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EVALUATING FOREIGN EXCHANGE MARKET INTERVENTION: SELF-SELECTION, COUNTERFACTUALS AND AVERAGE TREATMENT EFFECTS

Rasmus Fatum and Michael M. Hutchison

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Evaluating Foreign Exchange Market Intervention: Self-selection, Counterfactuals and Average Treatment Effects

Rasmus Fatum
University of Alberta

and

Michael M. Hutchison*
University of California, Santa Cruz
Hong Kong Institute for Monetary Research

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Abstract

Estimating the effect of official foreign exchange market intervention is complicated by the fact that intervention at any point entails a self-selection choice made by the authorities and that no counterfactual is observed. To address these issues, we estimate the counterfactual exchange rate movement in the absence of intervention by introducing the method of propensity score matching to estimate the average treatment effect (ATE) of intervention. To derive the propensity scores we introduce a new intervention reaction function that includes the difference between market expectations and official announcements of macroeconomic developments that can influence the decision to intervene. We estimate the ATE for daily official intervention in Japan over the January 1999 to March 2004 period. This sample encompasses a remarkable variation in intervention frequencies as well as unprecedented frequent intervention towards the latter part of the period. We find that the effects of intervention vary dramatically and inversely with the frequency of intervention: Intervention is effective over the 1999 to 2002 period and ineffective (or possibly counterproductive) during 2003 and 2004. These results hold up to a variety of robustness tests. Only sporadic and relatively infrequent intervention appears to be effective.

Key words: Foreign Exchange Intervention, Bank of Japan, Self-Selection, Matching Methods.

JEL Classifications: E58, F31, G15.

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Rasmus Fatum
School of Business
University of Alberta
Edmonton, Alberta
Canada, T6G 2R6
Email: rasmus.fatum@ualberta.ca

Michael M. Hutchison
Department of Economics
University of California
Santa Cruz, CA 95064
USA
Email: hutch@ucsc.edu

1. Introduction

Central Bank and Ministry of Finance officials make decisions to intervene in the foreign exchange market based on particular economic conditions, political factors, changes in policy and so on. Intervention is typically not random, but a process by which officials self select when deciding to intervene. Since an exchange rate movement at any given point in time coincides with either intervention or no intervention, we cannot directly observe both the exchange rate movement coinciding with intervention and what would have been the counterfactual, i.e. what would have been the exchange rate movement if intervention *had not* occurred when the authorities did in fact intervene. In other words, the counterfactual is not directly observable and, as such, this constitutes a missing data problem. These inherent issues of self-selection and missing data complicate the assessment of the effects of intervention. Following the modern literature on treatment effects (see Imbens, 2004, for a recent survey), we address the issue of self-selection and the missing counterfactual by estimating the average treatment effect (ATE) of intervention on the exchange rate using a propensity score matching methodology.

The approach taken here to evaluate the effectiveness of intervention, while addressing the aforementioned methodological issues, is to postulate a counterfactual and, in turn, match pairs of observations (or an average of control observations) of exchange rate movements – each pair consisting of an exchange rate movement coinciding with intervention and one that coincides with no intervention — on similar observable characteristics. Although the Japanese yen (JPY) appreciated strongly against the U.S. dollar (USD) over our sample period, intervention may still have been effective in reducing the magnitude of this appreciation. This highlights the necessity of estimating a relevant counterfactual. We consider intervention as a treatment and, using matched counterfactuals, investigate the exchange rate movements with and without intervention in otherwise identical circumstances (as far as can be determined by observable market characteristics that lead up to the decision by the central bank to intervene). By using similar economic circumstances that lead to intervention (similar probabilities of intervention) for matching up observations that differ only in whether intervention occurs or not, we are able to address the missing observations and the sample selection bias issues.

The focus of our matching analysis is to examine the effects of daily Japanese official interventions in the JPY/USD exchange rate market over the January 1999 to March 2004 period. This is a fascinating and unprecedented period in the history of foreign exchange market intervention and fits our methodological framework perfectly. Firstly, the magnitude of intervention was extremely large. Japanese foreign exchange market intervention jumped in 2003, shown in Figure 1, with the official selling of JPY 20.2 trillion (USD 177 billion) in exchange for USD — an amount surpassing that of any other country for any given year. Massive intervention operations in support of the USD continued in the first quarter of 2004, during which time the authorities sold another JPY 14.8 trillion (USD 139 billion). Although Japan has been the most active amongst the larger industrial economies in its foreign exchange market operations during the past decade and more, the recent magnitude dwarfs all previous experience. Secondly, there are distinct periods of intervention frequency during this sample period. Fatum and Hutchison (2005) and several others observe that a sharp departure from past Japanese intervention policy began in early 2003 when the frequency of interventions jumped dramatically. Official intervention continued in the first quarter of 2004 and, in fact, this quarter stands out with an intervention frequency of 73% of business days. Moreover, Fatum and Hutchison (2005) demonstrate that intervention operations in Japan during this time were automatically sterilized and had no independent effect on monetary growth (i.e. over and above what would otherwise have been the case in the absence of intervention operations). Consistent with the studies by Ito (2003 and 2005) and Kearns and Rigobon (2005), we identify (three) sub-samples of separate intervention regimes according to, in our case, highly noticeable changes in the official Japanese intervention frequency. Formal tests of reaction function parameter instability across the sub-samples confirm the existence of three separate intervention regimes by Japanese officials.[1]

The basic methodology consists of two parts. In the first part, models of the decision to intervene (decisions by the Ministry of Finance, carried out by the Bank of Japan as its agent through the Foreign Exchange Fund Special Account) are estimated separately across the full and across the three sub-samples. From the model estimates, the probability of intervention (a propensity score) for each day in the given sample is derived. The sample is then split into a group of days when intervention occurs and a group of days when no intervention occurs. Regardless of whether or not intervention occurs on a given day, there is a uniquely defined intervention probability associated with each day in both groups as well as a realized (day-to-day) change in the JPY/USD exchange rate. In the second part, a matching algorithm – the so-called nearest neighbor algorithm where each intervention observation is matched with the no-intervention observation that has the nearest propensity scores – is implemented and the ATE of intervention on exchange rates is examined using difference-in-means tests.[2]

Focusing on all intervention days and the general issue of effectiveness, the results of the ATE-matching analysis show that the effect of official intervention in Japan varies dramatically across the three sub-samples under study: significant effect (in the anticipated direction) during the period of infrequent interventions, no significant effect during the period of relatively frequent interventions, and either an insignificant or perverse (counterproductive) effect during the period of very frequent interventions. Furthermore, we find a systematic pattern of non-uniform intervention effects across specific types of intervention days, indicating structural parameter instability within different intervention regimes. These findings are consistent with the view that infrequent intervention operations may surprise markets and prove an effective policy strategy, while frequent intervention operations – even very large scales – are incorporated into market expectations with little or even counterproductive effects.

The rest of the paper is organized as follows. Section 2 describes the official Ministry of Finance Japanese intervention data. Section 3 further discusses the matching methodology and its application to the study of intervention. This section also describes the reaction function estimations necessary for extracting the propensity scores used in the matching. Section 4 presents the main results. Section 5 considers several robustness tests, including radius matching, using trimmed samples, and a procedure to deal with serial dependence. Section 6 discusses the results in light of other recent intervention papers. Section 7 concludes.

1 Despite the evident departure from past intervention policies, there was no official announcement of a policy change in January 2003 or in January 2004. Furthermore, there was no official announcement made when the active BoJ intervention policy ended abruptly on 16 March 2004 and no intervention took place in the remainder of 2004.

2 Robustness tests also employ the “radius” algorithm, trimmed sample estimates and corrections for serial dependence.

2. Data and Summary Statistics

The official Japanese intervention data is provided by the Ministry of Finance and consists of official, daily operations in the JPY/USD foreign exchange market. During the period under study, 1 January 1999 to 31 March 2004, all official interventions in the JPY/USD market are sales of JPY against purchases of USD. The U.S. government (i.e. the U.S. Treasury, with the Federal Reserve Bank of New York operating as its agent) did not intervene in the JPY/USD exchange rate market during this period.

Table 1 shows that during the full sample period, Japan intervened in the JPY/USD exchange rate market on a total of 159 days. On most intervention days the magnitude of intervention was substantial, with purchases of over USD 1,000 million and larger dominating (46 intervention days of less than USD 1,000 million were reported and only 20 intervention days consisted of less than USD 250 million).

Table 1 also shows that only 30 of the intervention days occurred during the first four years of our sample (between January 1999 and December 2002), 82 intervention days occurred during 2003, while a remarkable 47 intervention days occurred during the last three months of our sample (between January 2004 and March 2004). The described variation in intervention frequencies across the three sub-samples suggests that the January 1999 to March 2004 time period encompassed not one but three different intervention regimes.

We follow Ito (2003) and others in using New York close quotes of the daily JPY/USD exchange rate. The exchange rate data are obtained from Global Financial Data (GFD).

Table 2 presents some basic summary statistics of the exchange rate and intervention data. The number of intervention days associated with three categories of exchange rate changes – small, medium and larges – is displayed for the full and for each sub-sample period. The number of intervention days falling into a given category, as a percentage of total intervention days, is shown in parentheses below the absolute number of intervention days. The number of days as a percentage of total days in the sample falling into the particular exchange rate change category is given in brackets below the number of intervention days.

For the entire sample period (January 1999 – March 2004), the first column of Table 2 shows that the JPY/USD fell (JPY appreciation) by -6.86 percent and that the average daily absolute change in the exchange rate was 0.5%. 47% of the interventions (74 days) occurred during days of small exchange rate movements, 33% (52 days) occurred during days of medium exchange rate changes, and 21% (33 days) occurred during days of large exchange rate changes. The percentage of days in the full sample corresponding to small, medium and large exchange rate changes are 37%, 39% and 23%, respectively. Clearly, intervention operations were distributed over all days and not just associated with large exchange rate changes. In fact, intervention occurred relatively more frequently on days of small exchange rate changes (47%), while the percentage of days in the full sample experiencing small exchange rate changes is only 37%.

Intervention operations were mainly concentrated on days of large exchange rate changes in the first sub-sample (1999-2002) when 47% of the intervention days were in this category, compared to only 26% of the exchange rate changes. By contrast, the last sub-sample (2004Q1) saw most of the intervention concentrated during days of small exchange rate changes (49% of the intervention days) while 48% of the observations were in this category. It appears that a shift in exchange rate policy took place. Intervention was mainly concentrated during episodes of (relatively infrequent) large exchange rate changes in the first sub-sample sample, and shifted to a fairly uniform distribution (percentage of intervention days corresponding to percentage of days in the sample) of intervention across all days in the second and third sub-samples.[3]

3. Method of Matching and Average Treatment Effects (ATE)

The advantage of the matching method is that it addresses the issue of non-random sample selection.[4] Furthermore, as a non-parametric statistical method, it avoids the estimation of a time-series model of daily exchange rate changes. This is a further advantage, given the lack of consensus on what is the proper exchange rate model.[5]

The effect of the official Japanese intervention on the JPY/USD exchange rate is assessed by matching observations with similar characteristics in terms of intervention propensities (i.e. the likelihood of intervention on a given day), in which one group of observations consists of days when the government intervened (the treatment group) while the other group consists of days when the government did not intervene (the control group). In turn, the matching of observations allows us to capture the effect of intervention (the treatment effect) by measuring the difference in the average JPY/USD exchange rate change between the two groups.

In order to characterize the similarity among observations with and without official intervention, we consider a set of observable variables that can explain the decision by the Ministry of Finance to intervene. These variables, described in detail in the next sub-section, include the standard explanatory variables used when estimating official intervention reaction functions extended to also include a number of news variables that have not heretofore been employed in this literature. One approach would be to

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[3] Table 2 also displays the number of intervention days coinciding with Reuters news reports of intervention. Some studies question the accuracy of such news reports (see Fischer, 2006 and Osterberg and Wetmore Humes, 1993) and we do not incorporate this information into our analysis.

[4] See Glick, Guo and Hutchison (2006) for a recent application of the matching method to an analysis of the effects of capital account liberalization on the risk of currency crises. See also Persson (2001) for a very useful exposition of the matching methodology. An excellent textbook treatment is provided by Wooldridge (2002) and a comprehensive recent survey is provided by Imbens (2004).

[5] It is, of course, necessary to estimate a parametric model of the decision to intervene in order to facilitate the extraction of the propensity scores.

match each intervention observation with a no-intervention observation that has the same observed values of a vector of intervention determinants. This multidimensional matching is, however, difficult to implement given the relatively limited number of intervention observations and, as it turns out, not necessary. Instead, as shown by Rosenbaum and Rubin (1983 and 1985) in a general context, it is sufficient to match according to the one-dimensional probability of an observation being subject to the treatment. Therefore, by using the observable intervention determinants for estimating the probability of an intervention occurrence for each day in our sample, the matching is carried out according to the estimated probability of official intervention (the propensity scores).

The nearest neighbor algorithm matches each intervention observation to the no-intervention observation that has the nearest propensity score. After the no-intervention observation is used, it is returned to the pool of no-intervention observations. The effect of intervention, i.e. the treatment effect, is computed as a simple average of the differences in outcomes across the paired matches.

We conduct several robustness tests. Firstly, we implement the radius algorithm. The radius algorithm matches each intervention observation to the average of all the no-intervention observations with propensity scores falling within a pre-set radius from the propensity score of the intervention observation. The effect of intervention is again computed as an average of the difference in outcomes, weighted by the number of no-intervention observations used in the construction of each match. Secondly, we address the fact that our observations are not independent draws, thereby leading to a possible correlation between the treatment and the control sample observations. We adjust for this concern by excluding from the control sample (consisting of days of no intervention) all observations immediately following an intervention operation.[6]

3.1 Propensity Scores: Foreign Exchange Intervention Reaction Functions

In order to estimate the Japanese official intervention reaction function and, in turn, extract the propensity scores, we follow Ito and Yabu (2007). Ito and Yabu (2007) build on the friction model developed by Almekinders and Eijffinger (1996) and use daily data for estimating an ordered probit threshold model of Japanese intervention operations over the 1991 to 2002 time period. They develop their reaction function model from first principles by assuming that the Japanese authority has a loss function (in exchange rate deviations from a target) that it seeks to minimize by intervening in the foreign exchange rate market. Furthermore, they assume that the exchange rate is a random walk, and that there are political costs associated with intervention. These political costs are independent of the size of intervention and may help explain why intervention tends to be correlated such that intervention on day t is likely to be followed by intervention on day t+1.

6 We also control for the possibility that macroeconomic news surprises affect the exchange rate by excluding from both the intervention and the no intervention samples days coinciding with news surprises. Unsurprisingly, our results are not affected by this exclusion of data points. Very few news surprises coincide with intervention days.

As explanatory variables, Ito and Yabu (2007) use three measures of the past exchange rates (the first lag of the JPY/USD rate, the 21 business day moving average of the JPY/USD rate, and the one-year moving average of the JPY/USD rate) as well as the first lag of a (-1, 0, 1) intervention indicator variable that takes on non-zero values on intervention days only.[7] In addition, they employ (potentially asymmetric) threshold values of intervention in order to capture when the costs of intervening are exceeded by the benefits.[8]

We extend this approach by including news announcements that could influence the Ministry of Finance in directing its intervention operations.[9] We are interested in the surprise component of news, defined as the difference between official announcements regarding GDP, CPI, the unemployment rate and the trade balance, and results of surveys of expectations of these announcements conducted by Bloomberg during the days preceding the announcements. The official value of these news variables is announced once a month, or at a lower frequency. Our news variables capture the associated surprise element on announcement dates, thus these variables are non-zero only on announcement dates and only when the announcement differs from market expectations. To our knowledge, this is the first attempt to introduce these types of news variables into Japanese intervention reaction functions.

As mentioned earlier, all the official intervention operations in our sample period are sales of JPY against USD purchases. This allows us to use a standard binary choice model, reflecting that there are only two (intervention or no intervention) as opposed to three (intervention purchases of USD, no intervention, and intervention sales of USD) possible actions taken by the authorities.[10]

We use a logit model framework and estimate the following regression model:

$$IINT_t = \beta_0 + \beta_1 JPYUSD_{t-1} + \beta_2 TARGET_{t-1} + \beta_3 MADAY_{t-1} + \beta_4 MAYEAR_{t-1} + \beta_5 NEWS \\ \beta_6 IINT_{t-1} + \varepsilon_t \tag{1}$$

where IINT is the (0, 1) indicator variable that takes on the value 1 on days when there is intervention and 0 otherwise, JPYUSD is the first-difference of the log of the JPY/USD exchange rate, TARGET is the first-difference of the log of the JPY/USD deviation from an exchange rate target of 125 JPY/USD, MADAY is the 21-day moving average of the log of the JPY/USD exchange rate and MAYEAR is the

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7 While Almekinders and Eijffinger (1996) use the intervention amount as their dependent variable, consistent with Baillie and Osterberg (1997), Ito and Yabu (2007) use the indicator variable of intervention as their dependent variable in a binary choice modeling framework. They argue that the decision of the monetary authority to intervene or not is more important than the magnitude of intervention.

8 By following closely the Ito and Yabu (2007) framework, our reaction function estimates are free of simultaneity bias due to the focus on lagged regressors. A shortcoming, however, is that we do not account for the possibility of within-day exchange rate movements and volatility triggering some interventions.

9 Bonser-Neal and Tanner (1996) and, more recently, Galati, Melick and Micu (2005) and Fatum and Scholnick (2006) have found news variables to impact day-to-day exchange rate changes. Our reaction function specification allows such news to influence the decision to intervene.

10 In other words, analyzing the entire April 1991 to March 2004 period of publicly available Japanese intervention data (encompassing both purchases and sales of USD) using the binary treatment framework is not possible.

one-year moving average of the log of the JPY/USD exchange rate.[11] NEWS is the vector of variables capturing the unexpected component of Japanese macroeconomic news on days when an official macroeconomic announcement differs from market expectations. NEWS surprises cover GDP (GDP-UNEXP), CPI (CPI-UNEXP), the unemployment rate (UNEM-UNEXP) and the trade balance (TRDE-UNEXP). NEWS variables are daily observations (on the announcement date) that are divided into positive and negative surprises (giving us a total of eight NEWS announcement variables), in order to take into account the fact that *only* dollar support intervention operations were observed during the five-year sample period.

In order to take into account the possibility of heteroskedasticity in the error term, ε_t, all our logit model estimations are carried out using White's (1980) heteroskedasticity-consistent (robust) standard errors. The constant term, β_0, is included to allow for the possibility of a threshold value consistent with the political costs of intervention.[12] The reaction function model is estimated over the full sample period and separately across the three sub-samples. The sub-samples are demarked by the striking changes in intervention frequencies.[13] As noted earlier, despite no official announcement of a policy change in January 2003 when the frequency of intervention jumped to 32% of business days, or in January 2004 when the frequency of intervention jumped even further to 73% of business days, it seems evident that the de-facto intervention policy of the Japanese monetary authority changed twice during the period under study.

We first estimated the model including all the explanatory variables in equation (1). A common characteristic of the estimated equations for all four sample periods is that the variables TARGET and six of the NEWS variables are insignificant.[14] We then excluded the insignificant explanatory variables and re-estimated the reduced models. These models are reported in Table 3. The significant variables for the full sample (column 1) are the lagged exchange rate change JPYUSD, lagged MAYEAR, lagged intervention, positive GDP announcement surprises and positive CPI announcement surprises. The significant variables differ across the sub-sample estimations.

The displayed model diagnostics suggest that the four regressions fare reasonably well in explaining intervention operations. The McFadden R^2 ranges from 0.27 to 0.41 in the estimated equations, and they all pass the Likelihood Ratio test against the constant only alternative. None of the models are rejected

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11 The variable TARGET is included (and significant) in the reaction function estimations displayed in Ito (2003), but not included in Ito and Yabu (2007). Inclusion of TARGET is possibly problematic due to a high degree of collinearity with JPYUSD. As it turns out, TARGET is insignificant in all our estimations and dropped from the analysis.

12 As explained in Ito and Yabu (2007), this "conventional" reaction function specification is a linearization of the general friction model of central bank intervention.

13 This is similar to Kearns and Rigobon (2005), Ito (2003), and Ito and Yabu (2007) who identify June 1995 as a turning point in the BoJ intervention policies of the previous decade due to the noticeable change in the frequency of intervention.

14 These results are not reported for brevity but are available from the authors upon request.

by the Hosmer-Lemeshow test and, lastly, all four models improve the predictive ability against the naïve constant probability model. These results compare favorably with other models explaining official intervention policy in Japan.[15]

The model coefficient estimates reported in Table 3 indicate structural breaks across the sub-samples, in line with the observed shifts in the Japanese intervention intensity. We use a standard Wald test (F-test) to asses whether the coefficient estimates of the reduced models are significantly different across the three sub-samples. Table 4 displays the results. Overall, the Wald tests show that the estimated reaction function parameters are generally unstable, indicating the existence of three separate intervention regimes.

Our estimations suggest that the Ministry of Finance reacts systematically to previous day exchange rate changes in deciding to intervene, i.e. a depreciation of the JPY against the USD leads to a lower probability of USD-support intervention the following day. This holds true in every sub-sample except sub-sample 3 (first quarter of 2004). Intervention on a given day is an excellent predictor of intervention the following day in every sub-sample, indicating that intervention operations are frequently clustered. The 21-day moving average of the exchange rate (MADAY) is also a good predictor of intervention in the first and the second sub-sample (1999-2002 and 2003), indicating that trend depreciation of the JPY reduces the likelihood of intervention. Over the entire sample (1999-2004) the one-year moving average exchange rate change (MAYEAR) is a significant predictor of intervention, i.e. long-term trend depreciation of the JPY leads to a lower likelihood of USD-support intervention (even when controlling for contemporaneous exchange rate changes). Unexpected increases in Japanese GDP growth (growth announcements less expected growth based on survey data) increases the likelihood that the Ministry of Finance will intervene to support the USD in the full sample and in the third sub-sample (columns 1 and 4). Unexpectedly high CPI announcements also lead to USD-support intervention in the full sample and the first sub-sample.

4. Results

As a benchmark for comparison, we test for significant effects of intervention without invoking the matching technique. In order to do so, we use a standard t-test for assessing whether the average exchange rate change across intervention days is significantly different from zero. We do so across the full sample as well as separately across the three easily identified sub-samples.

Table 5 shows that intervention is, on average, effective over the full January 1999 to March 2004 period. Table 6 shows that the effects of intervention vary across the three samples and that the full-sample effectiveness result is driven by the intervention that occurred during the January 1999 to December 2002 sub-period. Once again, the idea of uniform effects of intervention across different regimes, therefore, seems questionable and we focus our matching analysis on the sub-samples separately rather than on the full sample.

[15] Although not directly comparable with our results, it is noteworthy that R-Bar squares for Ito's (2003) intervention functions, estimated by GMM, was 0.345 over April 1991 to June 1995, but only 0.025 over June 1995 to March 2001 (the period closest to our sample), and 0.026 for his full sample (April 1991 to March 2001).

4.1 Nearest-neighbor Matching

Using the cumulative logistic distribution function it is straightforward to extract from the estimated intervention reaction functions the conditional probability of observing an intervention on any given day in our sample.[16] These probabilities constitute the propensity scores necessary for the paired matching of observations. Based on these propensity scores, we employ the nearest-neighbor matching algorithm to evaluate the effect of intervention (computed as a simple weighted average of the differences in the outcomes across the paired matches). We first focus on all intervention days (ALL) and address the general issue of effectiveness, and we pay particular attention to the results from analyzing separately the three sub-samples. We then compare these effectiveness results to the previously discussed preliminary results obtained from testing for significant effects of intervention on the average exchange rate changes across the intervention days without using matching.

4.2 Nearest Neighbor Matching Results: All Intervention Days (ALL)

The result of the matching analysis of all intervention days (ALL) across the full sample is displayed in Table 7. The propensity scores underlying the matching procedure are derived from the model reported in column 1 of Table 3. Without taking into account the possibility of different intervention effects associated with different time periods, this result indicates that intervention has a small impact on the JPY/USD rate once we correct for the selection bias. The 0.0014 estimate suggests that, on average, days of USD-support intervention over 1999-2004 led to a same-day 0.14% appreciation of the USD against the JPY. This finding is significant at the 90% level of confidence. This estimated effect of intervention is similar to the 0.11% appreciation estimated without matching, reported in Table 5.

Table 8 shows the estimated effects of intervention for the three sub-samples. The underlying propensity scores are derived from the models displayed in columns 2, 3 and 4, respectively, of Table 3. The row labeled ALL again displays the results of the effectiveness analysis when all of the intervention days for the sub-sample in question are considered.

Focusing first on the January 1, 1999 to December 31, 2002 sub-sample, Table 8 shows that Japanese intervention (USD purchase against JPY sale) is associated with an exchange rate movement of the correct sign (USD appreciation vis-à-vis the JPY). The point estimate suggests that an intervention day is, on average, associated with a 0.61% same-day increase in the JPY/USD exchange rate. This finding is significant at the 95% level.

Turning to the January 1, 2003 to December 31, 2003 sub-sample, the displayed point estimate of 0.02% is of the correct sign, but insignificant. This suggests that during 2003, intervention had, on average, no effect on the JPY/USD exchange rate.

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16 In accordance with the cumulative logistic distribution function, the conditional probability of observing an intervention operation on day t is given by $P(INT_t = 1) = \frac{e^{X\beta}}{1+e^{X\beta}}$, where X is the vector of intervention determinants and β is the associated coefficient vector. See, for example, Humpage (1999) for an earlier application of the logit model to the study of intervention.

Finally, the effectiveness result based on the January 1, 2004 to March 31, 2004 sub-sample suggests that intervention is associated with a JPY/USD movement of the wrong sign. Specifically, a BoJ intervention (USD purchase) is, on average, associated with a 0.13% decrease in the JPY/USD exchange rate (USD depreciation). This finding is significant at the 90% level and illustrates that intervention may not only be ineffective (as is the case of the 2003 sub-sample), but even counterproductive. We discuss some caveats about this finding in Section 6.

In order to assess the importance of carrying out the matching procedure and the importance of addressing the sample selection issue in the context of an intervention study, we compare the discussed ALL sub-sample results based on matching (displayed in Table 8) to the previously mentioned ALL sub-sample results based on no matching (displayed in Table 6).

For the first sub-sample, both approaches yield very similar results. Although the estimated effect of intervention based on no matching is slightly higher (and associated with significance at the 99% level) a standard t-test rejects that the two point estimates are significantly different from each other. This indicates that official intervention in Japan works as intended during this period. For the second sub-sample, the sign of the point estimate changes when the matching methodology is employed. However, regardless of whether we use matching or not, we find no significant effects of intervention during the 2003 time period. Assessing the effects of intervention without the use of matching in the third sub-sample, however, yields a correctly signed though insignificant point estimate while employing the matching procedure yields an unexpected directional sign and a significant point estimate.[17]

It is important to acknowledge that while our ATE estimations address the fundamental issue of sample selection, our treatment methodology does not solve the inherent endogeneity problem. Endogeneity is a concern in the context of any daily data analysis of the effects of intervention and simultaneity bias potentially leads to an underestimation of the impact of intervention. Accordingly, in terms of quantitative accuracy, our estimates of the ATE of intervention should be interpreted with some caution. However, since our binary treatment framework by construction is not intended to quantify the magnitude of an exchange rate movement associated with a per USD amount of intervention, in our context the endogeneity problem seems less of a concern. Furthermore, it is also important to point out that the possible presence of simultaneity bias in no way invalidates our comparison of the matching results to the no-matching benchmark results. If simultaneity bias leads to an underestimation of the impact of intervention, it does so uniformly whether we use matching or no-matching.

[17] The rows labeled SA and CL in Table 8 display the results of the matching procedure when the effects of intervention across SA (stand-alone or single-day interventions) and CL (clusters of two or more intervention days in succession) are analyzed separately. We also distinguish between the first intervention day in a cluster (CLFD), the intervention day(s) surrounded by other intervention days (CLMD), and the last intervention day in a cluster (CLLD). Overall, we find that the previously discussed significant results found across the 1999 to 2002 period can be ascribed to the impact of single-day intervention operations. The significant results found across the first quarter of 2004 can be ascribed to the impact of the first-day of a string of consecutive intervention operations. From the perspective of the non-governmental exchange rate market participant, on a given intervention day that does not succeed a previous intervention day there is observational equivalence between a single-day intervention and the first intervention day in a cluster. Therefore, we also pool the single-day interventions with the first intervention day of clusters, denote these as general “first days” (FD), and redo the matching exercise in order to assess the effect across these two types of intervention days. Consistent with the previous findings, FD is significant and of the correct sign in the first sub-sample, while it is insignificant in the second sub-sample. Since there are no SA intervention days in the third sample, the effect of FD mirrors, by definition, the effect of the first intervention day of clusters (CLFD).

5. Robustness Tests

5.1 Radius Matching and Periods of Intense Intervention Operations

An alternative matching procedure to nearest neighbor is radius matching. This procedure chooses control sample observations with propensity scores that fall within a given radius of the propensity score associated with a given intervention observation. The control exchange rate change is then calculated as the average of the exchange rate changes within the radius. The radius has two advantages over nearest-neighbor matching. Firstly, it averages over the control sample so the results are not only dependent upon individual matches but rather rely on a control group of matches with similar propensity scores. Secondly, unlike nearest-neighbor matching which takes the closest match regardless of the propensity score distance, radius matching will drop intervention observations from the sample that do not find support for the specified propensity score radius within the set of control observations. Radius matching, therefore, effectively trims the intervention sample for outliers among the intervention (treated) observations.

The selection of the radius distance is somewhat arbitrary, however, and the choice is usually determined judgmentally by the range and distribution of propensity scores over the [0, 1] spectrum. In order to be comprehensive in our robustness tests we choose three alternative values for the radius: 0.01, 0.05 and 0.10.

The results are reported in Table 9. As expected, a number of intervention observations are dropped in calculating the radius measure since in some cases no matches fell within the designated radius. The number of dropped intervention observations is noted in the table. For the first two sub-samples the results are very similar across the different radius measures and also very similar to the previously discussed ALL sample results displayed in Table 8. Radius matching has no substantive effect on the results – highly significant positive effect of intervention of the expected sign in the 1999-2002 sub-sample (with point estimates ranging from 0.59% to 0.77%) and no measurable effect in the 2003 sub-sample. Moreover, the results are not very sensitive to the radius distance.

The third sub-sample of intense intervention operations, however, does not lend itself to radius matching since so few days are available in the control sample, i.e. most days of the sample are associated with intervention. Indeed, nearest-neighbor matching results are also problematic since the nearest-neighbor control observation is the same for many of the intervention observations. In general, any sample with a large number of intervention observations and few control observations will not be well suited to matching methods. Moreover, in our third sample, the problem is more acute since the intervention function is entirely dominated by the lagged intervention term, meaning that the propensity scores are largely segmented into two groups – those in which intervention occurred the day before and those in which it did not.

To address this problem, we extend the third sub-sample to augment January 1, 2004 to March 31, 2004 (first quarter of 2004) with daily observations over April 1, 2004 to June 30, 2004 (second quarter of 2004). The radius results from the extended third sub-sample (first and second quarters) are reported in the second column in the lower part of Table 9. The point estimate is again negative (-0.16%) and very similar to the radius and nearest-neighbor estimate (-0.13%). The results are therefore robust both to the matching algorithm and the extension of the third sub-sample period. Given the severe methodological problems associated with matching in the third sample, however, we do not want to emphasize this coefficient estimate. Rather, we prefer to interpret this as no evidence that intervention was effective during this period.

5.2 Independence of Observations

The basic matching methodology assumes that each observation is independent and drawn from an identical distribution from the underlying population. This assumption implies that the effect of intervention on the exchange rate (treatment on unit t) does not affect exchange rates on another day (another unit's outcome). This is called the *stable unit treatment value assumption* (see Wooldridge, 2002). In our context, it is possible that this assumption is violated if, for example, an intervention operation on day t affects the exchange rate movement on day t+1. If this is the case, then the t+1 observation is contaminated by the treatment (i.e. the intervention) of observation t.

To address this issue, we drop from the sample days of no intervention that immediately follow intervention days. By this adjustment we ensure that the control sample, and potential matches that are calculated in the ATE, do not include observations that are potentially correlated with the previous day's intervention operation. This analysis was only completed for the first and second sub-samples. Dropping no intervention days following intervention days during the third sub-sample of very frequent intervention did not leave enough control observations to make this a meaningful exercise. The results are shown in Table 10, where the row labeled ROB1 (ROB2), or robustness test one (two), denotes the case where we have dropped *one* (*two*) no-intervention day(s) immediately following an intervention day. In addition, the rows labeled TRIMMED denote cases where the sample in question is limited to those intervention observations with propensity scores that fall within the range of propensity scores supported by the control sample of no-intervention observations. The number of excluded observations (dropped either because the no-intervention observation followed an intervention operation or from subsequent trimming) is reported for each estimation. This is a standard method designed to improve comparability of treatment and control sample matches by removing outliers from the sample.

As shown in Table 10, the robustness results pertaining to sub-sample 1 are qualitatively identical to our baseline findings. Without taking into account the potential issue of contaminated control observations, our previously discussed effectiveness result suggests that USD-support intervention is, on average, associated with a 0.61% appreciation of the USD against the JPY, while the robustness analysis suggests an appreciation between 0.50 and 0.53%. Similarly, the sub-sample 2 robustness results show even less variation compared to the baseline findings, i.e. as before there is no significant effect of intervention over the second sub-sample. The remarkable robustness of our results suggests that the potentially contaminated observations that we removed from the control sample did not dramatically affect our analysis, thereby validating the applicability of the matching methodology to the study of intervention.

6. Discussion

We find significantly large and robust effects of intervention in the right direction in the January 1999 to December 2002 sub-sample. This general finding is consistent with several other studies analyzing Japanese intervention over a similar period. For example, Fatum and Hutchison (2006) find significant support for effectiveness over the April 1991 to December 2000 interval, Kearns and Rigobon (2005) find effectiveness over the April 1991 to June 2002 time period, and Ito (2003) finds effectiveness over the June 1995 to March 2001 period. Furthermore, Dominguez (2003) finds evidence of significant effects in the right direction when focusing on the January 1999 to April 2000 and the September 2001 to June 2002 periods. These studies use very different empirical techniques yet arrive at very similar conclusions with respect to effectiveness of official intervention in Japan.

By contrast, we find a complete lack of significant effects of official intervention in the 2003 sub-sample. This no-effect result is in itself not too surprising given that several (especially less recent) intervention studies have suggested that intervention is often futile and has little or no detectable impact on exchange rate markets.[18] It is, however, interesting and perhaps surprising that what seemed to work in 1999-2002, i.e. effective intervention, stopped working in 2003.

Finally, looking at the first quarter of 2004, and the extended sub-sample that includes the first and second quarters of 2004, the effects of official intervention are once again significant, but this time in the wrong direction. Intervention appears to be counterproductive. Nonetheless, this finding of significant yet counterproductive effects of official Japanese intervention is not completely unique, but also found in Ito (2003) when he analyzes effectiveness of intervention over the April 1991 to June 1995 period. In fact, his regression analysis shows that the counterproductive effects of intervention associated with that particular sample period are significant at the 95% level.

It is interesting that intervention is effective during the first sub-sample of infrequent interventions (3% of business days), ineffective during the second sub-sample of more frequent interventions (32% of business days) and possibly counterproductive during the third sub-sample where the interventions occur at an extremely high frequency (73% of business days). Interestingly, this pattern is repeated in Ito (2003), who finds that intervention during the 1991 to 1995 period characterized by frequent interventions (16% of business days) is ineffective while intervention during the 1995 to 2001 period characterized by infrequent interventions (3% of business days) is effective. Although not a testable hypothesis, given that the three sub-samples of our analysis essentially constitute three observations, it seems plausible that the dramatic increase in the official intervention frequencies in Japan constitute an important element towards understanding why intervention in one direction, in one exchange rate, carried out by one central bank over a total time span of little more than 5 years, turns from effective to ineffective and, perhaps, counterproductive.

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[18] See Dominguez and Frankel (1993), Edison (1993), Humpage (2003) and Sarno and Taylor (2001) for comprehensive surveys of the intervention literature and Neely (2005a) for a critical assessment and a very useful overview of recent studies of intervention. See Galati, Melick and Micu (2005) for a recent study that finds little or no effects of Japanese intervention. The latter study focuses on perceived intervention rather than actual intervention and, therefore, their findings are not directly comparable to ours.

The suggested frequency explanation seems consistent with the theoretical work by Vitale (1999). He shows that in order for a central bank to achieve the desired effects of sterilized intervention, it is necessary that the goals of intervention are undisclosed and, therefore, that secrecy of intervention is desirable.[19] With intervention frequencies of 32% of business days in 2003 and 73% of business days over the first quarter of 2004 and, importantly, all interventions carried out in the same direction, it is evident to the exchange rate markets what the goals of the intervention are. In addition, it can be argued that the higher the intervention frequency, the stronger the market awareness, thereby making it virtually impossible for the Japanese government to carry out secret interventions towards the end of our sample period.

This explanation is also consistent with our finding that the significant results of the 1999 to 2002 period stem from single-day intervention operations and, furthermore, that the significant results of the first quarter of 2004 stem from the first rather than the subsequent intervention days, i.e. when interventions are carried out on consecutive days. This result is in line with work by Chaboud and Humpage (2005), Fatum (2002) and Ito (2003), and again points to effectiveness being conditional on the surprise element of intervention. This interpretation is in contrast to Mussa’s (1981) argument that the main channel of intervention is through signaling, and that the authorities must be predictable and transparent in order for it to be effective in moving exchange rates in the desired direction.

We do not wish to emphasize or reach too far for an interpretation of the counterproductive result that we found in the third sub-sample. As discussed above, the matching ATE methodology is not well suited to circumstances where there are few good counterfactuals in the control sample. In our context, this means that the authorities were intervening too frequently in the first quarter of 2004 to allow a balanced set of control observations. This same characteristic of the data also precluded us from checking the robustness of our counterproductive results in our attempt to control for dependency between the treatment and control observations. We also attempted to address this issue by extending the sub-sample through the second quarter of 2004, but even this exercise proved somewhat problematic. Moreover, the selection function (intervention reaction function) is not well identified in the third sub-sample where intervention was related to past intervention, but other factors did not seem to play a role. In sum, although several plausible explanations may explain the estimated counterproductive effects of Japanese intervention during the first quarter of 2004, it may also simply be the case that the matching ATE methodology is not well suited to this sub-sample period.

7. Conclusion

The authorities make a conscious choice to enter the foreign exchange market when they intervene. The self-selection of the timing of an official intervention operation, and the fact that we don’t observe what would have occurred in its absence, is a key methodological challenge in estimating the effect on the exchange rate. Estimating an appropriate counterfactual under these circumstances in order to

[19] It should be noted, however, that earlier theoretical work by Bhattacharya and Weller (1997) suggests that the central bank should keep its interventions secret only when the intervention objective differs from the fundamental or “true” value of the exchange rate.

properly evaluate the effects of intervention on exchange rate movements is a central methodological problem. We address the issue of self-selection and the missing counterfactual by estimating the average treatment effect (ATE) of intervention on the exchange rate. We use a propensity score matching methodology to do so.

In our analysis, the exchange rate movement is the outcome variable and intervention is the treatment. Our propensity score matching compares pairs of observations of exchange rate movements – each pair consisting of an exchange rate movement coinciding with intervention and one that coincides with no interventions – that are similar in observable characteristics (and associated with similar probabilities of intervention. To derive the propensity scores we introduce an intervention function that includes Reuters wire reports capturing the differences between market expectations and announcements of macroeconomic developments that can influence the decision to intervene. The ATE is the average difference in terms of exchange rate movements across these matched pairs (or, for radius matching, an average of several control observations that are matched with the intervention observation). The focus of our matching analysis is to examine the effects of daily official intervention in Japan for the JPY/USD exchange rate market over the January 1999 to March 2004 time period.

As a benchmark for comparison, we test for significant effects of intervention on the average exchange rate changes across all the intervention days without invoking the matching technique. We do so across the full sample as well as separately across three sub-samples. Consistent with the existing literature, the sub-samples are identified according to highly noticeable changes in the intervention frequency. The results imply that the effects of Japanese intervention vary sharply across the three sub-samples. This exercise, and regime shifts estimated in the reaction function, strongly suggests that the matching analysis should focus on the sub-samples separately rather than on the full sample.

The results of the matching analysis show that the effects of Japanese intervention vary dramatically across the three sub-samples under study. For the January 1999 to December 2002 time period, the effects are significant and in the right direction. By contrast, the results are insignificant in the January to December 2003 sub-sample, and are significant but in the wrong direction during the first quarter of 2004. All our results are robust to various methodological changes.

The apparently inverse relationship between intervention frequency and effectiveness over the three sub-samples is interesting. Intervention is effective during the first sub-sample of infrequent interventions, ineffective during the second sub-sample of more frequent interventions and, finally, possibly counterproductive during the third sub-sample where the interventions occur at an extremely high frequency.

We have some methodological reservations about the ATE matching results from the 2004 sub-sample when intervention operations were intense and very frequent. Therefore, we interpret the results with caution and conclude as follows: Our results strongly support effectiveness of official Japanese intervention during an extended period of relatively infrequent operations (1999-2002), while no evidence to support effectiveness is found during periods of frequent and large-scale intervention operations (2003-04).

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Table 1. Official Japanese Intervention, 1 January 1999 - 31 March 2004

Full sample: 1 January 1999 – 31 March 2004

Purchases of USD (million USD)	Number of Days	Cumulated Amount
> 1000	113	443,796
> 500	21	16,613
> 250	5	1,694
> 0	20	2,148
Total	159	464,251

Sample 1: 1 January 1999 - 31 December 2002

Purchases of USD (million USD)	Number of Days	Cumulated Amount
> 1000	28	147,629
> 500	2	1,799
> 250	0	1,694
> 0	0	2,148
Total	30	149,428

Sample 2: 1 January 2003 - 31 December 2003

Purchases of USD (million USD)	Number of Days	Cumulated Amount
> 1000	52	165,101
> 500	11	8,864
> 250	4	1,465
> 0	15	1,671
Total	82	177,101

Sample 3: 1 January 2004 - 31 March 2004

Purchases of USD (million USD)	Number of Days	Cumulated Amount
> 1000	33	131,066
> 500	8	5,950
> 250	1	229
> 0	5	477
Total	47	137,722

NOTES:

(a) Daily Bank of Japan intervention data obtained from the Japanese Ministry of Finance data bank.

(b) Daily intervention operations of USD 1000 million or greater: >1000; daily intervention operations of USD 500 million or greater, but less than USD 1000 million: >500; daily intervention operations of USD 250 million or greater, but less than USD 500 million: >250; daily intervention operations of less than USD 250 million: > 0.

Table 2. Summary Statistics on Exchange Rate Changes and Intervention

	Full Sample: January 1999 - March 2004	Sample 1: January 1999 - December 2002	Sample 2: January 2003 - December 2003	Sample 3: January 2004 - March 2004
Change in JPY/USD (pct.)	-6.8574	6.1600	-10.6271	-2.6628
Average absolute daily pct. change in JPY/USD	0.4832	0.5124	0.3888	0.3926
Standard deviation of daily pct. change in JPY/USD	0.4317	0.4539	0.3244	0.3772
Intervention on days of SMALL JPY/USD changes	74 (47%) [37%]	6 (20%) [35%]	45 (55%) [46%]	23 (49%) [48%]
Intervention on days of MEDIUM JPY/USD changes	52 (33%) [39%]	10 (33%) [40%]	25 (30%) [37%]	17 (36%) [36%]
Intervention on days of LARGE JPY/USD changes	33 (21%) [23%]	14 (47%) [26%]	12 (15%) [17%]	7 (15%) [16%]
REPORTED intervention	100	27	49	24
Total intervention	159	30	82	47

NOTES:

(a) SMALL JPY/USD changes are defined as daily changes (in pct.) that are smaller than the sum of the average of the daily JPY/USD exchange rate (in pct.) and (-1/2) the standard deviation of the daily JPY/USD exchange rate (in pct.).

(b) MEDIUM JPY/USD changes are defined as daily changes (in pct.) that fall in between the range of the sum of the average of the daily JPY/USD exchange rate (in pct.) +/- 1/2 the standard deviation of the daily JPY/USD exchange rate (in pct.).

(c) LARGE JPY/USD changes are defined as daily changes (in pct.) that are larger than the average of the daily JPY/USD exchange rate (in pct.) plus (1/2) the standard deviation of the daily JPY/USD exchange rate (in pct.).

(d) REPORTED intervention includes days of Reuters news reports of intervention coinciding with actual intervention.

(e) Parentheses ( ) report intervention days as a percent of total intervention days, and brackets [ ] report number of days as a percent of total days in the sample, that were associated with days of SMALL, MEDIUM or LARGE exchange rate changes.

Table 3. Logit Model Estimates of Japanese Intervention Function

Dependent Variable: IINT

	Full Sample: January 1999 - March 2004	Sample 1: January 1999 - December 2002	Sample 2: January 2003 - December 2003	Sample 3: January 2004 - March 2004
Constant	-3.60*** (0.18)	-4.69*** (0.31)	-2.05*** (0.24)	-1.10* (0.58)
JPYUSD(-1)	-95.30*** (16.99)	-116.75*** (27.59)	-57.24* (31.69)	n.a.
MADAY(-1)	n.a.	-32.92*** (5.05)	-18.60** (9.10)	n.a.
MAYEAR(-1)	-4.60*** (1.11)	n.a.	n.a.	n.a.
IINT(-1)	4.05*** (0.26)	2.84*** (0.71)	2.60*** (0.34)	3.23*** (0.75)
GDP-UNEXP>0	1.50** (0.63)	n.a.	n.a.	40.18*** (1.11)
CPI-UNEXP>0	1.70** (0.80)	2.02*** (0.77)	n.a.	n.a.
Total Obs	1364	1040	260	64
Obs with IINT = 1	159	30	82	47
McFadden R^2	0.41	0.27	0.28	0.33
LR statistic	406.14	73.45	90.00	24.24
P(LR)	0.00***	0.00***	0.00***	0.00***
H-L Statistic	11.00	3.54	9.05	8.31
Total Gain	77.04	79.02	56.82	59.38
Percent Gain	43.18	15.38	43.18	40.18

NOTES:

(a) * denotes significance at 90%; ** denotes significance at 95%; *** denotes significance at 99%.

(b) Heteroskedasticity and autocorrelation consistent standard errors in parentheses below the point estimates.

(c) Logit models are defined in Equation (1) in the text.

(d) The dependent variable IINT is a (0, 1) indicator variable that takes on the value 1 on intervention days and 0 otherwise.

(e) The independent variables are defined as follows: JPYUSD is the first-difference of the log of the JPY/USD exchange rate; TARGET is the first-difference of the log of the JPY/USD deviation from a target rate of 125 JPY/USD; MADAY is the 21-day moving average of the log of the JPY/USD exchange rate; MAYEAR is the one-year moving average of the log of the JPY/USD exchange rate; (-1) denotes the first lag of a variable. The control variables measure the surprise element of Japanese macroeconomic announcements concerning GDP (GDP-UNEXP), CPI (CPI-UNEXP), Unemployment (UNEM-UNEXP), and Balance of Trade (TB-UNEXP). Positive (>) and negative (<) surprises are controlled for separately.

(f) n.a. indicates that an independent variable is omitted from the second model due to lack of significance.

(g) The Likelihood Ratio (LR) statistic tests the overall significance of the estimated model against a constant only alternative. P(LR) shows the p-value of the LR test statistic.

(h) Hosmer-Lemeshow test statistic for goodness-of-fit based on a $\chi^2(8)$-distribution. The 95 (90) percent critical value for rejecting the null of a fitting model is 15.51 (13.36).

(i) The estimated logit models' prediction evaluation is based on expected value calculations: Total Gain captures the percentage point gain/loss of correct predictions when compared to the naïve constant probability model; Percent Gain shows the percent of incorrect predictions according to the naïve model corrected by the estimated model.

Table 4. Wald Tests of Reaction Function Parameter Stability

	Sub-sample 1	Sub-sample 2	F-Statistic
Constant	-4.85	-2.05	71.04***
JPYUSD(-1)	-116.85	-57.24	4.65**
MADAY	-35.66	-18.60	8.04***
IINT(-1)	2.84	2.60	0.11
GDP-UNEXP>0	n.a.	n.a.	n.a.
CPI-UNEXP>0	2.02	n.a.	n.a.

	Sub-sample 1	Sub-sample 3	F-Statistic
Constant	-4.85	-1.10	131.33***
JPYUSD(-1)	-116.85	n.a.	n.a.
MADAY	-35.66	n.a.	n.a.
IINT(-1)	2.84	3.23	0.30
GDP-UNEXP>0	n.a.	40.18	n.a.
CPI-UNEXP>0	2.02	n.a.	n.a.

	Sub-sample 2	Sub-sample 3	F-Statistic
Constant	-2.05	-1.10	15.85***
JPYUSD(-1)	-57.24	n.a.	n.a.
MADAY	-18.60	n.a.	n.a.
IINT(-1)	2.60	3.23	5.76**
GDP-UNEXP>0	n.a.	40.18	n.a.
CPI-UNEXP>0	n.a.	n.a.	n.a.

NOTES:

(a) * denotes significance at 90%; ** denotes significance at 95%; *** denotes significance at 99%.

(b) The null hypothesis of the Wald test is that a coefficient estimate is the same across two sub-samples (parameter stability); the alternative hypothesis is that they are different (parameter instability).

(c) The dependent variables are defined in the notes to Table 2. The displayed coefficient estimates are associated with the models described in Table 2 (columns 2, 3 and 4). Sub-sample 1 covers January 1, 1999 to December 31, 2002; sub-sample 2 covers January 1, 2003 to December 31, 2003; and sub-sample 3 covers January 1, 2004 to March 31, 2004.

Table 5. Without Matching: Effectiveness of Bank of Japan Intervention

	Full Sample: 1 January 1999 - 31 March 2004
ALL	0.00107** (0.00524)
Intervention Days	159

NOTES:

(a) The table shows the mean of the JPY/USD exchange rate changes on intervention days; significance of whether the mean is different from zero is assessed using a standard t-test.

(b) * denotes significance at 90%; ** denotes significance at 95%; *** denotes significance at 99%.

(c) Standard errors in parentheses below the point estimate.

(d) ALL includes all intervention days; IINT is a (0, 1) indicator variable that takes on the value 1 on days when the Bank of Japan intervenes and 0 otherwise.

Table 6. Without Matching: Effectiveness of Bank of Japan Intervention

	Sample 1: January 1999 - December 2002	Sample 2: January 2003 - December 2003	Sample 3: January 2004 - March 2004
ALL	0.0065*** (0.0021)	-0.0003 (0.0005)	0.0001 (0.0008)
Intervention Days	30	82	47

NOTES:

(a) The table shows the mean of the JPY/USD exchange rate changes on intervention days; significance of whether the mean is different from zero is assessed using a standard t-test.

(b) * denotes significance at 90%; ** denotes significance at 95%; *** denotes significance at 99%.

(c) Standard errors in parentheses below the point estimates.

(d) ALL includes all intervention days; IINT is a (0, 1) indicator variable that takes on the value 1 on days when the Bank of Japan intervenes and 0 otherwise.

Table 7. Nearest Neighbor Matching: Effectiveness of Intervention

	Full Sample: 1 January 1999 - 31 March 2004
ALL	0.0014* (0.0008)
Obs with IINT = 1	159

NOTES:

(a) Matching based on the nearest neighbor algorithm.

(b) * denotes significance at 90%; ** denotes significance at 95%; *** denotes significance at 99%.

(c) Standard errors in parentheses below the point estimate.

(d) ALL includes all intervention days; IINTDUM is a (0, 1) indicator variable that takes on the value 1 on days when the Bank of Japan intervenes and 0 otherwise.

Table 8. Nearest Neighbor Matching: Effectiveness of Intervention

	Sample 1: January 1999 - December 2002	Sample 2: January 2003 - December 2003	Sample 3: January 2004 - March 2004
ALL	0.0061**	0.0002	-0.0013*
	(0.0021)	(0.0008)	(0.0008)
SA	0.0080**	0.0009	n.a.
	(0.0032)	(0.0028)	
CL	0.0027	0.0001	-0.0013*
	(0.0021)	(0.0008)	(0.0008)
CLFD	0.0043	-0.0000	-0.0031**
	(0.0035)	(0.0019)	(0.0015)
CLMD	n.a.	-0.0002	-0.0010
		(0.0010)	(0.0009)
CLLD	-0.0003	0.0010	-0.0022
	(0.0025)	(0.0015)	(0.0036)
FD	0.0072***	0.0003	-0.0031**
	(0.0026)	(0.0029)	(0.0015)
Obs with IINT = 1	30	82	47

NOTES:

(a) Matching based on the nearest neighbor algorithm.

(b) * denotes significance at 90%; ** denotes significance at 95%; *** denotes significance at 99%.

(c) Standard errors in parentheses below the point estimates.

(d) ALL includes all intervention days; SA includes only “stand alone” intervention days, i.e. intervention days not immediately preceded or succeeded by another intervention day; CL includes all intervention days belonging to clusters, i.e. intervention days immediately preceded or/and succeeded by an intervention day (ALL minus SA); CLFD includes CL intervention days that are not immediately preceded by an intervention day; CLMD includes only CL intervention days that are both immediately preceded and succeeded by other intervention days; CLLD includes only CL intervention days that are not immediately succeeded by another intervention day; FD includes only SA and CLFD intervention days; IINT is a (0, 1) indicator variable that takes on the value 1 on days when the Bank of Japan intervenes and 0 otherwise.

(e) n.a. indicates that the sample does not include the given type of intervention days.

Table 9. Radius Matching: Effectiveness of Intervention

	Sub-sample 1: January 1999 - December 2002	**Sub-sample 2: January 2003 - December 2003**
R=0.01	0.0059*** (0.0022) Dropped Int obs: 6	0.0001 (0.0003) Dropped Int obs: 11
R=0.05	0.0069*** (0.0022) Dropped Int obs: 2	0.0001 (0.0004) Dropped Int obs: 1
R=0.10	0.0077*** (0.0023) Dropped Int obs: 1	0.0003 (0.0017) Dropped Int obs: 1
Obs with IINT = 1	30	82
	Sub-sample 3: January 2004 - March 2004	**Sub-sample 4: January 2004 - June 2004**
R=0.01 0.05 0.10	-0.0013* (0.0008) No-Int obs: 17	-0.0016** (0.0008) No-Int obs: 82
Obs with IINT = 1	47	47

NOTES:

(a) Matching based on the radius algorithm; R denotes the radius.

(b) * denotes significance at 90%; ** denotes significance at 95%; *** denotes significance at 99%.

(c) Standard errors in parentheses below the point estimates.

Table 10. Nearest Neighbor Matching: Robustness

	Sub-sample 1: January 1999 - December 2002	**Sub-sample 2: January 2003 - December 2003**
ALL	0.0061*** (0.0021) No-Int obs: 1009	0.0002 (0.0008) No-Int obs: 178
ROB1	0.0050** (0.0018) Dropped No-Int obs: 24	0.0002 (0.0007) Dropped No-Int obs: 24
ROB1 TRIMMED	0.0053** (0.0019) Dropped Int obs: 1	0.0003 (0.0017) Dropped Int obs: 57
ROB2	0.0050** (0.0018) Dropped No-Int obs: 40	0.0002 (0.0007) Dropped No-Int obs: 41
ROB2 TRIMMED	0.0052** (0.0019) Dropped Int obs: 1	0.0002 (0.0017) Dropped Int obs: 57
Obs with IINT = 1	30	82

NOTES:

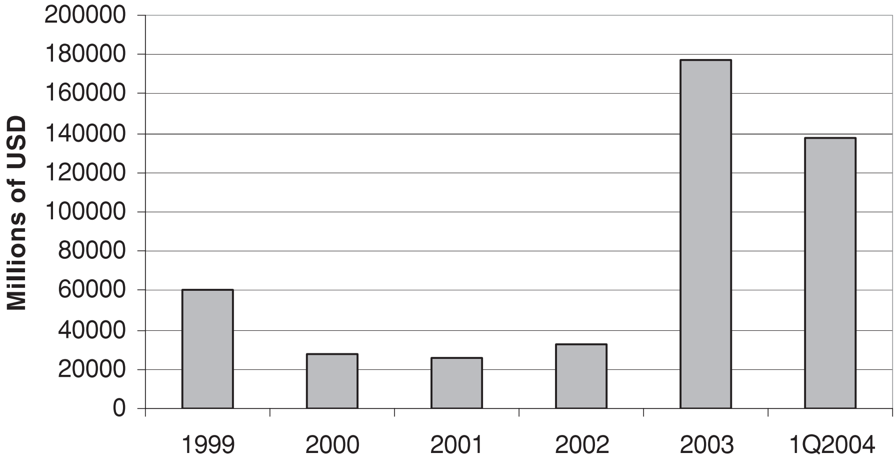
(a) Matching based on the nearest neighbor algorithm.

(b) * denotes significance at 90%; ** denotes significance at 95%; *** denotes significance at 99%.

(c) Standard errors in parentheses below the point estimates.

(d) ALL includes all intervention days. ROB1 denotes the sample with the no-intervention day immediately following an intervention day dropped. ROB2 denotes the sample with the two no-intervention days immediately following an intervention day dropped. TRIMMED denotes the sample that includes only intervention observations with propensity scores that fall within the range of the propensity scores of the no-intervention observations.

Figure 1. Official Japanese Intervention 1999 to 2004



Notes:

a) Yearly aggregates of daily intervention in the JPY/USD exchange rate market. The daily intervention data obtained from the Japanese Ministry of Finance data bank.

b) There has been no Japanese intervention since March 2004.