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International tax evasion, state purchases of confidential bank data and voluntary disclosures

Dirk Bethmann [a,*], Michael Kvasnicka [b,c,d]

[a] Korea University, 145 Anam-ro, Seongbuk-gu, Seoul 02841, Republic of Korea
[b] Otto von Guericke University Magdeburg, Germany
[c] RWI – Leibniz Institute for Economic Research, Essen, Germany
[d] IZA, Bonn, Germany

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ABSTRACT

International tax evasion is a major source of discontent for tax authorities worldwide. State purchases of whistleblower bank data on suspected tax evaders from international tax havens is a potentially powerful policy tool tax authorities may use unilaterally to combat such tax evasion. Tax authorities in North-Rhine Westphalia (NRW), Germany's most populous federal state, have been the most active internationally in acquiring such confidential bank data in the first half of the 2010s. Using unique self-compiled press data on the timing and content of altogether 10 data acquisitions from Switzerland, a major offshore financial center and tax haven, and on monthly voluntary disclosures of international tax evasion involving Swiss banks, we study the response of voluntary disclosures to news of such acquisitions in the period 2010–2015. Our results show that media reports on new purchases of whistleblower data had a positive, immediate and sizeable effect on the number of voluntary disclosures filed. Various robustness checks and additional explorations corroborate this finding. For NRW, these purchases generated additional tax revenue of more than half a billion euros.

1. Introduction

International tax evasion has become a major source of discontent for tax authorities worldwide. Evidence produced in Zucman (2013) and Alstadsæter et al. (2018) suggests that about 8% of the global financial wealth of households is held in tax havens, of which three-quarters goes unrecorded, and that offshore assets are owned in the main by residents of rich countries, particularly by Europeans.[1] Of these assets (recorded and unrecorded), a large fraction is held or managed in Europe itself, often in small landlocked Switzerland, a major offshore financial center and tax haven famous for its strict bank secrecy and data privacy laws.[2]

* Corresponding author.
E-mail addresses: dirk@korea.ac.kr (D. Bethmann), Michael.Kvasnicka@ovgu.de (M. Kvasnicka).

[1] Earlier estimates of household offshore wealth are even larger than the $5.9 trillion in 2008 estimated by Zucman (2013). They include $6.7 trillion in 2008 (Boston Consulting Group, 2009) and $8.5 trillion in 2002 (Cap Gemini and Merrill Lynch, 2002). According to Palan et al. (2010), the global rich in 2007 held wealth worth $12 trillion in tax havens. For 2022, Boston Consulting Group too reports an estimate of $12.0 trillion (Boston Consulting Group, 2023).

[2] According to Boston Consulting Group (2009), about a third of the world's offshore wealth in 2008 was held in Switzerland. Zucman (2013) estimates that one third of the global missing wealth could be traced back to Switzerland (estimated at $1,545 billions). According to data published by Swiss tax authorities, about 80% of the wealth held by Europeans in Switzerland at the time seemed to be evading taxes (Zucman, 2014). According to Boston Consulting Group (2023), Switzerland's share in the world's offshore wealth has declined to one fifth in recent years. Although Switzerland remains the most important booking center, it is forecast to be surpassed by Hong Kong by the year 2027.

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Lack of information on offshore assets is the most important obstacle tax authorities face in their fight against tax evasion. Tax havens are naturally reluctant to share such information.[3] Growing pressure amidst the financial and economic crisis of 2008/09, however, has led to numerous bilateral tax treaties that provide for an exchange of information between countries. Today, such treaties form the main policy instrument against offshore tax evasion. Recent evidence, however, suggests that the effectiveness of such treaties is limited.[4] Information-on-request treaties typically prohibit broad 'fishing expeditions' and they still require tax haven cooperation. The latter holds true also for automatic exchange of information agreements in tax matters, such as the Common Reporting Standard (CRS), which took effect in Switzerland in 2017, a multilateral framework that is still expanding in membership with select countries.[5]

In recent years, however, governments have made use also of unilateral measures to identify tax evaders and enforce tax compliance — by purchasing confidential bank data from whistleblowers. Germany's most populous federal state and economic powerhouse, North-Rhine Westphalia (NRW), has been particularly active in this regard.[6] Between 2010 and 2014, NRW acquired (and then shared with other German states) ten datasets containing the names of suspected tax evaders with Swiss accounts.[7] While controversial,[8] German courts have upheld the legality of using such data. According to NRW's finance ministry, voluntary disclosures related to offshore wealth in 2010 to 2015 have generated roughly 1.2 billion euros in additional tax revenue for the state (Landtag Nordrhein-Westfalen, 2016), while NRW spent only 19.4 million euros for all 10 data purchases from Switzerland (plus one from Luxembourg) it made in this period.[9] However, the extent to which these disclosures (and associated tax revenues) were actually triggered by acquisitions of whistleblower data has not yet been quantified and systematically analyzed — a gap in the literature this paper addresses.

State acquisitions of confidential bank data from tax havens increase the risks of detection for tax evaders. News of such acquisitions in the media are hence likely to spur timely voluntary disclosures of tax evaders for fear of facing criminal charges. Voluntary disclosure programs exist in most OECD countries (see OECD (2015) for a concise review of existing country-specific programs). Voluntary disclosure is an act by which a delinquent taxpayer discloses voluntarily information not previously reported to tax authorities in return for immunity from prosecution for tax fraud. A voluntary disclosure (essentially an amended tax return) is only possible if tax authorities have not started investigations into fraud.[10] If effective, a tax evader has to pay the tax liability plus interest and possibly some penalty surcharges, but generally no longer faces the risk of being prosecuted for tax fraud.[11] The effects on voluntary disclosures of such state-sponsored data purchases, however, have received surprisingly little attention in the literature. Hardly anything, as a consequence, is known on the effectiveness of such data acquisitions in helping states to tax hitherto untaxed wealth of domestic residents held abroad.

Using self-compiled and unique data for North-Rhine Westphalia (NRW) for the period January 2010 to November 2015 on the timing and content of altogether ten state acquisitions of bank data from Switzerland made public in the press and on monthly voluntary disclosures by NRW residents involving Swiss banks, we study in time-series regressions the effects news of such acquisitions had on the evolution over time of voluntary disclosures of untaxed wealth held in Switzerland.[12] Our results show that these exogenous shocks to the probability of detection had a positive and sizeable effect on the number of such voluntary disclosures. Various robustness checks corroborate this conclusion, as do additional explorations, such as the study of Google keyword searches for "voluntary disclosure" in Germany. As expected, the latter also show a positive response following the emergence of a new purchase. Furthermore, and also in line with expectations, this response both materializes more quickly and exhibits less persistence than the response of actual disclosures. We also find evidence for a more timely response of voluntary disclosures to news of purchases of data from Switzerland which have been linked in the press to specific banks in Switzerland. This more timely response may be explained by the perceived greater need of tax evaders to respond quickly by way of voluntary disclosure to such purchases of data, being faced with a greater and more imminent risk of identification and hence prosecution by tax authorities.

Overall, our findings show that the ten purchases of whistleblower data by NRW caused an enormous surge in voluntary disclosures in the years 2010 to 2015, generating additional tax revenue of 576 million euros. Unilateral state acquisitions of

[3] Strict bank secrecy and data privacy laws are a core feature of tax havens. It is for this reason that tax havens are also referred to as 'secrecy jurisdictions'.

[4] Menkhoff and Miethe (2019) show that information-on-request treaties with tax havens not only reduce deposits in tax havens, but also deposits of tax havens in high-tax countries. Menkhoff and Miethe (2019) also show that both reactions dissipate over time and that information-on-request treaties signed after 2010 trigger no further reactions. Evaluating the G20 tax haven crackdown in 2009, Johannesen and Zucman (2014) produce evidence that the information exchange treaties forced upon tax havens under the threat of economic sanctions caused tax evaders to shift deposits to havens not covered by a treaty with their home country, rather than to repatriate funds.

[5] The CRS did not put an end to cross-border tax evasion (Casi et al., 2020), as full cooperation and complete information exchange is not always guaranteed (see Langenmayr and Zyska, 2023 or Bomare and Le Guern Herry, 2022). There is also evidence that CRS enforcement has caused reallocation of funds into non-CRS jurisdictions, rather than bringing these funds "home" or making them taxable by authorities suffering from tax evasion (see e.g. Menkhoff and Miethe, 2019, Casi et al., 2020).

[6] The state of NRW accounts for roughly one fifth of German GDP (according to 2019 World Bank data). Its GDP exceeds that of European states such as Switzerland, Sweden, Poland or Belgium, or of countries like Saudi Arabia, Turkey, or Argentina.

[7] Information on these purchases of Swiss data is provided in Table A.1 in the Appendix.

[8] See, for example, the New York Times, April 2, 2012: "Swiss Seek Arrest of 3 German Tax Officials Over Stolen Banking Data".

[9] For the whole of Germany (not just for NRW) and for all kinds of voluntary disclosures (not only those involving untaxed wealth abroad), the additional revenue generated in the same period amounted to more than six billion euros (Landtag Nordrhein-Westfalen, 2016).

[10] It must also be complete and free of error to be effective and guarantee immunity.

[11] Rules, however, do vary across countries. In particular, not everywhere does a voluntary disclosure minimize, or eliminate, the risk of criminal prosecution.

[12] Data on voluntary disclosures involving Swiss banks are not available for any other of the 16 federal states in Germany in the period under investigation, which precludes doing a panel-data event study. However, even if such data were available, states other than NRW would not be valid controls, as information from data CDs was shared between the tax authorities of all federal states.

whistleblower data by countries suffering from tax evasion are hence an effective policy tool to identify tax evaders and promote tax compliance. For this reason, purchases of data, or mere threats thereof, are also a valuable policy tool to secure and sustain cooperation from tax havens in the negotiation, enforcement, and potential renegotiation of bilateral and multilateral information exchange agreements, such as the CRS.[13]

There is a large body of literature that is of broader relevance for our paper. First, there are numerous studies on tax evasion by individuals (see Slemrod (2007) for an early review of and introduction to this strand of literature). Theoretical work on individual tax evasion started with Allingham and Sandmo (1972) and Yitzhaki (1974) (see Sandmo (2005) for an overview). Alm (2012) and Collin (2020) review the empirical work on the subject which is – despite the obvious difficulties in quantifying tax evasion – surprisingly large.[14] Second, there is also some (although mostly theoretical) literature on temporary tax amnesties programs (see, for example, Malik and Schwab, 1991, or Alm and Beck, 1993), which however differ significantly in nature and regulation from permanent voluntary disclosure programs.[15] Finally, and of more immediate relevance to our paper, there is a small but growing body of literature on voluntary disclosure programs. Three studies have explored such programs from a predominantly theoretical perspective (see Andreoni, 1991, Langenmayr, 2017, and Gould and Rablen, 2020). Recent studies have added an empirical perspective, highlighting the importance of enforcement measures in triggering voluntary disclosures. These enforcement measures include investigative pressure on banks and their boards (Johannesen et al., 2020), the exploitation of data leaks and the criminalization of tax evasion (Londoño-Vélez and Ávila-Mahecha, 2020), and the quality of equipment and access to information of tax investigators (Leenders et al., 2023). Several recent working papers also underline the continued strong interest in this research area (see, for example, Tortarolo and Londoño-Vélez, 2024 and Baselgia, 2025).

Of the aforementioned studies, only (Langenmayr, 2017) has inquired empirically into the effects of a state purchase of confidential bank data on voluntary disclosures. Langenmayr (2017) analyzes a 2010 whistleblower data acquisition in Germany using two years of state-level data from the country's 16 federal states, a simple difference-in-differences (DiD) design,[16] and two very broad tax aggregates — one plausibly affected by voluntary disclosures (self-reported income taxes) and one not (payroll taxes). Between 2009 and 2010, changes in the former exceeded those in the latter by an average of 690 million euros per state, i.e. by more than 11 billion euros overall for the whole of Germany — a difference that is interpreted as the causal tax effect of the 2010 whistleblower data purchase. This effect estimate seems implausibly large.[17] The lack of use of actual disclosure data in the analysis and potential violations of the common trend assumption may sign responsible for this overestimation.[18]

In terms of breadth of focus, methodology, and the quantity and quality of data used, our study contributes to the existing literature in several important ways. In our analysis for North-Rhine Westphalia, we consider almost six years of data (January 2010 to November 2015), study a total of ten (news reports on) data purchases linked to Switzerland, employ high-frequency monthly data,[19] and use a better-suited direct measure of response (actual number of voluntary disclosures) that only covers voluntary disclosures involving Swiss banks (i.e., illicit wealth held abroad) in a battery of time-series regressions that control for both underlying trends, potential pre-treatment (anticipatory) effects, as well as time-varying confounders. We also look into the potential persistency of any effects.

2. Data and empirical strategy

2.1. Data and summary statistics

From January 2010, Germany's largest federal state North-Rhine Westphalia (NRW) began to collect data on the number of voluntary disclosures involving Swiss banks that are filed each month by NRW residents.[20] Although publicly available from the

[13] Note that CRS effectiveness itself (in Switzerland on January 1st, 2017) seems to have had little effect on the numbers of monthly disclosures involving Swiss banks that were filed by NRW residents. These rose from 38.5 in July–December 2016 to just 40.2 disclosures in January–June 2017.

[14] Recent additions to this strand of literature include (Leenders et al., 2023) and Schmal et al. (2023). The former exploits a tax amnesty in the Netherlands to study the link between offshore tax evasion and wealth inequality. The latter investigates the effects of the Offshore Leaks (2013), the Panama Papers and the Bahamas Leaks (both 2016), and the Paradise Papers (2017) on the reporting behavior of U.S. companies with subsidiaries in implicated tax havens.

[15] Tax amnesties are discretionary, temporary programs (they run for about a quarter), which, unlike voluntary disclosure programs, usually do not fine tax evaders, are often open also to individuals already under investigation by tax authorities, and allow also only partial disclosure.

[16] The model does not control for pre-trends, pre-treatment anticipatory effects, or time-varying confounders.

[17] Self-reported income taxes in Germany rose by only 4.7 billion euros from 2009 to 2010, increasing from 26.4 billion in 2009 (see Figure A.1 in Langenmayr, 2017).

[18] Self-reported income taxes tend to grow faster than payroll taxes during economic recoveries (2010 marked such a recovery). Moreover, payroll tax revenues in 2010 were reduced by two tax policy changes, an increase in the basic personal allowance and expanded deductibility of health insurance contributions.

[19] To be valid, voluntary disclosures require very timely responses of tax evaders in the face of a pending investigation. Once authorities have started an investigation, tax evaders no longer can become tax compliant by filing a voluntary disclosure and thereby evade prosecution. Higher frequency monthly data is hence better suited to study disclosure response dynamics than annual data.

[20] Noticing sharply rising numbers of voluntary disclosures with links to Switzerland after the first data CD was acquired in February 2010, the Ministry of Finance in NRW commanded tax authorities to record henceforth, and retrospectively too from January 2010, such disclosures on a monthly basis for analytical purposes (see Landtag Nordrhein-Westfale (2015), p.2). These data contain only monthly counts of disclosures filed, but no information on either the value (in euros) of these disclosures, nor personal characteristics of individuals who filed them. NRW did not make available earlier (pre-2010) data on voluntary disclosures by NRW residents involving Swiss banks. It also did not collect monthly data on the total number of voluntary disclosures, which include also domestic disclosures and disclosures involving assets in foreign countries other than Switzerland. As noted in Section 1, NRW is the only federal state in Germany for which data on voluntary disclosures involving Swiss banks has been made available.

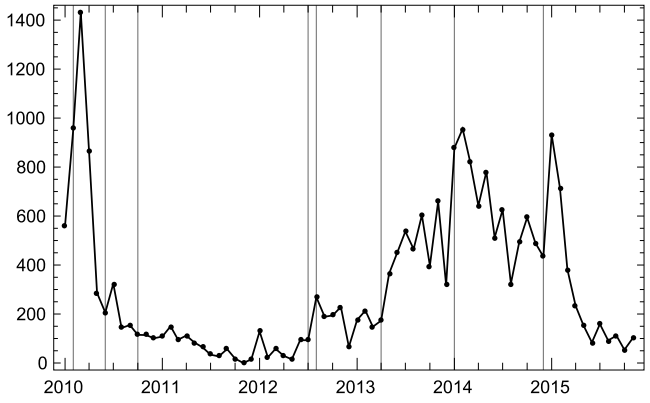

Fig. 1. Monthly voluntary disclosures and months of data purchases.
The black curve shows monthly voluntary disclosures involving Swiss banks of NRW residents to NRW tax authorities in the period January 2010 to November 2015. Zero disclosures were filed in November 2011 (the only month). The eight gray vertical lines indicate the respective calendar months in which it became public that a new data CD with information on potential tax evaders holding illicit wealth in Switzerland has been bought by tax authorities in NRW. Data on monthly voluntary disclosures stem from the Ministry of Finance of NRW. Details on individual purchases of data CDs have been compiled by the authors from various press articles (references for these articles and further information on data purchases is provided in Table A.1 in the Appendix).

Ministry of Finance of NRW, these unique data surprisingly have not been used yet in research of any kind, economic or other. The data set we use in the empirical analysis covers the period January 2010 to November 2015 and consists of two main types of data, (i) the aforementioned official data on the monthly absolute number of voluntary disclosures involving Swiss banks made by NRW residents to tax authorities in NRW, and (ii) self-compiled data on the respective calendar months in which purchases of data on potential tax evaders from NRW by tax authorities in NRW have been made public in the press.[21] We use (i) to construct our dependent variable and (ii) to construct our explanatory variables of main interest. Some of our robustness checks and additional explorations make use of further data, including data on keyword searches from Google trends and monthly exchange rates between the euro and the Swiss franc. We defer a discussion of these additional data until we first make use of them in the analysis.

Fig. 1 plots our raw data for the period January 2010 to November 2015, provided by the Ministry of Finance of NRW, on the monthly absolute number of voluntary disclosures involving Swiss banks made by NRW residents to tax authorities in NRW. Also documented in Fig. 1 are the respective eight calendar months in which the altogether ten purchases of data over this period by tax authorities in NRW containing information on potential NRW tax evaders holding illicit wealth in Switzerland have become public.[22] Three major insights may be gained from Fig. 1. First, the evolution over time of the absolute number of voluntary disclosures involving Swiss banks made by NRW residents to tax authorities in NRW roughly follows a U-shaped pattern. After a peak in early 2010, voluntary disclosures decline rapidly, then remain relatively constant at low numbers in 2011 and the first half of 2012, and begin to rise again from 2013 onwards (from the second quarter of 2014, however, this rise looses pace and eventually levels off, despite a marked, albeit short-lived, resurgence in January 2015). Second, the surge in voluntary disclosures in January 2015 is surprising, as the rules governing voluntary disclosures were significantly tightened from January 1st, 2015. Many, including NRW's Finance Minister at the time, had predicted a marked drop in voluntary disclosures for January 2015 (from their level in December 2014). Evidently, this drop did not materialize. Instead, voluntary disclosures surged in January 2015. Third, and of greatest relevance for our research question, the monthly number of voluntary disclosures increases substantially in the first few months after data purchases have become public, but shows no local hike immediately before such purchases emerge. This pattern in relative time (not calendar time) is suggestive that data purchases were unanticipated, i.e. indeed a shock, and that these data acquisitions exerted a sizeable and time-limited effect on voluntary disclosures.

2.2. Empirical strategy

NRW shared the whistleblower data with the other German states, which means that our setting does not permit identification of a non-parametric counterfactual in a classic treatment-control group design analysis at the state level. This limitation calls for caution, as it is difficult to disentangle treatment effects from potential trends in time series data for just one state (NRW). For this reason, we use a very flexible parametric approach in our analysis that considers both polynomial time trends and year fixed

[21] Data acquisitions have generally not been announced (nor were any details revealed later) by tax authorities in NRW. Information on the timing and content of data acquisitions has been compiled from press items in print and online media. Information on the first press coverage of individual data purchases, including the exact publication date, is provided in Table A.1 in the Appendix.

[22] Both in June 2010 and August 2012, two sets of data purchases have been revealed in the press. The only purchase of data by tax authorities in NRW which did not involve Swiss banks (bank data from Luxembourg, made public in the press in October 2011) is not shown in Fig. 1.

effects. Specifically, to identify the effect that news of purchases by NRW tax authorities of Swiss bank data have on the number of voluntary disclosures by NRW residents, we estimate variants of the following equation for time-series data:

$$y_t = \alpha + year_t + \sum_{i=1}^{z} \gamma_i \times month_t^i + \sum_{j=-1}^{n} \beta_j \times data_{t-j} + \varepsilon_t, \tag{1}$$

where y_t is the log of the number of monthly voluntary disclosures (plus one[23]) involving Swiss banks made by NRW residents to tax authorities in NRW in calendar month t, $year_t$ is a set of year dummies, $\sum_{i=1}^{z} month_t^i$ is a polynomial of degree z in (monthly) calendar time, and $\sum_{j=-1}^{n} data_{t-j}$ is a set of indicator variables capturing whether the current period (calendar month) t is leading a data purchase press report by one month, coinciding with a period of data purchase announced in the press, or lagging a period of data acquisition news by one to n months. In the empirical analysis, we choose n, i.e. the number of lags, to equal 5. Finally, ε_t is an error term.

The treatment effect of interest is measured by the β_j coefficients, for $j \geq 0$. These coefficients capture the contemporaneous ($j = 0$) and delayed responses ($j > 0$) of voluntary disclosures to news of a data purchase. The coefficient β_{-1} on our indicator for the month before a new purchase has become public captures any anticipatory effects on voluntary disclosures. Its estimate, $\hat{\beta}_{-1}$, should be statistically indistinguishable from zero if our dating of the first media reports on purchases is correct and emergences in the press are indeed shocks that change the information set and detection risk of potential tax evaders. The set of year dummies $year_t$ controls for level changes in voluntary disclosures between different calendar years, and the polynomial of degree z in (monthly) calendar time $\sum_{i=1}^{z} month_t^i$ controls in a flexible way for any underlying monthly trend affecting the evolution of voluntary disclosures over time, as could be caused, for instance, by trend changes in the efficiency of tax authorities, in economic conditions, or in tax honesty. In the empirical analysis, we consider $z \in \{0, 1, 2\}$, i.e. no monthly trend of any kind, a linear monthly trend, and a linear–quadratic trend. The latter is the most flexible specification, and arguably also the most suitable for our purposes, given the U-shaped evolution of voluntary disclosures over time (see Fig. 1). Throughout, we use robust standard errors.

3. Results

Table 1 reports the main regression output for four different specifications of Eq. (1).[24] The first three specifications differ only in the type of underlying monthly trend considered in the analysis. Specification (I) considers no monthly trend of any kind, specification (II) a linear monthly trend, and specification (III) a linear–quadratic monthly trend. Being the most flexible, specification (III) will constitute our baseline specification that we later use both for robustness checks and additional explorations (see Tables 2 and 3, as well as the left diagram in Fig. 2).

As it turns out, the estimated coefficient $\hat{\beta}_{-1}$ of the lead variable $data_{t+1}$ is insignificant in all three specifications. This lack of a pre-treatment effect is reassuring, as it suggests no anticipatory effects on monthly disclosures prior to the month in which a data purchase has been made public in the news.[25] The immediate response of voluntary disclosures to the emergence of a new data purchase in month t, given by the estimated coefficient $\hat{\beta}_0$ of $data_t$, is positive and of much larger magnitude than $\hat{\beta}_{-1}$ of the lead variable $data_{t+1}$, although also imprecisely estimated in the first two specifications. Finally, and in all three specifications, estimated coefficients $\hat{\beta}_1$ through $\hat{\beta}_5$ on the remaining set of indicator variables $data_{t-1}$ to $data_{t-5}$ show a strong positive and hump-shaped response of monthly voluntary disclosures in the first four months after a new data purchase by tax authorities in NRW has been made public in the press. In our most flexible specification (III), voluntary disclosures are on average 1.265 log points higher, which translates into an increase of 254%, in the first month after a new data acquisition has emerged compared to what they would have been, had no such purchase taken place, and still 1.138 log points (212%) higher in the second month after news of a new data purchase.[26] In the third and fourth month after the emergence of a new data purchase, a sizeable (yet also smaller) positive effect on disclosures is still observable; only in the fifth month do voluntary disclosures cease to show signs of a systematic positive response. Voluntary disclosures hence appear to react strongly to news of purchases of bank data that could identify potential tax evaders holding untaxed wealth in Swiss bank accounts. Observable in all three specifications, this hump-shaped pattern proves robust to the way we model any underlying monthly trend in voluntary disclosures that may arise from changes in unobservables over time. According to the findings from our most flexible specification (III), voluntary disclosures in 2010–2015 have risen by 11,298 as a result of state data acquisitions (or about half the total number of disclosures recorded in this period, i.e. 22,487).[27] Assuming

[23] In November 2011, zero disclosures were filed. For this reason, we take the log only after adding one to each monthly disclosure observation. Excluding November 2011 from our sample and using as dependent variable instead the log of disclosures does not affect the estimated response over time of voluntary disclosures to press reports of a new data acquisition by the tax authorities.

[24] The endogenous variable in all four specifications is the log of the monthly absolute number of voluntary disclosures involving Swiss banks made by NRW residents to tax authorities in NRW. All four specifications furthermore contain as regressors a set of year dummies for 2011–2015 (base year is 2010) and a set of indicator variables that capture systematic deviations in relative time (and not in calendar time) of the log of monthly voluntary disclosures in the vicinity of a month in which a new data purchase by the tax authorities in NRW has been made public in the press.

[25] The lack of a pre-treatment effect also suggests that our news-based dating of the month in which a data purchase has been made public does not suffer from systematic mis-measurement in the form of too late a recording of the month in which information on a new purchase actually became available.

[26] Percentage effects are obtained from the transformation $(e^{\hat{\beta}_j} - 1) \times 100\%$.

[27] For our back-of-the-envelope calculation, we use a correction (Stata command `levpredict`) to obtain level predictions from our log-dependent-variable regression to reduce retransformation bias (see section 3.6.3 in Cameron and Trivedi (2009) for further details). We obtained these level predictions for two scenarios: a treatment world and a control world (for the latter, we set all dummy indicators for a new data acquisition with a significant coefficient to zero). The summary total difference in monthly level predictions between these scenarios over the observation period is 11,298.

Table 1
Main regression results.

	(I)	(II)	(III)	(IV)
$data_{t+1}$	0.249 (0.300)	0.305 (0.310)	0.300 (0.259)	0.248 (0.259)
$data_t$	0.361 (0.318)	0.334 (0.298)	0.435* (0.249)	
$data_t \times (\leq 2$ weeks)				-0.061 (0.236)
$data_t \times (> 2$ weeks)				0.735*** (0.272)
$data_{t-1}$	1.035*** (0.333)	0.974*** (0.298)	1.265*** (0.290)	1.229*** (0.287)
$data_{t-2}$	0.720** (0.316)	0.781*** (0.267)	1.138*** (0.276)	1.133*** (0.276)
$data_{t-3}$	0.701*** (0.205)	0.667*** (0.208)	0.837*** (0.225)	0.846*** (0.221)
$data_{t-4}$	0.506* (0.267)	0.604** (0.236)	0.788*** (0.199)	0.804*** (0.207)
$data_{t-5}$	0.053 (0.295)	0.103 (0.287)	0.268 (0.244)	0.257 (0.241)
year indicators (2011–2015)	yes	yes	yes	yes
monthly trend (linear)	no	yes	no	no
monthly trend (linear–quadratic)	no	no	yes	yes
R^2	0.63	0.69	0.75	0.76
N	71	71	71	71

Notes: The endogenous variable is the log of the monthly number of voluntary disclosures submitted by residents of NRW which involve illicit wealth held in Switzerland. Dummy variable $data_t$ takes the value one if it became public in period t that tax authorities in NRW had bought a new data CD with potential tax evaders. Dummy variable $data_{t+1}$ brings forward this information by one month to account for potential anticipatory effects, and dummy variables $data_{t-1}$ to $data_{t-5}$ capture the (the possibly highly non-linear) response over time of voluntary disclosures to news reports of a new data purchase on potential tax evaders with illicit wealth in Switzerland. All regressions include year dummies (base year is 2010). The estimation sample comprises voluntary disclosures submitted in the months January 2010 to November 2015. Robust standard errors are used.

* Significance at the 10% level.

** Significance at the 5% level.

*** Significance at the 1% level.

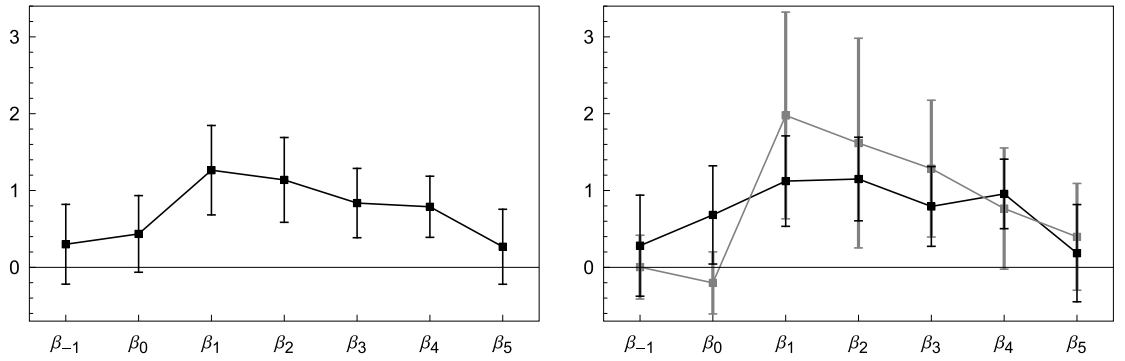

Fig. 2. Graphical representation of treatment leads and lags.
Left diagram: Plotted estimates are from our baseline specification, as reported in column (III) of Table 1. Right diagram: Plotted estimates are from a regression that uses two sets of dummy variables $data_{k,t-j}$, with $j \in \{-1, 0, 1, \ldots, 5\}$ and $k \in \{1, 2\}$, one for purchases that did implicate specific Swiss banks ($k = 1$, shown in black), and one for purchases that did not ($k = 2$, shown in gray), but that is otherwise identical to our baseline specification. Point estimates in both diagrams are marked by a dot, and the vertical bands indicate the 95% confidence interval of each estimate.

an average additional tax revenue of 51,000 euros per disclosure (Landtag Nordrhein-Westfale, 2015), this translates into an extra 576 million euros for NRW. As the ten data acquisitions cost less than 20 million euros in total, this implies an enormous financial gain for the state of North-Rhine Westphalia. The left diagram in Fig. 2 illustrates the findings of our baseline specification (III) graphically.

On several occasions, news of a new data purchase emerged only late in a calendar month, which left tax evaders possibly too little time (days) to file a voluntary disclosure in that same month.[28] In our analysis so far, we have disregarded this heterogeneity, which may explain (at least in part), why the contemporaneous effect of news of a new data purchase is positive but less

[28] In three out of the eight calendar months in which a new purchase of data became public, the news of such a data acquisition emerged only in the last two weeks of the respective calendar month. Exact dates of the first media reports on individual data acquisitions are provided in Table A.1 in the Appendix.

precisely estimated. To investigate this possibility, we re-run a slightly changed variant of our baseline specification (III). This new specification (IV) is identical to specification (III) but uses two dummies ($data_t \times (\leq 2$ weeks) and $data_t \times (> 2$ weeks)) instead of one ($data_t$) for the month in which it became public that a new data CD with potential tax evaders has been bought by tax authorities in NRW. Dummy $data_t \times (\leq 2$ weeks) is equal to one when two weeks or less within such a month remain for potential tax evaders to file a disclosure, and zero otherwise. Dummy $data_t \times (> 2$ weeks), in turn, equals one if more than two weeks remain, and zero otherwise.[29] Given that it takes time to file a disclosure, we would expect news on a new data purchase to exert a stronger positive effect on absolute monthly disclosures if such news break early rather than late within a calendar month, i.e. the estimated coefficient on $data_t \times (> 2$ weeks) should be positive and larger than that on $data_t \times (\leq 2$ weeks). As shown in the last column of Table 1, our regression results are consistent with this conjecture. Furthermore, estimated coefficients on our lead and our five lags variables, i.e. on $data_{t-j}$ for $j \neq 0$, show the same hump-shaped response of voluntary disclosures to news of a new data purchase.[30] In fact, estimated coefficients differ little in magnitude from those of the baseline specification.

We conduct several further tests to assess the robustness of our findings. First, we trim our estimation sample by omitting the first 7, respectively the last 11 and the last 14 months of our observation period (January 2010 to November 2015). Our observation period starts in January 2010, just one month before the first news of a data CD purchase broke, news that furthermore saw the largest absolute rise in voluntary disclosures in our data. Omitting the first seven months (January to July 2010) from the estimation sample allows us to check the robustness of our results to the omission of this first data purchase and the marked global peak in voluntary disclosures we observe in its vicinity. By omitting the last 11 months of our observation period, in turn, we effectively omit all 2015 observations (January 2015 to November 2015). This restriction provides for a more homogeneous policy environment, as rules for voluntary disclosures were significantly tightened and fines increased from 2015. Finally, omitting the last 14 months from our observation period (October 2014 to November 2015) provides not only for a more homogeneous policy environment by excluding all months from 2015, but also helps to assess whether the last months of 2014, i.e. the last months under the old policy regime, differed somehow from preceding months because tax evaders, in anticipation of the approaching tightening of the rules for voluntary disclosures, filed in larger numbers a voluntary disclosure so as to become tax compliant still under the old 'lower cost' policy regime. As shown in Table 2, however, our finding of a positive and hump-shaped response in the monthly number of voluntary disclosures proves robust to such alterations in the starting, respectively end points of our observation period.

Table 2
Robustness checks I (trimmed samples).

	Baseline specification	Trimmed sample, excluding:		
		$t \leq 7$	$t \geq T-11$	$t \geq T-14$
	(1)	(2)	(3)	(4)
$data_{t+1}$	0.300 (0.259)	0.405 (0.283)	0.104 (0.247)	0.161 (0.237)
$data_t$	0.435* (0.249)	0.427* (0.246)	0.384 (0.302)	0.617** (0.264)
$data_{t-1}$	1.265*** (0.290)	1.171*** (0.313)	1.093*** (0.242)	1.114*** (0.222)
$data_{t-2}$	1.138*** (0.276)	1.086*** (0.269)	0.982*** (0.233)	1.037*** (0.232)
$data_{t-3}$	0.837*** (0.225)	0.912*** (0.189)	0.751*** (0.224)	0.747*** (0.231)
$data_{t-4}$	0.788*** (0.199)	0.868*** (0.183)	0.688*** (0.215)	0.607*** (0.214)
$data_{t-5}$	0.268 (0.244)	0.330 (0.248)	0.259 (0.226)	0.293 (0.199)
year indicators (2011–2014)	no	no	yes	yes
year indicators (2011–2015)	yes	yes	no	no
monthly trend (linear–quadratic)	yes	yes	yes	yes
R^2	0.75	0.75	0.79	0.81
N	71	64	60	57

Notes: Column (1) re-produces the results from our baseline specification (III) in Table 1. Column (2) omits the first seven months from the observation period (i.e. January to July 2010)), column (3) omits all months from 2015, and column (4) omits all months after September 2014. Otherwise, estimation samples, specifications and covariates considered in these regressions are identical to those of our baseline specification (III) of Table 1 (see notes to Table 1). Robust standard errors are used.

* Significance at the 10% level.

** Significance at the 5% level.

*** Significance at the 1% level.

Second, we add to the set of regressors in our baseline specification the log (mean) monthly exchange rate between the Swiss franc and the euro. The CHF/EUR rate has seen some major changes in our period of analysis.[31] The Swiss franc appreciated steadily

[29] Two data acquisitions became public in the same calendar month in June 2010 (on the 9th and one the 24th) and in August 2012 (on the 8th and on the 22nd). In each case, we classify the respective month based on the earlier date.

[30] Note that this response pattern also proves robust, both for our baseline specification (III) and for specification (IV), when we account for seasonal effects in our empirical specification by including quarter-of-the-year dummies or allow for an even more flexible trend specification, i.e. a linear–quadratic–cubic trend.

[31] Floating against the euro, the Swiss franc appreciated steadily against the euro in 2010 and the first half of 2011, increasingly harming Swiss exporters. To halt this rise of the Swiss franc, the Swiss National Bank (SNB) introduced a peg in September 2011, intended to prevent the euro from trading below 1.20 Swiss francs. However, as the outlook for the euro darkened, the policy of defending the peg became increasingly costly for the SNB. Without prior notice, on

Table 3
Robustness checks II (exchange rate, German-Swiss tax agreement) and additional explorations (Google trends).

	(1)	(2)	(3)	(4)
$data_{t+1}$	0.300 (0.259)	0.293 (0.257)	0.317 (0.270)	-0.112 (0.135)
$data_t$	0.435* (0.249)	0.418* (0.246)	0.475* (0.256)	0.568** (0.225)
$data_{t-1}$	1.265*** (0.290)	1.199*** (0.314)	1.269*** (0.285)	0.370*** (0.127)
$data_{t-2}$	1.138*** (0.276)	1.100*** (0.304)	1.142*** (0.269)	0.218 (0.210)
$data_{t-3}$	0.837*** (0.225)	0.803*** (0.246)	0.742*** (0.218)	0.176* (0.103)
$data_{t-4}$	0.788*** (0.199)	0.743*** (0.244)	0.590*** (0.193)	0.026 (0.113)
$data_{t-5}$	0.268 (0.244)	0.252 (0.251)	0.165 (0.269)	-0.117 (0.109)
CHF/EUR exchange rate (in logs)		3.417 (5.993)		
tax agreement failure			0.858* (0.441)	
R^2	0.75	0.75	0.76	0.67
N	71	71	71	71

Notes: Column (1) re-produces the results from our baseline specification (III) in Table 1. Column (2) reports results when we add the log of the CHF/EUR exchange rate to the set of regressors in our baseline specification, and column (3) results when we add a dummy variable that takes value one from November 2012 (and zero before that month). Finally, column (4) reports results from our baseline specification when we use as dependent variable the log of the monthly relative search frequency in Google for the German keyword 'Selbstanzeige' (voluntary disclosure) in Germany in our observation period (January 2010 to November 2015). Estimations samples, specifications and covariates considered in these regressions are otherwise identical to those of our baseline specification (III) of Table 1 (see notes to Table 1). In particular, all regressions include year dummies (base year is 2010) and a second-order polynomial in time (calendar months). Robust standard errors are used.

* Significance at the 10% level.

** Significance at the 5% level.

*** Significance at the 1% level.

against the euro throughout 2010 and most of 2011 (until the introduction of a peg to the euro in September 2011) and soared sharply in January 2015, when the Swiss National Bank (SNB) without prior notice and to the surprise of market participants suddenly abandoned the peg. The sharp appreciation of the Swiss franc may have affected the number of voluntary disclosures, which increased strongly, yet only temporarily, in January 2015. As shown in column (2) of Table 3, however, the CHF/EUR rate has an insignificant (albeit positive) effect on the number of voluntary disclosures per month. As such, there is little evidence that the marked rise in disclosures from December 2014 to January 2015, a surprising and unexpected development given the tightening of the rules for voluntary disclosure in January 2015, was driven by this change in the CHF/EUR exchange rate. The data purchase in December 2014 together with the hump-shaped response of voluntary disclosures we find to such purchases in our data, however, can explain this local hike in voluntary disclosures at the beginning of 2015. In any case, our key finding again proves robust, i.e. voluntary disclosures continue to show a hump-shaped response to news of a data purchase by tax authorities in NRW. In fact, the pattern of this response differs only little from that in our baseline specification (reported again, for convenience, in column (1) of Table 3).

Third, we add to our baseline set of regressors a dummy variable, which takes value zero before November 2012, and value one from November 2012, to capture any level effect on voluntary disclosures of the failure on November 23rd 2012 of a long-awaited tax treaty between Germany and Switzerland in Germany's upper legislative chamber, the Bundesrat. The accord aimed to ensure the equal treatment of the wealth of German citizens, whether located in Germany or in Switzerland, and to restore tax equity for the past by means of a lump sum tax payment.[32] The failure of the tax treaty meant that voluntary disclosure was to remain the only option available to Germans to legalize their untaxed assets in Switzerland, i.e. become tax compliant, and escape prosecution for tax evasion. It is possible that some tax evaders refrained from disclosing undeclared wealth in Switzerland because they awaited the tax agreement to take force. If so, its failure may have spurred voluntary disclosures. As seen in column (3) of Table 3, we do indeed find evidence for a positive effect of the failure of the German-Swiss tax agreement on voluntary disclosures. Our core finding, however, proves robust once again. Voluntary disclosures still show a hump-shaped response to the emergence of new data purchases by tax authorities in NRW.

We also conducted a number of additional explorations. First, we obtained a measure from Google trends of the relative search frequency in Germany in our observation period of the German keyword 'Selbstanzeige' (voluntary disclosure) and used the log of

15 January 2015, the SNB stunned markets by abandoning the currency peg, which caused the Swiss franc to soar immediately and strongly against the euro. In our observation period, the monthly exchange rate between the Swiss franc and the euro averaged 1.22 CHF/EUR (standard variation of 0.10). It was lowest in April 2015 (1.04 CHF/EUR) and highest in January 2010 (1.48 CHF/EUR).

[32] Under the agreement, planned to take effect on January 1st 2013, undeclared wealth of Germans in Switzerland over the last 10 years would have been taxed at a rate between 21 and 41 percent, and tax evaders would have remained anonymous. From 2013, they would have been taxed at normal German rates. Similar Swiss accords (withholding tax deals) had been signed, for example, with Austria and the UK.

this measure as dependent variable in our baseline specification to see whether news of data purchases by tax authorities in NRW spurred public interest in voluntary disclosures.[33] Increased keyword searches may stem from tax evaders which, shocked by news of a data purchase, seek information on how to file a timely disclosure. If so, news of a new data purchase should have a positive effect on keyword searches for voluntary disclosure. Furthermore, such a positive effect should materialize more quickly (following the emergence of a new purchase) and show less persistence than the effect on actual disclosures. As shown in column (4) of Table 3, all three predictions find empirical support. Keyword searches from Germany for voluntary disclosure in Google do respond positively to news of a new data purchases, and this response is both more immediate and less persistent than that of voluntary disclosures (see baseline estimates, re-produced in column (1) of Table 3). Of course, this auxiliary piece of evidence is only suggestive, since we do not know if it is indeed tax evaders who account for this increase in keyword searches. The time structure of the effect (i.e. an immediate and short-lived response), however, is supportive of such an interpretation and also consistent with a causal interpretation of our core finding on the evolution of actual disclosures following a new data acquisition.

Second, we differentiate in our baseline specification between purchases of data from Switzerland which have been linked in the press to specific Swiss banks and purchases which have not.[34] A priori, it is unclear which type of data purchase generates more voluntary disclosures. In the former case, a smaller absolute number of tax evaders is potentially at risk of being identified by the tax authorities. However, this smaller group is also facing a much more elevated risk of detection, given that it is their very own bank from which tax authorities in NRW have allegedly acquired data. To explore this question empirically, we replace in our baseline specification the set of dummy variables $data_{t-j}$, where $j \in \{-1, 0, 1, \ldots, 5\}$, with two such sets $data_{k,t-j}$, one for purchases which are known to involve data from specific Swiss banks ($k = 1$), and one for purchases that are not ($k = 2$). Estimated coefficients on these two sets of dummy variables are plotted in the right diagram of Fig. 2.[35] As is evident, voluntary disclosures after news of both type of purchases do show a hump-shaped response, but one that differs somewhat in shape and magnitude, not only between these two types of purchases, but also from the estimated structure of response to a press report of a data purchase (of any kind) in our baseline regression (see left diagram in Fig. 2). A data purchase known to involve specific Swiss banks (black line) exerts a statistically significant positive effect (at the 5% level of significance) already from the very month it emerged (unlike in our baseline regression), whereas no such significant contemporaneous effect emerges for the subgroup of purchases of bank data which were publicized without implicating specific financial institutions (gray line). This more timely response may be explained by the greater need of tax evaders to respond quickly by way of voluntary disclosure as they face a greater and more imminent risk of identification and hence prosecution by tax authorities following such a purchase. Reassuringly, for both subgroups of purchases, the estimated lead coefficient on $data_{k,t+1}$ is again insignificant, which speaks against anticipatory effects of any kind also at the level of these subgroups of purchase types, and any positive stimuli provided by these data acquisitions again have fully faded out 5 months after a purchase had become public.

4. Conclusion

International tax evasion is a major concern to policy makers and tax authorities. Lack of information on domestically owned assets held abroad is at the heart of states' inability to enforce tax compliance of its citizens. Tax havens, for obvious reasons, are reluctant to share information on unrecorded wealth with countries suffering from tax evasion. This hampers the effectiveness of bilateral tax treaties. By acquiring confidential bank data from tax havens, however, countries suffering from tax evasion may identify (at least) some tax evaders and increase risks of detection for all those domestic residents who hold untaxed wealth abroad. What is more, this policy tool is subject only to own discretion, and hence can be used unilaterally by countries suffering from tax evasion.

In this paper, we investigated the discretionary and unilateral use of such state-sponsored purchases of whistleblower data to identify untaxed wealth held abroad. For this purpose, we compiled and analyzed press reports on data acquisitions involving Swiss banks by tax authorities from Germany's most populous federal state North-Rhine Westphalia (NRW) in the years 2010 through 2015, along with information on monthly voluntary disclosures made by NRW residents over this period. Our identification strategy relies solely on time-series variation, as NRW shared whistleblower data with other German states, which precludes standard treatment-control group design analyses at state level. This limitation calls for caution. We addressed this limitation by using a very flexible parametric approach in our analysis that considers both polynomial time trends and year fixed effects to study behavioral responses around multiple well-identified information shocks.

Our results strongly suggest that publicized purchases of whistleblower data were effective in promoting tax compliance by encouraging tax evaders to come forward and declare untaxed assets through voluntary disclosures. These unilateral data acquisitions also yielded substantial fiscal returns for the state of North-Rhine Westphalia, generating an extra 576 million euros in tax revenue

[33] Google trends does not provide monthly information on the relative search frequency at the level of individual federal states in Germany. We therefore use information for the whole of Germany. Data on a keyword search from Google are normalized by the absolute number of search queries in the geographical region of interest to remove any trends from growth in internet usage or from changes in the relative popularity of Google as a search engine. The normalized data are then rescaled to an index with a maximum value of 100. In our observation period, the average value of the index for the keyword search 'Selbstanzeige' (voluntary disclosure) was 18.4 (s.d. 15.8) with a minimum of 9 and a maximum (by construction) of 100.

[34] Of the altogether 10 purchases, which were made public in the news in eight different calendar months (multiple purchases occurred in two months), seven purchases (in six calendar months) were known to affect certain Swiss banks. Purchases emerging in June 2010 and December 2014 were publicized to involve Swiss accounts, but did not name any particular Swiss bank. For more details, See Table A.1 in the Appendix.

[35] The full tabulated regression output is available from the authors upon request.

at a purchase cost of less than 20 million euros. Various robustness checks support this conclusion, as do further analyses. State purchases of confidential bank data from tax havens therefore appear to be a valuable supplementary policy tool in the fight against international tax evasion. They help governments identify tax evaders and promote compliance — even in the absence of cooperation from tax havens. For this reason, their use (or even the mere threat of their use) can serve as a viable policy tool to induce tax havens to participate in, and remain part of, automatic exchange of information agreements such as the CRS.

CRediT authorship contribution statement

Dirk Bethmann: Writing – review & editing, Writing – original draft, Methodology, Conceptualization. **Michael Kvasnicka:** Writing – review & editing, Writing – original draft, Methodology, Conceptualization.

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Appendix

Table A.1
Data sources for individual CD purchases.

CD	First made public		Allegedly involved bank	
	on	by	bank	source
1	2010–02–04	HB (2010)	Credit Suisse	HB (2010)
2a	2010–06–09	Stern (2010)	–/–	–/–
2b	2010–06–24	SPON (2010)	–/–	–/–
3	2010–10–17	Spiegel (2010)[a]	Julius Bär	Zeit Online (2010)
4	2012–07–13	Stern (2012)	Coutts	Stern (2012)
5a	2012–08–08	HB (2012)	UBS & an unknown bank	HB (2012)
5b	2012–08–22	MM (2012)	Julius Bär	MM (2012)
6	2013–04–16	FAZ (2013)	Neue Aargauer Bank, Clariden Leu, Credit Suisse	FAZ (2013)
7	2014–01–12	SPON (2014)	Leumi	SPON (2014)
8	2014–12–20	BamS (2014)	–/–	–/–

[a] Information was released one day before publication of the print issue.

- BamS (2014): "NRW erwirbt neue Steuer-CD", *Bild am Sonntag*, 2014–12–20.
- HB (2010): "Steuer-CD soll 400 Mio. Euro wert sein", *Handelsblatt*, 2010–02–04.
- HB (2012): "Steuersünder-CDs", *Handelsblatt*, 2012–08–08.
- FAZ (2013): "Durchsuchungen nach Kauf von Steuer-CD", *Frankfurter Allgemeine Zeitung*, 2013–04–16.
- MM (2012): "Julius-Bär-Kunden im Visier der Fahnder", *Manager Magazin*, 2012–08–22.
- Spiegel (2010): "Geschäfte mit Ganoven", *Der Spiegel*, 2010–10–18.
- SPON (2010): "Spanien will Milliarden Euro von Steuersündern eintreiben", *Spiegel Online*, 2010–06–24.
- SPON (2014): "Ermittler spüren Dutzende mutmaßliche Steuersünder auf", *Spiegel Online*, 2014–01–12.
- Stern (2010): "Bund und Niedersachsen kaufen Steuer-CD", *Der Stern*, 2010–06–09.
- Stern (2012): "NRW kauft weitere Steuer-CD aus der Schweiz", *Der Stern*, 2012–07–13.
- Zeit Online (2010): "Fiskus erhält Zugriff auf Schweizer Konten", *Die Zeit Online*, 2010–10–17.

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