KIEL INSTITUTE FOR THE WORLD ECONOMY

KIEL WORKING PAPER

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No. 2171 November 2020

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Kiel Institute for the World Economy
ISSN 1862–1155

Leibniz Association

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Abstract

We construct a novel database of monthly foreign exchange interventions for 49 countries over up to 22 years. We build on a text classification approach that extracts information about interventions from news articles and calibrate our procedure to data about actual interventions. Our new dataset allows us to document stylized facts about the use of foreign exchange interventions for countries that neither publish their data nor make them available to researchers. Moreover, we show that foreign exchange interventions are used in a complementary way with capital controls and macroprudential regulation.

JEL classification: F31 (foreign exchange); F33 (international monetary arrangements); E58 (central banks and their policies).

Keywords: Foreign exchange intervention; capital controls; macroprudential regulation; international capital flows.

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*We thank for comments and discussions during presentations held at several seminar, the 2020 EEA annual conference and the 2020 German Economic Association (VfS) annual conference. Moreover, we thank all participating central banks and the respective staff members for their extremely cooperative approach in providing and explaining the foreign exchange intervention data. We thank the European Central Bank (ECB) which hosted the very early phase of this research project. Lukas Menkhoff and Tobias Heidland gratefully acknowledge financial support by the German Research Foundation (DFG).

1 Introduction

The continued rise of financial globalization and the related openness of countries have brought new challenges for economic policymaking. Many countries believe that they need better shields against volatile international capital flows and the resulting instabilities of their domestic economy. These challenges have led to policy responses over the last decade, which include reliance on foreign exchange (FX) interventions to keep control over international capital flows, the exchange rate and ultimately over the domestic economy. This goes along with a remarkable change in the international policy debate, which has become much more open to the application of capital flow management tools, such as FX interventions. An example of this change is the new stance of the IMF (2012), which states that capital flow management tools can be useful in realizing macroeconomic and financial stability during surges of capital inflows or strong capital outflows.

FX interventions are an established policy tool that has been used in all kinds of exchange rate regimes (Eichengreen, 2019). At the beginning of the floating era in the 1970s and 1980s, there was some agreement that interventions may have a signaling effect (Ghosh, 1992) and a small portfolio effect (Dominguez and Frankel, 1993), thus giving FX interventions some role in managing international capital flows. Doubts about the effectiveness of FX interventions increased with the rapid growth of global financial markets. The global financial crisis of 2008/09 changed the assessment again. Recently, several studies have applied an event study or a matching approach and provided evidence that FX interventions can be effective (see, e.g., Dominguez, 2020). Accordingly, FX interventions seem to smooth exchange rate fluctuations and also to impact the level and trend of exchange rates to some degree (e.g., Fatum and Hutchison, 2003; Fratzscher et al., 2019). Much of the new attention towards FX interventions is due to the increased relevance of emerging markets in the world economy, where interventions are quite frequently used (Menkhoff, 2013; Frankel, 2019), while central banks in the US, the Euro area or the UK hardly intervene any more.

The use of FX interventions has also gained from new theoretical work which extends the still accepted view that FX interventions can provide more freedom for monetary policy (Klein and Shambaugh, 2015). For example, Cavallino (2019) shows that in his model a mix of monetary policy and FX intervention (as an alternative to capital controls) is an optimal policy response to portfolio flow shocks. Hassan et al. (2017) argue that intervention can be a means to achieving lower risk-free interest rates, higher capital accumulation and higher wages because keeping a currency close to that of a larger anchor currency helps the domestic currency to depreciate less in bad times.

In some contrast to the politically and theoretically motivated interest in FX interventions, there is a lack of empirical studies systematically analyzing their impact on exchange rates and capital flows. Despite a long tradition of country studies relying on precise FX intervention data, there is a gap regarding cross-country studies. The latter typically have to rely on changes in foreign currency reserves as a proxy for interventions ("reserve proxy"). The disadvantage of this approach is, however, that there are large differences between reserve changes and interventions, as reserve changes may occur for many reasons, only one of which is interventions. Therefore, it is the main contribution of this paper to introduce a new database containing an FX intervention proxy which we make publicly available and which provides more reliable information about FX interventions than pure reserve changes.

Our FX intervention proxy is based on publicly available news articles and reserve data. We implement a support vector machine to classify individual news data based on a quantitative representation of the text in order to extract relevant information about FX interventions that can then be used to create the proxy. This algorithm is trained and tested on the dataset of hand-coded news of Fratzscher et al. (2019), in the following abbreviated as FGMSS; the algorithm captures 99% of relevant news at a monthly frequency. We show that our news-based approach delivers a far more precise proxy for FX interventions than reserve changes. Then, we use this algorithm to construct proxies for FX interventions for a broader set of countries and a longer time series compared to the data in FGMSS. At the end we get a new set of 49 country-specific time series of approximate FX interventions over the period 1995 to 2016.

We use these new data to provide two novel findings. First, we report stylized facts about the use of FX interventions for 49 countries depending on various market characteristics and exchange rate regimes. Results extend those in Fratzscher et al. (2019) for a larger dataset and provide information in which ways our FX intervention proxy represents actual interventions. Second, we study the occurrence of FX interventions in conjunction with other common tools of capital flow management, i.e. capital controls and macroprudential regulations. While these tools have been analyzed in isolation in prior research (e.g., Jeanne and Korinek, 2010; Jeanne, 2012; Klein and Shambaugh, 2015; Ghosh et al., 2017; Bianchi and Mendoza, 2018; Korinek, 2018), there has been little attempt to analyze them jointly (an exception being Gosh et al., 2017). We find that the use of these three tools is positively correlated: countries with a higher level of capital controls and countries which increase macroprudential measures intervene more often, suggesting that an impact analysis of one instrument should control for the use of other instruments in order to avoid confounding effects.

Our research is mainly related to the empirical literature on FX interventions. There has so far been no comprehensive, publicly available database on FX interventions. Instead, researchers follow three strands in order to analyze FX interventions: (i) case studies based on true FX intervention data, (ii) case studies relying on reserve-based proxies in order to compensate for missing FX intervention data, and (iii) a few attempts of cross-country analysis. Let us discuss briefly these three strands. First, studies based on true intervention data are typically country case studies because only very few countries make their intervention data publicly available (e.g., Fischer and Zurlinden, 1999; Melvin et al., 2009; Chamon et al., 2017; Kuersteiner et al., 2018). While these studies can often rely on quite detailed data, it remains unclear to which extent their results can be generalized. Second, due to the very restricted data availability, researchers often cannot work with true intervention data but use proxies for FX interventions. The two kinds of publicly available proxies are based either on news, such as reports about FX interventions in newspapers (Fischer, 2006), or on data about reserve changes (e.g., Blanchard et al., 2015; Daude et al., 2016; Adler et al., 2019). News-based proxies are so far used for country case studies because the data are laborious to compile. Reserve-based proxies are attractive in this respect as they are readily available; however, it is known that reserves can change for many reasons of which only one is FX interventions. Empirically they change basically every month while true FX interventions occur in the same countries statistically only every fifth month and do not follow regular patterns, as we show later. Thus reserve changes may be not very reliable as a proxy for FX interventions but still better than having no data at all. Third, from a research perspective, it is desirable to have reliable cross-country datasets. In practice, however, the problems mentioned above apply: either one has good data but for only very few countries (Dominguez and Frankel, 1993; Dominguez, 2003; Menkhoff et al., 2020), or one has many countries but relies on FX reserves (Blanchard et al., 2015; Daude et al., 2016), or one has good data on sterilized FX interventions and many countries but data are confidential (Fratzscher et al., 2019).

This paper consists of four further sections. Section 2 describes the development of the new database on FX interventions and shows its relation to actual interventions. Resulting stylized facts are documented in Section 3. Section 4 analyzes these data to examine relations between FX interventions, and the two other tools of capital flow management, i.e. capital controls and macroprudential policies. Section 5 provides conclusions.

2 Creating the new database

In Section 2.1, we detail the construction of our news-based proxy for FX interventions. In Section 2.2 we analyze the suitability of the news-based proxy and the widely used reserve change data to pin down intervention activity, and compare their performance to each other. Finally, we provide a summary of the important characteristics of our new FX intervention database in Section 2.3.

2.1 The news-based proxy for FX interventions

If researchers aim for more general insights based on the analysis of many countries, they have to rely on publicly available proxies. Two kinds of data can be used as proxies: first, publicly available changes in FX reserves, and second, published news about interventions.

Reserve data and news data as proxies for FX interventions. For any cross-country study, data on reserves have the great advantage of wide availability. Accordingly, reserve data are the proxy of choice in macro-oriented studies that want to exploit cross-country variation (e.g., Blanchard et al., 2015; Daude et al., 2016; Adler et al., 2019). Nevertheless, it is well-known that there are various reasons for reserves to change which are unrelated to FX interventions (see, e.g., Neely, 2000). These include (i) the central bank acting as an agent for the government regarding its FX transactions, (ii) valuation changes in reserve holdings and (iii) domestic monetary policy operations that may affect reserve holdings. It is an empirical question to which degree reserve changes do capture FX interventions. Central banks may sometimes deliberately choose forms of interventions that do not show up in reserves immediately. However, most of the recent research on the effectiveness of FX intervention suggests that FX interventions will lose much of their effectiveness if there is no signal to the market. Thus it is to be expected that FX interventions which are to some degree surprising, such as in flexible exchange rate regimes, will often be accompanied by communication. This can be either explicit communication of the central bank or an implicit communication, i.e. that the central bank accepts that market participants learn about the FX intervention.

This important role of communication is picked up by the news-based approach to identify FX interventions. While news data will tentatively underreport interventions, and will not work well at an intra-day frequency (Fischer, 2006), news data have the potential advantage that there is hardly any reason to incorrectly report intervention, so one of the major drawbacks of reserve-change-based proxies should not apply. Unfortunately, news data have to be extracted from respective databases, access to which is costly and their coverage of many smaller economies used to be patchy. Extracting and then manually coding individual news items is very laborious and thus often not feasible. However, two changes have made news data a far more attractive source for quantitative research recently: first, the coverage of news across countries has improved over time, and, second, there is the option to apply text classification approaches to extract efficiently the information of interest from huge amounts of news data (see, e.g., Hansen et al., 2018).

Processing news data using text classification. Our goal is to extract information about interventions from a large number of news articles and to provide a database on FX interventions which can be updated and maintained in the future. To this end, we rely on automatized pre-processing of the news data to bring the text into a quantitative format and then to use a support vector machine (SVM) model to classify the text.

We use news from Factiva, a major platform that was jointly created by Reuters and Dow Jones. We download headlines of articles that have been sampled using a standardized protocol (details in Appendix A) that was also used by FGMSS. All news items for the time in which actual intervention data were available are hand-coded according to this protocol.[1] The coding captures rumors, reports and official confirmations of intervention, all of which we suspect to proxy intervention. This is the first out of five steps to create the final database, see Figure 1.

[Figure 1 about here]

For six out of the original 33 countries used in FGMSS there are not enough news items to exploit (Azerbaijan, Bolivia, Costa Rica, Georgia, Kyrgyzstan, and Moldova). Although some of these countries intervene regularly, we only find an average of eight news items in the Factiva database for each of them using our search query. We therefore remove these countries from our database. For the rest of the countries, we compile all Factiva news reports, then pre-process and classify these documents (step 2 according to Figure 1).

Pre-processing of the data involves detecting and deleting common words that do not convey meaning (e.g.,"and"), stemming (i.e., reducing "intervene", "intervenes", and "intervention" to the common root "interven*") and summarizing the occurrence of individual terms in a text in matrix form. This matrix can then be used in quantitative models to classify news into those indicating intervention and those that do not.[2]

[1]For most countries, this means that all relevant news items according to the filter that were published on Factiva in 1995-2011 have been hand-coded.

[2]Note that our algorithm does not attempt to distinguish between sterilized and unsterilized inter-

For this classification, we use a standard Support Vector Machine (SVM, available from the open-source python package "sklearn") instead of other approaches that can run on a quantitative representation of the text data, such as simple logistic regressions or multinomial logit, because the SVM is known to yield better performance.[3] A support vector machine separates the data by choosing the hyperplane that optimally divides the training data along the outcome of interest. We apply a "modified Huber" loss function to penalize incorrect predictions, again chosen for best performance. The algorithm is trained using hand-coded data in which research assistants have manually conducted the same classification that we seek to automate using the algorithm. We then use 10-fold cross-validation to train the algorithm. This means that the complete dataset is randomly divided into ten sub-samples, each consisting of ten percent of the original data. Iteratively, nine of these samples are then used to train the algorithm and the remaining ten percent of the data are used to assess out-of-sample performance. The parameters that optimize out-of-sample performance are chosen automatically by the algorithm and used to classify all observations. To provide a systematic overview of the resulting algorithm on detecting FX intervention information in publicly available news, we then first compare manual and automatic classification before using the classified news data in combination with reserve data.[4]

Quality of automated news classification. Comparing the performance of the

vention. This is mainly because this distinction is seldom made in the financial press. It seems reasonable to expect that interventions discussed in the press are generally sterilized, at least in part, given that they are considered and discussed in the news as an instrument different from monetary policy. In any case, it remains possible that a small fraction of unsterilized intervention operations picked up by the algorithm generates measurement error in the resulting proxy FX intervention we provide, relative to the data used in Fratzscher et al. (2019) which only include sterilized intervention. In the news data used in this paper, only 17 out of the over 29,000 news items contain the substring "unsteril". All of these concern Japan and some state that the BOJ did not leave recent intervention unsterilized. Unsterilized intervention was, however, confirmed or hinted in news from September 2001, May 2002, and September 2010; in each case covered by several news items. Three monthly data points in our final dataset will thus contain unsterilized intervention. This will not systematically affect any results that make use of the full dataset we provide.

[3]For more details, see also Appendix B.

[4]A practical problem when using news data is the assignment of news to specific intervention days. While it is often possible to assign retrospective reports or confirmations to the intervention days when coding manually, a machine learning algorithm will struggle to do this reliably because it requires a detailed understanding of the text. Since reserve changes are merely available monthly, this problem is not that pressing for our paper, because we aggregate news data up, thus reducing the possible assignment-error to a minimum.

chosen algorithm to the manual classification as used in the FGMSS data, we see that 94 percent of all truly available (non-) intervention news items are correctly classified. A success rate of 94 percent may be considered acceptable even from human coders. Then we aggregate this daily information at the monthly frequency, calculating the incidence and number of news items suggesting intervention for each country and month. This aggregation to monthly frequency reduces the impact of errors in those cases where the algorithm has missed information on intervention because typically intervention is mentioned in more than one news item. Therefore, at the monthly frequency, even 99 percent of the aggregate manual coding can be reproduced. Overall, the use of the algorithm to classify news that indicate FX interventions seems to be successful.

Definition of news-based proxy. Having classified and aggregated news data, we get a binary indicator that captures whether Factiva news items have provided any evidence of intervention during a respective month. The extensive margin is thus based on an aggregate of news-based information which is the result of our step 2 in creating the database (see Figure 1). For those months with intervention according to the binary proxy, we then use reserve changes to define the intensive margin, since there is a reasonably strong correlation between reserve changes and intervention amounts in these particular months (which is our step 3). The proxy for currency c in month t can thus be written as

<u>Extensive margin:</u>

$$\text{News-based intervention dummy}_{ct} = \begin{cases} 1 \text{ if news dummy}_{ct} = 1 \\ 0 \text{ otherwise} \end{cases}$$

<u>Intensive margin:</u>

$$\text{News-based intervention proxy}_{ct} = \begin{cases} \text{reserve change}_{ct} \text{ if news dummy}_{ct} = 1 \\ 0 \text{ otherwise} \end{cases}$$

By construction, this proxy will miss intervention months during which there were no news in the press. Using the actual intervention data from FGMSS we know that larger monthly intervention volumes are more likely to trigger news. Therefore we capture a larger share of actual intervention volumes than the share of intervention months.

We will now first discuss the resulting quality from our procedure for automatically classifying news. Then, we turn to reserve changes before assessing the quality of the news-based intervention proxy and comparing it to using reserve changes on their own.

2.2 Judging the quality of proxies

To assess the quality of (competing) proxies for FX interventions one needs a benchmark. The problem we analyze can be seen as a standard problem of information retrieval, i.e. to retrieve the months of actual interventions from all months by relying on imperfect signals. The goal of any proxy is to realize a high share of correct predictions relative to the possible mistakes. To evaluate this, we can use a simple matrix (see Table 1) that relates actual interventions to predicted interventions. The resulting four fields are labeled as "true positive" (A: actual intervention and predicted intervention), "false positive" (B: no intervention but predicted intervention), "false negative" (C: intervention not predicted) and "true negative" (D: no intervention and no prediction).

[Table 1 about here]

To condense information from this table, we consider two aggregated success measures: (i) The "probability of detection" is the share of correctly predicted interventions over all actual interventions (A/(A+C)); (ii) the "probability of false alarm" is the share of false positives denominated by all actual non-intervention (B/(B+D)). Why these measures? First of all, there is a trade-off between type I and type II errors because a measure that will detect a very large share of interventions tends to predict too many interventions and thus comes at the cost of a higher rate of false alarm. We prefer a low probability of false alarm because this ensures that results are largely based on true FX interventions and thus informative. However, we explicitly examine both types of errors in the following.

Poor performance of reserve changes. In order to assess the performance of the reserve proxy, we visualize the relationship between actual FX interventions and the reserve proxy. Therefore, in Figure 2 we plot publicly available FX intervention data from Japan at the monthly frequency. We add Japan's contemporaneous reserve changes because these are often used as proxy for FX interventions. The figure shows the monthly volume of interventions and thereby also informs about the incidence of intervention. In the top panel, black bars indicate actual intervention volumes while white bars indicate the predicted size of the intervention. From the difference between black and white bars it can be inferred whether the proxy over- or underestimates the true intervention and how large the error is approximately. The bottom panel shows grey bars which highlight those months without FX interventions but changes in reserves (false positives). The bottom panel thus shows when this proxy errs in the extensive margin. By comparing black and white bars in the top panel, one can see the performance of the proxy in the intensive margin. Overall, the reserve proxy is able to capture a large share of the actual FX interventions, leading to a correlation between actual interventions and reserve changes of 0.77. However, this coefficient may be misleading as visualized by the grey bars: there are many and enduring periods of large discrepancies between these two time series during which reserve changes are a misleading proxy for actual interventions.

[Figure 2 about here]

A major source of noise that causes false positives when using reserve changes as intervention proxy is that most countries' reserves fluctuate monthly even without intervention. Consequently, one may think about considering only major reserve changes and leaving aside the many small changes that may have technical reasons rather than being the consequence of interventions. We implement this approach gradually, i.e. we start from considering all reserve changes, and then order all interventions across countries according to their size relative to the respective GDP. We start by dropping the single smallest FX intervention volume (relative to domestic GDP), then the second smallest etc. until there is just the largest intervention left in the sample. Dropping more and more small interventions will lead to an increase in precision, i.e. the share of correct classifications among the months labelled as intervention by the proxy. However, this comes at the price that fewer actual FX interventions will be detected, i.e. a reduced probability of detection. The result of this procedure is shown in Figure 3. When using no cutoff for reserve changes (at the very left of the figure), the precision starts at about one third and the proxy would indicate over a thousand intervention months. Excluding the 50 percent smallest reserve changes per country increases precision to only about 40 percent. Overall, using a reserve change cutoff cannot increase the precision of the proxy to above 60 percent.[5]

[Figure 3 about here]

Suitability of intervention news. News reports about interventions typically cover actual intervention. The most widely available form of news about FX intervention is a report that market participants have noticed that the central bank intervened in the market (i.e., a rumor). These are available in 353 months across our sample. Although rumors might be seen as unreliable, all of them have made it across the filter of a financial journalist reporting on them (as reported by Factiva). As a result, they are quite precise. During 93.8 percent of months with hand-coded rumors, actual interventions occurred. Overall, using our methodology the results will be largely driven by news about market rumors because these far outnumber confirmations by central banks. There are only 48 months out of over 13 thousand months in the full sample, where the text classification algorithm has classified a confirmation and there is no additional rumor in that month. In total, about 1500 months either see news that cover rumors, confirmations or both in our data.

To better understand what triggers news on FX interventions, we study the determinants in Appendix C. We test, for example, whether interventions in larger markets, under certain exchange rate regimes, and in countries with freer press receive more coverage. A plausible result is indeed, that larger reserve changes or larger intervention

[5] There are no substantial improvements of the reserve proxy based on four other cutoff definitions we have tried. First, we used a cutoff defined separately by intervention direction, secondly in terms of the log absolute reserve change instead of relative to a country's GDP, third based on the monthly number of news items of a country, and fourth based on the overall coverage of countries on Factiva.

volumes increase the probability of any relevant news. In larger countries there are more news on an intervention of a given size, but due to aggregating news at the monthly level, the impact on our proxy is small, and smaller than one might expect ex ante.

Comparing the proxies of reserve changes and intervention news. Based on the 27 countries for which we have actual intervention data (and manually collected news data), we now compare the two proxies (based on reserves and news) for several criteria (which is step 4 according to Figure 1). For a visual indication of the quality of news in filtering out intervention months and comparison to the reserve proxy in Figure 2, we plot our news-based proxy for the case of Japan. The major advantage of our proxy is that the interventions that are picked up are almost always actual interventions.

[Figure 4 about here]

Let us turn to the success criteria introduced above and our full sample of countries. The outcomes quite consistently indicate the advantage of the news proxy over the reserves proxy (see Table 2). Since reserves change every month, any month would be considered an intervention month by a proxy that exclusively relies on reserve changes. This fact is easily overlooked when simply relying on the correlation or explained variance between reserve changes and intervention, which does not appear being too bad, having a R^2 of 0.498. However, the probability of false alarm is 1, i.e. all non-interventions are falsely classified as interventions when using reserve changes. The reserve change proxy with a cutoff does not fare much better. By contrast, the probability of false alarm of the news proxy is only 3.7 percent, indicating that only few non-intervention months are incorrectly classified as intervention months. Moreover, 77.9 percent of classified interventions are actual interventions.

[Table 2 about here]

Thus the news proxy reduces the amount of noise considerably. When compared with actual intervention data, it captures about one-third of actual intervention months and about 54 percent of the actual absolute net monthly intervention amounts. Its performance is imperfect but constitutes a significant improvement over using reserve changes.

2.3 Characteristics of the new database for monthly FX interventions

The news-based intervention proxy appears to be useful in the data universe of 27 countries over a long sample period (13 years on average across countries). Using the same methodology, we, therefore, extend the proxy across time and countries (which is step 5 in Figure 1), thus creating a new database on FX interventions with country-level monthly intervention proxies. Our database includes almost all countries that provide data for monthly foreign currency reserves in the International Financial Statistics (IFS) database of the IMF and the sample period is 1995-2016 for the majority of countries.

Minimum number of news required. Countries are covered to a different extent in the Factiva news database. A lack of news reports on intervention should hence not be automatically interpreted as evidence of non-intervention. Thus we define a minimum degree of coverage for countries to be considered in our database. We use a simple cutoff of at least ten FX intervention-related news items over the full sample period (the cutoff is varied in a robustness check without changing results qualitatively). This rule results in four countries from the IFS database being dropped, i.e. Estonia, Lithuania, Mongolia, and Kuwait.[6] The country remaining in our working sample that is closest to the cutoff is Iceland, which has been actively building up reserves in the aftermath of the 2008 crisis, with 24 relevant news items.

Coverage over time. The data cover the period 1995-2016. We do not extend the data back into the 1980s because the relevant news coverage of emerging markets during those times was very poor. The panel is unbalanced because some countries do not provide reserve data for the whole period. Since we need those data for the intensive margin of the reserve changes, it is impossible to create it in full for those cases, i.e., both

[6] This cutoff also implies dropping Azerbaijan, Bolivia, Costa Rica, Georgia, Kyrgyzstan, and Moldova that were part of FGMSS's original 33 countries. Several of these countries are also not in the IFS database, thus lacking comparable monthly reserve data to work with.

regarding the incidence and the size of interventions.

Country and regime coverage. Our new intervention proxy covers 48 countries plus the EMU: Argentina, Australia, Brazil, Bulgaria, Canada, Chile, China, Colombia, Croatia, Czech Republic, Denmark, the European Monetary Union (EMU), Hong Kong, Hungary, Iceland, India, Indonesia, Israel, Japan, Kenya, Latvia, Lebanon, Malaysia, Malta, Mexico, New Zealand, Nigeria, Norway, Peru, the Philippines, Poland, Romania, Russia, Saudi Arabia, Singapore, Slovak Republic, Slovenia, South Africa, South Korea, Sweden, Switzerland, Thailand, Turkey, Ukraine, United Kingdom, United States, Uruguay, Venezuela, and Vietnam (see also Table A4). Thus, this dataset includes 37 of the 38 most important currencies covered in the BIS triennial survey (2017), missing only Bahrain. Furthermore, the data also extend to the currencies of Iceland, Kenya, Lebanon, Nigeria, Uruguay and Venezuela, which are not part of the BIS survey, and include a number of countries whose individual intervention history ended when they joined the Euro, which applies to Latvia, Malta, Slovak Republic and Slovenia between 2007 and 2014. Our main working sample thus covers most of the worldwide trade in FX, and the currencies of far over 80 percent of the world economy. Our new dataset is in this respect much more comprehensive than publicly available FX intervention data or the FGMSS data.

Finally, the new database provides broad coverage of different exchange rate regimes, in particular by strongly improving the coverage of managed exchange rate regimes. These typically do not provide their intervention data publicly and they are thus difficult to study empirically. Nonetheless, they make up the majority of exchange rate regimes. For example, in the Ilzetzki et al. (2019) database of exchange rate regimes, there were only four[7] free floaters out of 193 countries at the beginning of 2015; 83 countries fall under the "narrow" and " broad" bands. While some of these do not report reserve data to the IMF and are therefore not covered in our database, broad and narrow band regimes still make up 58 percent of our dataset in early 2015.

[7] Plus the Eurozone countries that they code as having fixed their exchange rates to each other.

3 Stylized facts of FX interventions

In this section, we describe several stylized facts of the new database about FX interventions. These have two purposes. First, stylized facts provide a perspective on FX intervention patterns; second, they allow us to get a better sense of whether there are relevant differences between these proxy data and the actual intervention data from FGMSS.

Frequency of intervention. We know from FGMSS that interventions occur in about 20 percent of months in their shorter and smaller sample of 33 countries. Comparing this to the same statistics based on the news-based proxy shows that the proxy underestimates the true incidence of interventions. Taking a sub-sample of our data by using all available countries that are also included in FGMSS, intervention is estimated to occur in 11.3 percent of months. This indicates that the proxy picks up just over half of the intervention months. As discussed in Appendix C, this discrepancy is greatest in the most rigid regimes for which we often lack relevant news items that capture interventions, because in these regimes FX intervention seems to be taken for granted.

To test whether there is any trend in the frequency of the intervention data, we plot the share of countries with interventions at the monthly frequency over time.[8] If anything, there appears to be a slow decrease in intervention incidence over time (Figure 5) that is interrupted by the times of the global boom of the mid-2000s and the Great Financial Crisis and its aftermath. These simple statistics mask differences by currency regime. For example, broad band regimes were more likely to intervene in FX markets in periods of market turmoil and during large capital inflows.

[Figure 5 about here]

The comparison with the same information from actual intervention data of FGMSS (on a smaller sample, Figure A2 in the Appendix) suggests that broad swings in intervention activity of country groups can be observed with the help of our proxy even if the proxy may miss some intervention episodes that remain unobserved by market participants.

[8] Since the proxy somewhat underestimates intervention, we correct the averages over time using the procedure described in Appendix C.

Interventions come in episodes. As we know from daily data (see e.g., FGMSS), interventions occur in sequences. The length of intervention episodes varies: according to our proxy, about two-thirds of episodes last only one month. The distribution has a mean of 1.7 months and a long tail. The probability of an intervention being followed by a second intervention month is about 16 percent. In the actual intervention data of FGMSS, this probability is 18 percent. A marked difference is that in the actual data intervention spells are substantially longer, with a mean of 5 months. This is driven by some extremely long intervention spells (e.g., monthly intervention over more than 5 years). Medians are much closer at 1 and 2 months, respectively. This is explained by episodes being at times split up under the proxy and not counted as belonging to the same spell. This happens because longer intervention spells tend not to receive monthly coverage in the news database we exploit.

Majority of interventions buys foreign currency. The current version of the proxy uses the reserve change to approximate the direction of the intervention. This will create a measurement error if central banks lean against the wind and, for example, sell currency at a lower rate than is compensated by reserves' appreciation. Nonetheless, the approximate direction of intervention can be informative. We find that, on average across all regimes, central banks more often build up reserves (60.3 percent of all intervention months) rather than decrease them. This makes sense because in the long run having foreign reserves in combination with economic growth and globalization is expected to require buying foreign currency. This share is higher in less rigid regimes like broad bands and free floaters, a fact that can also be found with the confidential intervention data of FGMSS. Also, during the study period many economies, especially in East and Southeast Asia, have acquired large amounts of reserves.

Intervention size is imperfectly approximated. In order to approximate intervention size, we identify intervention months by the news proxy and take the average reserve changes per country in these months as the estimation of intervention size; however, in contrast to the reserve proxy discussed above, we only use data for months highlighted by news items that predict intervention. Based on the FGMSS country sample we can compare the difference between estimated and actual intervention sizes. According to the news-based estimation procedure, reserves change by 1.3 billion USD in intervention months and by 1.05 billion USD in non-intervention months. The actual intervention amounts are 1.1 billion USD and 0 USD, respectively. This indicates that the news proxy picks up slightly stronger than average interventions because these are more likely to be reported (see Table A2), leading to an overestimation of average intervention size (1.3 instead of 1.1 billion).

More interventions in turbulent times. As can be seen in Table 3, where we provide additional information on the intervention proxy across different regimes, all countries are significantly more likely to intervene in turbulent times (defined here by the VIX deviating more than two standard deviations from its mean) regardless of their exchange rate regime.[9] According to the proxy, free floaters are, for example, more than twice as likely to intervene in a given month if markets are in turmoil. For other regimes, the increase in odds is smaller. This makes sense because these regimes are expected to intervene more often regardless of market conditions.

[Table 3 about here]

4 Relations between FX interventions, capital controls, and macroprudential policies

Having developed our news-based proxy, we are equipped to analyze linkages between FX interventions, capital controls, and macroprudential regulations. In Section 4.1 we analyze the relationship between FX interventions and the long-time established instrument of capital controls. In Section 4.2 we study the relation between FX interventions and macroprudential policies.

[9] Comparison data can be found in Table A5.

4.1 FX interventions and capital controls

The earlier literature on capital controls has mostly been critical about the consequences of using such tools, the main reason being the potential misuse in trying to avoid otherwise necessary adjustments (see also more recently Klein, 2012). Several studies highlight the distortions created by capital controls (e.g., Costinot et al., 2014; Forbes et al., 2015; Alfaro et al., 2017). At the same time it has been acknowledged that capital inflows and outflows can be heavy relative to the size and capacity of a domestic financial market (in particular in emerging economies), so that controls can be a useful instrument to moderate such extreme flows (e.g., Ostry et al., 2011; Benigno et al., 2016; Dominguez, 2020).

Empirically, we find in this section a positive association between FX interventions and the level of capital controls. Several sample-splits shed more light on this relationship.

Empirical setup. In order to test whether the policy instrument of interest in country/currency i at time t is systematically associated with FX intervention conditional on the regime and the year, we estimate:

$$Intervention_{it} = \alpha + \beta Instrument_{it} + regimeFE_i + yearFE_t + \epsilon_{it} \tag{1}$$

Intervention is included as a dummy variable. As a measure of the intensity of capital controls we add up different categories to form an overall index.[10] Controlling for the exchange rate regime and country differences is important because countries should see less need for capital controls as they develop (see, e.g., Korinek and Sandri, 2016) and are at the same time less likely to closely manage their exchange rate through intervention.[11] In all of the following, we use "coarse" grid regime fixed effects as defined by Ilzetzki et al. (2019), i.e. a classification that results in four main exchange rate regimes. Standard errors are clustered at the country level.

Data on capital controls are available from different sources. A prominent dataset

[10]There are 14 inflow and outflow controls each, all measured by an index that is scaled between 0 and 1. We add up all inflow and outflow controls, respectively, and divide each resulting aggregate index by 14 such that it is scaled between 0 and 1 again. The resulting sample means for inflow controls and outflow controls are 0.34 and 0.41, respectively.

[11]Results hold when we provide an alternative approach with detrended variables.

that is widely used is Chinn and Ito (2008), updated to 2015. Novel databases have been building on earlier works, leading us to pick the most comprehensive one that was established by Fernández et al. (2016). In its update, it covers 100 countries for the years 1995-2015. From our perspective, these data have the disadvantage of being available only at annual frequency but we are not aware of a better alternative source for capital control measures. Due to this frequency, the FX intervention proxy is aggregated accordingly when studying the correlation of changes in capital controls and changes in intervention incidence.

Finally, aggregation at the yearly level also requires an adjustment of the monthly data about the exchange rate regime.[12] First, we investigate capital controls, both their changes and levels.

Levels vs. changes in capital controls. A priori, it is not obvious whether either levels or changes are the most relevant unit of analysis. Capital controls are quite a persistent instrument and much more persistent than FX interventions. In our data, there are relatively few changes in capital controls despite the capital control data's annual frequency. For example, there are only five observed changes for free-floating regimes and a mere six for rigid regimes.

When analyzing the relationship of levels of capital controls and FX interventions, we use month-to-month variation in intervention and treat capital controls more akin to a contextual factor that changes little over time. Both with regime and year fixed effects as well as unconditionally (Table 4, Panel A, columns 1 and 2), we find that interventions occur significantly more often in countries that have higher levels of capital controls in place. This positive correlation becomes stronger when using actual intervention data instead of our news-based proxy. Most of this increase seems to be due to lower noise as the comparison of columns 3 and 4 suggest, which are based on the same sample but use different outcome variables. In terms of the strength of the correlation, a one

[12] In general, we choose that regime in which a country was for between six and twelve months in a given year. For the time being, countries that have spent less than six months in a regime or exactly six months on two regimes each are excluded from the analysis. In the case of 53 observations in our working sample, a country has spent less than six months in its longest regime setting in that year (e.g., three regimes in a year, each of them for four months).

standard deviation increase in the standardized capital controls index is associated with 8.3 percentage point higher probability of FX intervention in a given month. Using a similar analysis at the yearly frequency to estimate the relationship of changes in FX intervention and changes in capital controls yields no systematic pattern (Table A7). Thus, there do not seem to be broad, synchronous swings in capital controls and FX intervention policy. Rather, capital controls provide a stable background against which decisions about FX intervention are taken. Next, we disaggregate the data by country characteristics and policy characteristics.

[Table 4 about here]

Advanced economies vs. emerging economies. As predicted by the theoretical literature (e.g., Korinek and Sandri, 2016), there is a strong negative correlation between capital controls and GDP per capita in our data. Emerging economies not only far exceed advanced economies in their use of capital controls, but they also intervene more often in the FX market. In emerging markets capital controls increased strongly during and after the crises of the late 1990s, then being reduced slightly over time until the global financial crisis. In advanced economies the pronounced change has been a reduction in capital controls starting in the late 1990s until about 2005.[13]

When we run the analysis of Table 4 separately for advanced and emerging economies in Panel B, the OLS-based coefficient for advanced economies is larger than that for emerging markets, indicating that a given change in the level of capital controls is associated with a larger change in the probability of intervention (columns 5 and 6).[14] This seems to be driven by changes occurring at vastly different levels of the respective covariate in each of the groups with the average advanced economy having far lower capital controls in place than the average emerging market.

[13]Table A3 in the Appendix further breaks down these data by exchange rate regime and direction of capital control. It shows that more rigid exchange rate regimes are also more likely to have capital controls in place. Comparing types of controls, the most striking difference is that the outflow index has twice the value of the inflow index for free floaters. Broad band regimes made the greatest use of new macroprudential policies as well and had a higher share of months with FX intervention according to the news-based proxy.

[14]Re-estimating the mean marginal effect at the mean of covariates using a logit model suggests this is not merely a result of the linear model (see Table A6).

Exchange rate regimes. When distinguishing exchange rate regimes, an interesting and, to our knowledge, previously unknown pattern emerges (see Table A8). Among narrow and broad band regimes there are strong positive correlations between interventions and capital controls. By contrast, among free floaters, there is a negative relationship. This could be interpreted as tentative evidence that countries that do not normally intervene in the FX market and that have capital controls in place need to intervene less frequently. The negative correlation is confirmed when using actual intervention data (available only for a subset of countries) instead of the news-based proxy.

Inflow vs. outflow controls. Distinguishing between inflow and outflow controls across countries or regimes in levels and changes suggests that both kinds of capital controls are positively correlated with interventions – a consequence of many countries controlling both inflows and outflows (Table A9). While levels matter, interventions are not systematically more likely in years when capital controls have been changed (Table A9, Panel B).

4.2 FX interventions and macroprudential policies

The literature on macroprudential policies is very broad (e.g., Farhi and Werning, 2016). Some papers particularly relevant for us highlight the beneficial role of macroprudential policies in managing international capital flows, such as Korinek (2018). Cerutti et al. (2017a) provide the broadest documentation of macroprudential policies with 64 countries covered at quarterly frequency and find that these policies are generally able to impact credit growth. Aside from those studies which discuss the usefulness of macroprudential regulation to stabilize the economy (and the financial sector in particular), such as Jeanne and Korinek (2017), only a few studies consider FX transactions. While Korinek (2018) identifies reasons why specific forms of capital inflows to emerging markets might be taxed, Ahnert et al. (forthcoming) show that borrowing in foreign currency is reduced due to stricter macroprudential regulation but that risks may shift from the regulated banking sector to unregulated firms.

In our empirical analysis we find, in contrast to the quite persistent capital controls, that macroprudential policies[15] change more often, creating variation in both changes and levels that can be exploited empirically. Reserve requirements that restrict borrowing in foreign currency are the single macroprudential policy that is most likely to be relevant for our paper because it is the most closely connected to inflows, outflows, or changes in valuations of the currency. Using the news-based proxy, we find a clear positive relationship between macroprudential policies and FX interventions.

Empirical setup. The empirical procedure mirrors the procedure on capital controls (see Section 5.1): a panel estimation including country and year fixed effects; however, the left-hand-side variable now refers to macroprudential policy. There are less datasets available to us than for capital controls, probably because these instruments received less systematic attention in the past. Since the last major crisis of 2008/09, however, policy makers have been eager to expand their toolset for capital flow management. Accordingly, there are recent efforts to collect such measures over time. In our paper, we rely on a database which offers large coverage across countries, instruments, and over time, which is provided by Cerutti et al. (2017a), spanning the years 2000-2014 at the quarterly frequency for 64 countries. The definition of macroprudential policies is taken from Cerutti et al. (2017a, 2017b) and is a measure of the intensity of macroprudential policy: it adds up the number of measures that are taken in defined categories. A quarter-on-quarter change can then take values 1, 0, and -1, indicating increasing, stable and decreasing macroprudential policies. We reflect the quarterly frequency of the macroprudential policy data by conducting the analysis at the quarterly level. To study the correlation between the two kinds of policies we run a regression where FX interventions are the left hand side variable and macroprudential policies enter on the right hand side (see equation 1). To allow incorporating differences by exchange rate regime accordingly, we aggregate exchange rate regimes from the monthly level up to the quarterly frequency, meaning that a country is classified as the specific regime prevailing over at least two out of three months.

FX interventions and changes of macroprudential policies. The estimates in

[15] Denoted by an index counting net changes in macroprudential policies. The vertical level, which starts a little over 0, reflects the cumulative change and not the starting level in December 1999.

Table 5 show a systematic positive relationship between quarterly changes in macroprudential policies and FX intervention. In two-thirds of cases, these changes are increases in the degree of macroprudential regulation, meaning that more intense macroprudential policies are associated with a higher probability of FX intervention. While the correlation retains its sign when reducing the sample size in column 2 to the countries with actual intervention data, the estimate becomes statistically insignificant in this smaller sample that overlaps with FGMSS. By contrast, when using actual intervention data instead of the proxy, the relationship remains weakly significant. Measurement error of the proxy plays some role in the subsample analysis, but sample composition is another important reason for the differences between columns 1 and 2, as the following analysis shows.

[Table 5 about here]

Advanced vs. emerging economies. The first reason for rather weak correlations between FX intervention and macroprudential policies is sample composition. Several papers - such as Korinek and Sandri (2016) - discuss that as economies become more advanced, countries should be in less need of capital controls but their use of macroprudential policies should remain stable. This is a pattern that we indeed observe within our data.[16] Macroprudential policies remain relevant, these authors argue, because they help mitigate boom and bust cycles. In the data, we find that emerging markets' macroprudential policies started increasing in the mid-2000s and that this trend accelerated in the early 2010s. In advanced economies, by contrast, there had been little change before 2010 but a similarly strong rise afterwards (see also column 4 of Table 5). For emerging economies (see column 5), by contrast, the point estimate is more than twice that for advanced economies. Any evidence on the joint use of intervention and the introduction of prudential policies is thus largely driven by emerging markets.

Results on specific instruments. The second reason for the weak correlation in the full sample is the broad set of policies that fall under the umbrella term macroprudential policies. The database of Cerutti et al. (2017a) distinguishes ten different macropruden-

[16] The pattern holds also when accounting for regimes, country characteristics, and time effects, each covered by adding dummy variables to the regression.

tial policy instruments. Of these, only the two instruments of reserve requirements for foreign currency loans and locally denominated loans are both significantly and strongly related to FX intervention across countries, regimes and time (see Table 5, columns 7 and 8). As reserve requirements for local loans are highly correlated with those for foreign currency loans, this may indicate that reserve requirements aim at reducing credit growth and are thus used as a counter-cyclical measure. Accordingly, the macro-environment also plays a role in understanding the use of instruments for capital flow management.

Levels of macroprudential policies instead of changes. Looking also at levels of macroprudential policies that a country has put in place since the start of the Cerutti et al. data in the year 2000[17] (Table A11 in the Appendix), we find positive and statistically significant correlations between *cumulative* changes ("levels") of macroprudential policies and FX intervention in managed exchange rate regimes. These exchange rate regimes with more active use of FX intervention are also more common in emerging markets. At the same time, they impose greater macroprudential controls on foreign currency. Thus, macroprudential policies and foreign exchange intervention often go broadly hand in hand. Countries that intervene more frequently tend to have put more macroprudential policies in place since the year 2000.

Exchange rate regimes. The positive relationship between FX intervention and a higher level of prudential policies since the year 2000 is driven by one exchange rate regime in particular, broad bands (see Table A10 in the Appendix) and, among these, by emerging markets. Among free floaters, the pattern is indistinguishable from zero. While especially broad band regimes have introduced macroprudential policies, free floaters introduced them to a much smaller extent. Tellingly, changes to the specific macroprudential policy of most interest in this paper, i.e. reserve requirements on foreign currency, are positively correlated with FX intervention for broad bands, while we do not see any changes of this policy indicator for free floaters in the sample period.

Summarizing. Our results indicate that FX intervention and the level of macroprudential policies are independent of each other in many countries, especially in advanced

[17]For interpretation, it is important to note that Cerutti et al. only provide changes as well as a cumulative sum of changes but no initial level for each country at the start of their sample.

ones. However, overall, there is a positive association between interventions and increases in macroprudential policies, indicating some complementarity in emerging markets.[18]

For capital controls, we also find a correlation between the levels of certain capital controls and FX intervention but not for changes. This dissimilarity to the case of macroprudential policies may be partly due to the annual frequency of the capital controls data and may in addition highlight different use of both sets of instruments. Capital controls are quite persistent and thus changed less frequently than macroprudential policies, indicating that countries may favor regimes where they permanently shield their economy against influence from international financial markets. By contrast, macroprudential measures vary more over time and seem to be introduced or reduced in combination with FX interventions under specific circumstances.

5 Conclusion

This paper contributes to the large literature on FX interventions by compiling a new database of FX interventions for a broad cross-section of currencies. We provide stylized facts about FX interventions and show the relationship between the main policy tools of capital flow management, i.e. FX interventions, capital controls, and macroprudential regulation.

The new database relies on the news provided by a financial news platform to identify FX intervention episodes. Information about intervention is derived from these news items implementing a text classification approach. We train this algorithm using thousands of hand-coded documents. The results are then assessed using two sources of information: The performance of the news classification is compared to hand-coded data, and success in correctly identifying interventions is evaluated by comparison to the confidential, actual intervention data of Fratzscher et al. (2019).

We then first use the new database to provide stylized facts about FX interventions, documenting that, for example, interventions are frequent, come in episodes, are char-

[18] Additional analyses of joint use of capital controls, macro-prudential policies and FX interventions did not yield consistent results. A major reason seems to be the multitude of possible combinations of the three sets of policies and a relatively low number of observations for each case.

acterized generally by purchases of foreign currency, and occur more often in turbulent times. All of these findings are in line with what we know from country studies and the cross-country study by Fratzscher et al. (2019), thereby lending credibility to the new dataset. The advantage of the new database is, however, its broader coverage and the fact that we can make it publicly available for use by other researchers now as well as in updated form in the future.

Relating FX interventions to two other policy tools of interest, i.e. capital controls and macroprudential regulation, we find that their use is positively correlated. FX interventions are used more often in countries that have more capital controls. This link between levels of capital controls and FX interventions is highly robust. Joint policy changes, i.e. increases or decreases in capital controls at times of FX intervention, are far less common, yet also difficult to identify because the capital controls data come at annual frequency. Regarding macroprudential regulation, we find a different pattern: Their level is unrelated to FX intervention, but they have a higher likelihood of being increased during times of FX intervention. In all of these cases, our database allows distinguishing common trends in policy use from patterns that hold across countries and many years, because the database is both broad and wide enough to control for country and time fixed effects.

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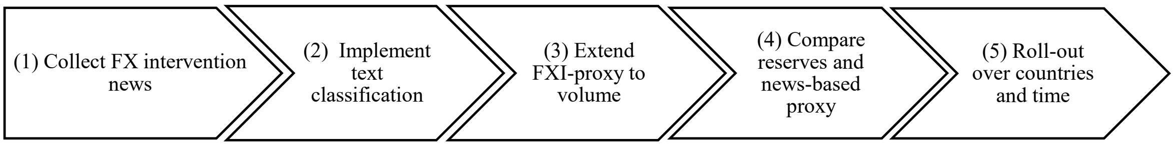
Figures and Tables

Figure 1: Creating a new database of FX interventions

This figure explains the work flow used to create the new foreign exchange intervention proxy database.



Hand-coding of headline news from database mentioning FXI etc. (33 countries – 6 with few news) → training dataset	SVM approach: crossfold-validation to identify FX intervention news according to FGMSS: 94% success, 99% at monthly frequency → monthly binary indicator of news-based FXI proxy	Complement FXI proxy dummy by FX proxy volume: if FXI, then take reserve change for volume → FXI proxy	Reserve-based proxy and news-based proxy to capture actual FXI at monthly frequency: Our proxy succeeds → in-sample assessment of quality	Roll-out of our proxy: A. collect news for more countries and longer time periods B. apply the SVM approach → our new database on FX interventions

Figure 2: Reserve proxy and actual intervention for the case of Japan

This figure reports the performance of an intervention proxy that is solely based on reserve changes for the case of Japan, where intervention data are public and results can thus be shown. Each bar provides monthly information. The top panel reports all months where the reserve proxy correctly predicts any intervention. The shading allows comparing the true and predicted size of the respective intervention. Actual intervention amounts (black) and predicted intervention amounts that are based on cleaned reserve changes (white). Each bar in the top panel thus consists of a white and a black part. A black bar without a clearly visible white bar indicates excellent fit of the proxy. A white bar on top of a black bar indicates that the proxy overestimated the true intervention. A white bar smaller than a black bar indicates an underestimated true intervention. The bottom panel shows erroneously predicted intervention months and the reserve changes during those months which would be interpreted as intervention volumes under such a proxy. In all of the cases in this panel, there is thus no actual intervention but a predicted volume based on the cleaned reserve change (grey). Figure based on publicly available data for Japan. The probability of detection is 1, the probability of false alarm is 1. The coefficient of correlation between the actual and predicted volumes is 0.77.

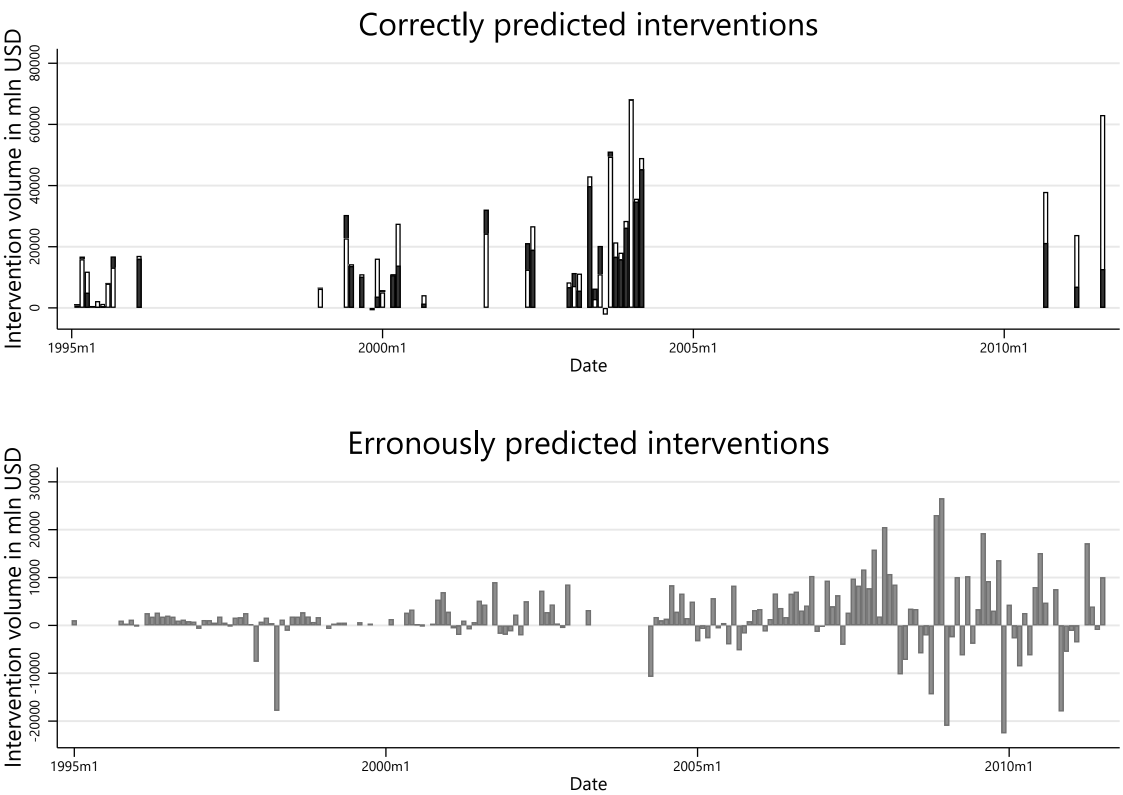


Figure 3: Precision and reserve change cutoffs

The figure shows the performance of an intervention proxy based solely based on reserve changes for different levels of filtering. Filtering is done by using the relative size of the absolute monthly reserve change for a given country relative to its GDP. The horizontal axis provides the percentiles used as cutoff. The solid line is the share of true positive intervention months (i.e. precision) and the dashed line the number of true positives. The number of correctly classified intervention months (dashed line) refers to the second vertical axis. As the cutoff increases, precision increases slowly while the number of true positive interventions decreases linearly. These statistics refer to all countries and times covered in the data.

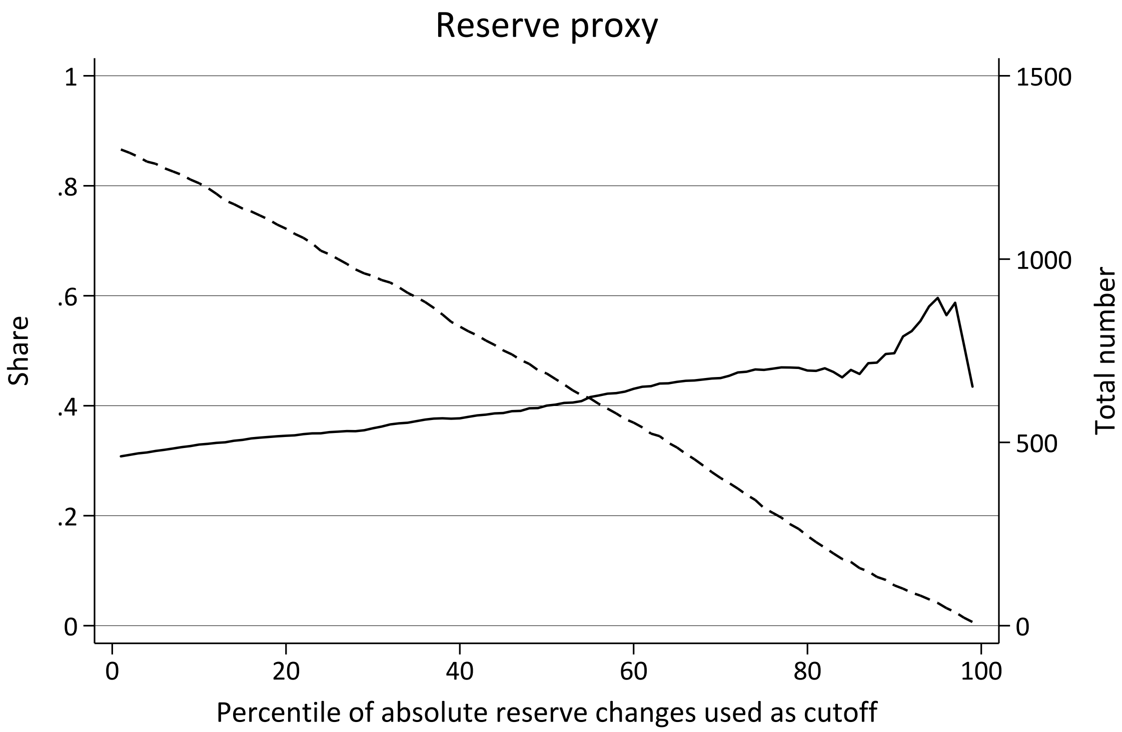


Figure 4: News proxy and actual intervention for the case of Japan

This figure reports the performance of the new intervention proxy for the case of Japan, where intervention data are public and results can thus be shown. Each bar provides monthly information. The top panel reports all months where the reserve proxy correctly predicts any intervention. The shading allows comparing the true and predicted size of the respective intervention. Actual intervention amounts (black) and predicted intervention amounts that are based on cleaned reserve changes (white). Each bar in the top panel thus consists of a white and a black part. A black bar without a clearly visible white bar indicates excellent fit of the proxy. A white bar on top of a black bar indicates that the proxy overestimated the true intervention. A white bar smaller than a black bar indicates an underestimated true intervention. The bottom panel shows erroneously predicted intervention months and the reserve changes during those months which would be interpreted as intervention volumes under such a proxy. In all of the cases in this panel, there is thus no actual intervention but a predicted volume based on the cleaned reserve change (grey). The probability of detection is 0.31, the probability of false alarm is 0.04. Performance is thus slightly worse than in the full sample. The coefficient of correlation between the actual and predicted volumes is 0.77. The performance can be compared directly to Figure 2 and indicates that the incidence of intervention is much better measured when using the new intervention proxy. These statistics refer to all countries and times covered in the data.

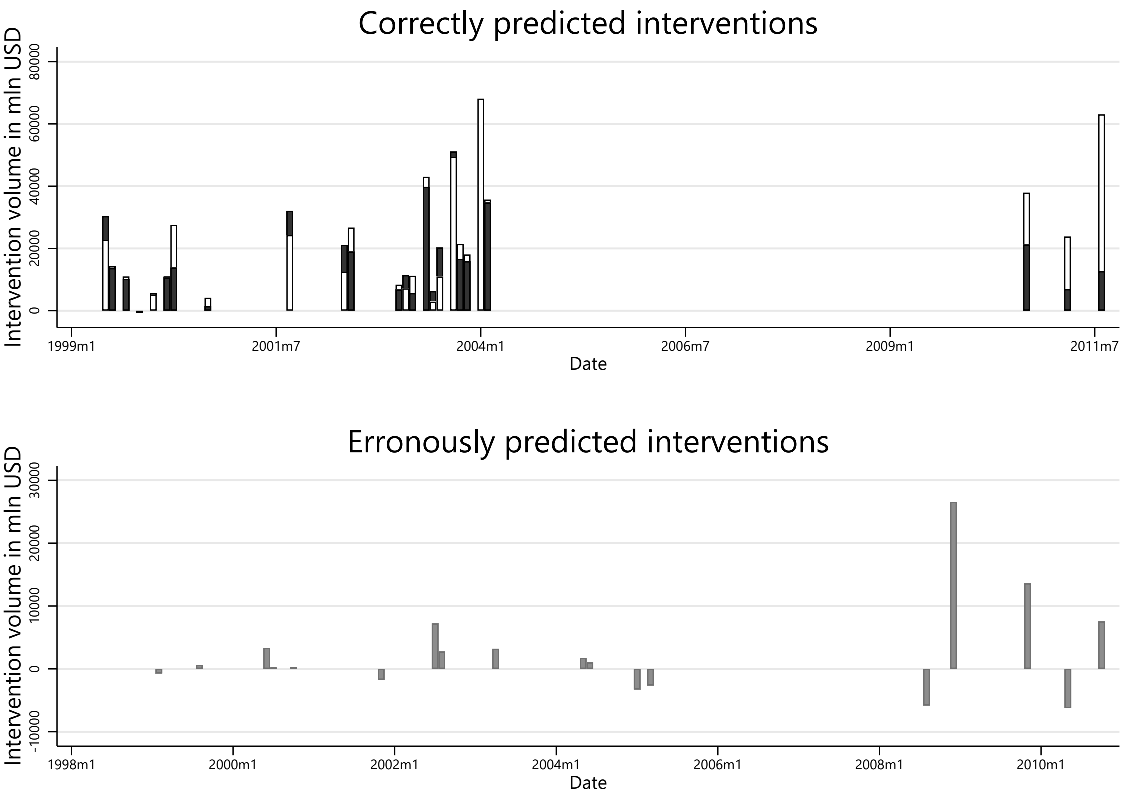


Figure 5: Foreign exchange intervention, capital controls, and changes in macroprudential policies over time

This figure compares estimates for the use of foreign exchange interventions, capital controls and macroprudential policies. The reported share of intervening central banks is estimated using the news-based proxy created in this paper. Capital controls and cumulative macroprudential policy index come from Fernández et al. (2016) and Cerutti et al. (2017a), respectively. These are rescaled as described in the text. Average FX intervention data from our new database and scaled up to reflect estimated underreporting (cf. Figure A2) and smoothed using a rolling 6 month window around each point in time. These statistics refer to all countries and times covered in the data for which all three data series are available.

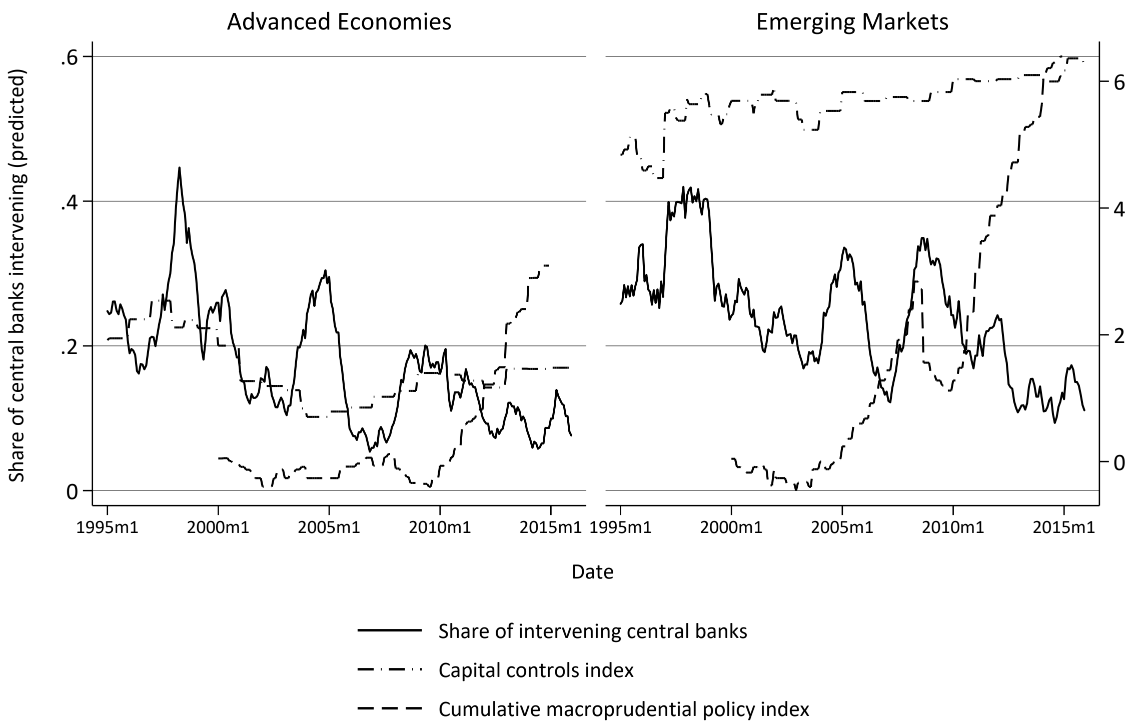


Table 1: Prediction quality measures

		Actual intervention	
		Yes	No
Classified as intervention	Yes	True positive (A)	False positive (B)
	No	False Negative (C)	True negative (D)

Table 2: Performance of proxies

Panel A: Frequency tables of classifications by proxy

Panel A plots the distribution of true and false positives (cf. Table 1 for an intervention proxy that is solely based on reserve changes. Panel B plots the same distribution for the news-based intervention that we develop in this paper. These statistics refer to all countries and times covered in the data.

Reserve proxy

		Actual intervention Yes	No	Total
Classified as intervention	Yes	1,340	2,929	4,268
	No	0	0	0
	Total	1,340	2,929	4,268

News proxy

		Actual intervention Yes	No	Total
Classified as intervention	Yes	377	107	484
	No	963	2,821	3,784
	Total	1,340	2,929	4,268

Panel B: Measures of predictive quality by proxy and explained variance of actual intervention explained by proxy

This panel provides additional estimates of the predictive quality of the reserves-based and the news based proxy to summarize the information in Panel A. The R^2 is calculated using a regression of actual intervention (dummy or volume) on the respective proxy (dummy or volume). The R^2 for intervention incidence is calculated using the respective intervention dummy of a proxy as explanatory variable for a dummy measuring actual intervention from true intervention data. The overall R^2 indicates the R^2 in the full sample. The R^2 at the bottom only includes those cases when the proxy indicates an intervention. The news proxy thus dominates the reserve proxy both regarding its performance on incidence and regarding the level conditional on predicted incidence. These statistics refer to all countries and times covered in the data.

	Reserve proxy	News proxy
Indicators for incidence		
Probability of detection	1.000	0.273
Probability of false alarm	1.000	0.043
R^2	0.000	0.112
Indicators for overall variance		
Overall R^2	0.496	0.581
R^2 if proxy indicates intervention	0.496	0.740

Table 3: Summary of interventions using our intervention proxy

This table provides summary statistics of intervention characteristics according to our news-based proxy for the aggregate sample and by exchange regime. Country-regime refers to unique combinations of country and exchange regime. * assuming 20 trading days per month and using interpolated data from the BIS triennial survey. Reading example: net intervention volume of 100% indicates monthly intervention volume is as large as 1/20th of daily FX turnover in the respective market. Mean absolute size where indicated.

	Total	Free Floaters	Broad Bands	Narrow Bands	Rigid Regimes	Other regimes
Number of country-regime observations	106	6	33	28	20	19
Months covered	12485	1087	5327	3034	2556	481
Size of reserve changes in mill USD (mean abs)	1880	3456	1303	2660	1652	999
Size of reserve changes (mean abs %/GDP)	.53	.13	.47	.48	.84	.5
Months with intervention	11%	9.5%	13%	12%	4.9%	2.0%
Months with net FX purchase intervention	6.6%	6.8%	8.1%	6.7%	2.8%	8.5%
Months with net FX sale interventions	4.3%	2.7%	4.5%	5.3%	2.2%	12%
Size in mill USD (mean abs)	2795	8387	2120	3231	1849	1142
Size in % of GDP (mean abs)	.58%	.21%	.58%	.5%	1.1%	.54%
Size in % of FX turnover (mean abs)	40%	1.9%	34%	52%	95%	76%
Months in turbulent times	7.4%	7.6%	8.0%	6.9%	7.6%	3.5%
Months in turbulent times with intervention	1.5%	1.7%	1.6%	1.5%	9.8%	4.7%

Table 4: FX intervention and the level of capital controls

The table reports estimates of the relationship between FX interventions and capital controls. Interventions are included as dummy variables. We either use our proxy or actual intervention data in different columns. Capital controls data from Fernández et al (2016) and included as levels. Intervention data are monthly while capital controls data are yearly, hence we do not use changes in capital controls and treat capital controls as background level. The sample period is from 1995-2015. All estimates are based on OLS models. These include year and regime fixed effects where indicated. Heteroskedasticity-robust standard errors throughout. *** p<0.01, ** p<0.05, * p<0.1. Panel A provides a comparison of intervention proxy and true data. Panel B provides estimates of the relationship for advanced and emerging countries, respectively, as well as differentiating between inflow and outflow controls.

Panel A: Comparison of proxy and actual data

	(1)	(2)	(3)	(4)
Outcome variable	Intervention proxy	Intervention proxy	Intervention proxy	Actual intervention
Subgroup	All	All	Sample of column 4	If actual data available
Covariate of interest				
Capital controls	0.0538***	0.0585***	0.0763***	0.265***
(levels)	(0.00890)	(0.00885)	(0.0211)	(0.0296)
Year FE	yes	no	yes	yes
Regime FE	yes	no	yes	yes
Observations	11,731	11,731	3,971	3,971
R-squared	0.030	0.004	0.022	0.110

Panel B: Subgroup analysis for the intervention proxy

	(5)	(6)	(7)	(8)
Covariate of interest	All controls	All controls	Outflow controls	Inflow controls
Subgroup	Advanced economies	Emerging markets	All	All
Covariate of interest				
Capital controls	0.124***	0.0317**	0.0544***	0.0427***
(levels)	(0.0217)	(0.0130)	(0.00787)	(0.00955)
Year FE	yes	yes	yes	yes
Regime FE	yes	yes	yes	yes
Observations	5,214	6,517	11,731	11,742
R-squared	0.030	0.037	0.031	0.029

Table 5: FX intervention and the changes in prudential policies

The table reports estimates of the relationship between FX interventions and macroprudential policies. Interventions are included as dummy variables. We either use our proxy or actual intervention data in different columns. Macroprudential instruments are from Cerutti et al (2017a). Since these data are quarterly, intervention data are aggregated up to quarterly data. The sample period is from 2000-2014. All estimates are based on OLS models. These include year and regime fixed effects where indicated. Heteroskedasticity-robust standard errors throughout. *** p<0.01, ** p<0.05, * p<0.1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Intervention proxy		Actual intervention data	Intervention proxy			
Subgroup	All	Sample as in column 3	Countries where actual data available	Advanced Economies	Emerging Markets	All	All
Covariate of interest							
Prudential Policies	0.0462**	0.0585	0.0823*	0.0219	0.0486*		
	(0.0203)	(0.0480)	(0.0468)	(0.0326)	(0.0258)		
Reserve requirements (foreign)						0.0734**	
						(0.0331)	
Reserve requirements (local)							0.0465*
							(0.0246)
Regime FE	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes
Observations	2,815	991	991	1,319	1,496	2,815	2,815
R-squared	0.043	0.031	0.135	0.044	0.054	0.043	0.042

INTERNET APPENDIX

Foreign exchange intervention: A new database

Appendix A: News data

The source of our news data on foreign exchange intervention is Factiva. We structured a search query which is able to provide coverage of known intervention episodes that were public while omitting highly irrelevant news. This reduction in search outcomes is required because it is not legally possible to download the full Factiva news database on a specific topic. Using our search query (see box for exact formulation), we identify all news items in which foreign currency and interventions are mentioned in combination with a relevant body such as the central bank and the country name.

Factiva Search Query:
"(foreign exchange or fx or forex or currenc*) and (intervene* or operation?) and (countrystub near10 interven*) and (rst=trtw or rst=tprw or rst=tdjw) and (central bank or ministry of finance or treasury ministry or monetary authority)", where countrystub is, for example, "australia*"
Other settings: language=English, Region=respective country, all dates, all sources, all authors, etc.

Examples of news items that are thus found are:

> AUD/USD Softer After RBA's Kent's Comment on Intervention – Market Talk
> Dow Jones Institutional News, 04:23 GMT, 13 November 2014, 1507 words, (Englisch)
> 0423 GMT [Dow Jones] The Australian dollar is displaying a softer tone against the greenback Thursday after Reserve Bank of Australia Assistant Governor Christopher Kent said intervention on the Aussie has not been ruled out. The spot ...
> Document DJDN000020141113eabd000i3

> RBA Keeps Currency Intervention as Option
> Dow Jones Institutional News, 02:15 GMT, 20 August 2014, 480 words, (Englisch)
> SYDNEY–Australia's central bank Gov. Glenn Stevens said intervention in currency markets to help drive the Aussie dollar lower remained a real option.
> Document DJDN000020140820ea8k000mo

For each of these news items, the full summary (see above) is then downloaded for each country which reports reserve data in the IFS.

These news items can then be used to code each article. Manual coding was done based on the article summaries for countries for which we have actual intervention data. A standardized codebook and double-entry by separate research assistants was used to standardize coding. Research assistants were asked to identify separate categories of news, including rumors about intervention and confirmations of interventions by central banks or the treasury that we use as indication of relevant news.

Rumors are defined as immediate rumors of market participants of central bank interventions on the same day. Reports are defined as ex-post reports about previous intervention activity, for example, reporting net intervention amount or simply activity at the end of a month. Confirmations are defined as announcements by central bank or government authorities that confirm an intervention has taken place.

Appendix B: Brief summary of the machine learning algorithm

We use a standard text classification algorithm from the python library scikit-learn (link), which is open-source and includes a large variety of different machine learning and classification approaches. The algorithm is used to classify individual Factiva news items, i.e. short pieces of text such as the following:

> "UPDATE 1-Bank of Israel buys $200 mln of forex -dealers
> Reuters News, 11:25 GMT, 6 October 2009, 337 words, (English) (Adds details, dealer comment) JERUSALEM, Oct 6 (Reuters) - The Bank of Israel bought as much as $200 million of foreign currency in its first intervention in the forex market in three weeks, dealers said."

These data include both relevant ("Bank of Israel buys $200 mln", "intervention in the forex market") and irrelevant information (such as "UPDATE", "Oct 6", "Reuters"). Hence the algorithm needs to be trained to distinguish relevant from irrelevant information and to make a classification based on the relevant substrings.

For this hand-coded data (see Appendix A) are used. The algorithm thus receives several thousand text items and the hand-coded information, for example which news reports include "rumors."

The algorithm then extracts features from the text files, which involves turning text into numerical vectors. These vectors can be a simple count of the number of occurrences of each word in a text file, leading to typically hundreds of thousands of features for each news item. This yields a high-dimensional, very sparse (mostly zeros) dataset.

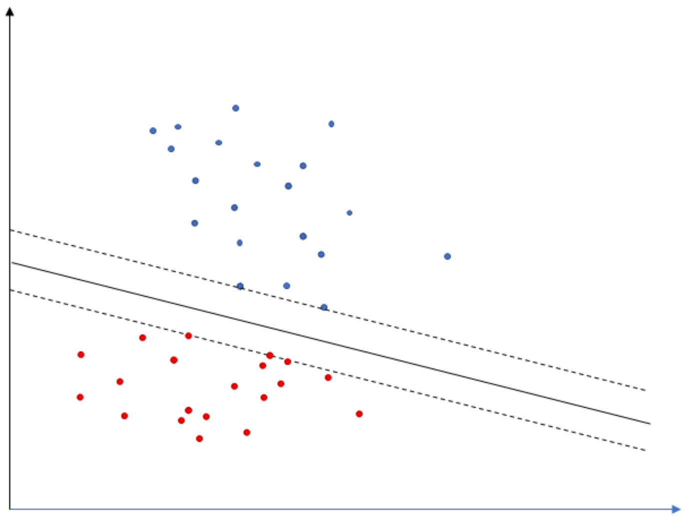
The next step is text preprocessing, filtering and some automatized editing to aid the classification. This means cutting words or N-gram (combinations of e.g. 15 consecutive characters like "foreign currenc"). An important part is furthermore the elimination of stopwords. These are words that occur often in language but do not carry any predictive quality, so we would not want to make the algorithm use this information in prediction. Examples of stopwords are "of", "as" and "is".

Next, the classifier is trained to distinguish relevant from irrelevant news. For this we use a random sample of 90 percent of the hand coded data. The remaining 10 percent of news items are held back as a test dataset. The methods we use in prediction are regularized linear models such as Support Vector Machines (SVM).

SVMs classifies observations into different classes based on the hyperplane that best separates the observations into those classes (Vapnik, 2000). In their most basic form in two-dimensional space and with two classes of observations to distinguish, a linear SVM draws the line that best separates both groups of observations. This is also illustrated in Figure A1. As can be seen in the graph, an important assumption of the approach is that groups can be distinguished based on the observations that that are closest to those of the other group. The remaining observations, e.g. top-right and bottom-left in the illustration, do not contribute the optimal choice of the dividing line.

Figure A1: Illustration of a support vector machine in two dimensional space

The graph shows how two groups of observations (blue, red) can be distinguished with the help of a support vector machine approach. The algorithm chooses the dividing line (solid) that best separates the blue and red points.



Prediction then works by determining on which side of the separating line an observation lies. The approach can easily be generalized to higher dimensional cases, which in fact typically makes distinguishing groups of observations much easier because there are more dimensions in which to draw the separating hyperplane. Also, by using polynomials instead of assuming a linear functional form for the hyperplane, a group of observations that at first seem "surrounded" by the other group can be distinguished with this method. SVMs have the advantage of being able to learn independent of the dimensionality of the data. Hence, contrary to a simple regression model, it is possible to have more potentially relevant dimensions (here, for example, counts of 1 million different words, i.e. 1 million potential regressors) than observations (here: news items). Furthermore, they can work with extremely sparse data (the excess zero problem, known for example from international trade). To implement the SVM, we use sklearn's linear model SGDClassifier (see the following link for formulae and more explanation: link), which implements regularized linear models with stochastic gradient descent learning. Excellent results are achieved using a "modified Huber" loss function and the standard l2 penalty setting.

Finally, the model that is selected on the basis of the training is used to predict other data. To check the quality of the prediction, news items in the test data are classified. To create the working sample, we however automatically code ALL data. That means no matter whether the data were hand-coded or not, their labels are based on the algorithm. This means there will be no systematic difference in quality of labels between training data and out-of-sample data as long as we use the predicted labels. Since the algorithm is excellent but not perfect, there are some small deviations from the hand-coded data. Predicted labels are then matched to the data first on a day-by-day level. These data are then aggregated to the monthly level and matched with the IFS and all the other data we plan to use.

Appendix C: Determinants of news on FX intervention

To provide a basis for assessing the performance of the news-based proxy and comparing it to the one based on reserve changes, we systematically analyze whether there are any determinants of news data that may create systematic biases when using such data in the construction of a proxy. We hypothesize that (i) economies with larger GDP, (ii) larger currency markets, or (iii) freer press have greater news coverage. We also test whether there are systematic differences (iv) during crisis times or (v) in different exchange rate regimes.

In Table A1 we estimate regressions that explain the number of or incidence of news in months with and without intervention. These are identified using our confidential actual intervention data. Columns 1 and 2 use the log number of news yielded by our search query and an indicator of whether any of these are classified as rumors or confirmations of intervention by our machine learning algorithm in a given month. Columns 3 and 4 are restricted to months with and without intervention, respectively. They thus help assess the probability of detection and precision. Column 5 measures the intensive margin of the indicator from column 3. Results suggest that news will not introduce large systematic biases along most of the dimensions considered above. Specifically, (i) The procedure we use to identify relevant news items does not generate a systematically greater number for larger economies. Still, in larger countries, an intervention is more likely to be covered by rumors or confirmations as a doubling of a country's GDP means approximately 0.5 additional news items per intervention month. The rather low level of statistical significance indicates that this pattern is not as strong as one might expect.

(ii) Unconditionally, the estimated FX trading volume of the respective currency is positively correlated with more news but, after controlling for GDP, no systematic association remains. The size of the economy thus matters more for overall news coverage and the coverage of interventions than a currency's trading volume. (iii) Countries that have a freer press, approximated by having a lower Freedom House score, are covered by more news, but FX interventions are not more likely to be covered. (iv) Furthermore, there is no evidence of systematic differences in reporting during times of crisis, captured by the VIX. (v) While exchange rate regimes do not generally explain differences in news coverage of intervention, the case of ERM-II membership seems to be important as we observe significantly less news if a country is an ERM-II member. This result should be treated with caution though, because among the countries from FGMSS that we can use there are only two ERM-II-countries: Denmark and Slovakia. This may therefore result from pure chance or unobserved characteristics of these countries. However, we rather suspect that ERM-II countries are expected to intervene frequently, thus rendering intervention not very newsworthy. This finding would suggest caution is needed when applying our proxy to ERM-II countries. (vi) In Table A2 we study whether adding covariates helps explain the probability that news is triggered at times of actual interventions. These regressions highlight that larger reserve changes or larger intervention volumes increase the probability of any relevant news. The news-based intervention proxy will thus typically detect the larger interventions, which are also the ones that are more likely to affect outcomes such as the level of the exchange rate. In additional tests, we study persistence of news reports. Contrary to what might be expected, interventions that come after a month without intervention do not have a higher likelihood of resulting in news, even after controlling for country and regime fixed effects. The basic results from above thus can be generalized. Also, during episodes of ongoing intervention, those that have been covered by news during previous months are more likely to receive coverage even after controlling for many country characteristics.

Correction of sample mean intervention series. Our intervention proxy sometimes misses interventions if these are not covered in the news reports. As discussed above, reporting behavior differs by how common interventions are in a given exchange rate regime. To account for these differences, we estimate the degree of underreporting by using separate linear regression models by regime r for each currency c in month t. Using no intercept, a model of the form

$$\text{Actual intervention dummy}_{it} = \beta \text{News-based intervention dummy}_{it} + \epsilon_{it}$$

yields an inflator β that we can then use to correct for the expected amount of underreporting. Assuming that this coefficient is stable over time, we can then calculate a corrected aggregate intervention proxy for each regime my multiplying β_r with the average intervention proxy for exchange rate regime r at time t.

We plot the underlying actual data, the uncorrected proxy and the corrected proxy for in Figure A2. Note that the solid line is the "corrected" number for the full sample, not the subsample for which we have actual intervention data. The corrected proxy series is used in Figure 5 in the main text where we aggregate countries that belong to different regimes by emerging market status.

Figure A2: Intervention incidence over time according to the proxy in comparison to actual data

The graph indicates the shares of intervening central banks according to the actual data and the proxy for the same subsample. The time series are smoothed using a rolling 6 month window around each point in time. The underlying data are monthly and the sample is restricted to the 1995-2011 time period used by Fratzscher et al. (2019) to be able to compare an identical set of countries. The "corrected" number is calculated by scaling up the proxy time series with a correction factor as described in Appendix C.

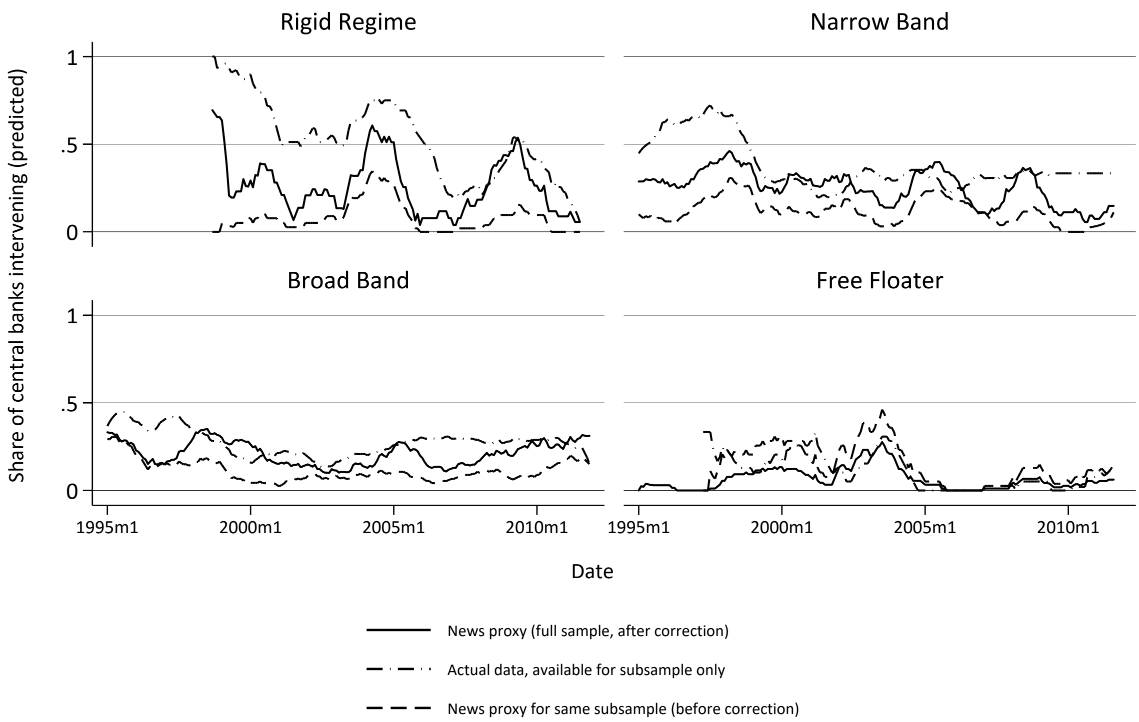


Figure A3: Quality of proxies by intervention size relative to turnover

The graph plots the rate of share of true positives and false positives for the reserve proxy and news-based proxy, respectively. The horizontal axis is a cutoff value that is defined by intervention size relative to FX turnover. FX turnover from the BIS triennial survey. True and false positives are differentiated using the actual intervention data from Fratzscher et al. (2019). The sample is therefore restricted to their 1995-2011 time period. The statistics are calculated for each cutoff and the graph is then automatically smoothed using an epanechnikov kernel of degree 0 and a 0.1 bandwith.

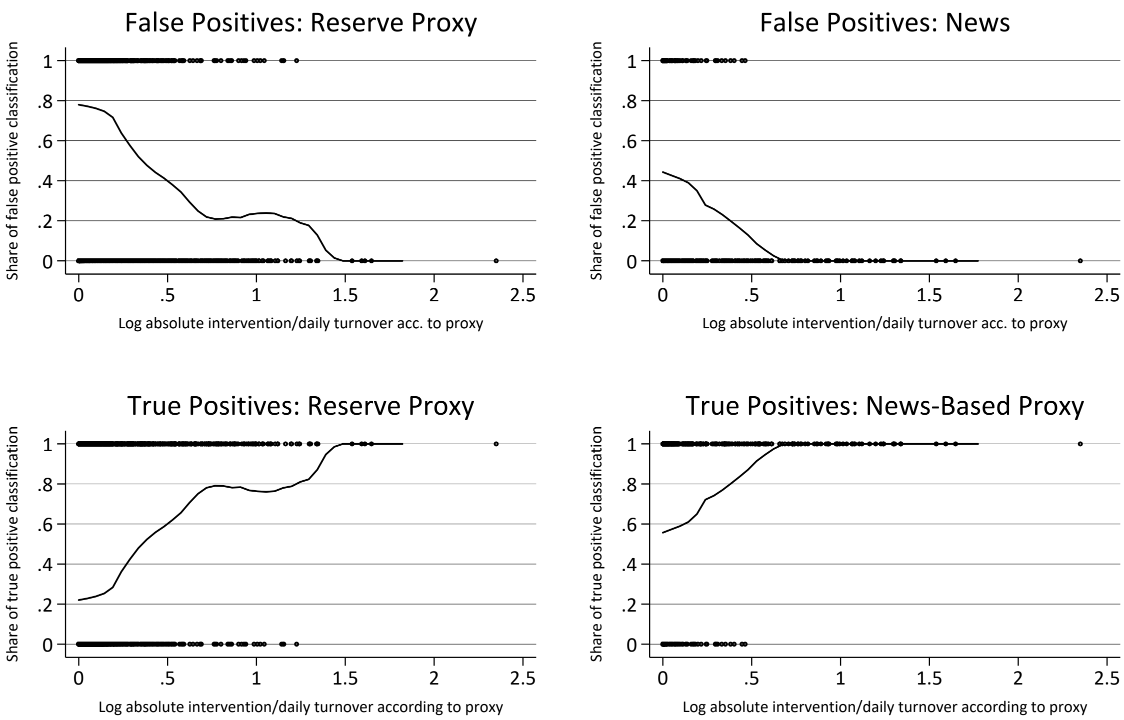


Table A1: Determinants of observing relevant news items

The table provides estimates of the determinants of observing relevant news items about foreign exchange intervention on Factiva in a given month and country. The dependent variable is given at the top of the column. Column 1 uses the total number of news items yielded by our search query for a country in a given month. Columns 2 to 5 use the number of news items among these that were classified as "rumor" or "confirmation" by our text classification algorithm. Standard errors that cluster at the country level in parentheses.

	(1)	(2)	(3)	(4)	(5)
Outcome	Log number of news per month	Any rumor or confirmation in given month	Any rumor or confirmation in given month	Any rumor or confirmation in given month	Log news items per month with actual intervention
Covariate of interest					
log(GDP)	0.103	0.0686	0.0356	0.00198	0.0723
	(0.182)	(0.0439)	(0.0844)	(0.0593)	(0.162)
log(BIS FX turnover)	-0.147*	-0.112***	-0.0630	-0.0271	-0.0417
	(0.0847)	(0.0292)	(0.0367)	(0.0437)	(0.0747)
Freedom house score	-0.00619	0.000215	0.000447	0.000986	-0.00776**
	(0.00394)	(0.00263)	(0.00211)	(0.00334)	(0.00353)
Vix (rolling, 6 months)	-0.00446	0.000160	-0.00296	0.00277*	-0.00179
	(0.00515)	(0.00182)	(0.00274)	(0.00156)	(0.00631)
ERM II Member	-0.523*	-0.334**	-0.466***	0.0645	-0.349
	(0.259)	(0.140)	(0.121)	(0.136)	(0.223)
Absolute log reserve change	0.130**	0.0126	0.00693	-0.0223	0.108**
	(0.0577)	(0.0161)	(0.0233)	(0.0130)	(0.0384)
Absolute log exchange rate change	2.132	-1.078	-0.742	-0.920	-2.139
	(2.027)	(0.818)	(1.226)	(0.775)	(2.118)
Constant	1.025	0.743**	0.931	0.378	0.526
	(1.333)	(0.327)	(0.728)	(0.403)	(1.574)
Observations	1,104	1,104	433	671	433
R-squared	0.146	0.110	0.133	0.030	0.417

Table A2: Determinants of observing relevant news items during intervention months

This table provides estimates for an OLS regression without fixed effects that predicts whether, for a given country, a month has rumors about intervention or confirmation of these on Factiva news according to our text classification algorithm. Heteroskedasticity-robust standard errors used throughout.

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome			Any rumor or confirmation in month			
Covariate of interest						
log(GDP)	0.0500	0.0325	-0.0163	0.0264	-0.00301	-0.118
	(0.0663)	(0.0784)	(0.0798)	(0.0854)	(0.0848)	(0.124)
log(BIS FX turnover)	-0.0642	-0.0627	-0.0462	-0.0535	-0.0506	-0.0111
	(0.0374)	(0.0372)	(0.0392)	(0.0392)	(0.0395)	(0.0597)
VIX Rolling 6 months	-0.00360	-0.00373	-0.00363	-0.00478	-0.00346	-0.00356
	(0.00249)	(0.00255)	(0.00278)	(0.00298)	(0.00248)	(0.00285)
Narrow Band (0/1)	0.0615	0.0857	0.141	0.147	-0.0587	0.127
	(0.124)	(0.124)	(0.123)	(0.120)	(0.164)	(0.123)
Broad Band (0/1)	-0.150	-0.129	-0.0735	-0.0717	-0.284	-0.107
	(0.108)	(0.115)	(0.123)	(0.118)	(0.173)	(0.118)
Free Floater (0/1)	0.303*	0.323*	0.359**	0.372**	0.177	0.305*
	(0.162)	(0.170)	(0.167)	(0.165)	(0.219)	(0.150)
Other Regime (0/1)	0.295**	0.309**	0.637***	0.577***	0.383	0.539***
	(0.141)	(0.143)	(0.203)	(0.199)	(0.236)	(0.184)
ERM II Member (0/1)	-0.355***	-0.357***	-0.377***	-0.360***	-0.495***	-0.279***
	(0.0424)	(0.0408)	(0.0403)	(0.0396)	(0.102)	(0.0923)
Log(absolute reserve change)		0.0176	-0.00610	-0.0152	-0.0137	0.00734
		(0.0212)	(0.0236)	(0.0247)	(0.0261)	(0.0214)
Log(absolute intervention volume)			0.0534***	0.0301	0.0460**	0.0498***
			(0.0161)	(0.0180)	(0.0162)	(0.0168)
Log(absolute intervention volume/GDP)				8.598***		
				(2.627)		
Freedom house score					0.000470	
					(0.00214)	
Log(population size)						0.104
						(0.0764)
Constant	0.647	0.723	0.960	0.678	1.113	1.548*
	(0.663)	(0.708)	(0.721)	(0.742)	(0.732)	(0.875)
Observations	457	457	457	457	433	457
R-squared	0.119	0.121	0.142	0.150	0.147	0.155

Table A3: Summary table: Distribution of key policy instruments

The table provides summary statistics of the frequency of use of foreign exchange intervention, capital controls, and macroprudential policies for different country groups. Capital controls from Fernández et al. (20106) and macprus from Cerutti et al. (2017). The sample is restricted to the lowest common denominator in terms of time frame, which is 2000-2014 from the Cerutti et al database. Data are included at the monthly level. Quarter-on-quarter changes counted in in column 4 to reflect the structure of the macpru data. Capital control indices scaled between 0 and 1. Changes in policy index can take values between 1 and -1. Cumulative index takes values between -8 and 25.

	Any intervention according to proxy	Inflow controls index	Outflow controls index	Changes in macroprud. policy index	Cumulative macroprud. policy index
All countries	0.109	0.337	0.411	0.057	1.131
Advanced Economies	0.098	0.140	0.193	0.044	0.387
Emerging Markets	0.119	0.494	0.584	0.069	1.787
Narrow bands	0.119	0.438	0.477	0.041	1.253
Broad bands	0.127	0.311	0.417	0.069	1.666
Free floaters	0.095	0.080	0.164	0.045	0.452
Other regimes	0.073	0.382	0.427	0.051	0.196

Table A4: Summary statistic of main working sample

This table provides summary statistics of the intervention database that is used in most parts of the text. Exchange rate regimes based on annual coarse classification by Ilzelzki et al. (2017a, 2017b). 1 represents rigid regimes, 2 narrow bands, 3 broad bands, 4 free floaters, 5 and 6 other regimes. FX turnover is based on the BIS triennial survey (e.g. BIS, 2017) thus not always available.

Country	First year covered	Last year covered	Average GDP in billion USD	Average daily FX turnover in billion USD	Regimes
Argentina	1995	2016	349	1.4	1,2,5
Australia	1995	2016	848	187	4
Brazil	1995	2016	1315	21	2,3,5
Bulgaria	1995	2015	33	0.74	1,5
Canada	1995	2016	1173	128	2,3
Chile	1995	2016	148	5.7	3
China	1995	2016	4011	40	1,2
Colombia	1995	2016	196	3	3
Croatia	1995	2016	43		1,2
Czech Republic	1995	2016	139	7.7	2,3
Denmark	1995	2016	258	20	1,2
EMU	1999	2016			4
Hong Kong	1997	2016	210	58	1
Hungary	1995	2016	97	9.4	2,3
Iceland	1995	2016	13		2,3
India	1995	2016	1029	23	1,2,3
Indonesia	1995	2016	458	4.2	2,3,5
Israel	1995	2016	179	5.7	3
Japan	1995	2016	4875	590	4
Kenya	1995	2016	29		2,3
Latvia	1995	2015	18	0.28	1,2,3
Lebanon	1995	2016	27		1
Malaysia	1995	2016	177	7.4	1,2,3,4
Malta	1995	2016	7		1,2,3
Mexico	1995	2016	841	49	3,5
New Zealand	1995	2016	112	42	3
Nigeria	1995	2015	232		2,3,5
Norway	1995	2016	313	39	3
Peru	1995	2016	103	1.3	2,3
Philippines	1995	2016	146	3.5	1,2,3,5
Poland	1995	2016	331	19	2,3,6
Romania	1995	2016	110	3.9	1,2,3,5
Russia	1995	2015	995	28	2,3,5,6
Saudi Arabia	1995	2016	379	3.1	1
Singapore	1995	2016	166	35	3
Slovak Republic	1995	2016	57		1,2
Slovenia	1995	2015	36		1,2
South Africa	1995	2016	245	24	3,6
South Korea	1995	2016	870	36	2,3,5
Sweden	1995	2016	396	55	2,3
Switzerland	1995	2016	455	167	1,3
Thailand	1995	2016	239	7.4	1,3,5
Turkey	1995	2016	489	26	3,5
Ukraine	1995	2016	97		1,3,5,6
United Kingdom	1995	2016	2224	363	3,4
United States	1995	2016	12767	2429	4
Uruguay	1995	2016	30		2,3,5
Venezuela	1995	2016	206		1,2,5
Vietnam	1995	2015	85		2

Table A5: Overview of overlap between actual intervention data and main working sample

This table provides an overview of the overlap between the new proxy sample and Actual intervention data as in Fratzscher et al. (2019). Exchange rate regimes based on annual coarse classification by Ilzelzki et al. (2017a, 2017b). 1 represents rigid regimes, 2 narrow bands, 3 broad bands, 4 free floaters, 5 and 6 other regimes. FX turnover is based on the BIS triennial survey (e.g. BIS, 2017) thus not always available.

Country	First year covered	Last year covered	Average GDP in billion USD	Average daily FX turnover in billion USD	Regimes
Argentina	2003	2011	282	1.1	1
Australia	1997	2011	731	148	4
Canada	1995	2011	1016	98	2,3
Chile	2001	2011	145	3.9	3
Colombia	1999	2011	167	1.7	3
Croatia	1996	2011	40		1,2
Czech Republic	1995	2011	122	4.8	2,3
Denmark	1995	2011	238	15	1,2
Hong Kong	1998	2009	184	47	1
Iceland	1995	2011	12		2,3
Israel	1995	2011	150	3.6	3
Japan	1995	2011	4784	441	4
Kenya	1999	2011	25		2
Mexico	1997	2011	798	26	3
New Zealand	1995	2010	90	27	3
Norway	1995	2011	268	29	3
Peru	1995	2011	79	0.58	2,3
Poland	1995	2010	277	14	3
Slovak Republic	1999	2008	45		2
South Africa	1999	2011	233	21	3
Sweden	1995	2006	300	21	2,3
Switzerland	1995	2001	301	93	3
Turkey	2002	2011	534	12	3,5
United Kingdom	1995	2011	2071	294	3,4
United States	1997	2011	12179	2072	4
Venezuela	1997	2011	177		1,2

Table A6: Logit Estimates on capital controls and intervention corresponding to Table 4

This table provides logit estimates of the relationship between foreign exchange intervention and capital controls. Panel A provides a comparison of intervention proxy and true data. Panel B provides estimates of the relationship for advanced and emerging countries, respectively, as well as differentiating between inflow and outflow controls. The dependent variable is the foreign exchange intervention proxy or actual intervention data from Fratzscher et al. (2019). Capital controls data from Fernández et al. (2016) and included as levels. Intervention data are monthly while capital controls data are yearly. Hence, we do not use changes in capital controls here and treat capital controls as a background variable. The sample period is from 1995-2015. The logit models include year and regime fixed effects where indicated. *** p<0.01, ** p<0.05, * p<0.1.

Panel A: Comparison of proxy and actual data

	(1)	(2)	(3)	(4)
Outcome variable	Intervention proxy	Intervention proxy	Intervention proxy	Actual intervention
Subgroup	All	All	Sample of column 4	If actual data available
Covariate of interest				
Capital controls	0.573***	0.580***	0.779***	1.361***
(levels)	(0.0920)	(0.0858)	(0.206)	(0.153)
Year FE	yes	no	yes	yes
Regime FE	yes	no	yes	yes
Observations	11,731	11,731	3,971	3,971

Panel B: Subgroup analysis for the intervention proxy

	(5)	(6)	(7)	(8)
Covariate of interest	All controls	All controls	Outflow controls	Inflow controls
Subgroup	Advanced economies	Emerging markets	All	All
Estimate	1.094***	0.327**	0.584***	0.449***
	(0.171)	(0.137)	(0.0825)	(0.0958)
Year FE	yes	yes	yes	yes
Regime FE	yes	yes	yes	yes
Observations	5,214	6,503	11,731	11,742

Table A7: FX intervention and the changes of capital controls

The table reports estimates of the relationship between FX interventions and changes in capital controls. Interventions are included as dummy variables. We either use our proxy or actual intervention data in different columns. Capital controls data from Fernández et al (2016) and included as changes. Intervention data are monthly while capital controls data are yearly, hence aggregate intervention variables up to the yearly level. To reflect that monthly intervention data are aggregated up, Panel A and B use different outcome variables. Panel A uses a dummy variable which takes the value 1 if there was any intervention during the respective year (i.e. the maximum of the intervention series per country-year). Panel B uses the number of months with interventions (i.e. the sum of the intervention series per country-year). The sample period is from 1995-2015. All estimates are based on OLS models. These include year and regime fixed effects where indicated. Heteroskedasticity-robust standard errors throughout. *** p<0.01, ** p<0.05, * p<0.1.

Panel A: FXI proxy measuring whether any FX during the year

	(1)	(2)	(3)	(4)
Outcome variable	Intervention proxy	Intervention proxy	Intervention proxy	Actual intervention
Subgroup	All	All	Sample of column 4	If actual data available
Estimate	-0.007	-0.008	-0.004	-0.004
	(0.007)	(0.006)	(0.010)	(0.010)
Year FE	yes	no	yes	yes
Regime FE	yes	no	yes	yes
Observations	922	931	305	305
R-squared	0.074	0.002	0.050	0.050

Panel B: FXI proxy measuring number of months with any FX intervention during year

	(5)	(6)	(7)	(8)
Outcome variable	Intervention proxy	Intervention proxy	Intervention proxy	Actual intervention
Subgroup	All	All	Sample of column 4	If actual data available
Estimate	-0.001	0.000	0.002	0.001
	(0.002)	(0.002)	(0.003)	(0.001)
Year FE	yes	no	yes	yes
Regime FE	yes	no	yes	yes
Observations	922	931	305	305
R-squared	0.073	0.000	0.052	0.050

Table A8: Capital controls and FX intervention by exchange rate regime

This table provides estimates of the relationship between foreign exchange intervention and capital controls. Panel A uses inflow controls, Panel B uses outflow controls. The dependent variable is the foreign exchange intervention proxy or actual intervention data from Fratzscher et al. (2019). Capital controls data from Fernández et al. (2016) and included as levels. Intervention data are monthly while capital controls data are yearly. Hence, we do not use changes in capital controls here and treat capital controls as a background variable. The sample period is from 1995-2015. The OLS models include year and regime fixed effects where indicated. *** p<0.01, ** p<0.05, * p<0.1.

Panel A: Levels of inflow controls

	(1)	(2)	(3)	(4)
Covariate of interest	Inflow controls			
Subgroup	Narrow Bands	Broad Bands	Free Floaters	Other regimes
Covariate of interest				
Inflow controls	0.100***	0.0353**	-0.354***	-0.000
(levels)	(0.0183)	(0.0152)	(0.0744)	(0.0153)
Year FE	yes	yes	yes	yes
Observations	2,779	5,209	1,057	2,697
R-squared	0.033	0.020	0.149	0.065

Panel B: Levels of outflow controls

	(5)	(6)	(7)	(8)
Covariate of interest	Outflow controls			
Subgroup	Narrow Bands	Broad Bands	Free Floaters	Other regimes
Covariate of interest				
Outflow controls	0.110***	0.0443***	-0.222***	0.0326**
(levels)	(0.0169)	(0.0115)	(0.0534)	(0.0144)
Year FE	yes	yes	yes	yes
Observations	2,768	5,209	1,057	2,697
R-squared	0.037	0.022	0.149	0.067

Table A9: Capital controls and FX intervention by direction of flow

This table provides estimates of the relationship between foreign exchange intervention and capital controls. Panel A uses levels of controls by direction and development level of the economy. Panel B uses changes (here at the monthly level, cf. Table A7). The dependent variable is the foreign exchange intervention proxy or actual intervention data from Fratzscher et al. (2019). Capital controls data from Fernández et al. (2016) and included as levels. Intervention data are monthly while capital controls data are yearly. Hence, we do not use changes in capital controls here and treat capital controls as a background variable. The sample period is from 1995-2015. The OLS models include year and regime fixed effects where indicated. *** p<0.01, ** p<0.05, * p<0.1.

Panel A: Levels of capital controls

	(1)	(2)	(3)	(4)
Subgroup	All	All	Advance Economies	Emerging Markets
Covariate of interest				
Inflow controls	0.0427***		0.0853***	0.0248*
	(0.00955)		(0.0231)	(0.0133)
Outflow controls		0.0544***		
		(0.00787)		
Year FE	yes	yes	yes	yes
Regime FE	yes	yes	yes	yes
Observations	11,742	11,731	5,214	6,528
R-squared	0.029	0.031	0.025	0.037

Panel B: Changes in capital controls

	(1)	(2)	(3)	(4)
Subgroup	All	All	Advance Economies	Emerging Markets
Covariate of interest				
Inflow controls	0.0256		-0.170	0.0902
	(0.104)		(0.191)	(0.121)
Outflow controls		-0.0252		
		(0.0913)		
Year FE	yes	yes	yes	yes
Regime FE	yes	yes	yes	yes
Observations	11,702	11,690	5,197	6,505
R-squared	0.027	0.027	0.023	0.036

Table A10: Prudential policies and FX intervention by exchange rate regime

This table provides estimates of the relationship between foreign exchange intervention and prudential policies by exchange regime. Panel A uses levels of prudential policies. Panel B uses quarterly changes for reserve requirements for foreign currency, which we expect to be more closely linked with foreign exchange intervention than others macroprudential policies. The dependent variable is the foreign exchange intervention proxy or actual intervention data from Fratzscher et al. (2019). Capital controls data from Cerutti et al. (2016). Intervention data are monthly. Macpru data are quarterly, hence we aggregate intervention data up accordingly.The sample period is from 2000-2014. The OLS models include year . *** p<0.01, ** p<0.05, * p<0.1.

Panel A: Levels of prudential policy index

	(1)	(2)	(3)	(4)
Covariate of interest	Level of macroprudential policies			
Subgroup	Narrow Bands	Broad Bands	Free Floaters	Other regimes
MacPrus	0.00547*	0.0291***	-0.0317	-0.0171***
(levels)	(0.00304)	(0.00435)	(0.0231)	(0.00441)
Year FE	yes	yes	yes	yes
Observations	596	1,315	264	640

Panel B: Changes in reserve requirements for foreign currency

	(5)	(6)	(7)	(8)
Covariate of interest	Level of macroprudential policies			
Subgroup	Narrow Bands	Broad Bands	Free Floaters	Other regimes
MacPrus	0.0439	0.130**	*No changes*	0.0457
(levels)	(0.0588)	(0.0624)	*to exploit*	(0.0716)
Year FE	yes	yes	yes	yes
Observations	596	1,315	264	640
R-squared	0.040	0.022	0.212	0.068

Table A11: FX intervention and the levels of prudential policies

The table reports estimates of the relationship between FX interventions and the macroprudential policy level. The table provides the equivalent to Table 5. Interventions are included as dummy variables. We either use our proxy or actual intervention data in different columns. Macprus are from Cerutti et al (2017a). Since macpru data quarterly, intervention data are aggregated up to quarterly data. The Cerutti et al database is based on changes and does not have a start level, so we use the cumulative changes variable they provide. This should be interpreted not as a level of intensity but as a level in addition to the policies that were already in place in the end of 1999 in the given country. The sample period is from 2000-2014. All estimates are based on OLS models. These include year and regime fixed effects where indicated. Heteroskedasticity-robust standard errors throughout. *** p<0.01, ** p<0.05, * p<0.1

	(1)	(2)	(3)	(4)	(5)
Outcome	Intervention proxy		Actual intervention data	Intervention proxy	
Subgroup	All	Sample of column 3	Countries where actual data available	Advanced Economies	Emerging Markets
Covariate of interest					
Cumulative PruC	0.00523***	-0.00370	-0.00401	0.00992***	0.00280**
	(0.000978)	(0.00315)	(0.00403)	(0.00197)	(0.00116)
Regime FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
Observations	8,445	2,997	2,997	3,957	4,488
R-squared	0.024	0.015	0.069	0.030	0.026