information exchange had a positive effect on aggregated tax compliance. The authors consider the US enforcement actions beginning in 2008 and, using administrative micro data from the IRS, estimate that US taxpayers disclosed around USD 120 million held in foreign accounts as a result of the TIEAs with tax havens and the ad hoc legal measures to obtain U.S. customer information from Swiss banks. Still the authors highlight the limited scope of such enforcement initiatives and the necessity for stronger policy tools, such as a global CRS, to achieve an effective taxation of foreign accounts.

The first step towards a multilateral approach to exchange of information occurred in 2003 when the European Savings Directive was issued forcing the automatic exchange of information on private saving income among EU member states. Still, empirical evidence suggests that no overall reduction in cross-border tax evasion was achieved, instead tax evaders relocated their deposits to non-EU tax havens (Johannesen (2014); Caruana-Galizia and Caruana-Galizia (2016)). Further supportive evidence is provided by Omartian (2017), who by considering the leaked data from the Panama Papers, tests the impact of the amendment to the European Savings Directive in 2005[9] and FATCA on foreign asset ownership. De Simone et al. (2020) exclusively focus on FATCA, and building on the empirical analysis of Hanlon et al. (2015) offer strong evidence of reallocation behavior by U.S. citizens. They find that FATCA induced a significant reduction of equity foreign portfolio investments into the United States from tax havens as well as an increase in alternative investment options not subject to FATCA reporting, e.g., real estate and artwork. Finally, they also document additional negative externalities such as increased renunciation of U.S. citizenship. The strong effect of FATCA can be explained by the high degree of commitment to information sharing, both at the country and financial institution level, as documented by Belnap et al. (2019).

2.2. The CRS

Back in April 2013, the G20 endorsed the automatic exchange as the expected new global standard and in February 2014, the OECD published the final version of its Model for the AEOI, namely the CRS. In order to start exchanging information under the CRS, four distinctive steps need to be taken. First, participating countries express their commitment to introduce the CRS into national law by signing the Competent Authority Agreement (CAA). The CAA enables the introduction of a CRS system into national law by establishing its legal basis. Although a bilateral and a multilateral CAA model exists, so far all countries committed to CRS introduced it

[9]For more details on the amendment to the European Savings Directive, see European Council (2005/60/EU).

under a multilateral model (also called MCAA). The first countries, which committed to the CRS, signed the MCAA on 29 October 2014 during the 7th Global Forum. As of November 2019, 107 countries signed the MCAA but the list is constantly growing (OECD (2019)). Once the MCAA is signed, the national law introducing the CRS can be drafted and eventually published in the official gazette in each respective participating country. Table 1 offers an overview of the countries in our sample, the exact date when the law has been published in the official gazette and when it becomes effective in the country.

As visible in Table 1, the date of the law publication may differ from the date from which the CRS becomes effective. The date when the CRS becomes effective is stated in the national CRS law and from that date on, each foreign financial institution automatically collects and transmits the information requested under the CRS to the respective national authorities. All data collected under CRS requirements are exchanged across participating countries on September each year starting from 2017. First wave adopters are those countries that request the collection of financial information starting from January 1, 2016, and exchanged the financial information in 2017 for the first time. While second wave adopters are those countries that request the collection of financial information starting from early 2017 and exchange the financial information in 2018 for the first time.

Table 2 provides a comparison between TIEAs, FATCA and CRS with respect to the relevant time period, the tax haven coverage, the type of information exchanged and the related empirical literature. Overall, the CRS overcomes significant drawbacks of these previous initiatives in the field of information exchange.[10] First, it constitutes a multilateral approach similar to the European Savings Directive, but different from bilateral approaches such as FATCA and classical TIEAs. This is because the CRS eliminates the requirement to negotiate single treaties on a country-by-country basis. To date 107 countries around the world signed the multilateral agreement, meaning they commit to the exchange of information under the CRS requirements in the near future OECD (2019). Second, under the CRS participating countries agree to request local financial institutions to collect information on accounts held by non-resident reportable persons. Thus, while FATCA is mainly about receiving information, the CRS is mainly about providing information. Third, the collected information is automatically exchanged with any other participating counterparty. In this way, in contrast to normal TIEAs and FATCA (for information on foreign deposits in the United States), the information is no longer exchanged only upon request. Fourth, the CRS not only has a larger country coverage than any previous initiative

[10]For a comprehensive overview of the CRS laws at the national level, see Casi et al. (2019).

Table 1: CRS Introduction and Effective Date at National Level – Exact Date

Country	CRS Introduction Month	CRS Effectiveness Month
Gibraltar	29 October 2014	01 January 2016
UK	15 April 2015	01 January 2016
Lithuania	25 June 2015	01 January 2016
Austria	14 August 2015	01 October 2016
Slovenia	28 August 2015	01 January 2016
Cayman Islands	16 October 2015	01 January 2016
Isle of Man	23 October 2015	01 January 2016
Romania	27 October 2015	01 January 2016
Spain	17 November 2015	01 January 2016
Guernsey	01 December 2015	01 January 2016
Jersey	01 December 2015	01 January 2016
Bulgaria	04 December 2015	01 January 2016
Malta	04 December 2015	01 January 2016
Sweden	10 December 2015	01 January 2016
Hungary	11 December 2015	01 January 2016
Slovakia	15 December 2015	01 January 2016
Belgium	16 December 2015	01 January 2016
Norway	18 December 2015	01 January 2016
Iceland	19 December 2015	01 January 2016
Germany	21 December 2015	01 January 2016
Latvia	23 December 2015	01 January 2016
Luxembourg	24 December 2015	01 January 2016
France	28 December 2015	01 January 2016
Italy	28 December 2015	01 January 2016
Netherlands	28 December 2015	01 January 2016
Cyprus	30 December 2015	01 January 2016
Denmark	30 December 2015	01 January 2016
Ireland	31 December 2015	01 January 2016
Mexico	12 January 2016	01 January 2016
New Zealand	12 February 2016	01 July 2017
South Africa	02 March 2016	01 January 2016
Australia	18 March 2016	01 July 2017
Portugal	30 March 2016	01 January 2016
Czech Republic	06 April 2016	01 January 2016
Finland	08 April 2016	01 January 2016
Greece	14 April 2016	01 January 2016
Croatia	19 May 2016	01 January 2016
Hong Kong	29 June 2016	01 January 2017
Japan	29 July 2016	01 January 2017
Estonia	02 August 2016	01 January 2016
Mauritious	01 September 2016	01 January 2017
Lebanon	27 October 2016	30 June 2017
Panama	27 October 2016	30 June 2017
Singapore	02 December 2016	01 January 2017
Canada	15 December 2016	01 July 2017
Rep. of Korea	15 December 2016	01 January 2016
Switzerland	18 December 2016	01 January 2017
Brazil	29 December 2016	01 January 2017
Bahamas	29 December 2016	01 January 2017
Samao	31 December 2016	01 January 2017
Poland	20 March 2017	01 January 2016
Bermuda	12 April 2017	01 January 2016
Bahrain	30 April 2017	30 June 2017
Turkey	20 May 2017	01 July 2017
Barbados	22 May 2017	30 June 2017
Curacao	27 June 2017	01 January 2017
Chile	21 July 2017	01 July 2017
Aruba	19 December 2017	01 January 2017
Chinese Taipei	11 November 2018	01 January 2019
Israel	01 January 2019	01 January 2018
United States	Not Committed	Not Committed
Macao	Committed - Not yet Introduce	Committed - Not yet introduced
Philippines	Committed - Not yet Introduce	Committed - Not yet introduced

Notes: The table provides an overview of the countries in our sample. It displays both the exact date when the CRS law has been published in the Official Gazette (CRS Introduction Month) and the exact date from which domestic financial institutions begin to automatically collect information on foreign accounts (CRS Effectiveness Month).

but also a broader scope.[11] Reportable financial institutions are forced to provide detailed information on financial assets held by non-resident taxpayers, which is not limited to interest income and covers deposits held by individuals as well as entities. This is why we expect to find a significant effect of the CRS, even for those tax havens, which already implemented bilateral TIEAs, the European Savings Directive, and FATCA. Consequently, our first test focuses on the CRS's effectiveness in reducing wealth and related income held in traditional tax havens.

In the second part of the analysis, we test to what extent and to which countries deposits are shifted to, given that those traditionally considered as tax havens now automatically exchange financial account information. The United States is the only important financial center around the world, which did not commit to the CRS and does not plan to do so any time soon (Goulder, 2019, p. 139).[12] This is unsurprising, since under FATCA the United States already receive the information that could be obtained from the CRS. However, information transmitted on foreign residents owning deposits in the United States is limited because FATCA has been passed with the key intention to collect information on U.S. citizens owning deposits abroad. The IRS transmits data on foreign financial account holders only upon request and only if such request comes from countries, which signed the FATCA Model 1a Intergovernmental Agreement (IGA). The transmitted information is further limited to the gross interest paid for depository accounts, only if held by an individual, and U.S. source interest and dividends for custodial accounts, only if the accounts are already subject to reporting and only for individuals and entities in partner countries. No information on the last beneficial owners of passive non-financial entities (NFEs) is collected and transmitted to IGA partners (Cotorceanu, 2015, p. 1053). Country evidence even suggests that the U.S. duty to exchange information based on FATCA agreements is not fully respected.[13]

The United States was already before the introduction of the CRS an attractive location for wealthy individuals seeking secrecy and tax evasion opportunities.[14] Non-resident individuals investing in the United States enjoy advantageous tax-free facilities. This includes tax exemption on domestic-

[11] The CRS has a similar scope as FATCA when considering the information the United States receives on its citizens having bank accounts outside national borders. However, under FATCA, the United States provides to counterparties only information on the gross interest paid for foreign depository accounts.

[12] Other than the United States, non-CRS-abiding countries generally cannot provide an attractive and stable financial sector and are not OECD or EU member states. Countries not committed to CRS so far include Algeria, Armenia, Bangladesh, Egypt, Maldives, Oman, Palestine, Philippines, Sri Lanka, Thailand, the United States and Vietnam. See http://www.crs.hsbc.com/, accessed on 14.01.2020.

[13] For more details, see Sueddeutsche Zeitung (2018).

[14] For more details, see Financial Times (2016).

source portfolio interest or reinvested dividends (Brunson, 2014). Further, the United States provides high levels of bank secrecy. Currently, no U.S. state or federal law obliges legal entities to maintain beneficial ownership information or even requests legal entities to disclose the beneficial owners' identity when they are established.[15] Last, on the grounds of an extensive cross-country randomized field experiment, Sharman (2010) and Findley et al. (2015) find that in contrast to non-U.S. providers, U.S. service providers for shell company incorporation are less likely to comply with international transparency standards. In this way the complexity of setting up a shell company in the United States is reduced (Findley et al., 2015, p. 153, 157).[16] Thus, while already offering low tax rates for foreign residents and high bank secrecy, as traditional tax heavens do, in the post CRS world, the United States may become even more attractive than tax havens by refraining from the AEOI on tax matters.[17]

[15]In May 2016, under the Bank Secrecy Act, the Treasury's Financial Crimes Enforcement Network issued a new customer due diligence requirement imposing on certain domestic financial institutions the collection of a beneficial ownership information form for their respective clients' corporations and trusts. But the law has not yet been enacted. Even in case of execution, it has been labeled as fully ineffective because among others it allows senior managers of the company to be identified as beneficial owners (see Tax Justice Network (2018)).

[16]"Only 62 of the answers to the 2,336 inquiries in the United States asked for any document with a photo establishing identity" (Findley et al., 2015, p. 157).

[17]For more details, see The Economist (2016) or Bloomberg (2017).

Table 2: Comparison between TIEAS, FATCA and CRS

Agreement	Key Dates	Tax Havens	Information	Literature
TIEAs	• 19 May 1998: OECD Report 'Harmful Tax Competition: An Emerging Global Issue' was published • April 2002: the OECD launched the Model Agreement on Exchange of Information in Tax Matters (Model TIEA) • From early 2000 up to now: 887 TIEAs are signed • Between 2008 and 2013: 744 have been signed	Andorra, Anguilla, Antigua and Barbuda, Aruba, Bahamas, Bahrain, Barbados, Belize, Bermuda, British Virgin Islands, Cayman Islands, Cook Islands, Cyprus, Dominica, Gibraltar, Grenada, Guernsey, Hong Kong, Isle of Man, Jersey, Liberia, Liechtenstein, Luxembourg, Maldives, Malta, Marshall Islands, Mauritius, Monaco, Montserrat, Nauru, Netherlands Antilles, Niue, Panama, Saint Lucia, Saint Kitts and Nevis, Samoa, Saint Vincent and the Grenadines, San Marino, Seychelles, Singapore, Turks and Caicos Islands, Switzerland, Vanuatu	• Information is exchanged either upon request or spontaneously • The applicant country must send the identification information on the taxpayer and the tax purpose for which the information is required. Tax includes taxes on income or profits, taxes on capital, taxes on net wealth, and estate, inheritance or gift taxes (potentially also other taxes) • Information the requested country must provide includes the one held by financial institutions and in case of indirect ownership, information on all persons in the ownership chain should be provided	• Huizinga and Nicodeme (2004) • Bilicka and Fuest (2014) • Johannesen and Zucman (2014) • Menkhoff and Miethe (2019) • O'Reilly et al. (2019) • Beer et al. (2019)
FATCA	• In 2007: the IRS issued its report entitled "Reducing the Federal Tax Gap • 18 March 2010: FATCA provisions were passed • 8 February 2012: the U.S. Treasury issued exhaustive reporting guidelines on FATCA • 1 January 2013: the final FATCA legislation is issued • From June 2013 on: FATCA Intergovernmental Agreements (IGAs) become effective	Anguilla, Antigua and Barbuda, Bahamas, Bahrain, Barbados, Bermuda, British Virgin Islands, Cayman Islands, Cyprus, Dominica, Gibraltar, Grenada, Guernsey, Hong Kong, Isle of Man, Jersey, Liechtenstein, Luxembourg, Malta, Mauritius, Montserrat, Panama, Saint Lucia, Saint Kitts and Nevis, Saint Vincent and the Grenadines, San Marino, Seychelles, Singapore, Turks and Caicos Islands, Switzerland	Automatically exchanged information on accounts from US citizens abroad: • Identification information of the account holder, if indirectly owned, on the last beneficial owner • Financial information on the account, including the balance, the interest and/or dividend amount, the amount of other income generated with respect to the assets held in the account, the proceeds from the sale or redemption of financial assets, the amount paid or credited by the reporting financial institution in reference to the account Information exchanged upon request on accounts from foreign citizens in the U.S.: • Identification information of the account holder, if indirectly owned, on the last beneficial owner • Financial information on the account, including the interest and/or dividend amount, the amount of other income generated with respect to the assets held in the account	• Dharmapala (2016) • Belnap et al. (2019) • De Simone et al. (2020) • Johannesen et al. (2018) • Menkhoff and Miethe (2019) • O'Reilly et al. (2019) • Beer et al. (2019)
CRS	• 9 April 2013: G20 endorse the automatic exchange as the expected new global standard • 13 February 2014: the OECD published the CRS • From October 2014 on: participating jurisdictions started signing the MCAA • From 2015 on: participating jurisdictions started implementing the CRS into national law • From January 2016 on: financial institutions started collecting information under CRS • On September 2017: Information under CRS are exchanged for the first time	Andorra, Anguilla, Antigua and Barbuda, Aruba, Bahamas, Bahrain, Barbados, Belize, Bermuda, British Virgin Islands, Cayman Islands, Cook Islands, Cyprus, Dominica, Gibraltar, Grenada, Guernsey, Hong Kong, Isle of Man, Jersey, Liberia, Liechtenstein, Luxembourg, Malta, Marshall Islands, Mauritius, Monaco, Montserrat, Nauru, Niue, Panama, Saint Lucia, Saint Kitts and Nevis, Samoa, Saint Vincent and the Grenadines, San Marino, Seychelles, Singapore, Turks and Caicos Islands, Switzerland, Vanuatu	Automatically exchanged information: • Identification information of the account holder, if indirectly owned, on the last beneficial owner • Financial information on the account, including the balance, the interest and/or dividend amount, the amount of other income generated with respect to the assets held in the account, the proceeds from the sale or redemption of financial assets, the amount paid or credited by the reporting financial institution in reference to the account	• Menkhoff and Miethe (2019) • O'Reilly et al. (2019) • Beer et al. (2019)

Notes: The table provides an overview of key features of TIEAS, FACTA and CRS. In particular, information on key events, collaborative tax havens, scope and the related literature for each information exchange initiative.

3. Research Design

3.1. Data

Our main dataset is constructed based on the BIS Locational Banking Statistics (LBS). This database offers detailed information about the outstanding volume of claims and liabilities of internationally active banks located in reporting countries vis-a-vis counterparties residing in more than 200 countries around the world. For our analysis, we focus on the outstanding quarterly volume of cross-border deposits (in the following referred to as cross-border deposits). The data enables us, for example, to observe the total amount of deposits German residents owned in active banks located in Hong Kong. The main advantage of the BIS data is the extensive country coverage. The coverage rate on cross-border interbank business is around 93% as of 2016.[18] Additionally, the BIS data features sectoral decomposition into bank and non-bank sector.[19] We consider only non-bank deposits. As also highlighted in Johannesen and Zucman (2014), interbank deposits should not represent a channel for tax evasion.

The limitations of the data are as follows. First, we can only observe the immediate owner and not the final beneficiary of a deposit. Given the well-established evidence of the use of shell companies,[20] we address the role of shell companies in additional tests in Section 5. Second, the BIS statistics do not distinguish between individual and entity ownership of deposits. However, we do not see this as a limitation to our analysis. The CRS requires financial institutions to collect information on both, individual and entity accounts. In the case of the latter, financial institutions are required to conduct an accurate investigation regarding the final individual owner of the financial account. This means that upon the passing of the CRS laws, we expect a reaction from both if entity owned accounts are used for tax evasion purposes. Lastly, since the BIS statistics include only bank deposits, alternative channels for tax evasion, namely equity or bond portfolios and real estate, are excluded from our analysis. Especially, real estate may represent an important non-financial asset class for tax evaders because indirect investment in real estate is not reportable under CRS.[21] Yet, as suggested by Johannesen and Zucman (2014, p. 72), bank deposits can be considered

[18] For an overview of the BIS data, see BIS (2018).

[19] To be more precise, the BIS data at bilateral level are only available for all sectors aggregated together, which include bank, non-bank and unallocated, and for the non-bank sector.

[20] Johannesen and Zucman (2014, p. 85) state that the owners of 25% of all deposits in tax havens are recorded as residents of other havens.

[21] "An Entity the gross income of which is primarily attributable to investing, reinvesting, or trading real property" is not reportable under CRS (see OECD (2019)). This is a similar condition to FATCA and De Simone at al. (2019) provide evidence of an increase in investment in residential real estate upon the introduction of FATCA.

a sound proxy for testing the reaction to a shock in the scrutiny on wealth in tax havens.[22]

In our empirical analysis, as deposit country, we include all countries for which data at bilateral level is publicly available in the BIS LBS dataset. We divide them into tax havens (Guernsey, Hong Kong, the Isle of Man, Jersey, Luxembourg and Switzerland) and non-tax havens (Australia, Austria, Belgium, Brazil, Canada, Chile, Denmark, Finland, France, Greece, Ireland, Italy, Korea, Macau, Mexico, Netherlands, Philippines, South Africa, Spain, Sweden, Taiwan, the United Kingdom and the United States). As tax havens, we take those available from the list of Johannesen et al. (2020). They also study individual tax evasion, and they define tax havens as the OECD (2000) list of uncooperative tax havens plus Switzerland, Singapore, Hong Kong, and Luxembourg.[23] As for the location of the owner of the deposits, we select all EU and OECD member states arriving at a total of 41 countries.[24]

The BIS LBS offers data on more than 200 resident countries, but we restrict our sample of resident countries to EU and OECD member states. This choice overall ensures a higher comparability between the countries in the control group and rules out confounding factors that might lead to failure of our identifying assumptions in the difference in difference analysis. First, with few exceptions, EU and OECD member states have a high degree of political stability. We believe that this is an important factor to be considered in our analysis because movements in cross-border deposits might be driven for example by the occurrence of a war rather than the CRS, while we can expect politically stable locations to have more similar trends in cross-border deposits. Furthermore, EU and OECD member states face similar fiscal rules and regulations on fighting tax evasion.[25] Therefore, secrecy seekers from EU and OECD countries face similar incentives when it comes to holding wealth and related income outside their country of residence. Despite limiting our sample, we still make use of 84% of the total available data at BIS LBS, yet, we minimize the risk that other factors be-

[22]Heckemeyer and Hemmerich (2018) show that the reaction to increased information exchange on portfolio wealth held through tax havens mirrors the reaction on cross-border deposits held in tax havens that is observed by Johannesen and Zucman (2014). This suggests that our estimates on the effect of the CRS on cross-border deposits may similarly apply to other cross-border channels for tax evasion.

[23]Others, such as Hines and Rice (1994) and Johannesen and Zucman (2014) consider as tax havens a different selection of countries. We select the most recent one that relates to individual tax evasion, i.e., the one of Johannesen et al. (2020).

[24]We consider EU and OECD member states as of June 2018.

[25]For example, OECD member states have a sound network of TIEAs all based on the Art. 26 of the OECD Model Tax Convention, they all signed the Convention on Mutual Administrative Assistance in Tax Matters while EU member states have implemented the six directives on administrative cooperation.

sides the passing of the CRS laws drive movements in cross-border deposits in our sample period.

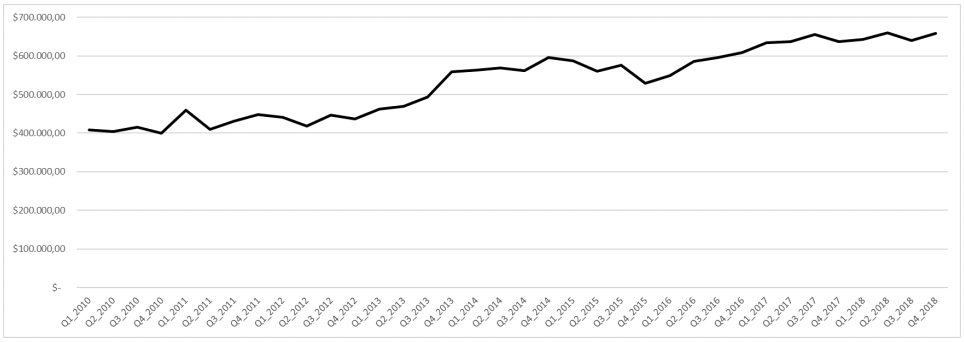
In Table 3 we provide descriptive statistics on cross-border deposits held by EU and OECD residents in the deposit countries considered in our sample. The period covered is from the last quarter of 2014 until the third quarter of 2017. The United Kingdom has the largest average volume of cross-border deposits (USD 28,241 Million), followed by the United States (USD 14,858 Million), and France (USD 11,186 Million). Despite the small size of the country, the Netherlands follow with USD 7,265 Million in cross-border deposits owned by EU and OECD residents. This may be related to the importance of the Netherlands as conduit country for financial flows of multinationals investing worldwide (European Parliamentary Research Service, October 2018). Among our group of tax havens, Switzerland, Luxembourg, and Hong Kong have the largest average volume of cross-border deposits with USD 3,920 Million, USD 2,424 Million, and USD 1,347 Million, respectively. The small islands of Jersey, the Isle of Man, and Guernsey still represent important countries for cross-border deposits with average values of USD 802 Million, USD 437 Million, and USD 411 Million, respectively. This may be because most of the cross-border deposits considered in our sample are owned by residents of EU member states, countries that are mostly in geographical proximity to these Islands.

Figure 1 shows the long-term series of cross-border deposits held in banks located in the United States by residents of EU and OECD member states. As visible in Figure 1, cross-border deposits held in U.S. banks start increasing in the second quarter of 2012 and continue to rise until the end of 2013. This trend may be related to the introduction of FATCA. The Congress passed FATCA in March 2010 but prior to 2012 it was not clear whether the law would be effective because of the missing commitment to cooperate by foreign countries and the limited guidance provided by the U.S. government. Only on February 8 2012, a final FATCA guidance was issued and a joint statement with five EU member states agreeing to automatically exchange information under FATCA was provided. After these events, De Simone et al. (2019) detect an increase in investment from U.S. citizens into the United States via EU member states. Figure 1, shows particularly strong increases in cross-border deposits in 2013. The year of the issuance of the final FATCA legislation and in which the bilateral FATCA agreements started to become effective. Finally, a steady increase in cross-border deposits held in U.S. banks begins after the last quarter of 2015, which coincides with the passing of the CRS laws at national level for the majority of the CRS participating countries. The trend becomes flat again after the third quarter of 2017. Cross-border deposits held in the United States do not seem to have changed substantially after the U.S. Tax Reform in 2018.

Table 3: Descriptive Statistics on Deposit Countries

Deposit Country	Observations	Mean (M $)	Stand. Deviation (M $)	Min (M $)	Max (M $)
United Kingdom	480	28,241	85,081	12.0	611,654
United States	480	14,858	52,946	6.0	373,090
France	480	11,186	26,315	4.0	168,198
Netherlands	300	7,265	12,804	8.0	67,782
Switzerland	480	3,920	5,787	23.0	27,310
Canada	433	3,348	16,685	0.8	131,631
Belgium	480	2,572	6,836	3.6	43,511
Luxembourg	480	2,424	5,266	14.0	31,037
Spain	480	1,630	3,695	1.0	25,168
Hong Kong	492	1,347	2,809	3.0	17,371
Ireland	480	1,321	3,593	0.0	20,661
Italy	437	1,230	3,364	1.4	21,162
Austria	478	949	3,054	2.2	25,179
Jersey	492	802	3,391	1.0	23,951
Finland	407	748	2,553	0.1	19,349
Sweden	468	733	1,543	0.6	16,022
Denmark	449	683	1,481	7.0	9,223
Australia	480	500	1,244	0.3	7,223
Isle of Man	492	437	1,838	0.5	13,416
Guernsey	486	411	1,370	0.0	8,366
Mexico	87	243	746	0.0	4,379
Taiwan	492	185	489	0.0	3,755
Macau	412	141	509	0.0	4,516
Korea	447	126	300	0.0	1,591
South Africa	384	95	329	1.0	2,448
Brazil	179	81	317	1.0	2,781
Greece	104	43	78	1.0	353
Philippines	163	30	100	0.0	671
Chile	367	13	74	0.0	1,281

Notes: The table depicts the quarterly cross-border deposits volume held by EU and OECD residents in the deposits countries considered in our sample from the fourth quarter of 2014 until the third quarter of 2017. The data are in millions of US dollars. The data originates from Table A6.2 available at the BIS LBS. An observation stands for the deposit value by country-pair and by quarter-year.



Notes: The figure shows the outstanding volume of cross-border deposits held in banks located in the United States by residents of EU and OECD member states. The figures are displayed in millions of US dollars for the time period from the first quarter of 2010 until the last quarter of 2018. The data originates from Table A6.2 available at the BIS LBS.

Figure 1: Cross-Border Deposits in the United States

Our sample period ranges from the last quarter of 2014[26] to the third

[26]We start from the last quarter of 2014 because data for Hong Kong are available only

quarter of 2017. Limiting our sample period to the fourth quarter of 2014 until the third quarter of 2017 enables us to exclude possible confounding impacts of the big wave of bilateral TIEA signatures in 2008-2011, the introduction of FATCA in 2010-2013 as well as the 2018 U.S. Tax Cuts and Job Act announced in Fall 2017.[27] For example by including observations before 2014, our results on the CRS effect on cross-border deposits from tax havens to the United States could be upward biased, due to the 2010 FATCA implementation. Similarly, if we include observations after 2017, our results on the CRS effect on cross-border deposits from non-tax havens and from tax havens to the United States could be upward biased, due to the economic effects of the 2018 U.S. Tax Cuts and Job Act.[28]

We manually collect information on both the exact date when the CRS law is passed at national level and the exact beginning of the automatic collection of information under the CRS by directly considering national laws. The OECD provides on its website the link to each CRS national law for each participating country.[29] When the information is not available through the OECD database, we search it using news alerts from the Customer and Investor Tax Transparency (CITT) News Blog by PwC.[30]

3.2. Empirical Strategy

In each subsection of this chapter we first describe our research design followed by a discussion and defence of the identifying assumptions.

3.2.1. Measuring reduction of cross-border deposits in tax havens

We first test whether cross-border deposits held directly in tax havens are reduced due to the CRS. We use two different empirical models, beginning with event studies, followed by a difference in difference analysis. The event studies are used to evaluate the common trends assumption in the pre-treatment period and to assess the dynamics of the response to the CRS so as to gain a more comprehensive picture of how the CRS affects tax evasion through the use of cross-border deposits. For identification, we exploit that

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from that date on.

[27]Possibly confounding events during the selected period are the implementation of Basel III between 2013 and 2015 and of the fourth EU Directive on prevention of the use of the financial system for the purposes of money laundering or terrorist financing issued in May 2015 (European Parliament and Council, 2015/849/EU). However, those reforms are not directly influencing the movement of cross-border deposits for tax evasion.

[28]The BIS data includes, both individual and entity cross-border deposits. Entity cross-border deposits in the United States may increase in response to the tax changes induced by the Tax Cuts and Job Act 2018. Heinemann et al. (2018) findings point to an increase in total FDI activity as a result of the lowered tax burden in the United States.

[29]See OECD "Automatic Exchange Portal – CRS by Jurisdiction", available at http://www.oecd.org/tax/automatic-exchange/crs-implementation-and-assistance/crs-by-jurisdiction/, accessed on 14.01.2020.

[30]For more details, see https://blogs.pwc.de/citt/, accessed on 14.01.2020.

while all countries in our sample are committed to the CRS, we only expect a reaction to the CRS in cross-border deposits in tax havens and not in non-tax havens. As elaborated in Section 2, we expect that cross-border deposits if held for tax evasion purposes in tax havens are on average reduced after the passing of the CRS laws in the deposit countries. Furthermore, we assume that the reaction by deposit holders to the CRS occurs after the CRS law is passed in the tax haven where the deposit is located, rather than in the country of their residence, because deposits held in tax havens are not immediately affected if only the residence country introduces the CRS. We test this assumption in an additional regression, reported in Section 4.3.3. We do not expect any significant reaction to the CRS in our control group, because changes of cross-border deposits in non-tax havens should mainly be driven by economic activity, which we reasonably expect to be unaffected by the CRS.

We begin our analysis with an event study design of the form:

$$log(Deposits_{ijt}) = \sum_{k=-4}^{4} \alpha_k D_{jt}^k * Havens_j + \gamma_{it} + \theta_{ij} + \epsilon_{ijt} \qquad (1)$$

The variables of interest are the dummies D_{jt}^k indicating a point in time k periods from the CRS treatment and interacted with $Havens_j$, which is a dummy taking value one when the deposit country is a tax haven. Here the CRS treatment is the passing of the CRS law in country j at time t. We measure the effect on the (log) volume of cross-border deposits ($log(Deposits_{ijt})$) between residence country i and deposit country j at the end of quarter t. As is the standard in the literature for event studies, we omit the indicator for period t-1. It, therefore, serves as a benchmark. We bin the treatment indicators at the endpoints.[31] Further, we include residence-country quarter-year fixed effects γ_{it} as well as ordered country-pair fixed effects θ_{ij}. The residence-country quarter-year fixed effects allow us to further control for common time trends affecting cross-border deposits such as globalization of financial markets and economic shocks, but also residence country-specific demand-side shocks. The ordered country-pair fixed effects allow us to control for all time-invariant country-pair factors such as distance or common language, which might affect the change in cross-border deposits as a reaction to the CRS. Overall, we employ the most comprehensive fixed effects structure that our data allows.[32] Our standard errors are cluster-robust,

[31] Binning implies here that the indicator t-4 stands for treatment at time t-4 or more periods ago and the indicator t+4 stands for time t+4 or more periods in the future. In general, we design our event studies based on Schmidheiny and Siegloch (2019) and Fuest et al. (2018).

[32] In Appendix D.1, we show that the only fixed effects that are actually needed for identification are country-pair fixed effects. All other fixed effects are just demonstrations of robustness of our findings.

with clustering at the deposit country level. The error term is denoted by ϵ_{ijt}.

While we use the event study mainly to establish that the common trends assumption holds in the pre-treatment period, we use the difference in difference design to estimate the average effect of the CRS on cross-border deposits held in tax havens. In the difference in difference analysis, we run regressions of the form:

$$\begin{aligned} log(Deposits_{ijt}) = \alpha &+ \beta_1 PostCRSIntroDepL_{jt} \\ &+ \beta_2 PostCRS_{jt} * Havens_j \\ &+ \gamma_{it} + \theta_{ij} + \epsilon_{ijt} \end{aligned} \tag{2}$$

Where the dependent variable and the definition of the treatment dummy $Havens_j$ is unchanged.[33] $PostCRS_{jt}$ is the post-period dummy. It switches to one after the CRS law is passed in the deposits country and stays switched on until the end of the sample period. As in the event study design, we include residence-country quarter-year fixed effects γ_{it} as well as ordered country-pair fixed effects θ_{ij} and standard errors are clustered at the deposit country level. If wealth and related income are moved away from tax havens after the CRS law is passed, the coefficient β_1 should be negative.

We first use the date when the CRS law is published in the official gazette as post-period for our baseline. As already highlighted, the CRS laws are not passed everywhere at the same time. In fact, there is variation in the dates across deposit countries, which we can exploit for identification. As a first alternative specification, we test deposit country-specific CRS effective dates. All variables and specifications of the fixed effects remain the same, except for the treatment dummy $PostCRS_t$, which in this first alternative is a dummy equal to one when the CRS becomes effective at the deposit country and zero otherwise. As a second alternative specification, we use a post-period dummy that is constant across all observations and not deposit country-specific. The treatment dummy $PostCRS_t$, in this third specification is a dummy equal to one starting on January 1, 2016 - the time when financial institutions of the first wave adopters started collecting information for CRS purposes and zero otherwise.

The two post periods used in our test are the passing of the CRS law and the beginning of the automatic collection of information under the CRS, i.e. when the CRS becomes effective, both, in the deposit country. Our event date selection is based on the following two reasons. First, we follow the previous literature on multilateral agreements. Johannesen (2014) considers as post-treatment period the moment the Savings Directive became effective,

[33]Since the treatment dummy is perfectly multicollinear with our country-pair fixed effects, we do not include it as non-interacted term.

i.e. the years from 2005 on. Similarly, for most of the tests in our study, we consider as post-CRS period the date when the information under CRS started to be automatically collected in most of the countries in our sample, namely January 1 2016, or the exact effectiveness date in each country in our sample. Secondly, to capture anticipation to CRS effectiveness and as alternative robustness check, we use the passing of the CRS law in the deposit country, since this event gave tax evaders and secrecy seekers the certainty that information on their deposits abroad will be collected soon.[34] We do not expect anticipation to the CRS passing into law, since deposits are highly mobile assets. Therefore, we expect individuals to set up alternative ways of avoiding collection of information on their wealth and related income, and then, on short notice, move the deposits once the CRS law is passed. This argument is corroborated by our findings in the event studies reported in Figure 3 and Figure 4, which show no pre-trends.

In the remainder of this section, we discuss our identifying assumptions in detail, how we test for their validity and name examples of when they could fail. The identifying variation results from differences in the timing of the passing of the CRS law across deposit countries and differences in the relevance of the CRS for tax havens and non-tax havens. The identifying assumption we rely upon is that, absent the CRS, cross-border deposits of EU and OECD residents in tax havens and non-tax havens would have trended in parallel within our sample period, and especially that there are no local shocks affecting cross-border deposits concurrent to the CRS. We show in event studies that in the pre-treatment periods both control and treated trend in parallel (Section 4.1). Given parallel pre-trends, our research design would still be invalid if local shocks systematically affected cross-border deposits around treatment. To address the issue of potential time series shocks from the side of the residence country of the deposit holder, we include residence-country x quarter-year fixed effects in our regressions. Thus, we can rule out in a non-parametric fashion that residence country-time shocks are biasing our results. In Appendix D.1, we estimate a simpler model excluding residence-country x quarter-year fixed effects. If confounding residence-country shocks were significant, our estimates should vary for this specification, which is not the case. Conversely, we cannot

[34] We do not use as post-CRS period the signature of the MCAA, which occurred for most countries in our sample on October 29, 2014, and represents the announcement that the country will join the CRS. Our decision is driven by the fact that several years can pass between the signature of the MCAA and the passing of the CRS law. For example, Israel singed the MCAA on May 2016 and passed the CRS law on February 2019. For more information on Israel signing the MCAA, see https://blogs.pwc.de/citt/2016/05/20/israel-russia-sign-mcaa-adopt-crs/ and on Israel passing the CRS law, accessed on 14.01.2020, see https://blogs.pwc.de/citt/2019/02/22/israel-publishes-crs-regulation-along-with-a-list-of-participating-and-reportable-jurisdictions/, accessed on 14.01.2020.

include deposit-country x quarter-year fixed effects to control for shocks at the deposit country level, since our identifying variation stems from deposit-country quarter-year changes.

Therefore, the main threat for identification in this staggered design lies in shocks at the level of treatment (deposit country) that affect the CRS treatment dates and cross-border deposits. We can directly test for violations of the identifying assumptions stemming from deposit country economic shocks by using deposit country level economic outcomes as dependent variables in our event study design. In Appendix B.3, we report the results of our event studies using GDP as left-hand-side variable. Examining the pre-treatment period, we find no significant pre-trends. This is in line with the assumption that the passing of the CRS law at national level is driven by political factors rather than economic cycles.

In Appendix B, we report further tests to alleviate restrictions of the identification posed by deposit country level changes. The staggered introduction of the CRS laws in the deposit countries reduces concerns about concurrent shocks, however many of the deposit countries in our sample adopt the CRS in the first adoption wave, i.e. in the last quarter of 2015, making the identification susceptible to deposit-country time series shocks in that period. We perform alternative event studies using only those countries that adopt the CRS well before and well after the first adoption wave, i.e. before September 2015 or after April 2016 instead (see Appendix B.8). As dynamics are unchanged, we demonstrate that CRS adoption matters for deposit patterns separately from shocks to the deposit countries during the first adoption wave. In a second robustness test, we show in a placebo analysis that there is no significant change in cross-border deposits at CRS effectiveness in our control group deposit countries (see Appendix B.6).[35] Furthermore, in additional tests we drop from our specification those deposit countries for which we find evidence of a potential shock to the economy that might significantly affect cross-border deposits in that country. We find no changes in effect size (see Appendix B.2). Overall, we are therefore confident that our results are not biased by concurrent events to the CRS.

3.2.2. Measuring the relocation of cross-border deposits to the United States

In the second part of our main analysis, we test for changes in cross-border deposits located in the United States after versus before the CRS becomes effective.[36] We begin again with an event study to explore pre-

[35] In this test we can demonstrate at the same time the absence of relocation of deposits to control group countries. Note also that repatriation does not lead to changes in the volume of the deposits in our control group because our sample is restricted to cross-border deposits.

[36] In robustness tests we also test the effect of the CRS in other secrecy locations, Section 4.3.3.

trends and dynamic effects of the CRS on cross-border deposits held in the United States versus in other non-tax havens, which formally reads as:

$$\begin{aligned} log(Deposits_{ijt}) = & \sum_{k=-4}^{4} \beta_k D^k * US + \beta_9 PostCRS_t * Havens_j \\ & + \gamma_{it} + \theta_{ij} + \epsilon_{ijt} \end{aligned} \tag{3}$$

All variables and specifications of the fixed effects remain the same as in equation 1 except for the added variables of interest, which are the dummies D^k_{jt} indicating a point in time k periods from the CRS treatment and interacted with US, which is a dummy taking value one when the deposit country is the United States. Here the CRS treatment is the date when the CRS becomes effective in the first wave adopters. We base this test on a non-staggered specification of the CRS treatment period, because there is no date when the CRS become effective in the United States. We control for the reduction of cross-border deposits held in tax havens by adding the interaction $PostCRSFirstWave_t * Haven_j$ from our first analysis. We confirm that the control group is not driving our results in a split sample test (Section 4.3.2.).

Next, we measure the average effect of the CRS on cross-border deposits in the United States in a difference in difference design. We run a new regression of the form:

$$\begin{aligned} log(Deposits_{ijt}) = & \alpha + \beta_1 PostCRS_t * Havens_j \\ & + \beta_2 PostCRS_t * US \\ & + \gamma_{it} + \theta_{ij} + \epsilon_{ijt} \end{aligned} \tag{4}$$

All variables and specifications of the fixed effects remain the same in equation 4 as in equation 2, except for the added interaction term of the $PostCRS_t$-dummy and the US_j-dummy. As in the event study, the Post-CRS period is measured using the non-staggered treatment dummy, which is only time and not country dependent and which is switching to one when the CRS becomes effective in the first wave adopters. The added interaction captures the effect of the CRS on foreign deposits held in the United States. Thus, while controlling for the effect of the CRS in tax havens, we compare the change in deposits held in the United States to the change in deposits held in other non-tax havens after the CRS becomes effective. β_2 is the coefficient of interest. If wealth and related income are relocated to the United States, the coefficient β_2 should be positive.

In the following we discuss the identifying assumptions on this part of the analysis. In both, the event study and the difference in difference design, we compare changes in cross-border deposits held in the United States with

those held in non-tax havens (control group) after the CRS becomes effective (post-period) controlling for the reduction of deposit in tax havens. The main assumption we rely upon is that absent the CRS cross-border deposits of EU and OECD residents in the United States and other non-tax havens would have trended in parallel within our sample period. Especially we rely on the assumption that there are no local shocks affecting cross-border deposits concurrent to the CRS. We show in event studies that in the pre-treatment periods both control and treated trend in parallel, i.e. we observe no U.S. specific local shocks (see Section 4.1). Given parallel pre-trends, our research design would still be invalid if local shocks systematically affected cross-border deposits around treatment. We address this issue from the side of the residence country of the deposit holder by including residence-country x quarter-year fixed effects in our regressions. In Appendix D.1, we estimate a simpler model excluding residence-country x quarter-year fixed effects. Our estimates do not vary significantly in this specification.

What remains is, that our identification in this part of the analysis relies crucially on the absence of local shocks concurrent to the CRS in the United States as the only treated deposit country. First, to evaluate this identifying assumption, we check the U.S. institutional environment for potentially confounding events around the first adoption wave of the CRS. As a result we exclude in our main period of analysis the years before 2014, in which our results are likely confounded by FATCA implementation and the period after 2017, in which the 2018 U.S. Tax Cuts and Job Act might confound our findings. For instance, a number of wealthy foreign residents may be US taxpayers and may have relocated financial assets to the United States due to the burden of FATCA. This would lead to an increase in foreign-owned U.S. deposits after FATCA implementation. Indeed, if we extend the sample period an effect of FATCA is visible (see Figure 1 and the long run event study in Appendix C.1). A central remaining concern is that cross-border deposits in the United States, a large economy, could change due to economic cycles. As in the first part of the analysis, we directly test for violations of the identifying assumptions due to economic shocks by investigating deposit country level economic outcomes (GDP) as left hand side variables in our event study design (Appendix B.3). We find no trends in GDP in the United States relative to the one in our control group within our sample period. To evaluate the identification assumptions further, we conduct additional tests. By a split sample analysis we rule out that our results are driven by concurrent shocks to the control group deposit countries. Cross-border deposits in the control group do not change, while they do increase significantly in the United States (see Section 4.3.2). As further placebo, we test what happens in other attractive deposit countries. In those other deposit countries, we find no increase in deposit holdings post-CRS (see Section 4.3.3). While we want to point out that not all changes to cross-border deposits after the CRS in the United States should be attributed to the CRS, our robustness

analysis makes us confident that the effect we measure in the short term period, should at least to a substantial amount be due to a response to the CRS.

4. Empirical Results

4.1. Event Study

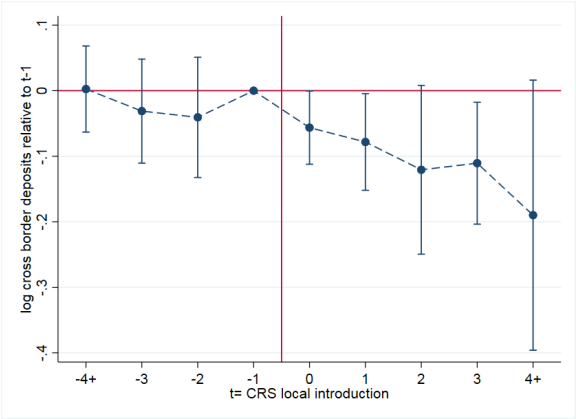

Figure 2: Event Study Test of Reaction to the CRS in Tax Havens

Notes: The figure charts the coefficients, which each mark the change in cross-border deposits held in tax havens versus non-tax havens around the CRS event dates (in event time). We estimate equation 1. The plotted coefficients are the interactions of the tax havens indicator with 8 separate indicator variables, each marking one quarter over the sample period relative to the quarter before the CRS event date (t=0). We bin the treatment indicators beyond t-4 to t+4 and omit the indicator for period t-1. It, therefore, serves as a benchmark and has a coefficient value of zero (and no confidence interval). The figure plots the coefficient estimates of the 8 quarters together with their 95% confidence intervals for the staggered CRS event date at the passing of the CRS law in the deposit country. We use the log of cross-border deposits as the dependent variable and residence-country quarter-year fixed effects as well as ordered country-pair fixed effects. We cluster at deposit-country level.

We commence our analysis by reporting graphical results from event-study regressions from equation 1. Figure 2 plots the coefficients, which each mark the change in cross-border deposits held in tax havens versus non-tax havens in one quarter over the sample period, relative to the quarter before the CRS treatment event date (t=0), with quarters beyond t-4 to t+4 being binned at the end points. The results are shown together with the 95% confidence interval. They corroborate the parallel trends assumption since in the pre-treatment period the coefficients lie close to zero and are statistically insignificant. In the post-treatment period, the effect size increases in absolute magnitude over time and remains significant through quarters t+4(+). The increase in the effect size suggests that some tax evaders wait until information collection under the CRS commences (i.e., when the CRS becomes effective) before moving their deposits from tax havens.

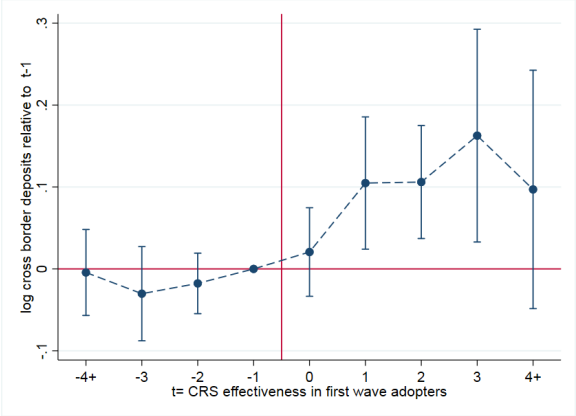

Figure 3: Event Study Test of Relocation Behavior after the CRS

Notes: The figure charts the coefficients, which each mark the change in cross-border deposits held in the United States versus non-tax havens around the CRS event dates (in event time). We estimate equation 3. The plotted coefficients are the interactions of the U.S. indicator and the 8 separate indicator variables, each marking one quarter over the sample period relative to the quarter before the CRS treatment event date (t=0). We bin the treatment indicators beyond t-4 to t+4 at the endpoints and omit the indicator for period t-1. It, therefore, serves as a benchmark and has a coefficient value of zero (and no confidence interval). The figure plots the coefficient estimates of the 8 quarters together with their 95% confidence intervals for the non-staggered CRS event date. We use the log of cross-border deposits as the dependent variable and residence-country quarter-year fixed effects as well as ordered country-pair fixed effects. We cluster at deposit-country level.

We continue with the event study results from equation 3, which is our first test of cross-border deposits relocation to the United States. Figure 3 plots the coefficients, which each mark the change in cross-border deposits held in the United States versus other countries, controlling for tax havens, in one quarter over the sample period, relative to the quarter before the CRS treatment event date (t=0), with quarters beyond t-4 to t+4 being binned at the end points. The results are shown together with the 95% confidence interval. The coefficients in the pre-period (t-1 to t-4(+)) are statistically indistinguishable from the benchmark quarter, showing that there are no significant pre-treatment trends. After the CRS treatment date we observe an increase in cross-border deposits in the United States relative to the control group, which is relatively immediate. From t=1 the coefficient size increases sharply and is significant until t=3.

4.2. Difference in Difference Estimates

We report the difference in difference results from our main test on the effect of the CRS on cross-border tax evasion in Table 4. The results from the estimation of equation 2, our test of whether the CRS leads to a reduction of deposits held in tax havens, can be found in Columns 1 to 3 of Table 4. Column 1 refers to the post CRS period specified as the passing of the CRS law in the deposit country, Column 2 refers to the post CRS period

specified as the date when the CRS becomes effective in the deposit country and Column 3 refers to the post CRS period specified as the date when the CRS becomes effective in the first wave adopters. Our coefficient of interest is the interaction term of the tax haven variable and the respective Post-CRS dummy. We observe a 11.9% reduction of cross-border deposits held by residents of the EU and OECD in tax havens upon the passing of the CRS laws in the deposit countries as compared to the change in cross-border deposits in the control countries. We reject the null hypothesis with p-value of 4%.

This effect is very similar in terms of size to what Johannesen and Zucman (2014) find in their test of the effect of bilateral TIEAs on cross-border deposits in tax havens, and it is more significant here.[37] On first inspection, the CRS, accordingly, seems to have a similar effect as a TIEA. However, for two reasons this result suggests that the CRS is considerably more effective than previously concluded TIEAs. The CRS is introduced on top of TIEAs in most of our sample country-pairs, and our sample mainly includes EU member states where also the European Savings Directive was in place. Thus, the information on interest income gained on the majority of the accounts considered in our sample has been already automatically exchanged across EU member states. In Section 4.3 below, we run the same regression analysis as above, but we limited our sample to non-EU member states as countries of residence of tax evaders. As expected, then the effect of the CRS is considerably larger. A further difference to Johannesen and Zucman (2014) is, that we evaluate a shorter time period in our main analysis.[38] This may result in a conservative estimate of the total effect.

We provide an intuition for the economic relevance of our main estimate, i.e., a 11.9% reduction in cross-border deposits in our sample of tax havens. In a given quarter-year, the average amount of cross-border deposits held by residence countries in the tax havens in our sample is USD 389 billion. Thus, in our sample, the average amount of deposits in tax havens is decreased by about USD 46 billion. This should be considered a lower bound estimate for three reasons. First, we get access to data on bilateral cross-border deposits located in a representative but limited sub-sample of tax havens. That is, we base our calculation on the residence and tax havens in our sample. Second, we are only analyzing deposits located in banks. While the CRS affects deposits in a wider range of financial institutions, including for example investment entities and specified insurance companies,[39] and sources of in-

[37] Johannesen and Zucman (2014) find an 11% decrease.

[38] Johannesen and Zucman (2014) examine the fourth quarter of 2003 up to the second quarter of 2011. Our sample period is also shorter than for example the one of Alstadsæter et al. (2018), who consider the period from 2001 until 2015. However, it is similar to the one of Huizinga and Nicodème (2004), who study the period from 1996 until 1999.

[39] For a complete list, see OECD (2018, p. 61).

Table 4: Change in Cross-Border Deposits after the CRS

CRS SPECIFICATION	(1) Country Introduction	(2) Country Effectiveness	(3) First Adoption Wave	(4) First Adoption Wave
VARIABLES	LogDeposits	LogDeposits	LogDeposits	LogDeposits
PostCRS * Havens	-0.119**	-0.118**	-0.115*	-0.109
	(0.0537)	(0.0558)	(0.0622)	(0.0645)
PostCRS * US				0.109*
				(0.0559)
Observations	11,884	11,884	11,884	11,884
R-squared	0.972	0.972	0.972	0.972
Country-Pair FE	YES	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of equation 2. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit location j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit location, in Column 2, for the effective date of the CRS in the deposit location, and, in Column 3 and 4, for the period of the first wave of information exchange. Havens is a dummy taking value one when the deposit country j is a tax haven. US is a dummy equal to one when the deposit country j is the United States. All regressions include ordered country-pair and residence country x quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at the deposit-country level.

Robust standard errors in parentheses

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

vestments other than bank deposits, e.g., equity or bond portfolios. Third, to the extent that the reduction observed is due to the declaration of income rather than relocation, tax evaders do not necessarily need to repatriate all income held in tax havens to make a declaration, just the amount needed to pay back taxes and fines (Hakelberg and Schaub, 2018, p. 356). In sum, this implies that a much larger total effect of the CRS on cross-border secrecy seekers and tax evasion can be expected, based on our findings.

Furthermore, we test the deposit country-specific CRS effective dates. The result of the test is reported in Column 2 of Table 4. After the CRS becomes effective in the deposit countries, cross-border deposits are on average 11.8% lower in the tax havens as compared to non-tax havens. We reject the null hypothesis with p-value of 4%. In Column 3 of Table 4, as an alternative specification of the post CRS period and robustness check, we chose a post-period dummy that is defined as the period after CRS becomes effective for the first wave adopters. Using this third alternative measure we find a very similar result, namely that in the post-treatment period deposits held in tax havens are on average 11.5% below those held in the control group countries (see Column 3 of Table 4). We reject the null hypothesis with p-value of 7%.

We report the results from our test of whether the passing of the CRS law leads to relocation of deposits to the United States (the estimation of equation 4) in Columns 4 to 5 of Table 4. Column 4 reports regression results, from running the difference in difference regression without further controls beyond the fixed effects structure. Our test shows that relative to all other countries in our sample and after controlling for the effect of the CRS on tax havens, deposits by EU and OECD residents in the United States significantly increase, on average by 10.9%, after the CRS becomes effective. We reject the null hypothesis with p-value of 6%. The effect size is substantial and, therefore, economically highly relevant. In a given year, the average amount of deposits held by all residence countries in our sample in the United States is USD 551 billion. Given our coefficient estimates, that amount is increased by USD 60 billion after the CRS becomes effective, which is large enough to assume that a substantial part of cross-border deposits, that after the CRS becomes effective were removed from tax havens (the estimate for our six tax havens is USD 46 billion), are relocated to the United States.

In Appendix B.1, we re-run the main tests for tax havens as well as for the United States but using this time a balanced sample. In this way, we lose around 9% of the observations. Yet, results are entirely in line with the above-presented ones. Furthermore, we show in Appendix C.1, that our results are confirmed even in an extended period from the first quarter of 2010 to the fourth quarter of 2018. The size of the coefficients of interest are larger, which may be due to confounding effects in the longer time window, foremost due to FATCA, or due to underestimation of the total effect in the

short run.

4.3. Robustness Checks

4.3.1. The CRS Effect Net of the European Savings Directive

In our baseline model, the sample of residence countries includes mainly EU member states. However, since 2003 and up to the passing of the CRS laws at national level, EU residents were subject to the European Savings Directive. This means that banks were either required to automatically report information on interest income earned by foreign EU households to local tax authorities, who further transmitted the information to the respective home country of the household (as in most EU member states and Guernsey, Isle of Man, Jersey) or a withholding tax of initially 15% - then increased to 20%-35% - was levied on interest income of foreign EU households (as initially in Austria and most of the tax havens). In order to net out the effect of the European Savings Directive and isolate the effect of the CRS, we re-run our baseline model including as residence countries only OECD member states, which are not EU member states, namely Australia, Canada, Chile, Israel, Iceland, Japan, South Korea, Mexico, New Zealand, Norway, Switzerland, Turkey, and the United States.[40]

Table 5: Non-EU Residents' Change in Cross-Border Deposits Upon CRS

	(1)	(2)	(3)
	Country Introduction	Country Effectiveness	First Adoption Wave
VARIABLES	LogDeposits	LogDeposits	LogDeposits
PostCRS * Havens	-0.279***	-0.277***	-0.325***
	(0.0844)	(0.0905)	(0.0912)
Observations	3,490	3,490	3,490
R-squared	0.968	0.968	0.968
Country-Pair FE	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of equation 2 excluding cross-border deposits owned by EU residents. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit location j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit country, in Column 2, for the effective date of the CRS in the deposit country, and, in Column 3, for the period of the first wave of information exchange. Included residence countries are Australia, Canada, Chile, Israel, Iceland, Japan, South Korea, Mexico, New Zealand, Norway, Turkey and the United States. Havens is a dummy taking value one when the deposit country j is a tax haven. All regressions include ordered country-pair and residence country x quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at deposit-country level.

Robust standard errors in parentheses

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

[40] In Appendix B.5, we present the results of our placebo tests where we only include the EU member states as residence countries.

Results are displayed in Table 5. We observe a short term 27.9% reduction of cross-border deposits held by residents of the non-EU OECD member states in tax havens after the CRS as compared to the change in cross-border deposits in the control countries (see Column 1 in Table 5). We reject the null hypothesis with p-value of 0.3%.

As expected, this finding shows that, while the overall short term effect of the passing of the CRS laws on the use of tax havens is 11.9%, the reduction in those countries unaffected by the European Savings Directive is much larger. A similar effect is detected when considering the country-specific effective dates (see Column 2 in Table 5). In Column 3 of Table 5, we consider the period after the first CRS adoption wave not measured at the country level, and we find that in the post-treatment period deposits held in tax havens are on average 32.5% below those held in the control group countries. We reject the null hypothesis with p-value of 0.1%. Event study results corresponding to the difference in difference estimates from Column 1 Table 5 are reported in Appendix A.1.

4.3.2. Sample Split Test of Relocation to the United States

To corroborate the robustness of our finding that the United States receives an increasing amount of cross-border deposits after the CRS becomes effective, we conduct a split sample analysis. We test relocation behavior to the United States only on the sub-sample of country-pairs where the deposit country is the United States, i.e., from our sample we drop all other observations for which deposits are held in non-U.S. deposit countries. The difference-in-difference regression design thus becomes a time trend test of deposits located in the United States, where we compare the change in deposits located within the United States after versus before the CRS becomes effective. This test rules out that our main findings are driven by changes in the control group rather than in the treated group.

As a placebo test, we investigate the change in non-tax haven to non-tax haven deposits as a reaction to the CRS. We adopt ordered country-pair fixed effects in both, the main test and the placebo test. In the placebo test, we use cluster robust standard errors, with clustering at the deposit country level. Since for the test on U.S. deposits we only have one deposit country, we cluster at the country-pair level. Results are displayed in Table 6.

The estimated effect of the CRS on the U.S. deposits reported in Table 6 Column 1 is larger but similar to the one in our main test, namely we detect an increase of cross-border deposits in the United States of 15.8% post CRS and we reject the null hypothesis with p-value of 0.0%. This corroborates our difference-in-difference results of the test of relocation behavior to the United States. In Column 2 of Table 6, the placebo test underscores that, as we expect, no statistically significant change in cross-border deposits of EU and OECD residents in non-tax haven deposits occurs after the CRS

Table 6: Reaction to CRS of Cross-Border Deposits in the United States vs. Non-Tax Havens

VARIABLES	(1) Deposits in U.S. LogDeposits	(2) Deposits in Non-Tax Havens & Non-U.S. LogDeposits	(3) Deposits in Non-Tax Havens LogDeposits
PostCRS	0.158***	0.0490	
	(0.0381)	(0.0550)	
PostCRS * US			0.113*
			(0.0561)
Observations	480	8,482	8,962
R-squared	0.989	0.966	0.970
Country-Pair FE	YES	YES	YES
Residence-Quarter-Year FE	NO	NO	YES
Clustering	Country-Pair	Deposit Country	Deposit Country

Notes: This table reports the time-trend estimates for the split sample in Column 1 (only cross-border deposits held in the U.S.) and 2 (only cross-border deposits held in non-tax havens excluding the U.S.) and in Column 3 the DiD estimates of equation 4 with our baseline sample excluding tax havens. The dependent variable is the log of cross-border deposits held by residences of country i in banks of deposit country j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is a dummy equal to one starting in the period of the first wave of information exchange. US is a dummy equal to one when the deposit country j is the United States. In Column 1 and 2, ordered country-pair fixed effects is included. In Column 3, ordered country-pair and residence country x quarter-year fixed effects are included. Cluster robust standard errors in parentheses, clustered at the country-pair level for Column 1 and at deposit-country level for Column 2 and 3.

Robust standard errors in parentheses

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

becomes effective.[41] Column 3 of Table 6 presents the result where the two samples from Column 1 and 2 are now added together in a diff-in-diff setting. This allows us to add residence-country quarter-year FE to control for time variant common shocks. Results confirm our baseline test. Supporting event studies corresponding to the Columns 1 to 3 of Table 6 are reported in Appendix A.2.

4.3.3. Alternative Attractive Countries for Relocation

To further rule out that other countries have become attractive places of relocation post-CRS, we test what happens in alternative potentially attractive countries after the CRS becomes effective. First, as potentially equally attractive secrecy locations, we consider countries listed among the top ten secrecy locations in the Financial Secrecy Index.[42] Next to the United States, we have data on Luxembourg, Guernsey, Hong Kong, and Switzerland. In contrast to the United States, they all passed the CRS into national law, and they are part of our tax havens sample. Our choice is motivated by the consideration that deposit countries that score very high in the index might offer more possibilities for exploiting loopholes in the CRS. Besides the degree of information exchange, other criteria considered by the Tax Justice Network when creating this index include legal entity transparency, owner-

[41] The p-value is 38.3%. We extend this placebo test to all Post-CRS treatment indicators in Appendix B.6.

[42] For the full index, see https://www.financialsecrecyindex.com/en/, accessed on 14.01.2020.

ship registration and integrity of tax and financial regulation.[43] Secondly, we test what happens after the CRS becomes effective in an alternative renowned financial centre for which we have bilateral data at deposit level, namely the United Kingdom. Differently from the United States the United Kingdom introduced the CRS into its national law.

To make results comparable to our test of relocation to the United States, we employ the same research design. We rerun our main test of relocation to the United States replacing the US-dummy with a dummy for Luxembourg, Guernsey, Hong Kong, Switzerland, and the United Kingdom respectively. We control for the reaction to the CRS in the respective other tax havens with an interaction of the CRS treatment dummy (PostCRS) and tax havens dummy (Havens), e.g., when testing for the effect in Luxembourg the reaction in all other tax havens is controlled for with this interaction term. As expected, in none of the alternative countries tested we observe a statistically significant increase in cross-border deposits (see Table 7).

4.3.4. Placebo Test of the CRS Introduction Effect in Residence Countries

In our main test, we assume that the reaction to the CRS occurs at the moment when the CRS law is passed in the deposit country rather than in the residence country. To test this claim, we run the following regression:

$$\begin{aligned} log(Deposits_{ijt}) = \alpha &+ \beta_1 PostCRSDepL_{it} \\ &+ \beta_2 PostCRSDepL_{it} * Havens_j \\ &+ \beta_3 PostCRSResL_{jt} \\ &+ \beta_4 PostCRSResL_{jt} * Havens_j \\ &+ \gamma_{it} + \delta_{ij} + \epsilon_{ijt} \end{aligned} \tag{5}$$

Where equation 5 is the same as equation 2, except for the post-treatment period dummies. $PostCRSDepL_{jt}$ denotes the passing of the CRS law in the deposit country, and $PostCRSResL_{it}$ denotes the passing of the CRS law in the residence country. Both dummies switch to one after the CRS law is passed and stay switched on until the end of the sample period. We are interested in comparing the two post-treatment period dummies, i.e., $PostCRSDepL_{it}$ and $PostCRSResL_{jt}$. We add quarter-year and ordered country-pair fixed effects. The fixed effects structure has to be adapted to allow us to test the effect of the CRS in the residence country. We expect the

[43] Although the index incorporates a large number of characteristics of which some are more and others are less relevant in the context of our study, we nevertheless chose not to adapt the scores for our study, to retain objectivity with regard to our choice of jurisdictions.

Table 7: Relocation of Cross-Border Deposits to the United States vs. Alternative Countries

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First Adoption Wave	First Adoption Wave	First Adoption Wave	First Adoption Wave	First Adoption Wave	First Adoption Wave	First Adoption Wave
VARIABLES	LogDeposits	LogDeposits	LogDeposits	LogDeposits	LogDeposits	LogDeposits	LogDeposits
PostCRS * Havens	-0.109	-0.139**	-0.111*	-0.122*	-0.122*	-0.112*	-0.188***
	(0.0645)	(0.0597)	(0.0653)	(0.0653)	(0.0649)	(0.0647)	(0.0656)
PostCRS * US	0.109*						0.113*
	(0.0559)						(0.0592)
PostCRS * LU		0.00524					0.0153
		(0.0533)					(0.0593)
PostCRS * GG			-0.137**				-0.127**
			(0.0538)				(0.0598)
PostCRS * HK				-0.0823			-0.0722
				(0.0530)			(0.0590)
PostCRS * CH					-0.0796		-0.0695
					(0.0541)		(0.0602)
PostCRS * GB						0.0599	0.0666
						(0.0562)	(0.0593)
Observations	11,884	11,884	11,884	11,884	11,884	11,884	11,884
R-squared	0.972	0.972	0.972	0.972	0.972	0.972	0.972
Country-Pair FE	YES	YES	YES	YES	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of equation 4, substituting the treated group in Column 2 to 7 with alternative countries beyond the United States. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit country j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is a dummy equal to one starting in the period of the first wave of information exchange. Havens is a dummy taking value one when the deposit country j is tax haven. US, LU, GG, HK, CH and GB is a dummy equal one when the deposit country j is the United States, Luxembourg, Guernsey, Hong Kong, Switzerland, or United Kingdom respectively. Cluster robust standard errors in parentheses, clustered at the deposit-country level. All regressions include ordered country-pair and residence country x quarter-year fixed effects.

Robust standard errors in parentheses

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

coefficient on $PostCRSDepL_{it}$ to be negative and significant at conventional levels and the $PostCRSResL_{jt}$ to be insignificant at conventional levels and close to zero. This is what we find in Table 8. The findings corroborate that the reaction to the CRS occurs when the CRS law is passed in the deposit country rather than in the residence country.

Table 8: CRS's Effect of Introduction in Residence vs. Deposits Country

	(1)
	Country Introduction
VARIABLES	LogDeposits
PostCRSDepL * Havens	-0.126**
	(0.0507)
PostCRSResL * Havens	0.0148
	(0.0598)
Observations	11,884
R-squared	0.970
Country-Pair FE	YES
Quarter-Year FE	YES
Clustering	Deposit Country

Notes: This table reports the DiD estimates of regression model 5. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit country j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS_ResL is a dummy equal to one after the CRS is introduced in the residents country, and PostCRS_DepL is a dummy equal to one after the CRS is introduced in the deposits country. Havens is a dummy taking value one when the deposit country j is a tax haven. All regressions include ordered country-pair and residence country x quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at the deposit-country level.

Robust standard errors in parentheses

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

5. The Use of Shell Companies in the Post-CRS Era

So far, we only address tax evaders and secrecy seekers who hold bank accounts in tax havens in their name, i.e., directly. Instead of directly holding a bank account in tax havens, tax evaders or secrecy seekers can first set up a company in a tax haven and through that company (a so-called shell company) hold a bank account. Shell companies are used to add layers of secrecy between the hidden account and its beneficial owner. We proceed by investigating how CRS affects the use of shell companies by tax evaders.

To detect shell companies, we follow the identification strategy proposed in Johannesen and Zucman (2014). Their identification strategy relies on the fact that cross-border deposits from the BIS include deposits owned by

both entities and individuals. For example, when an Italian tax evader holds assets in Jersey through a shell company in Hong Kong, the BIS assigns the funds to Hong Kong, i.e., we observe in our data these deposits as being held by a Hong Kong resident in Jersey. Although cross-border deposits might be held through tax havens for other reasons than tax evasion, there is vast anecdotal and empirical evidence on bank accounts in tax havens being held by individuals indirectly through shell companies such as the evidence reported in the context of the Paradise and Panama Papers for the purpose of tax evasion. Therefore, we assume that changes in deposits held by tax haven residents around the passing of the CRS laws are associated with shell companies held by tax evaders and other secrecy seekers who are responding to CRS reporting requirements.

We first test whether a decrease in deposits held by residents of tax havens in other tax havens is documented post CRS. Secondly we test whether the CRS leads to a relocation to the United States of cross-border deposits held through tax havens. Lastly, we test for the increasing relevance of the United States as a location for indirect deposit holdings. For these analyses, we maintain the same sample of tax haven deposit countries, but we have a larger sample of tax haven resident countries. This is due to the availability of bilateral data at the BIS on cross-border deposits held by residents of Aruba, Bahamas, Bahrain, Barbados, Bermuda, Cayman Islands, Curacao, Gibraltar, Guernsey, Hong Kong, the Isle of Man, Jersey, Lebanon, Macau, Mauritius, Panama, Samoa and Singapore in Guernsey, Hong Kong, the Isle of Man, Jersey, Luxembourg and Switzerland.[44] All tests in this section are time trends tests without control group, thus, compared to our main analysis the identification we can obtain is weaker.

We begin by testing whether the passing of the CRS law has led to a reduction of shell companies holding cross-border deposits in other tax havens. For that purpose, we restrict the sample to deposits held by tax haven residents (i.e., our proxy for cross-border deposits held through shell companies) in other tax havens. We regress these tax haven-to-tax haven deposits on the post-CRS dummy. The baseline regression takes the following form:

$$\begin{aligned} log(Deposits_{ijt}) = \alpha &+ \beta_1 PostCRSDepHaven_{it} \\ &+ \beta_2 PostCRSResHaven_{jt} \\ &+ \beta_3 PostCRSDepHaven_{it} * PostCRSResHaven_{jt} \\ &+ \gamma_t + \theta_{ij} + \epsilon_{ijt} \end{aligned} \tag{6}$$

The dependent variable is defined as in equation 2. $PostCRSDepHaven_{it}$ denotes the passing of the CRS law in the deposit tax haven and $PostCRSResHaven_{jt}$

[44] We select all countries listed as offshore countries at the BIS.

Table 9: Change in Cross-Border Deposits held through Tax Havens and the United States

	(1)	(2)	(3)	(4)	(5)
SAMPLE	Tax Haven to Tax Haven	Tax Haven to Tax Haven	Tax Haven to US	Tax Haven to US	US to Non-Tax Haven
VARIABLES	LogDeposits	LogDeposits	LogDeposits	LogDeposits	LogDeposits
PostCRSResHaven	0.0937		0.310***		
	(0.0683)		(0.104)		
PostCRSDepHaven	0.0452				
	(0.0538)				
PostCRSResHaven * PostCRSDepHaven	-0.223**				
	(0.0891)				
PostCRSFirstWave		-0.109**		0.177*	0.258*
		(0.0453)		(0.0990)	(0.129)
Observations	1,338	1,338	208	208	234
R-squared	0.975	0.975	0.988	0.985	0.964
Quarter-Year FE	YES	NO	YES	NO	NO
Country-Pair FE	YES	YES	YES	YES	YES
Clustering	Tax Haven Country-Pair	Tax Haven Country-Pair	Tax Haven Residence Country	Tax Haven Residence Country	Non-Tax Haven Deposit Country

Notes: This table reports the DiD estimates of regression model 6, 7 and 8 respectively. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit country j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. In Column 1 and 2, the sample is restricted to tax havens as residence and deposit country. In Column 3 and 4, the sample is restricted to tax havens as residence country and the United States as deposit country. In Column 5, the sample is restricted to the United States as residence country and non-tax havens as deposit country. All columns include country-pair fixed effects, but only Columns 1 and 3 include quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at country-pair level in Column 1 and 2, at residence-country level in Column 3 and 4 and at deposit-country level in Column 5.

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

in the residence tax haven. Since in this setting, both, residence and deposit country are tax havens, our coefficients of interest are the sum of the interaction effect of the two variables (β_1, β_2, β_3). The identifying variation comes from differences in the timing of CRS adoption across residence and deposit tax havens. Following Johannesen and Zucman (2014), standard errors are clustered at the treatment level, i.e. country-pair, and we include country-pair and quarter-year fixed effects.[45] In an alternative specification to this staggered design, we test the effect after the CRS is effective in the first adoption wave countries. [46]

Ex-ante, the direction of the effect is unclear. Anecdotal evidence suggests that the CRS could be circumvented by the setting up of shell companies in certain circumstances. According to the CRS guidelines, financial institutions are required to identify the controlling person(s) in case the account holder is an entity. However, it might not always be feasible to obtain information on the final beneficial owner. Thus, holding a financial account through shell companies located in a traditional tax haven may still represent a valuable strategy to hide wealth and related income outside the country of residence. In case individuals avoid CRS requirements by the use of shell companies in tax havens, we would expect a coefficient, which is insignificant or even positively significantly different from zero. If instead, the CRS is effective in addressing tax evasion by the use of shell companies in tax havens, we would expect a negatively significant coefficient. Indeed, this is what we find in the short-term. Cross-border deposits in tax haven-to-tax haven constellations decreased in the short term by 8.4% in our baseline (adding together the three coefficients of interest)[47] or alternatively by 10.9% around the first adoption wave, which indicates that the overall use of shell companies in tax havens decreased as a reaction to the CRS (see Table 9 Column 1 and 2). Based on average deposits held through the 18 tax havens in our sample in Guernsey, the Isle of Man, Jersey, Luxembourg and Switzerland, this is equivalent to a reduction of USD 13 billion in these six tax havens as of our lower bound estimate. These short term results are supported by event studies, which we report in Appendix A.3. Furthermore, in Appendix B.8, Table B.18 Column 1, we show robustness to dropping those countries that where part of the first adoption wave. Extended sample results are reported in Appendix C.1.

Secondly, we test whether shell companies in tax havens increased cross-

[45]Here we do not use clustering at the deposit country level, since the identifying variation results from country-pair level changes and not, as it is the case in our baseline estimations, from deposit country level changes.

[46]In the non-staggered design we can only include ordered country-pair fixed effects, given that we have no control group.

[47]The F-test of joint significance on the coefficients of β_1, β_2 and β_3 gives a p-value of 9.7%.

border deposit holdings in the United States after the CRS passing into national law in the tax haven in the short term. We restrict the sample to tax haven residence countries and the United States as deposit country. We then regress these haven-to-U.S. deposits on the post CRS dummy. The regression takes the form:

$$\begin{aligned} log(Deposits_{ijt}) = \alpha + \beta_1 PostCRSResHaven_{jt} \\ + \gamma_t + \theta_{ij} + \epsilon_{ijt} \end{aligned} \tag{7}$$

All variables are defined as in Equation 6. As best country-level proxy available, we consider the passing of the CRS into law in the residence tax haven country as the moment when treatment occurs. In an alternative specification to this staggered design, we test the effect after the CRS is effective in the first adoption wave countries. β_1 is the coefficient of interest, which we expect to be positive and significantly different from zero. In the baseline staggered specification we add ordered country-pair and quarter-year fixed effects. In the non-staggered design we can only include ordered country-pair fixed effects given that we have no control group. Standard errors are clustered at the residence-country level, i.e. the treatment level in this specification. We find a short term increase in cross-border deposits located in the United States and held through tax havens of 31.0% in the staggered design. In our alternative specification, we find an increase of 17.7% (see Table 9 Column 3 and 4). Based on average deposits held through the 18 tax havens in our sample in the United States, this effect is equivalent to an increase of USD 83 billion as of the lower bound estimate. Results are supported by event studies, which we report in Appendix A.3. In Appendix B.8, Table B.18 Column 2, we show robustness to dropping those countries that where part of the first adoption wave. Results in an extended sample period are shown in Appendix C.1.

In our last test, we investigate the role of the United States as a location for shell companies. As Sharman (2010) and Findley et al. (2015) show, not only traditional tax havens but also the United States offer very attractive conditions for setting up shell companies. Thus, we can expect that after the CRS becomes effective, given the compliance of all traditional tax havens, deposit holders may now find it more appealing to set up shell companies in the United States. Furthermore, through those entities, they may hold local as well as international deposits in non-tax havens, since wealthy individuals may both be unwilling to accumulate all their capital in one single country and present a home-bias investment attitude (Coeurdacier and Rey, 2012). Therefore, one can presume deposit holders to own also deposits located outside the United States indirectly via U.S. shell companies. This would represent a similar 'round-tripping' strategy as the one detected by Hanlon et al. (2015) in the context of U.S. taxpayers. For example, a German tax-

payer could set up an investment entity in the United States and through that entity hold deposits in a German bank account.

We test for the increasing relevance of the United States as a location of shell companies by comparing the change in cross-border deposits held by U.S. residents in non-tax havens after versus before the CRS becomes effective. Thus, we regress these U.S.-to-non-tax haven deposits on the post CRS dummy. The regression takes the form:

$$\begin{aligned} log(Deposits_{ijt}) = \alpha + \beta_1 PostCRS_t \\ + \gamma_t + \theta_{ij} + \epsilon_{ij} \end{aligned} \tag{8}$$

All variables are defined as in equation 2, but we do not have a control group and the setting allows only for a non-staggered treatment specification, which rules out the inclusion of quarter-year fixed effects. Results suggest a short term increase of 25.8% of deposits held by U.S. residents in non-tax havens after the CRS becomes effective in the first wave CRS adopters (see Table 9 Column 3). Based on average deposits held by U.S. residents in the non-tax havens in our sample this is equivalent to a USD 209 billion increase. This finding gives first evidence that after the CRS becomes effective also the use of U.S. shell companies could have substantially increased. Supporting event study results are reported in Appendix A.3, Figure A.6., panel (c). Furthermore, in Appendix B.7, we provide placebo tests substituting the United Kingdom for the United States in the tests that we conducted in this Section. In Appendix C.1 we also consider the effect in the longer term window.

Overall, when taking these findings as approximations of the short term CRS effect on the use of shell companies, the CRS is effective in tax havens but the US-non participation in the CRS, seems to increase the country's attractiveness as location for shell companies. However, our results need to be interpreted with caution since the identification in this additional analysis is weaker than in our main tests, as all country-pairs in our sample are treated and we do not employ a control group.

6. Conclusion

In this study, we analyze the short term impact of the CRS, an unprecedented standard for the automatic exchange of information, on cross-border tax evasion. In the period from the fourth quarter of 2014 to the third quarter of 2017, we document a statistically significant decrease of cross-border deposits ranging from 11.8% (held by residents of non-tax havens) to 8.4% (held by residents of tax havens) in major tax havens around the world after the CRS is passed into national law in the deposit country. Moreover, we do not find that the CRS truly puts an end to cross-border tax evasion, instead we document a change in the dynamics of cross-border tax evasion.

We add to the prior literature by providing evidence that after the CRS becomes effective, an unexpected country seems to attract wealth and related income, namely the United States. The United States represent the only major economy that so far did not commit to the CRS. In this analysis, we show that over our sample period cross-border deposits in the United States increase after the CRS becomes effective between 10.9% (held by residents of non-tax havens) to 17.7% (held by residents of tax havens). We are aware of the threat of confounding factors. To reduce this threat as far as possible, we carefully draft our empirical analyses. First, by employing a well-established empirical model for estimation of cross-border tax evasion and by conducting event study analyses. Second, we implement a demanding fixed effects structure going beyond that used in much of prior research. Adding residence-country quarter-year fixed effects enables us to control for residence country-specific demand-side shocks to cross-border deposits. Third, we limit our analysis to a narrow period (2014-2017) to avoid that other major events may influence our outcomes. Last, we test the robustness of our results in alternative samples and specifications (e.g., split sample and alternative relocation countries).

We believe that our study contributes substantially to the current international debate on cross-border tax evasion. A key finding is that the CRS leads to a reduction of deposits in traditional tax havens of USD 46 billion at the lower bound. However, our findings also suggest that the U.S. adoption of the CRS would remove a major means of avoiding information exchange.

In our study we make use of bank deposit data held by individuals and entities in tax havens. Although bank deposits are a sound proxy for analysing movements of wealth and related income held in tax havens, they fall under a special category of assets, i.e. they constitute a very mobile asset class. Thus, our results may not transpose to other forms of assets held in tax havens which are less easily moved across tax havens. This is especially true in the context of firms' tax planning activities. In particular, firms' ability to substitute across tax havens is not very elastic as shown by Suárez Serrato and Garrett (2019). It is therefore important to distinguish the use of tax havens for individual tax evasion as studied here and the one used for corporate tax planning as studied in Desai et al. (2006), Hines et al. (2016) and Suárez Serrato and Garrett (2019). Since the determinants of the use of tax havens for corporate tax planning differ substantially, we believe it is important to highlight that we do not take a stance on this topic.

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Appendix A. Additional Event Studies on Analysis in the Main Test

Appendix A.1. Dropping Countries Affected by EU Savings Directive - Event Study

We conduct an event study analysis on the difference in difference results reported in Table 5, i.e. on the CRS effect on those countries that where not subject to the European Savings Directive. Figure A.4 plots the coefficients, which each mark the change in cross-border deposits held in tax havens versus non-tax havens before and after the CRS treatment event date (t=0) only considering residence of non-EU countries. We detect no pre-trends

and an immediate and continuous reduction in the volume of cross-border deposits in tax havens post CRS.

Appendix A.2. Split Sample Test - Event Studies

In Figure A.5 we report graphically results from event-study regressions on the split sample tests corresponding to Table 6. First in Panel (a), we test the effect of the CRS on cross-border deposits held by residents of EU and OECD countries in the United States, dropping both non-tax havens and tax havens from our sample. Second in Panel (b), we test the effect of the CRS on cross-border deposits held by residents of EU and OECD countries in our control group, i.e. non-tax havens, dropping the United States and tax havens from our sample. In Panel (c) we combine the data sets from Panel (a) and (b) testing the effect of the CRS on cross-border deposits held by residents of EU and OECD countries in the United States versus non-tax havens. Overall, results confirm flat pre-trends and a statistically significant increase in cross-border deposits in the United States post CRS. Furthermore, the placebo test shows no time trends neither before nor around the CRS-post period in deposits held in our control group countries.

Appendix A.3. Use of Shell Companies - Event Studies

In Figure A.6 we report graphically results from event-studies on the tests on the change in deposits held through tax havens and the United States post CRS corresponding to Table 9. First in Panel (a), we test the effect of the CRS on cross-border deposits held through tax havens in other tax havens. Second in Panel (b), we test the effect of the CRS on cross-border deposits held through tax havens in the United States. Third, in Panel (c), we test the effect of the CRS on cross-border deposits held through the United States in non-tax havens. Overall, the graphs show flat pre-trends and changes in line with our difference in difference estimates.

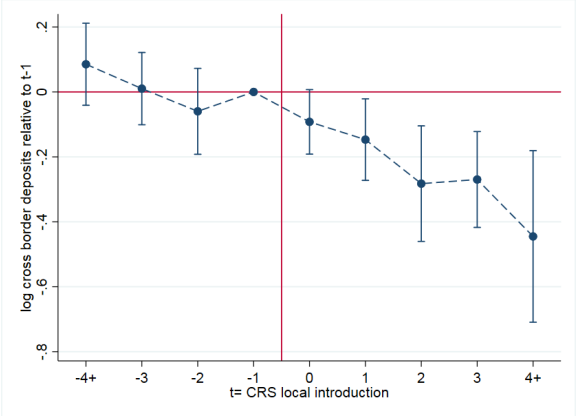
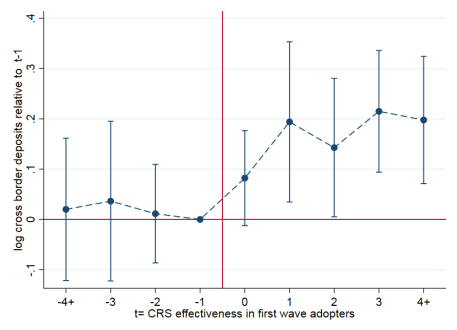
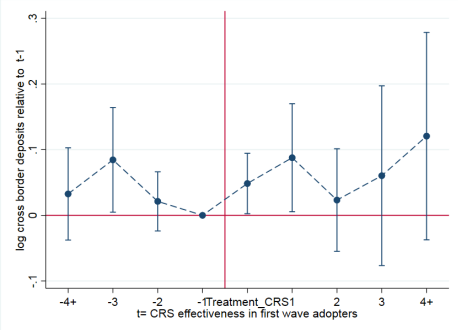


Figure A.4: Event Study Test on Non-EU Residents' Change in Cross-Border Deposits Upon CRS

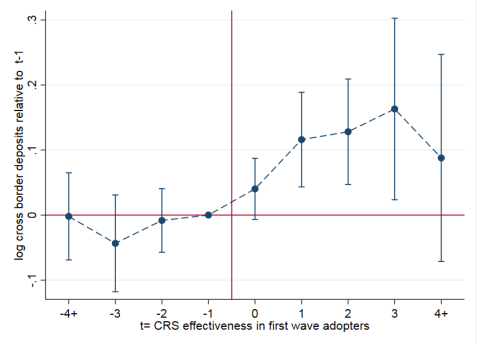
Notes: The figure charts the coefficients, which each mark the change in cross-border deposits held in the United States versus non-tax havens around the CRS event dates (in event time). Cross-border deposits of residents in EU countries are excluded. We estimate equation 1. The plotted coefficients are the interactions of the tax havens indicator with 8 separate indicator variables, each marking one quarter over the sample period relative to the quarter before the CRS event date (t=0). We bin the treatment indicators beyond t-4 to t+4 at the endpoints and omit the indicator for period t-1. It, therefore, serves as a benchmark and has a coefficient value of zero (and no confidence interval). The figure plots the coefficient estimates of the 8 quarters together with their 95% confidence intervals for the staggered CRS event date at the deposit country level. We use the log of cross-border deposits as the dependent variable and residence-country quarter-year fixed effects as well as ordered country-pair fixed effects. We cluster at deposit-country level.



(a) Deposit time-trends in the United States



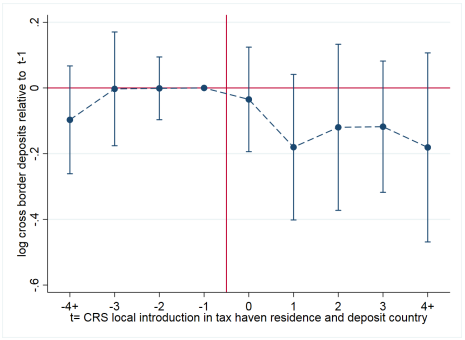
(b) Deposit time-trends in Non-Tax Havens



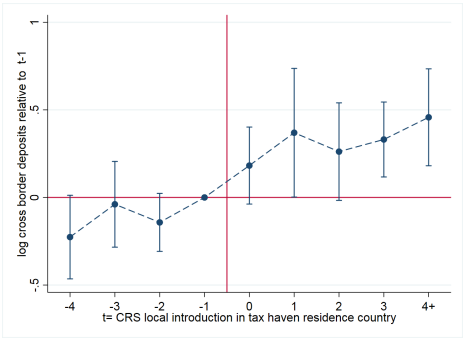
(c) Deposits in the United States vs. Non-Tax Havens Countries

Figure A.5: Event Study Graphs on Sample Split Test of Relocation Upon CRS Effectiveness

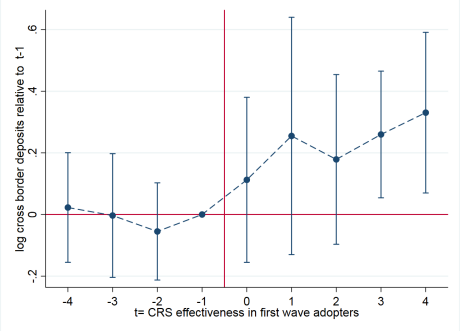
Notes: Panel (a) charts the coefficient estimates of cross-border deposits held by residents of EU and OECD countries in the United States around the CRS event date (in event time) and Panel (b) charts the coefficient estimates of cross-border deposits held by residents of EU and OECD countries in non-tax havens around the CRS event date (in event time). Panel (c) charts the coefficients, which each mark the change in cross-border deposits held in the United States versus non-tax havens around the CRS event dates (in event time). Cross-border deposits in the tax-havens are excluded in all regressions. In Panel (a) to (c), we estimate a variant of equation 1. The plotted coefficients are the interactions of the US or tax havens indicator, respectively, with 8 separate indicator variables, each marking one quarter over the sample period relative to the quarter before the CRS event date (t=0). We bin the treatment indicators beyond t-4 to t+4 at the endpoints and omit the indicator for period t-1. It, therefore, serves as a benchmark and has a coefficient value of zero (and no confidence interval). The figure plots the coefficient estimates together with their 95% confidence intervals. The CRS treatment event date is the adoption of the CRS in the first adoption wave (1 quarter of 2016). We use the log of cross-border deposits as the dependent variable and add residence-country quarter-year fixed effects as well as ordered country-pair fixed effects. We cluster at deposit-country level.



(a) Tax Haven to Tax Haven Deposit Changes after the CRS



(b) Tax Haven to US Deposit Changes after the CRS



(c) US to Non Tax Haven Deposit Changes after the CRS

Figure A.6: Event Studies - Change in Cross-Border Deposits held through Tax Havens and the United States

Notes: Panel (a) charts the coefficients, which each mark the change in cross-border deposits held by residents of tax havens in tax havens havens around the CRS treatment dates (in event time), Panel (b) charts the coefficients, which each mark the change in cross-border deposits held by residents of the United States in tax havens around the CRS treatment dates (in event time) and Panel (c) charts the coefficients, which each mark the change in cross-border deposits held by residents of the United States in non-tax havens around the CRS event dates (in event time). We estimate equation 6, 7 and 8, respectively, but replace the single coefficient of the post-CRS indicator with 8 separate indicator variables, each marking one quarter over the sample period relative to the quarter before the CRS treatment event date (t=0). We bin the treatment indicators beyond t-4 to t+4 at the end points and omit the indicator for period t-1. It, therefore, serves as a benchmark and has a coefficient value of zero (and no confidence interval). The figure plots the coefficient estimates together with their 95% confidence intervals. The CRS treatment event date in Panel (a) is the interaction of CRS passing into national law in the tax haven residence country with the tax haven deposit country. In Panel (b) the CRS treatment event date is the passing of the CRS into national law in the tax haven residence country. In Panel (c) the CRS treatment event date is the adoption of the CRS in the first adoption wave (1 quarter of 2016). We use the log of cross-border deposits as the dependent variable and residence-country quarter-year fixed effects as well as ordered country-pair fixed effects. We cluster at deposit-country level.

Appendix B. Robustness Checks Using Different Samples

Appendix B.1. Baseline Results using a Balanced Sample

In our main analysis we use an unbalanced sample in order to preserve the maximum number of observations possible. In a robustness check, we re-run our main regression analysis using a balanced sample. This leads to the loss of around 9% of the observations.

First, we present the results from event-study regressions on the main tests using a balanced sample in Figure B.7. Panel (a) plots the coefficients, which each mark the change in cross-border deposits held in tax havens versus non-tax havens before and after the CRS treatment event date (t=0). The event study results on the effect of the CRS on deposit from EU and OECD residents in tax havens using the balanced sample confirm previous results. As is visible in Panel (a), in the pre-treatment period the coefficients lie close to zero and are statistically insignificant, while in the post-treatment period, the effect size increases in absolute magnitude over time. In Panel (b), we test the effect of the CRS on cross-border deposits held by residents of EU and OECD countries in the United States versus those held in non-tax havens. Similarly in Panel (b), the results on the effect of the CRS on deposit from EU and OECD residents in the United States using the balanced sample confirm previous results. We detect no pre-trends and an increase in cross-border deposits in the United States in the post-treatment period. In Table B.10 we show that the difference-in-difference results are essentially unchanged to those in our main tests in Table 4.[48] Thus, we can rule out that our tests suffer from selection bias due to unbalanced sampling.

Appendix B.2. Baseline Results using a Reduced Control Group

One concern with our choice of the control group might be that concurrent changes in the depository countries may be driving the observed effects. Two concurrent events may be critical in this regard. First, Switzerland is likely to have experienced a shock to its cross-border deposits following the first quarter of 2015 when the Swiss central bank abandoned the 1.20 francs per euro cap.[49] Second, the Italian banking crisis surfacing again in the last quarter of 2016 is likely to have caused a negative shock on deposits held in Italian bank accounts.[50] To rule out that the effects, which we measure, are influenced by these countries' financial turmoil, we rerun our main tests in Table B.11 on a reduced sample excluding Switzerland and Italy as deposits countries. The results marginally lose significance as the sample

[48] The coefficient on the U.S.-interaction has a p-value of 10.0 %.

[49] See e.g., https://www.reuters.com/article/us-swiss-snb-cap/swiss-central-bank-stuns-market-with-policy-u-turn-idUSKBN0KO0XK20150115, accessed on 14.01.2020

[50] See e.g., https://www.theguardian.com/commentisfree/2016/nov/28/italy-failing-banks-new-japan, accessed on 14.01.2020

size decreases, but remain fundamentally unchanged suggesting that the two events in Switzerland and Italy are not influential on our main outcomes.

Appendix B.3. GDP Placebo Test

In the first part of our analysis a potential concern in our staggered design is that the CRS treatment dates at the deposit country level are non-random but respond to changes in local economic conditions, which could also affect cross-border deposits. We can test directly for this violation of the identifying assumptions by using local economic outcomes as left-hand side variable in the event study design. Since GDP data is available at the same aggregation level (country) and at the same frequency (quarterly) as the cross-border deposits data from the BIS and for most of the tax havens in our sample,[51] we consider it the best proxy of economic cycles for the purpose of our test. Figure B.8 depicts the event studies for GDP as left-hand side variable. Investigating the changes in tax havens vs. non tax havens, we find flat pre-trends in GDP and only slight and insignificant increases in GDP after treatment (Panel (a)). This is in line with the assumption that the passing of the CRS law at national level is driven by political factors rather than economic cycles, thus, alleviating the concern that the effects we observe are driven by economic factors rather than the CRS.

In the second part of the analysis, the U.S. setting, a large concern is that the effect we observe is driven by cross-border deposits reacting to economic conditions instead of the CRS, since in contrast to the tax havens in our sample, the United States is an important economic center. GDP trends in the United States relative to in the control group countries are flat around treatment, with only limited evidence of pre-trends. Importantly we observe no concurrent increase in GDP to CRS treatment (Panel (b)).

Appendix B.4. Baseline Results Excluding the United States as Residence Country and as Deposit Country

In our main test we control for the fact that post-CRS cross-border deposits might be relocated to the United States. Still the inclusion of the United States in our tests, where we measure the effect of the CRS on cross-border deposits located in tax havens, might cause some SUTVA-violations. We drop the United States from our sample of residence countries and from our sample of deposit country in a robustness test in which we re-run the same tests as in Table 4 and Table 5. Results from rerunning Table 4 reported in Table B.12 are mostly unchanged, namely we detect a statistically significant reduction of cross-border deposit in tax havens post CRS. In the specification using as post-CRS treatment indicator CRS effectiveness in the first wave adopters our coefficient is not significant, i.e. the p-value is

[51] We lack data for Guernsey, Isle of Man and Jersey.

13%. This might be due to a lack of power, since our sample is reduced by approximately 10%. Results in Table B.13 from rerunning Table 5 confirm the results in Table 5, namely, excluding countries already affected by the Saving Directives leads to larger coefficient estimates on the CRS effect on cross-border deposits.

Appendix B.5. Baseline Results - Placebo Test Using only EU member states as residence countries

In this Appendix, we extend the test we present in Table 5. We perform a placebo tests where only residents of EU member states are considered. In 2003 the Savings Directive became effective and it introduced a multinational AEOI programme within the EU member states. The goal of this Council Directive was to fight underreported interest income by giving to participating countries the option to either exchange bank account information on foreign EU residents or to levy a withholding tax on interest income owned by each reportable individual. Thus, one could expect that within the EU, an overall zero effect of the CRS is detected. As visible in Table B.14, this is what we find regardless of the selected CRS post period indicator (namely, the passing of the CRS law in the deposits country, when the CRS becomes effective in the deposits country, or when the CRS becomes effective for the first waive adopters). P-values range between 40 and 60%, and all three coefficients are small in size.

Appendix B.6. Baseline Results - Placebo Test Using only Control Group

In this Appendix, we extend the placebo test we present in Table 6, in which we perform a time trend test of changes in cross-border deposit located in our control sample (non-tax havens and non-U.S. deposit countries). We consider as post period first the passing of the CRS law in the deposits country, second when the CRS becomes effective in the deposits country, and third when the CRS becomes effective for the first waive adopters. Regardless of the selected post period, in Table B.15, we document no statistically significant change in cross-border deposit held in our control sample post-CRS (p-values range between 30 and 90%), and all three coefficients are small in size.

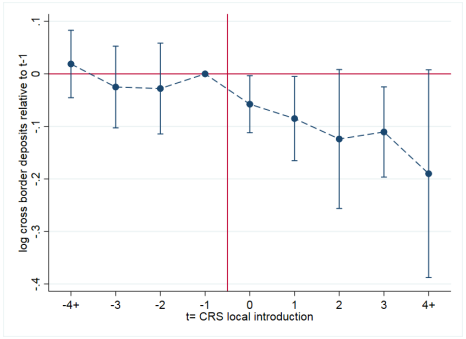
Appendix B.7. Use of Shell Companies - Placebo Test substituting United Kingdom for United States

As further placebo test for whether the changes we observe in the United States are driven by concurrent economic shocks to the financial system, we run a placebo analysis on our shell companies test, where we redo all tests in the shell companies section of our paper which involve the United States replacing the United States with the United Kingdom. We chose the United Kingdom as another very relevant financial center. Results are reported in Table B.16. We find no increase in tax haven deposits in the United Kingdom

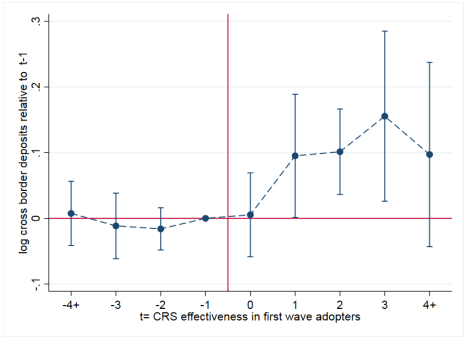
after passing of the CRS into national law (Column 1) or alternatively after the CRS is effective in the first wave adopter countries (Column 2). We also find no evidence of "round-tripping" through the United Kingdom (Column 3). Overall, coefficient estimates are insignificant at any conventional significance level and are negative in sign. Thus, at best there was a reduction in cross-border deposits held in the United Kingdom in the CRS-post period.

Appendix B.8. Baseline Results Excluding First Waive Adopters

The CRS is adopted by most countries around the so-called first adoption wave. The concentration of CRS adoption in many countries in a short time period (most countries adopt between September 2015 and April 2016) allows us to approximate the treatment period for the relocation of deposits to non-CRS participating jurisdictions, here the United States. It also means that there are fewer available control pairs and periods and the identification becomes closer to a time series comparison of flows between non-tax havens and tax havens country-pairs, which may make the results susceptible to confounding factors in the time series that change the relative desirability of tax haven countries as deposit locations. To rule out that other factors in this main adoption period are driving our results we drop those countries in which the CRS became effective between September 2015 and April 2016 and rerun our baseline test (as specified in equation 2) and the corresponding event study (equation 1), for which results are reported in Table B.17. The effect size and dynamics are unchanged in the event study and in the difference in difference analysis. However, potentially due to the reduced sample size resulting in only 9 deposit country clusters (standard errors are clustered at the deposit country level) and only two treated tax havens (Switzerland and Hong Kong), p-values of the coefficient of interest are only around 43% now. We also rerun our shell company tests on the effect of the CRS on tax haven-tax haven deposits as well as tax haven-US deposits together with the corresponding event studies dropping those tax haven countries from the sample that adopt the CRS between September 2015 and April 2016. Table B.18 displays the difference in difference results which are largely the same as in the baseline with p-values above 5% throughout. The event studies corresponding to Table B.17 Column 1 (main results) are reported in Panel (a) of Figure B.9 and corresponding to Table B.18 (shell company test) are reported in Panel (b) and (c) of Figure B.9 respectively. They show the expected dynamics without pre-trends.



(a) Reaction to the CRS in Tax Havens



(b) Relocation behavior after the CRS

Figure B.7: Event Studies - Balanced Sample

Notes: Panel (a) charts the coefficients, which each mark the change in cross-border deposits held in tax havens versus non-tax havens around the CRS event dates (in event time) using a balanced sample. We estimate equation 1. The plotted coefficients are the interactions of the tax havens indicator with the 8 separate indicator variables, each marking one quarter over the sample period relative to the quarter before the CRS event date (t=0). Panel (b) charts the coefficients, which each mark the change in cross-border deposits held in the United States versus non-tax havens around the CRS event dates (in event time) using a balanced sample. We estimate equation 3. The plotted coefficients indicate the interactions of the U.S. indicator with 8 separate indicator variables, each marking one quarter over the sample period relative to the quarter before the CRS treatment event date (t=0). In both equations we bin the treatment indicators at the endpoints and omit the indicator for period t-1. It, therefore, serves as a benchmark and has a coefficient value of zero (and no confidence interval). Panel (a) and (b) plot the coefficient estimates together with their 95% confidence intervals. We set the log of cross-border deposits as the dependent variable and add residence-country quarter-year fixed effects as well as ordered country-pair fixed effects. We cluster at deposit-country level.

Table B.10: Change in Cross-Border Deposits after the CRS - Balanced Sample

VARIABLES	(1) Country Introduction LogDeposits	(2) Country Effectiveness LogDeposits	(3) First Adoption Wave LogDeposits	(4) First Adoption Wave LogDeposits
PostCRS * Havens	-0.122**	-0.118**	-0.129**	-0.123*
	(0.0513)	(0.0541)	(0.0618)	(0.0644)
PostCRS * US				0.0952
				(0.0559)
Observations	10,968	10,968	10,968	10,968
R-squared	0.973	0.973	0.973	0.973
Country-Pair FE	YES	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of regression model 2 and 4 respectively using a balanced sample. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit country j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit location, in Column 2, for the effective date of the CRS in the deposit location, and, in Column 3 and 4, for the period of the first wave of information exchange. Havens is a dummy taking value one when the deposit country j is a tax haven. US is a dummy equal to one when the deposit country j is the United States. All regressions include ordered country-pair and residence country x quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at deposit-country level.

Robust standard errors in parentheses

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

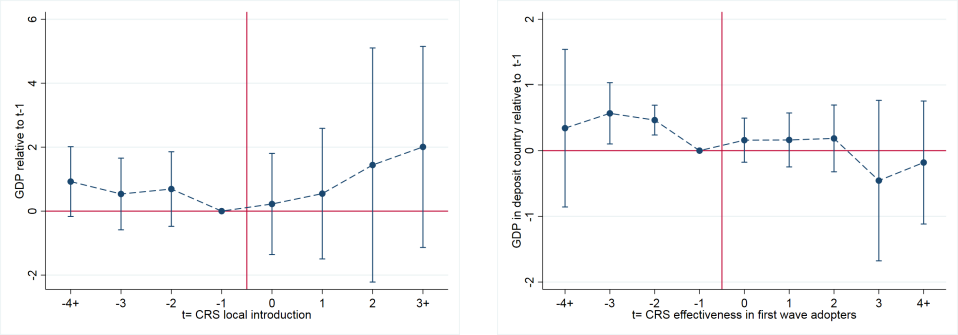
Table B.11: Change in Cross-Border Deposits after the CRS - Using a Reduced Control Group

VARIABLES	(1) Country Introduction LogDeposits	(2) Country Effectiveness LogDeposits	(3) First Adoption Wave LogDeposits	(4) First Adoption Wave LogDeposits
PostCRS * Havens	-0.115*	-0.115*	-0.121*	-0.114
	(0.0587)	(0.0612)	(0.0674)	(0.0699)
PostCRS * US				0.113*
				(0.0592)
Observations	10,967	10,967	10,967	10,967
R-squared	0.971	0.971	0.971	0.971
Country-Pair FE	YES	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of regression model 2 and 4 respectively excluding cross-border deposits held in Italian and Swiss banks. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit country j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit location, in Column 2 and 3, for the period of the first wave of information exchange, respectively depending on the CRS specification as reported above the regression results in the table. Havens is a dummy taking value one when the deposit country j is a tax haven. US is a dummy equal to one when the deposit country j is the United States. All regressions include ordered country-pair and residence country x quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at deposit-country level.

Robust standard errors in parentheses

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



(a) Tax Haven GDP

(b) United States GDP

Figure B.8: Event Studies: Local Economic Effects

Notes: The figure charts the coefficient estimates of GDP changes in tax havens (Panel a) and the United States (Panel b) vs. non-tax haven deposit countries around the CRS event date (in event time). We estimate 1 and 3, respectively, using as the dependent variable the quarterly GDP in the deposit country. The plotted coefficients are the interaction terms of the tax havens/US indicator with an indicator for each quarter over the sample period relative to the quarter before the CRS event date (t=0). We bin the treatment indicators at the endpoints and omit the indicator for period t-1. It, therefore, serves as a benchmark and has a coefficient value of zero (and no confidence interval). The figure plots the coefficient estimates together with their 95% confidence intervals. We use ordered country-pair fixed effects. We cluster at deposit-country level.

Table B.12: Change in Cross-Border Deposits after the CRS - Excluding the United States as Residence Country and as Deposit Country

	(1)	(2)	(3)
	Country Introduction	Country Effectiveness	First Adoption Wave
VARIABLES	LogDeposits	LogDeposits	LogDeposits
PostCRS * Havens	-0.110**	-0.111*	-0.101
	(0.0531)	(0.0547)	(0.0646)
Observations	11,098	11,098	11,098
R-squared	0.970	0.970	0.970
Country-Pair FE	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of regression model 2 and 4 respectively, excluding deposits owned by US residents as well as excluding deposits located in the US. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit location j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit location, in Column 2, for the effective date of the CRS in the deposit location, and, in Column 3 and 4, for the period of the first wave of information exchange. Havens is a dummy taking value one when the deposit location j is a tax haven. US is a dummy equal to one when the deposit country j is the United States. All regressions include ordered country-pair and residence country x quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at the deposit-country level.

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

Table B.13: The CRS Effect Net of the European Savings Directive - Excluding the United States as Residence Country and as Deposit Country

	(1)	(2)	(3)
	Country Introduction	Country Effectiveness	First Adoption Wave
VARIABLES	LogDeposits	LogDeposits	LogDeposits
PostCRS * Offsh	-0.262***	-0.265***	-0.305***
	(0.0868)	(0.0929)	(0.0989)
Observations	3,052	3,052	3,052
R-squared	0.960	0.960	0.960
Country-Pair FE	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of regression model 2 and 4 respectively, excluding cross-border deposits owned by EU and U.S. residents as well as excluding deposits located in the United States. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit location j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit location, in Column 2, for the effective date of the CRS in the deposit location, and, in Column 3 and 4, for the period of the first wave of information exchange. Havens is a dummy taking value one when the deposit location j is a tax haven. US is a dummy equal to one when the deposit country j is the United States. All regressions include ordered country-pair and residence country x quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at the deposit-country level.

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

Table B.14: Change in Cross-Border Deposits after the CRS for EU Residents - Placebo Test

	(1)	(2)	(3)
	Country Introduction	Country Effectiveness	First Adoption Wave
VARIABLES	LogDeposits	LogDeposits	LogDeposits
PostCRS * Havens	-0.0518	-0.0513	-0.0271
	(0.0632)	(0.0655)	(0.0688)
Observations	8,394	8,394	8,394
R-squared	0.973	0.973	0.973
Country-Pair FE	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of regression model 2 including only cross-border deposits owned by EU residents. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit location j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit country, in Column 2, for the effective date of the CRS in the deposit country, and, in Column 3, for the period of the first wave of information exchange. Included residence countries are Austria, Belgium, Bulgaria, Croatia, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden, and the UK. Havens is a dummy taking value one when the deposit country j is an tax haven. All regressions include ordered country-pair and residence country x quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at deposit-country level.

Robust standard errors in parentheses

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

Table B.15: Change in Cross-Border Deposits after the CRS held in the Control Sample - Placebo Test

	(1)	(2)	(3)
	Country Introduction	Country Effectiveness	First Adoption Wave
VARIABLES	LogDeposits	LogDeposits	LogDeposits
PostCRS	0.0365	-0.00521	0.0490
	(0.0985)	(0.112)	(0.0550)
Observations	8,482	8,482	8,482
R-squared	0.968	0.968	0.966
Country-Pair FE	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	NO
Clustering	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the time-trend test estimates for the change in the volume of cross-border deposits after versus before the CRS, excluding deposits owned by residents of the United States. The dependent variable is the log of cross-border deposits held by residences of country i in banks of deposit country j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit location, in Column 2, for the effective date of the CRS in the deposit location, and, in Column 3 and 4, for the period of the first wave of information exchange. All regressions include ordered country-pair and residence country x quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at deposit-country level.

Robust standard errors in parentheses

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

Table B.16: Shell Company Test on United Kingdom - Placebo Test

VARIABLES	(1) Tax Haven to UK LogDeposits	(2) Tax Haven to UK LogDeposits	(3) UK to Non-Tax Haven Log_VLD
PostCRSResHaven	-0.0233		
	(0.122)		
PostCRSFirstWave		-0.0878	-0.0347
		(0.0656)	(0.106)
Observations	232	232	242
R-squared	0.990	0.990	0.952
Quarter-Year FE	YES	NO	NO
Country-Pair FE	YES	YES	YES
Clustering	Tax Haven Residence Country	Tax Haven Residence Country	Non-Tax Haven Deposit Country

Notes: This table reports the DiD estimates of regression model 7 and 8 respectively. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit country j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. In Column 1 and 2, the sample is restricted to tax havens as residence country and the United Kingdom as deposit country. In Column 3, the sample is restricted to the United Kingdom as residence country and non-tax havens as deposit country. All columns include country-pair fixed effects, but only Columns 1 includes quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at residence-country level in Column 1 and 2 and at deposit-country level in Column 3.

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

Table B.17: Change in Cross-Border Deposits after the CRS - Excluding CRS Adopters Around First Adoption Wave

VARIABLES	(1) Country Introduction LogDeposits	(2) Country Effectiveness LogDeposits
PostCRS * Havens	-0.124	-0.132
	(0.149)	(0.158)
Observations	3,845	3,845
R-squared	0.970	0.970
Country-Pair FE	YES	YES
Residence-Quarter-Year FE	YES	YES
Clustering	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of regression model 2. The sample is adapted from the baseline estimation to exclude deposit countries that passed the CRS into national law after September 2015 up until April 2016. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit location j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit location and, in Column 2, for the effective date of the CRS in the deposit location. Havens is a dummy taking value one when the deposit location j is a tax haven. All regressions include ordered country-pair and residence country x quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at the deposit-country level.

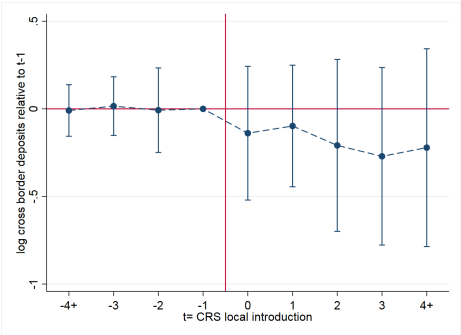
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.18: Change in Cross-Border Deposits Held Through Tax Havens or the US after the CRS - Excluding CRS Adopters Around First Adoption Wave

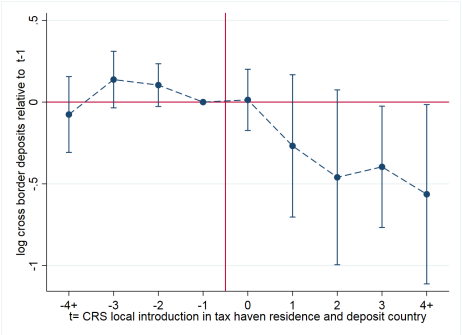
	(1)	(2)
	Tax Haven to Tax Haven	Tax Haven to US
VARIABLES	LogDeposits	LogDeposits
PostCRSResHaven	0.0246	0.393***
	(0.0730)	(0.110)
PostCRSDepHaven	0.0309	
	(0.0554)	
PostCRSResHaven* PostCRSDepHaven	-0.240**	
	(0.0959)	
Observations	1,065	182
R-squared	0.973	0.982
Quarter-Year FE	YES	YES
Country-Pair FE	YES	YES
Clustering	Tax Haven Country-Pair	Tax Haven Residence Country

Notes: This table reports the DiD estimates of regression model 6 and 7 respectively. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit country j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. In Column 1, the sample is restricted to tax havens as residence and deposit country. In Column 2 the sample is restricted to tax havens as residence country and the United States as deposit country. All columns include quarter-year fixed effects and country-pair fixed effects. Cluster robust standard errors in parentheses, clustered at country-pair level in Column 1 and at residence-country level in Column 2.

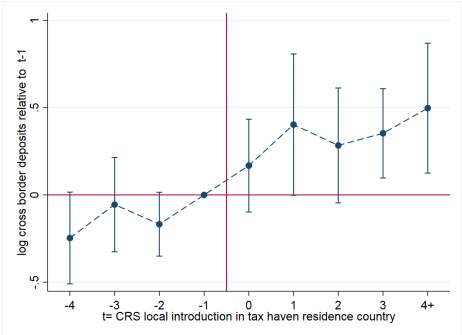
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1



(a) Baseline Estimation: Effect of the CRS on Tax Haven Deposits Held by Non-Tax Haven Residents



(b) Tax Haven Deposits Held by Tax Haven Residents



(c) US Deposits Held by Tax Haven Residents

Figure B.9: Event Studies - Excluding CRS Adopters Around First Adoption Wave

Notes: Panel (a) charts the coefficients, which each mark the change in cross-border deposits held in tax havens versus non-tax havens around the CRS event dates (in event time), Panel (b) charts the coefficients, which each mark the change in cross-border deposits held by residents of tax havens in tax havens havens around the CRS treatment dates (in event time), Panel (c) charts the coefficients, which each mark the change in cross-border deposits held by residents of the United States in tax havens around the CRS treatment dates (in event time). In Panel (a), (b), (c), cross-border deposits from countries that introduced the CRS between September 2015 and April 2016 are dropped. In Panel (a), we estimate equation 1. The plotted coefficients are the interaction of the tax havens indicator with 8 separate indicator variables, each marking one quarter over the sample period relative to the quarter before the CRS event date (t=0). In Panel (b) and (c), we estimate equation 6 and 7, respectively, but replace the single coefficient of the post-CRS indicator with 8 separate indicator variables, each marking one quarter over the sample period relative to the quarter before the CRS treatment event date (t=0). We bin the treatment indicators at the end points and omit the indicator for period t-1. It, therefore, serves as a benchmark and has a coefficient value of zero (and no confidence interval). The Figure plots the coefficient estimates of the 8 quarters together with their 95% confidence intervals. The CRS treatment event date in Panel (a) is the CRS passing into national law in the deposit country, in Panel (b) the interaction of CRS passing into national law in the tax haven residence country with the tax haven deposit country and in Panel (c) the CRS treatment event date is the passing of the CRS into national law in the tax haven residence country.

Appendix C. Robustness Checks Using an Extended Sample Period

In our main test we restrict the sample to the period from the last quarter of 2014 to the third quarter of 2017 in order to exclude possible confounding impacts of the big wave of bilateral TIEAs signatures in 2008-2011, the introduction of FATCA in 2010-2013 as well as the 2018 U.S. Tax Cuts and Job Act announced in Fall 2017. Nevertheless, we test here how our results change if we extend our sample. The extended sample period ranges from the first quarter of 2010 to the third quarter of 2018. We start by replicating Table 4 in the main text using the extend sample period and we support the analysis with the same event study analysis as the one in our main text but for the extended period.

To begin with, in the corresponding difference in difference analyses, reported in Table C.19, the direction of the effects is unchanged. However, the effect sizes increased. The event studies suggest that this may be due to the fact, that we now also capture some of the effect of the introduction of earlier bilateral treaties and FATCA in our pre-period. Panel (a) of Figure C.10 plots the coefficients using the same event study design as Figure 2 in our main text. While estimates are noisier in the extended sample, we find no pre-trends (coefficients are insignificant at the 5% level) and a statistically significant reduction in cross-border deposits upon the passing of the CRS into national law. These results are suggesting that the CRS effect on deposits held by EU and OECD residents in our tax haven sample was stronger than previous initiatives on the exchange of information in tax matters, e.g. FATCA and TIEAs. Next in Panel (b) of Figure C.10, we present the results from the event study on cross-border deposits relocation to the United States. The event study suggests that, the effect of FATCA on U.S. cross-border deposits was larger than the effect of the CRS. In Panel (b) of Figure C.10, we see a clear increase in cross-border deposits around FATCA introduction (from early 2012).

In Table C.20, we report the results of our test on the use of shell companies in this extended sample. Results on the effect of the CRS on deposits in tax havens held through tax havens are reported in Column 1 and 2, results on the effect of the CRS on deposits held in the United States through tax havens are reported in Column 3 and 4, and results on the effect of the CRS on deposits held in non-tax havens through the United States are reported in Column 5. Overall, the effects we find are much smaller in the longer term in Column 1 and 5. The corresponding event studies reported in Figure C.11. are much noisier in the long run.

With regard to tax haven to tax haven deposits, our main observation is that, while we still observe in the non-staggered design on average a decrease in cross-border deposits in the haven-to-haven constellation (Table C.20 Column 2), the event study graphs corresponding to the staggered

specification are noisy and show evidence of pre-trends (Figure C.11, Panel (a)). In particular, we see a decline in haven-to-haven deposits in the pre-treatment period. It is possible that pre-trends are biased by FATCA, which should have had a similar effect on haven-to-haven deposits as the CRS.

The main observation on the test involving tax haven to United States deposits is, that the estimates of the effect sizes do not change much in the long term window, when compared to our main specification (Table C.20 Column 3 and 4), however, in the staggered design the effect is no-longer significant. The corresponding event study (Figure C.11, Panel (b)) shows evidence of pre-trends in the years leading up to the passing of the CRS law in the tax haven.

The main observation on the extended sample results of our test of the change in United States to non-tax haven deposits is that we see in both the difference in difference table (Table C.20, Column 5) and the event study that this effect does not hold in a longer term window. Although there is an immediate increase in cross-border deposit holdings following the CRS, this increase exists only relative to the recent pre-treatment period (up until t-6).

Overall, it might well be the case that in the long-term window, our results are impacted by other initiatives such as FATCA that addressed cross-border tax evasion, as well as economic activity. Yet, also our identification in the shell company analysis is weaker than in our main tests. First, we do not have a control group in these tests. Second cross-border deposits between the United States and non-tax havens are much more likely to be affected by cross-border real economic activity. This is particularly critical in the longer term analysis.

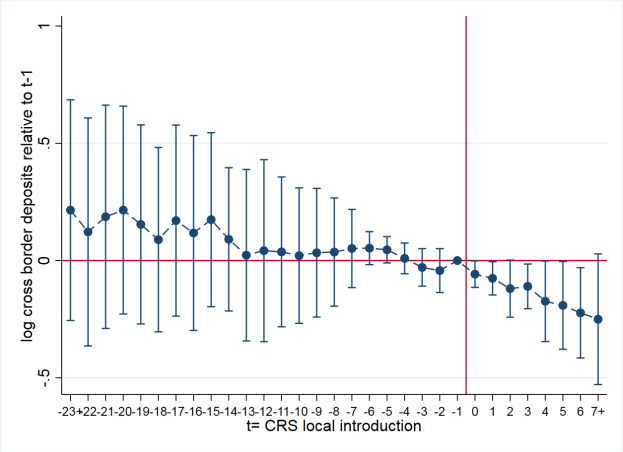
Table C.19: Change in Cross-Border Deposits after the CRS - Extended Sample Period

	(1)	(2)	(3)	(4)
	Country Introduction	Country Introduction	First Adoption Wave	First Adoption Wave
VARIABLES	Log_VLD	Log_VLD	Log_VLD	Log_VLD
PostCRS * Havens	-0.327**	-0.326**	-0.254**	-0.235*
	(0.120)	(0.122)	(0.123)	(0.127)
PostCRS * US				0.296***
				(0.101)
Observations	30,859	30,859	30,859	30,859
R-squared	0.944	0.944	0.943	0.943
Country-Pair FE	YES	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country

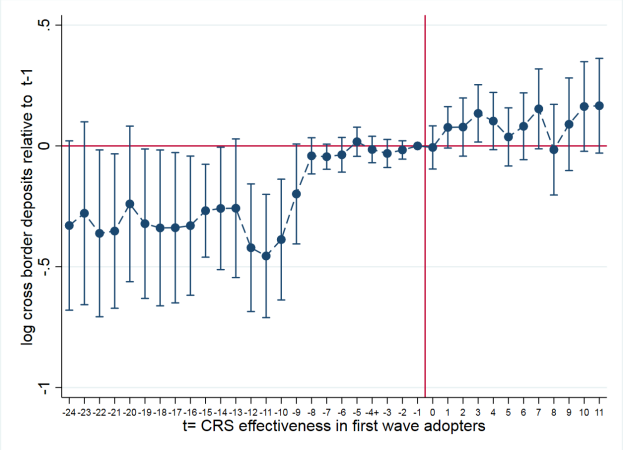
Notes: This table reports the DiD estimates of regression model 2 and 4 respectively. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit location j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the first quarter of 2010 to the fourth quarter of 2018. PostCRS is a dummy, which equals one in the period after the introduction of the CRS in the deposit location or the period of the first wave of information exchange, respectively, depending on the CRS specification reported above the regression results. Havens is a dummy taking value one when the deposit location j is a tax haven. US is a dummy equal to one when the deposit country j is the United States. All regressions include ordered country-pair and residence country x quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at the deposit-country level.

Robust standard errors in parentheses

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



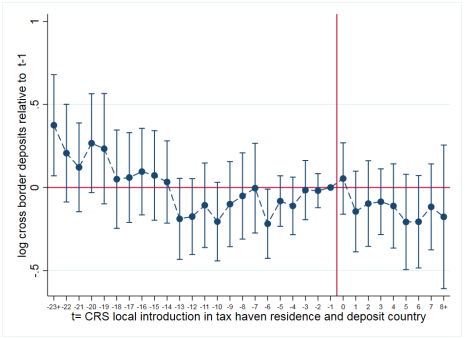
(a) Event Study Test of Reaction to the CRS in Tax Havens - Extended Sample Period



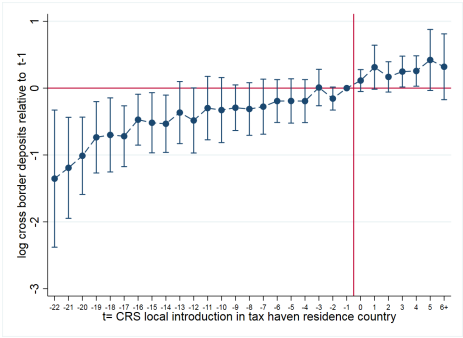
(b) Event Study Test of Relocation Behavior after the CRS - Extended Sample Period

Figure C.10: Event Studies –Extended Sample

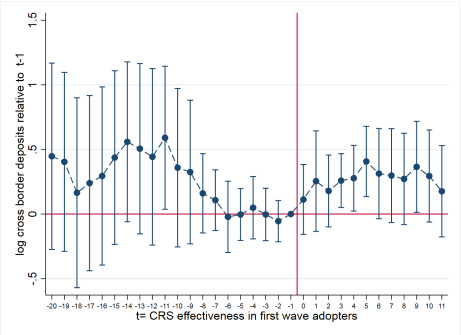
Notes: Panel (a) charts the coefficients, which each mark the change in cross-border deposits held in tax havens versus non-tax havens around the CRS event dates (in event time) using an extended sample period. Panel (b) charts the coefficients, which each mark the change in cross-border deposits held in the United States versus non-tax havens around the CRS event dates (in event time) using an extended sample period. We estimate equation 1 (Panel (a)) and equation 3 (Panel (b)) only that now the considered period ranges from the first quarter 2010 to the last quarter 2018. For Panel (a), we bin the interacted treatment indicators at the endpoints. In Panel (a) and (b), we omit the indicator for period t-1. It, therefore, serves as a benchmark and has a coefficient value of zero (and no confidence interval). The figure plots the coefficient estimates of the 36 quarters together with their 95% confidence intervals for the staggered CRS event date at the passing of the CRS law in the deposit country. We use the log of cross-border deposits as the dependent variable and residence-country quarter-year fixed effects as well as ordered country-pair fixed effects. We cluster at deposit-country level.



(a) Tax Haven to Tax Haven Deposit Changes after the CRS



(b) Tax Haven to United States Deposit Changes after the CRS



(c) United States to Non-Tax Haven Deposit Changes after the CRS

Figure C.11: Event Studies – Extended Sample

Notes: Using an extended sample period, which ranges from the first quarter of 2010 to the last quarter of 2018, panel (a) charts the coefficients, which each mark the change in cross-border deposits held by residents of tax havens in tax havens around the CRS treatment dates (in event time), panel (b) charts the coefficients, which each mark the change in cross-border deposits held by residents of the United States in tax havens around the CRS treatment dates (in event time) and panel (c) charts the coefficients, which each mark the change in cross-border deposits held by residents of the United States in non-tax havens around the CRS event dates (in event time). We estimate equation 6, 7 and 8, respectively, but replace the single coefficient of the post-CRS indicator with separate indicator variables, each marking one quarter over the sample period relative to the quarter before the CRS treatment event date (t=0). The CRS treatment event date in Panel (a) is the interaction of the CRS passing into national law in the tax haven residence country with the tax haven deposit country. In Panel (b) the CRS treatment event date is the passing of the CRS into national law in the tax haven residence country. In Panel (c) the CRS treatment event date is the adoption of the CRS in the first adoption wave (1 quarter of 2016). In the staggered treatment specifications (Panel (a) and (b)), we bin the treatment indicators at the endpoints. In all specifications we omit the indicator for period t-1. It, therefore, serves as a benchmark and has a coefficient value of zero (and no confidence interval). The figure plots the coefficient estimates together with their 95% confidence intervals. We use the log of cross-border deposits as the dependent variable and residence-country quarter-year fixed effects as well as ordered country-pair fixed effects. We cluster at deposit-country level.

Table C.20: Change in Cross-Border Deposits after the CRS Held Through Tax Havens and the United States - Extended Sample Period

VARIABLES	(1) Tax Haven to Tax Haven LogDeposits	(2) Tax Haven to Tax Haven LogDeposits	(3) Tax Haven to US LogDeposits	(4) Tax Haven to US LogDeposits	(5) US to Non-Tax Haven LogDeposits
PostCRSResHaven	0.0732		0.215		
	(0.0889)		(0.189)		
PostCRSDepHaven	0.172***				
	(0.0631)				
PostCRSResHaven * PostCRSDepHaven	-0.236**				
	(0.118)				
PostCRSFirstWave		-0.187***		0.356***	0.0217
		(0.0582)		(0.117)	(0.207)
Observations	3,206	3,206	519	519	544
R-squared	0.955	0.954	0.978	0.975	0.899
Quarter-Year FE	YES	NO	YES	NO	NO
Country-Pair FE	YES	YES	YES	YES	YES
Clustering	Tax Haven Country-Pair	Tax Haven Country-Pair	Tax Haven Residence Country	Tax Haven Residence Country	Non-Tax Haven Deposit Country

Notes: This table reports the DiD estimates of regression model 6, 7 and 8 respectively in an extended sample. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit country j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the first quarter of 2010 to the third quarter of 2018. In Column 1 and 2, the sample is restricted to tax havens as residence and deposit country. In Column 3 and 4, the sample is restricted to tax havens as residence country and the United States as deposit country. In Column 5, the sample is restricted to the United States as residence country and non-tax havens as deposit country. All columns include country-pair fixed effects, but only Columns 1 and 3 include quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at country-pair level in Column 1 and 2, at residence-country level in Column 3 and 4 and at deposit-country level in Column 5.

Robust standard errors in parentheses

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

Appendix D. Robustness Checks Using a Different Identification Strategy

Appendix D.1. Baseline Results Using Different Fixed Effects Structures

In this Appendix, we test the reliance of our identification on our choice of fixed effects, by modifying the fixed effects structure of our baseline estimates.

We begin by reporting graphical results from event-study regressions estimating the effect of the CRS on cross-border deposits held by residents of EU and OECD countries in tax havens versus non-tax havens. Panel (a) of Figure D.12 plots the results when we only include country-pair fixed effects (dropping residence country-quarter-year fixed effects from the baseline estimation), Panel (b) of Figure D.12 plots the results when we only include residence country-quarter-year fixed effects (dropping country-pair fixed effects from the baseline estimation) and Panel (c) of Figure D.12 plots the results when we do not include fixed effects. Excluding residence country-quarter-year fixed effects does not affect our results substantially. Thus, there seems to be no need to control for residence country time variant characteristics, nor for general time trends. While excluding country-pair and similarly including no fixed effects at all does affect our results. Country-pair fixed effects control for time invariant characteristics across and within country-pairs. Because of very different levels of cross-border holdings between country-pairs, a between country comparison may not be sensible. For example, Switzerland is geographically closer to most EU and OECD countries compared to Hong Kong and the relationship between Germany and Switzerland is again a closer one than between Norway and Switzerland. Thus, one expects that more deposits of EU and OECD residents, in particular, Germans are located in Switzerland than in Hong Kong. Not controlling for these between country-pair differences may introduce significant bias to our estimates. This is why in our view, for the purpose of our identification strategy, introducing country-pair fixed effects is fundamental. While inclusion of residence-country quarter-year fixed effects only proves robustness of our results.

Results from event study regressions estimating the effect of the CRS on cross-border deposits held by residents of EU and OECD countries in the United States versus non-tax havens are less sensitive to changes in the fixed effects structure. Country-pair fixed effects may be less essential here because the US-treatment-dummy, in the regressions without fixed effects, is controlling for the level difference in cross-border deposits in the only treated country, i.e. the United States, to the other deposit countries. As visible in Panel (a), (b) and (c) of Figure D.13, excluding residence-quarter-year fixed effects does not affect our results substantially, and excluding country-pair fixed effects or all fixed effects increases the size of the effect, however the overall effect dynamics remain similar. In general it holds that

the less stringent the fixed effects structure the stronger is the effect size measured.

Finally, we report the regression estimates for our baseline regressions reported in Table 4 (Equation 2 and 4) without residence-country quarter-year fixed effects in Table D.21, without country-pair fixed effects in Table D.22 and without fixed effects in Table D.23. Results confirm our overall findings from the event study graphs.

Appendix D.2. Baseline Results Using Different Weightings

In this section we explore different weighting strategies following closely recommendations by Solon et al. (2015). Weighting does not fundamentally change our results, which suggests that our estimates do not suffer from severe model misspecification. Further, the additional tests recommended by Solon et al. (2015) bring interesting evidence with regard to effect size heterogeneity in different tax haven deposit locations. The CRS effect is moderately larger for smaller tax havens, but overall the average effect we estimate in our baseline estimation, which can be understood as an average of the country level effects, is not far off from a weighted average effect with weights that capture the per-dollar relative importance of the tax haven location.

According to Solon et al. (2015), when trying to estimate the causal effect of a policy instrument, weighting is commonly used in the following three circumstances. First, to improve precision by correcting for heteroscedasticity because of varying sample sizes in subgroups (Solon et al., 2015, section III), second, to correct for endogenous sampling (Solon et al., 2015, section IV) and third, according to the authors incorrectly applied, to identify average partial effects in the presence of unmodeled heterogeneity (Solon et al., 2015, section V). We consider the first reason and the third reason to weight relevant for our estimation.

In section III, Solon et al. (2015) address weighting as an attempt to improve precision in the presence of heteroscedasticity. They argue, however, that weighting may not always increase efficiency in the presence of heteroscedasticity. The classical argument for heteroscedasticity based weighting arises when the dependent variable is a group average and the averages for different groups are based on widely varying within-group sample sizes. In our context that means weighting may be used to control for differing variance of the error term due to the varying number of cross-border deposit accounts in every deposit country. For example, there may be three times as many deposit accounts in Switzerland than in Jersey. The variance of the error term on the coefficient of interest in Switzerland will be lower as the larger sample size allows more precise estimation. In this context, weighting Switzerland higher than Jersey may result in more precise estimates. However, Solon et al. (2015) qualify this statement, because re-weighting in this way, may commonly lead to a less precise estimate if the individual-level

error terms within each subgroup (e.g. deposit country) are not independent, i.e. if they are clustered. Then it may be better not to weight, because the "cluster component" of the error term will be more influential in the weighted regression estimate of the standard errors leading to less precise estimates than under non-weighted regression. This is especially the case if the clustering is substantial and the sample size in every group (e.g. the deposits in every deposit location) is large. In this case OLS "is nearly the best linear unbiased estimator" (Solon et al., 2015, p. 307).

Solon et al. (2015) recommend to the practitioner to first conduct standard heteroscedasticity tests to identify whether weighted regression is necessary at all, second to report heteroscedasticity robust standard errors and third to compare weighted to unweighted regression results. Indeed, starting with standard OLS regression on our main identification and conducting the standard Breusch-Pagan-Test for heteroscedasticity we find, rather unsurprisingly, clear evidence of heteroscedasticity in our sample (the null is rejected at all standard levels of significance). Using cluster robust standard errors, we proceed to run differently weighted regressions and to compare the weighted to the standard unweighted regression.

The question is which weights to employ. The weights should be directly proportional to the sample size in each cluster (i.e. the number of deposit accounts in each deposit country). Based on the assumption that the volume of deposits held in a deposit country will be highly positively correlated with the number of accounts held in that deposit country, we believe the best available weight is a measure of deposits located in the deposit location. Differently stated, the underlying assumption is that the volume of deposits per account are on average approximately the same in each deposit country. We alternate our measure to test for robustness. The weights that we employ are: average deposits located in the deposit country over all years (Table D.24a and D.25a), by year (Table D.24b and D.25b) and in the first period where deposits are observed (Table D.24c and D.25c). In one set of regressions we take as measure of quarterly deposits in one deposit country hand-calculated aggregates of the bilateral data in our sample (Table D.24), this implies that only deposits owned by residents countries in our sample are accounted for (Table D.25). In another set of regressions we use aggregates provided directly by the BIS. As Solon et al. (2015) note, next to testing whether weighting improves precision, the comparison between weighted and unweighted regressions can additionally serve as diagnostic for model misspecification: "Under exogenous sampling and correct specification of the conditional mean of y [. . .], both OLS and [weighted regression] are consistent for estimating the regression coefficients." The degree of difference between the estimates in both regressions can therefore show the degree of model misspecification. We find that in our weighted regression the standard errors are more precisely measured (smaller p-values) and the coefficient estimates are close to our unweighted baseline estimates (the tax

haven estimate is very close and the U.S. estimate is of by about 4 percentage points) indicating increased precision in our weighted regressions and no sever issue of model misspecification. We test more directly for model misspecification in the next paragraph.

According to section V of Solon et al. (2015), another reason for weighting that is sometimes put forward is misspecification in the presence for unmodeled heterogeneous effects. As Solon et al. (2015) demonstrate weighting does not solve this problem and does not identify the population average partial effect. Instead, Solon et al. (2015) highlight that it is necessary to account for heterogeneous effects by modelling them explicitly. If the effect of the CRS differs for each tax haven deposit country, Solon et al. (2015) recommend to model the effect separately for each and then calculate the average effect by weighting the separately estimated effects appropriately. That is what we do in Table D.26. We find that the effect size is on average moderately larger in smaller tax havens. Luxembourg is the only tax haven where we do not find an economically meaningful effect.

The main reason for this might be the tax evasion strategy observed by Zucman (2013b). Luxembourg is a major player in the mutual fund industry. According to the detailed Swiss bank accounts data Zucman (2013b) has access to, Luxembourg is regularly entered through Switzerland by foreign investors. Directly holding deposits and investing in Luxembourg funds would not shield against taxation, instead, if the goal is to avoid taxation, Switzerland or other tax havens are to be used as conduit country (see the example made in Zucman (2013b) p. 10). Not observing an effect on cross-border deposits held directly in Luxembourg would be in line with these observations.

To calculate the overall "per-dollar" effect we weight each estimated coefficient by the average amount of deposits in the relevant deposit country. That weighted average effect, reported at the bottom of Table D.26, is slightly lower than what we find in our baseline estimate. Thus, estimating the effect at the country-level leads to a larger estimate than if we estimate the effect on a "per-dollar"-basis. This dollar weighted estimate lies at approximately 9.4% vs. 11.5% in our un-weighted baseline result. The corresponding P-value is 5.57%.

Appendix D.3. Baseline Results With Bootstrapping

Table D.27 reports the baseline estimates from Table 4 with bootstrapped standard errors.[52] Cluster robust standard errors as used in this study and commonly in economic analysis, may over-reject if the number of clusters is not sufficiently large. A common way to improve inference is to use bootstrapped standard errors. The number of clusters in our main analysis is 29

[52]We use Stata's "bootstrap" command setting the number of replications to 999.

(29 deposit countries). As robustness check we investigate how our results change if we use bootstrapped standard errors. As can be seen in Table D.27, our inference is largely unchanged when bootstrapped standard errors are used for estimation. All coefficients exhibit p-values above 10%. At most coefficients are more significant now. Therefore, overrejection does not seem to be a fundamental issue in our setting.

Table D.21: No Residence-Time Fixed Effect - Change in Cross-Border Deposits after the CRS

	(1)	(2)	(3)	(4)
	Country Introduction	Country Effectiveness	First Adoption Wave	First Adoption Wave
VARIABLES	LogDeposits	LogDeposits	LogDeposits	LogDeposits
PostCRS * Havens	-0.115**	-0.112**	-0.111*	-0.105
	(0.0534)	(0.0546)	(0.0612)	(0.0635)
PostCRS * US				0.109*
				(0.0547)
Observations	11,884	11,884	11,884	11,884
R-squared	0.970	0.970	0.970	0.970
Country-Pair FE	YES	YES	YES	YES
Residence-Quarter-Year FE	NO	NO	NO	NO
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of regression model 2 and 4 respectively. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit location j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit location, in Column 2, for the effective date of the CRS in the deposit location, and, in Column 3 and 4, for the period of the first wave of information exchange. Havens is a dummy taking value one when the deposit location j is a tax haven. US is a dummy equal to one when the deposit country j is the United States. All regressions include ordered country-pair fixed effects. Cluster robust standard errors in parentheses, clustered at the deposit-country level.

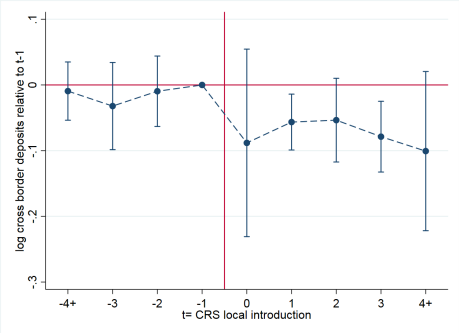
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.22: No Country-Pair Fixed Effects - Change in Cross-Border Deposits after the CRS

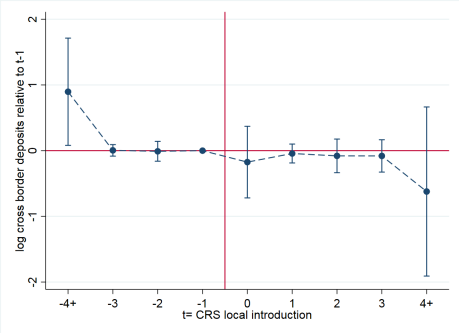
	(1)	(2)	(3)	(4)
	Country Introduction	Country Effectiveness	First Adoption Wave	First Adoption Wave
VARIABLES	LogDeposits	LogDeposits	LogDeposits	LogDeposits
PostCRS * Havens	-1.615**	-1.382**	0.0894	0.0985
	(0.734)	(0.658)	(0.129)	(0.131)
PostCRS * US				0.338**
				(0.127)
Observations	11,889	11,889	11,889	11,889
R-squared	0.366	0.339	0.316	0.352
Country-Pair FE	NO	NO	NO	NO
Residence-Quarter-Year FE	YES	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of regression model 2 and 4 respectively. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit location j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit location, in Column 2, for the effective date of the CRS in the deposit location, and, in Column 3 and 4, for the period of the first wave of information exchange. Havens is a dummy taking value one when the deposit location j is a tax haven. US is a dummy equal to one when the deposit country j is the United States. All regressions include residence country x quarter-year fixed effects. Cluster robust standard errors in parentheses, clustered at the deposit-country level.

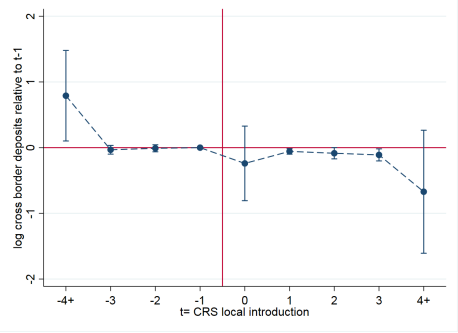
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1



(a) Only Country-Pair Fixed Effects



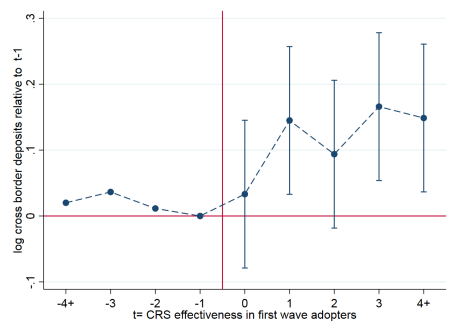
(b) Only Residence Country-Quarter-Year Fixed Effects



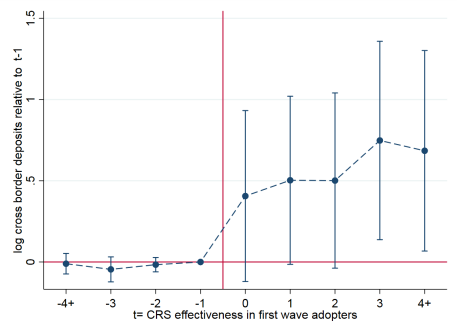
(c) No Fixed Effects

Figure D.12: Alternative Fixed Effects - Event Study Test of Reaction to the CRS in Tax Havens

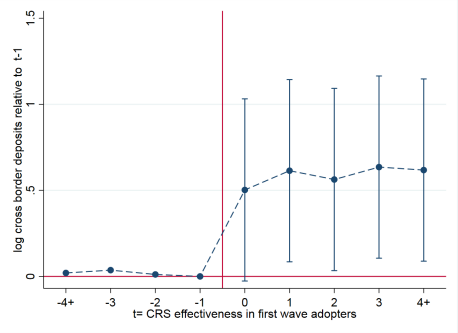
Notes: The figure charts the coefficients, which each mark the change in cross-border deposits held in tax havens versus non-tax havens around the CRS event dates (in event time). We estimate equation 1, but adapt the fixed effects structure in each figure in the graph. Panel (a) includes only country-pair fixed effects, Panel (b) includes only residence-country quarter year fixed effects, Panel (c) includes no fixed effects and Panel (d) includes only deposit-country fixed effects. We bin the treatment indicators at the endpoints and omit the indicator for period t-1. It, therefore, serves as a benchmark and has a coefficient value of zero (and no confidence interval). The figure plots the coefficient estimates per quarter together with their 95% confidence intervals for the staggered CRS event date at the deposit country level. We use the log of cross-border deposits as the dependent variable. We cluster at deposit-country level.



(a) Only Country-Pair Fixed Effects



(b) Only Residence-Quarter-Year Fixed Effects



(c) No Fixed Effects - Event Study Test of Reaction to the CRS in Tax Havens

Figure D.13: Alternative Fixed Effects - Event Study Test on Relocation Behavior after the CRS

Notes: The figure charts the coefficients, which each mark the change in cross-border deposits held in tax havens versus non-tax havens around the CRS event dates (in event time). We estimate equation 3, but adapt the fixed effects structure in each figure in the graph. Panel (a) includes only country-pair fixed effects, Panel (b) includes only residence-country quarter year fixed effects and Panel (c) includes no fixed effects. We bin the treatment indicators at the endpoints and omit the indicator for period t-1. It, therefore, serves as a benchmark and has a coefficient value of zero (and no confidence interval). The figure plots the coefficient estimates per quarter together with their 95% confidence intervals for the staggered CRS event date at the of CRS in the deposit country. We use the log of cross-border deposits as the dependent variable. We cluster at deposit-country level.

Table D.23: No Fixed Effects - Change in Cross-Border Deposits after the CRS

	(1)	(2)	(3)	(4)
	Country Introduction	Country Effectiveness	First Adoption Wave	First Adoption Wave
VARIABLES	LogDeposits	LogDeposits	LogDeposits	LogDeposits
PostCRS * Havens	-1.848**	-1.492**	0.0888	0.0984
	(0.754)	(0.656)	(0.134)	(0.136)
PostCRS * US				0.339**
				(0.129)
Observations	11,889	11,889	11,889	11,889
R-squared	0.040	0.026	0.012	0.043
Country-Pair FE	NO	NO	NO	NO
Residence-Quarter-Year FE	NO	NO	NO	NO
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of regression model 2 and 4 respectively. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit location j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit location, in Column 2, for the effective date of the CRS in the deposit location, and, in Column 3 and 4, for the period of the first wave of information exchange. Havens is a dummy taking value one when the deposit location j is a tax haven. US is a dummy equal to one when the deposit country j is the United States. All regressions do not include fixed effects. Cluster robust standard errors in parentheses, clustered at the deposit-country level.

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

Table D.24: Weighted Regressions Part 1: Weights based on own cross-border deposits aggregates at deposit country level

(a) Weighted by Average Cross-border Deposits by Deposit Country

	(1)	(2)	(3)	(4)
	Country Introduction	Country Effectiveness	First Adoption Wave	First Adoption Wave
VARIABLES	LogDeposits	LogDeposits	LogDeposits	LogDeposits
PostCRS * Havens	-0.136***	-0.117**	-0.129***	-0.118***
	(0.0449)	(0.0441)	(0.0341)	(0.0374)
PostCRS * US				0.0561**
				(0.0264)
Observations	11,884	11,884	11,884	11,884
R-squared	0.986	0.986	0.986	0.986
Country-Pair FE	YES	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country

(b) Weighted by Average Current Cross-border Deposits by Deposit Country

	(1)	(2)	(3)	(4)
	Country Introduction	Country Effectiveness	First Adoption Wave	First Adoption Wave
VARIABLES	LogDeposits	LogDeposits	LogDeposits	LogDeposits
PostCRS * Havens	-0.138***	-0.119***	-0.119***	-0.119***
	(0.0442)	(0.0432)	(0.0365)	(0.0365)
PostCRS * US				0.0553**
				(0.0260)
Observations	11,884	11,884	11,884	11,884
R-squared	0.986	0.986	0.986	0.986
Country-Pair FE	YES	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country

(c) Weighted by Average Initial Cross-border Deposits by Deposit Country

	(1)	(2)	(3)	(4)
	Country Introduction	Country Effectiveness	First Adoption Wave	First Adoption Wave
VARIABLES	LogDeposits	LogDeposits	LogDeposits	LogDeposits
PostCRS * Havens	-0.125***	-0.112**	-0.125***	-0.111***
	(0.0451)	(0.0442)	(0.0341)	(0.0374)
PostCRS * US				0.0634**
				(0.0264)
Observations	11,884	11,884	11,884	11,884
R-squared	0.986	0.986	0.986	0.986
Country-Pair FE	YES	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of regression model 2 and 4, respectively, with the difference that regressions are weighted: in panel (a) by average cross-border deposits over all years and all countries in our sample at the level of the deposit country, in panel (b) by average current cross-border deposits over all countries in our sample at the level of the deposit country and in panel (c) by average initial cross-border deposits over all countries in our sample at the level of the deposit country. Aggregates are hand calculated based on bilateral cross-border deposits as available by the BIS and used in our main analysis. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit location j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit location, in Column 2, for the effective date of the CRS in the deposit location, and, in Column 3 and 4, for the period of the first wave of information exchange. Havens is a dummy taking value one when the deposit location j is a tax haven. US is a dummy equal to one when the deposit country j is the United States. All regressions do include residence-quarter-year and country-pair fixed effects. Cluster robust standard errors in parentheses, clustered at the deposit-country level.

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

Table D.25: Weighted Regressions Part 2: Weights based on global BIS cross-border deposits aggregates at deposit country level

(a) Weighted by Average Cross-border Deposits by Deposit Country

VARIABLES	(1) Country Introduction LogDeposits	(2) Country Effectiveness LogDeposits	(3) First Adoption Wave LogDeposits	(4) First Adoption Wave LogDeposits
PostCRS * Havens	-0.137***	-0.114***	-0.131***	-0.115***
	(0.0369)	(0.0376)	(0.0271)	(0.0308)
PostCRS * US				0.0585**
				(0.0240)
Observations	11,721	11,721	11,721	11,721
R-squared	0.985	0.985	0.985	0.985
Country-Pair FE	YES	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country

(b) Weighted by Average Current Cross-border Deposits by Deposit Country

VARIABLES	(1) Country Introduction LogDeposits	(2) Country Effectiveness LogDeposits	(3) First Adoption Wave LogDeposits	(4) First Adoption Wave LogDeposits	(5) Country Introduction LogDeposits	(6) Country Effectiveness LogDeposits	(7) First Adoption Wave LogDeposits	(8) First Adoption Wave LogDeposits
1.PostCRS#1.Offsh	-0.137***	-0.114***	-0.116***	-0.116***	-0.137***	-0.114***	-0.116***	-0.116***
	(0.0368)	(0.0372)	(0.0309)	(0.0309)	(0.0368)	(0.0372)	(0.0309)	(0.0309)
1.PostCRS#1.US				0.0577**				0.0577**
				(0.0241)				(0.0241)
Observations	11,721	11,721	11,721	11,721	11,721	11,721	11,721	11,721
R-squared	0.985	0.985	0.985	0.985	0.985	0.985	0.985	0.985
Country-Pair FE	YES	YES	YES	YES	YES	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country	Deposit Country	Deposit Country	Deposit Country	Deposit Country

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

(c) Weighted by Average Initial Cross-border Deposits by Deposit Country

VARIABLES	(1) Country Introduction LogDeposits	(2) Country Effectiveness LogDeposits	(3) First Adoption Wave LogDeposits	(4) First Adoption Wave LogDeposits
PostCRS * Havens	-0.133***	-0.110***	-0.130***	-0.113***
	(0.0377)	(0.0377)	(0.0281)	(0.0315)
PostCRS * US				0.0599**
				(0.0239)
Observations	11,721	11,721	11,721	11,721
R-squared	0.985	0.985	0.985	0.986
Country-Pair FE	YES	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of regression model 2 and 4, respectively, with the difference that regressions are weighted: in panel (a) by average cross-border deposits over all years at the level of the deposit country, in panel (b) by average current cross-border deposits at the level of the deposit country and in panel (c) by average initial cross-border deposits at the level of the deposit country. Aggregates of cross-border deposits located in one deposit country are obtained directly form the bank of international settlements. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit location j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit location, in Column 2, for the effective date of the CRS in the deposit location, and, in Column 3 and 4, for the period of the first wave of information exchange. Havens is a dummy taking value one when the deposit location j is a tax haven. US is a dummy equal to one when the deposit country j is the United States. All regressions do include residence-quarter-year and country-pair fixed effects. Cluster robust standard errors in parentheses, clustered at the deposit-country level.

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

Table D.26: Heterogeneity in Tax Haven Treatment Effect - Change in Cross-Border Deposits

VARIABLES	(1) Country Introduction LogDeposits	(2) Country Effectiveness LogDeposits	(3) First Adoption Wave LogDeposits
PostCRS * CH	-0.125**	-0.122**	-0.0796
	(0.0538)	(0.0517)	(0.0541)
PostCRS * GG	-0.110**	-0.130***	-0.137**
	(0.0437)	(0.0443)	(0.0538)
PostCRS * HK	-0.0810*	-0.0729	-0.0823
	(0.0464)	(0.0488)	(0.0530)
PostCRS * IM	-0.230***	-0.230***	-0.236***
	(0.0430)	(0.0436)	(0.0530)
PostCRS * JE	-0.170***	-0.152***	-0.159***
	(0.0430)	(0.0436)	(0.0530)
PostCRS * LU	-0.000205	0.0119	0.00525
	(0.0434)	(0.0440)	(0.0533)
Observations	11,884	11,884	11,884
R-squared	0.972	0.972	0.972
Country-Pair FE	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country
Joint Coefficient Estimate	-0.0943	-0.0881	-0.0747
P-Value	0.0557	0.0714	0.174

Notes: This table reports the DiD estimates of regression model 2 replacing the "Haven"-Dummy with a seperate Dummy for each Tax Haven in our Sample. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit country j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is a dummy equal to one starting with the passing of the CRS into national law (Column 1), CRS effectiveness (Column 2) and in the period of the first wave of information exchange (Column3), respectively. Cluster robust standard errors in parentheses, clustered at the deposit-country level. All regressions include ordered country-pair and residence country x quarter-year fixed effects.

Robust standard errors in parentheses

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

Table D.27: Bootstrapped Standard Errors - Change in Cross-Border Deposits after the CRS

	(1)	(2)	(3)	(4)
	Country Introduction Standard	Country Effectiveness	First Adoption Wave	First Adoption Wave
VARIABLES	LogDeposits	LogDeposits	LogDeposits	LogDeposits
PostCRS * Havens	-0.119**	-0.118**	-0.115*	-0.109*
	(0.0560)	(0.0581)	(0.0625)	(0.0660)
PostCRS * US				0.109**
				(0.0549)
Observations	11,884	11,884	11,884	11,884
R-squared	0.972	0.972	0.972	0.972
Country-Pair FE	YES	YES	YES	YES
Residence-Quarter-Year FE	YES	YES	YES	YES
Clustering	Deposit Country	Deposit Country	Deposit Country	Deposit Country

Notes: This table reports the DiD estimates of regression model 2 and 4, respectively. The dependent variable is the log of cross-border deposits held by residents of country i in banks of deposit location j in the end of quarter q. The unit of observation is the ordered country-pair and the sample period goes from the last quarter of 2014 to the third quarter of 2017. PostCRS is an indicator variable, in Column 1, for the period after the introduction of the CRS in the deposit location, in Column 2, for the effective date of the CRS in the deposit location, and, in Column 3 and 4, for the period of the first wave of information exchange. Havens is a dummy taking value one when the deposit location j is a tax haven. US is a dummy equal to one when the deposit country j is the United States. All regressions do include residence-quarter-year and country-pair fixed effects. Bootstrapped cluster robust standard errors in parentheses, clustered at the deposit-country level.

Standard errors in parentheses

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

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ZEW – Leibniz-Zentrum für Europäische Wirtschaftsforschung GmbH Mannheim
ZEW – Leibniz Centre for European Economic Research

L 7,1 · 68161 Mannheim · Germany
Phone +49 621 1235-01
info@zew.de · zew.de