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Taro Esaka[†] and Shinji Takagi[††]

ABSTRACT

The paper tests the effectiveness of marginal reserve requirements employed by the Japanese authorities in the 1970s to influence short-term capital flows, thereby contributing to the ongoing debate on the use of capital controls—market- or price-based ones in particular. While the case for using market-based controls almost entirely relies on the mixed evidence from the experience of Chile with unremunerated reserve requirements in the 1990s, testing for their effectiveness on the volume of inflows is hampered by the endogeneity of such a measure, which is typically imposed or intensified when inflows surge. We address this problem by applying the method of propensity score matching and find that an increase in marginal reserve requirements modestly reduced the volume of short-term capital inflows through non-resident free-yen accounts. The impact was not statistically significant, however, implying that the price elasticity of short-term capital flows was small. We conclude that market-based controls must be nearly prohibitive, perhaps combined with administrative measures, to be effective in a meaningful way.

Keywords: market-based capital controls; price-based capital controls; effectiveness of capital controls; Japanese capital controls; propensity score matching

JEL classification codes: F32, N25

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I. INTRODUCTION

This paper applies the method of propensity score matching to test the effectiveness of marginal reserve requirements employed by the Japanese authorities in the 1970s to influence short-term capital flows, thereby contributing to the ongoing debate on the use of capital controls—market- or price-based ones in particular. Use of capital controls, once condemned by the mainstream thinking of the economics profession, has assumed greater respectability in recent years (IEO 2004). Following the global financial crisis of 2007-08, moreover, it has become increasingly accepted as a legitimate tool of protecting domestic financial markets from the vicissitude of cross-border capital flows (Ostry et al. 2011); the Group of Twenty (G20) finance ministers and central bank governors in October 2011, calling capital controls "residency-based capital flow management measures (CFMs)," stated that these and other CFMs "may constitute part of a broader approach to protect economies from shocks" and "can complement and be employed alongside, rather than substitute for, appropriate monetary, exchange rate, foreign reserve management and prudential policies."[1]

There is a long-standing debate on the effectiveness of capital controls especially as countries liberalize international payments (Dooley 1996; Edwards 1999; Magud and Reinhart 2006; Habermeier et al. 2011). An open current account allows disguised capital flows through leads and lags in current payments and receipts as well as through under- and over-invoicing. As the capital account is progressively liberalized, additional room is created for substitution between different types of capital flows, so that a measure to control one type of flows is to some extent offset by an increase in other types. As long as a country is committed to the principle of

[1] "G20 Coherent Conclusions for the Management of Capital Flows Drawing on Country Experiences," as endorsed by G20 Finance Ministers and Central Bank Governors, dated 15 October 2011.

free trade, there is a natural limit to how much it can feasibly control external payments and receipts, making it necessary to exempt certain types of capital transactions, such as trade credits or foreign direct investment (FDI). Inevitably, these would create a channel through which otherwise controlled capital inflows could take place. The longer controls are in place, the more creative economic agents will become in finding ways to circumvent them.

The philosophical question of whether capital controls should or could be used under certain circumstances is different from the practical issue of whether they are effective and, if so, whether the benefits outweigh the costs. Some, arguing against the use of capital controls, have based their case on the idea that administrative controls must be progressively tightened to remain effective over time and, in view of the potential for substitution, will become increasingly distortionary (e.g, Mathieson and Rojas-Suarez 1993). It is in this context that so-called market- or price-based controls have become the favorite device of emerging market economies. Such controls, instead of resorting to administrative interventions, attempt to influence capital flows through the incentives of market participants and are thus considered less distortionary. In 1991, Chile became the first country in recent history to introduce such a mechanism, called unremunerated reserve requirements (URR), which required a percentage of relevant inflows to be deposited in a non-interest-bearing account at the central bank for a certain period of time. The countries that have followed Chile in introducing URRs or other similar devices in recent years include, among others, Colombia (1993, 2007), Slovenia (1995), Croatia (2004), Russia (2004), Thailand (2006) and, in the aftermath of the global crisis, Brazil (2009) and Korea (2011).

The generally favorable view of market-based controls within the policymaking community had been evident even before the use of capital controls became widely accepted. This came about from around 1996 when a substantial empirical literature began to emerge on

the effectiveness of the Chilean URR. Although a review of the literature by Nadal-De Simone and Sorsa (1999) cautioned against prematurely accepting the Chilean experience as supportive of the effectiveness of such controls, most had interpreted the body of empirical evidence to believe that the URR had lengthened the maturity of capital inflows (if not reduced the total volume). Subsequent studies tended to be more supportive of the volume effect as well (Gallego et al. 2002; Le Fort and Lehman 2003). Fischer (2001), speaking on behalf of the International Monetary Fund (IMF), expressed support for "the use of market-based capital inflow controls: for example, those that impose a tax on capital inflows."

The case for market-based controls, however, rests almost entirely on a particular interpretation of the experience of the Chilean URR, the effectiveness of which "has often been exaggerated" (Edwards 1999). The available evidence on other countries is far from favorable. For example, Concha et al. (2011) suggest that the Colombia URR had no impact on either the volume or the composition of inflows for 1998-2008, a period that covers both the URR of 1993 and that of 2007 (though Cardenas and Barrera (1997) is more favorable to the compositional impact). Likewise, Clements and Kamil (2009), covering only the URR of 2007, show that the Colombian measure, while reducing external borrowing, had no impact on the much more significant, overall volume of non-FDI inflows. Coelho and Gallagher (2010) and Jittrapanum and Prasartset (2009) find evidence of a negative impact of the Thai URR on inflows, but the impact was either economically small or statistically not significant; moreover, the reduction in net inflows may have been achieved, not by limiting inflows, but through capital flight.[2] Brazil's

[2] Apparently, international investors punished Thailand for introducing the URR. On 18 December 2006, Thailand imposed a 30 percent URR on all portfolio inflows with maturities of less than one year but, faced with a sharp decline in equity prices, was compelled to lift the measure on the following day for equity flows.

foreign exchange tax had little effect on the volume or composition of inflows (Hebermeier et al. 2011).

Aside from the potential endogeneity of capital controls (i.e., controls are strengthened when inflows surge), the ambiguity of the empirical literature on the effectiveness of market-based controls is hardly surprising. After all the effectiveness of any measure depends, among other things, on the administrative capacity of the country concerned, the willingness of market participants to comply with the requirements, and the scope for substitution and circumvention, as determined by the coverage of the measure as well as country-specific conditions (Hebermeier et al. 2011). These conditions, for example, include the restrictiveness of the capital account regime, the sophistication of financial markets, including the extent to which derivative instruments are available, transactions costs involving a structure of fees and legal procedures, and the strength of incentives provided by interest rate differentials favoring domestic instruments and the prospect of economic growth over the medium term. In order to understand what measure works under what circumstances, and how it works when it does, we must sort out these factors and isolate the impact of each. This is where the experience of Japan in the 1970s becomes helpful.

Japan's use of a market-based control in the 1970s predates the Chilean URR by two decades or more. At that time, Japan had a highly closed capital account, with all capital transactions in principle subject to approval, though the process of capital account liberalization meant that the presumption of approval was applied to an increasing number of transactions. The exception was short-term capital flows through non-resident free-yen accounts (FYAs) held at a small number of commercial banks that the Ministry of Finance had authorized to do foreign exchange business. Foreign investors were freely allowed to move funds into and out of Japan

through FYAs without any formality. Free-yen deposits (FYDs) provided foreign investors with a vehicle of investing in yen assets (though purchasing domestic securities with the funds would have required approval), while Japanese banks used them as an additional source of funding. Only when the pace of capital inflows surged did authorities introduce or raise reserve requirements on FYDs. The measure was rescinded when capital inflows abated.

This experience of Japan provides a kind of controlled experiment in which to isolate the effectiveness of a market-based capital control by removing the influences of factors other than changes in the price incentive itself. Japan's closed capital account, administrative capacity in the environment of a strong regulatory culture, and high degree of legal compliance (especially given the fact that the control applied to a small number of tightly regulated banks as intermediaries) meant that there was little room for substitution among different types of capital transactions. For foreign investors, FYDs were the only vehicle of freely investing in the Japanese financial market. If the authorities imposed or raised reserve requirements, banks became less willing to accept new deposits. But foreign investors, who were thus cut out of the deposit market, had no other means of entering the Japanese market. We believe that, by analyzing how changes in the balance of FYDs responded to changes in the reserve requirements, we gain insight into how market-based controls do or do not work in practice and the conditions required to make them work.

The rest of the paper is organized as follows. Section II reviews the origin and other specifics of non-resident free-yen accounts and explains the institutional arrangement under which short-term capital flows in general were regulated in Japan prior to the de jure opening of Japan's capital account in 1980. Section III presents a preliminary analysis of the impact of capital control measures on FYD flows, by identifying capita control episodes and using an event

study methodology. In testing how the behavior of FYD flows differed between the capital control and the non-capital control episodes, the section uses as the counterfactual the sample average rate of increase in FYDs and the rate of increase obtained from a simple prediction model. Section IV estimates the average treatment effect of what we identify as intensive capital controls by the method of propensity score matching. In particular, we estimate a probit model of the probability of intensive controls and, in identifying the counterfactual for each capital control episode, use the probit score to find the closest match. Finally, section V presents concluding remarks.

II. Non-resident Free-yen Accounts as a Channel of Short-term Capital Flows

The system of non-resident free-yen accounts was established on 1 July 1960 in order to provide non-residents with a means to keep the proceeds from current exports to Japan in yen. Along with the authorization to use the yen as a currency of trade settlement, this marked the modest beginning of the yen as an international currency. The non-resident holders of FYAs at authorized foreign exchange banks were freely allowed to take the deposits out of the country at any time. Thus, this also represented the establishment of external convertibility for the yen. These decisions came about against the background of foreign pressure to liberalize Japan's trade and payment system as the Japanese economy had recovered from the devastation of World War II and had recorded a double-digit growth year after year. Pressure intensified after major West European countries had restored external convertibility for their currencies at the end of 1958. The establishment of free-yen accounts and the authorization for the yen to be used for trade settlement purposes were the very first measures implemented under the Trade and Payments Liberalization Plan announced by the Japanese government in June 1960 (JMOF 1999).

The establishment of FYAs had far-reaching implications. Because accounts could be opened not only with yen proceeds from current exports to Japan but also with the sale of foreign exchange to onshore banks, this meant that non-residents could freely bring short-term capital into Japan through the banking system. In this sense, Japan's capital account liberalization followed a non-standard sequence, by liberalizing short-term capital flows before FDI and long-term portfolio flows;[3] the liberalization of short-term capital flows even predated the full opening of the current account in 1964 (Takagi 1997). Free-yen deposit rates were subject to maximum ceilings stipulated by law, which could be higher or lower than the prevailing world interest rates.[4] Overseas branches of Japanese banks also participated in this market, by transferring Eurodollar deposits, converting them into yen, and depositing the proceeds with the head offices.

FYDs were placed under the overall framework of regulatory supervision over short-term capital flows during the 1960s. First, from 1962 to 1972, the authorities applied liquidity requirements to short-term external liabilities, including non-resident free-yen deposits. Under this system banks were required to keep a certain percentage of short-term external liabilities (such as foreign currency deposits, FYDs, and unsecured borrowing from foreign banks) in liquid foreign assets, such as cash, deposits, call loans, and foreign government securities, and bankers' acceptances. Initially, the liquidity requirements were set at 10 percent, which were raised to 25 percent in 1964 before being lowered to 15 percent in 1966; from 1963 to 1965, marginal liquidity requirements of 35 percent were additionally imposed (on an increase in

[3] Ishii et al. (2002) discuss what has been widely accepted as the conventional wisdom on the pace and sequencing of capital account liberalization. Japan's liberalization of FDI proceeded in several stages from 1969 to 1978, while the liberalization of portfolio investment was not completed until the mid-1980s, at least on the outflow side.

[4] For example, the ceilings were 5.84 percent for time deposits with maturities of more than three months in the case of non-resident financial institutions; in the case of non-financial firms and individuals, 4.3 percent for three months, 5.5 percent for six months, and 6 percent for a year.

liabilities) in order to discourage inflows. The primary purpose of the liquidity requirements, however, was prudential. According to Inuta (2000), they were for the most part not binding on the behavior of foreign exchange banks, especially after 1966 (when the rate was reduced to 15 percent), because most banks for internal risk management purposes kept a sufficiently high share of their external liabilities in liquid foreign currency assets in any case.

Second, from 1964 to 1972, the Japanese authorities applied outright quantitative controls on the amount of new short-term capital inflows, including through FYAs. This required, for each bank, the ratio of external liabilities to external assets to remain within the ratio actually achieved during a particular period in the past. Basically, what this intended was to allow an increase in external liabilities, only to the extent that the banks were willing to increase external lending, thereby limiting the net inflow of short-term funds. Third, in 1968, the authorities introduced a limit on the amount of so-called "yen conversion," defined as the amount obtained by subtracting the balance of relevant foreign assets from the balance of relevant external liabilities.[5] The amount of yen conversion essentially refers to the amount of foreign borrowing that could be lent out to domestic borrowers. Under this regulation, which was to be terminated under the Japan-US Yen-Dollar agreement in 1984 (Frankel 1984), the authorities determined the yen conversion limit for banks on a monthly average basis (initially 103 percent of the

[5] Consider the following simplified representation of a commercial bank balance sheet:

Assets	Liabilities
Foreign currency assets (A)	Foreign currency liabilities (B) Non-resident free-yen deposits (C)

The amount of yen conversion is equivalent to B + C – A. In December 1973, free-yen deposits were excluded from the liabilities side except for the claims to overseas branches of Japanese banks.

average balance during December 1967 and January 1968). The yen conversion limit was tightened over time as speculation built up on the sustainability of the Bretton Woods parities.[6]

Our assessment of market-based capital controls in Japan covers the period 1971-79, and is based not on the foreign currency liquidity requirements on all short-term inflows (which changed only three times during 1963-1966) but on the average and marginal reserve requirements on FYDs, which replaced the liquidity requirements in June 1972. Not only do the newly introduced reserve requirements provide a richer set of data for our purpose (as they were changed no less than 9 times in a variety of circumstances over a 7-year period), but the period 1971-79 is one in which a transparent legal framework replaced the opaque "window guidance" of the 1960s.[7] This means that any regulatory change in the 1970s was a legal measure binding on foreign exchange banks, and that any response of banks was a result of their own economic decision, and not of arm-twisting by the authorities. In 1979 the marginal reserve requirements were reduced to zero, with the system of non-resident free-yen deposits itself abolished altogether in December 1980, when the new Foreign Exchange and Trade Control Law came into effect making the distinction between residents and non-residents all but meaningless.

Except at the very beginning, non-resident free-yen deposits during most of our sample period were subject to two sets of regulations: (i) yen conversion limits and (ii) statutory reserve requirements. The application of these regulations changed in response to the tightening or easing of pressure on the exchange rate as well as the size and direction of net capital inflows. For example, following the announcement on 15 August 1971 by the United States government to suspend the gold convertibility for the dollar, the authorities "requested" the foreign exchange

[6] For example, the yen conversion limit for domestic banks was set equal to zero after February 1970.
[7] Whereas the authorities had administered the regulation of banks with respect to short-term capital flows through administrative fiat, in the immediate aftermath of the currency crisis of August 1971, they placed it within the framework of a ministerial ordinance in September.

banks to improve their compliance with the yen conversion limits; following the temporary floating of the yen, in early September 1971, the authorities further tightened the conversion limits by changing the application from monthly average balance to daily balance basis. The conversion limits were then eased as the yen began to face depreciation pressure with the quadrupling of oil prices in November 1973. In December 1973, free-yen deposits were removed from the application of conversion limits, except for those held by the overseas branches of domestic banks.

The adjustment of reserve requirements followed a similar pattern during the capital flow cycle of 1971-73 (except that the system itself was not introduced until June 1972); from 1974 on, reserve requirements became the only instrument, which the authorities used to control short-term capital flows through FYDs (Figure 1). When reserve requirements were introduced, they were made to apply only to an increase over the 21 April-20 May 1972 average balance, at the rate of 25 percent, which was then raised to 50 percent a month later.[8] With the oil crisis, the requirements were reduced to 10 percent in December 1973 and further to zero in September 1974. Average reserve requirements (on outstanding balances) were introduced, at the very low rate of 0.25 percent, only in June 1977 when the yen began to experience appreciating pressure again. This was followed by the reinstatement of marginal reserve requirements, at the rate of 50 percent in November 1977 and the requirements became prohibitive in March 1978. The requirements were then reduced to 50 percent in January 1979 and further to zero a month later.

It should be noted that these regulatory actions with respect to FYDs took place within a broader policy context. For example, as appreciation pressure on the yen mounted in the early summer of 1971, the government eased restrictions on the holding of foreign currency assets by

[8] This decision, made on 1 July, followed the reopening of the Tokyo foreign exchange market which had been closed for four days after the floating of the British pound.

residents. Following the suspension of gold convertibility for the dollar, it introduced far-reaching restrictions on foreign exchange transactions, including the requirement that authorized foreign exchange banks limit the growth in foreign liabilities beyond the August 18 level. As the yen began to float permanently on 14 February 1973, the yen was under selling pressure, which intensified with the October oil crisis. The authorities intervened in the foreign exchange market by selling dollars ($2.44 billion in November; $1.45 billion in December). At the same time, they took measures, from November 1973 to February 1974, to promote capital inflows and discourage capital outflows, including the abolishment of all restrictions on purchases by non-residents of domestic securities. From June to August 1974, they further tightened exchange and capital controls, including the raising of the yen conversion limit for resident foreign banks and the liberalization of purchases by non-residents of short-term government securities.

III. THE IMPACT OF CONTROL MEASURES ON FYD FLOWS: AN EVENT STUDY

We have grouped these and other control measures taken with respect to free-yen deposits over 1971-79 into nine "episodes," which are summarized with dates and additional details, where relevant, in Table 1. It was necessary to cluster two or more measures into a single event because they were taken very close to each other, making it impossible to isolate the impact of one from that of the other(s). The first two episodes concerned (the tightening or easing of) the application of yen-conversion limits and a quantitative restriction. The next two involved a market-based measure (a change in average or marginal reserve requirements) combined with a change in the application of yen conversion limits, while the remaining five episodes entailed only a market-based measure. Strictly speaking, the concurrent use by the Japanese authorities of non-market-based controls may to some extent diminishes the integrity of our tests to measure the impact of changes in reserve requirements on FYD flows, but we believe

that the problem is not so serious especially if we use monthly data as we do. Except for the imposition or elimination of a quantitative ceiling, the non-market-based measures in all other cases only involved a change in the way the benchmark balance was calculated, whether daily or monthly, and no change was made in the yen conversion limit itself. In a later section, when we estimate the impact of controls on FYDs, we exclude the first two episodes in one specification to focus on the impact of market-based controls alone.

In assessing the impact of these measures on FYD flows, we must note that the FYD balance steadily rose over the period, increasing from a mere 350 billion yen in January 1971 to over 7.5 trillion yen in November 1980; the rate of increase seldom turned negative (Figure 2). The steady rise in the balance of FYDs could be explained not only by the increasing desire of foreign investors to participate in the growing Japanese financial market with attractive returns, but also by the expanding volume of Japanese trade, to the extent that a part of FYDs reflected the volume of trade settlement. Even when the controls became binding or prohibitive, the balance of FYDs could still increase as banks had no choice but to accept deposits from their foreign corresponding banks received for trade settlement purposes or from their long-standing foreign customers who desired to keep their current receipts in yen. Thus, in measuring the impact of controls, a change in the FYD balance must be compared to the hypothetical change in the absence of such controls, both of which could be positive. The key is finding the right counterfactual.

Obviously, the simplest counterfactual would be the average rate of increase in FYDs obtained from the sample. An event study was performed on this basis (Table 2). In view of the limited number of events, we perform no formal test except to note whether each event was "successful" or "not successful" relative to the counterfactual. In particular, we judge a

tightening (easing) event to be "successful" if the increase in the balance of FYDs over the post-event window (r^{post}) was smaller (larger) than the counterfactual ($r^{[post]}$), which is calculated as the average one-month and two-month changes in the sample that excludes the observations for the event and three succeeding months. That is to say, the event is judged to be successful if $r^{post} < r^{[post]}$ in the case of tightening and $r^{post} > r^{[post]}$ in the case of easing.

Compared to the sample average increases of 2.75 and 5.29 percent for one-month and two-month changes, respectively, four and two out of the nine events are judged to have been successful. Only the 6th and 8th events are judged to have been successful in terms of both one-month and two-month changes. The first of these two events involved only the introduction of small average reserve requirements of 0.25 percent. This may explain why the balance of FYDs increased over a two-month horizon, though the pace of increase (1.98) was smaller than the sample average of 5.29. In contrast, the latter event entailed a 50-percentage increase in marginal reserve requirements, from 50 percent to the prohibitive level of 100 percent. The balance of FYDs declined over both one-month and two-month horizons.

Alternatively, following a model used by Fratzscher (2009) for exchange rate forecasting, we can obtain the counterfactual from a simple linear prediction model, such as:

$$r_{t+k} = \alpha^k + \sum_i \beta^k F_{i,t} + \varepsilon_t$$

Where *t* is a time subscript; *i* refers to the ith factor; r_{t+k} is the realized rate of change in the balance of FYDs over the forthcoming k months; α^k and β^k are coefficients for the kth period ahead; ε is a while-noise error; $F_{i,t}$, a set of factors (explanatory variables), is assumed to include: the 3-month moving average rate of change in FYD, 3-month moving average Japan-

U.S. interest rate differential favoring Japanese instruments,[9] and 3-month moving average exchange market pressure index (EMPI). Following Kaminsky and Reinhart (1999), we define EMPI as a weighted average of monthly changes in the yen-US dollar exchange rate and foreign exchange reserves (an increase in EMPI means greater depreciation pressure for the yen), where the weights are given by the respective variables' standard deviations. All data, except for FYDs (obtained from the Bank of Japan, *Economic Statistics Monthly*), come from the International Monetary Fund, *International Financial Statistics*, CD-ROM.

The model is estimated separately for each time horizon, by using the sample that excludes the observations for the event and three succeeding months. The results of an event study based on the predicted value of FYDs as the counterfactual are summarized in Table 3. Compared to the results based on the sample mean rates of increase as the counterfactual, the success rate for one-month changes declines (from 4 to 3) while that for three-month changes rises (from 2 to 4). Given the fact that the first event represents perhaps the tightest measure (enforced by a quantitative ceiling), this result in which it was found successful for both one-month and two-month changes appears more plausible. Likewise, the 7th event also represents an intensively tight control measure (an introduction of marginal reserve requirements at 50 percent). It makes therefore intuitive sense that, with the counterfactual obtained from a prediction model, the 7th event becomes successful though only for the two-month horizon. For the one-month horizon, the event (the second event) that was found successful with the sample mean rate of change but ceased to be successful with the counterfactual calculated from the prediction model, involved an easing of the controls in place. The positive assessment of the 6th

[9] The interest rates are the Treasury bill rate for the US and its equivalent for Japan.

and 8th episodes of controls does not change whether we use the predicted value or the sample average as the counterfactual.

IV. Estimating the Average Treatment Effect by Propensity Score Matching

The event study methodology we have employed has three conceptual problems. First, the number of events is too small to test the statistical significance formally. Second, the choice of counterfactual is rather arbitrary. Finally, as a consequence, it is not capable of dealing with the problem of endogeneity (as the incidence or tightening of a capital control tends to coincide with increasing free-yen deposits). In econometric terms, this is the problem of self-selection. To the extent that capital controls are introduced precisely because short-term capital inflows are surging, a failure to account for this endogeneity is likely to bias our assessment against concluding that the capital controls were effective.

To address this problem, we use the methodology of propensity score matching, whose basic idea is to identify as the counterfactual a similar episode in which capital inflows surged but no control was imposed. Propensity score matching was developed by Rosenbaum and Rubin (1983) and has been used widely in the program evaluation literature (Caliendo and Kopeinig 2008). More recently, the methodology has also been applied successfully to monetary and international economics, for example, by Glick et al. (2006) (capital controls and currency crises), Lin and Ye (2007) (inflation targeting) and Fatum and Hutchison (2010) (foreign exchange market intervention). The advantage of this methodology is that it can formally control for the non-random selection problem while avoiding need to specify the functional form because it is nonparametric (Heckman et al., 1998; Dehejia and Wahba, 2002; Imbens, 2004; Imbens and Wooldridge, 2009).

The empirical literature employing a variety of propensity score matching methods is often called the treatment effect literature, because it involves testing the difference between "treated" observations (those for which there is a presumed impact of the event or measure concerned) and "control" (non-treated but otherwise similar) observations (in order to avoid confusion with "capital control," we avoid using the term "control observations" in the rest of the paper) In the terminology of this literature, therefore, what we attempt to do is to estimate the average treatment effect (ATE) of a market-based capital control measure on the volume of FYD flows.

Our assessment of the effectiveness of capital controls is based on the average treatment effect of what we call "intense" capital controls (which excludes the introduction of average reserve requirements at 0.25 percent, which would remain in place until December 1980). These are not events, as was the case previously, but periods or succession of months where the controls measures were in place. Because an easing of controls essentially represents the absence of controls, an episode of easing can only be considered to be part of the non-treated sample. Specifically, the "treated" observations (for which controls were applied intensively) include: (i) August-November 1971; (ii) May 1972-November 1973; and (iii) November 1977-January 1979. These correspond to the periods when a maximum limit was imposed on the outstanding balance of free-yen deposits, marginal reserve requirements on free-yen deposits were 25% or higher, or the yen conversion limit was strengthened. Except for the first episode, the tightening of control entailed a change in the price incentive, with any administrative measure playing a secondary role.

By observing how the balance of FYDs changed from month to month (Figure 3), we find no clear pattern that the rate of increase was consistently negative during the months of

intense capital controls (indicated by the shaded areas). During the first episode (when the controls were entirely administrative), the rate of increase did fall but the fall started prior to the intensification of controls and it started to rise before the tightened controls were eased. Nor is there evidence to suggest that rate of increase was smaller during the periods of intensive controls than the rate of increase during the other months. Rather, except for the latter part of the second episode, the rate of increase in FYDs appears to be generally larger when the controls were intensive.

Formally, we estimate the average treatment effect on the treated (ATT) defined as:

$$ATT = E[Y_i(1) \mid T_i = 1] - E[Y_i(0) \mid T_i = 1], \qquad (1)$$

where T is the dummy variable for treatment ($T = 1$ when treated; $T = 0$ when not treated); $Y_i(1)$ denotes the outcome when observation *i* involves the treatment (i.e., intensive capital controls) and $Y_i(0)$ is the outcome that obtains in the absence of intensive capital controls.[10] The fundamental problem in estimating equation (1) is that the second term on the right-hand side ($Y_i(0) \mid T_i = 1$) is not observable, that is to say, the key point is to identify the counterfactual for $E[Y_i(0) \mid T_i = 1]$. If we assume that the treatment decision (i.e., the imposition of intensive controls) is random, the sample mean of the outcome for the non-treated reference group ($E[Y_i(0) \mid T_i = 0]$) can be used to estimate the ATT (Table 4). Then, we cannot reject the hypothesis that the imposition of intensive controls raised (rather than reduced) the rate of increase in FYDs at the 10 percent significance level. This is consistent with what we have

[10] Both continuous and discrete scalar variables can be applied as the outcome variable in the estimation of the average treatment effect. In this analysis, only one of the potential outcomes is observed for each *i,* and the other is unobserved or missing. The unobserved outcome is called the counterfactual outcome (Caliendo and Kopeinig 2008). The matching estimators impute the missing potential outcome by using average outcomes for observations with similar values of covariates.

previously gathered from looking at Figure 3. When all non-treated observations are used as the reference group, the capital controls had only a small impact, if any, on the volume of short-term capital flows through FYDs.

The problem of this approach is obvious because the imposition of intensive controls is in practice not random. If the treatment decision (i.e., the imposition of intensive controls) is systematically correlated with a set of observable covariates that also affect the outcome, the estimate of the ATT will be biased (Dehejia and Wahba, 2002; Caliendo and Kopeinig, 2008). To identify the second term on the right-hand side of equation (1), conditional independence must first be assumed as follows:

$$Y_i(0), Y_i(1) \perp T_i \mid X_i . \qquad (2)$$

The conditional independence (or unconfoundedness) assumption implies that, given the pre-treatment variables or covariates *X*, the treatment assignment is independent of the potential outcomes. Therefore, for observations with similar values of *X*, the treatment assignment is random with respect to the potential outcomes. We further assume the following so-called common support (or overlap) condition:[11]

$$0 < P(T_i = 1 \mid X_i) < 1, \qquad (3)$$

where $P(T_i = 1 \mid X_i) = P(X_i)$ is the conditional probability of adopting the treatment given the observed covariates *X*. This probability is what is commonly called a "propensity score." This condition requires the existence of some comparable reference observations for each treatment observation.

[11] According to Heckman et al. (1998), we only need to assume that $Y_i(0) \perp T_i \mid X_i$ (unconfoundedness for controls) and $P(T_i = 1 \mid X_i) < 1$ (weak overlap) to identify the ATT.

Under the conditional independence assumption and the common support condition, as specified above, equation (1) can be rewritten as

$$ATT = E[Y_i(1) \mid T_i = 1, X_i] - E[Y_i(0) \mid T_i = 0, X_i], \quad (4)$$

where $E[Y_i(0) \mid T_i = 1, X_i]$ is replaced with $E[Y_i(0) \mid T_i = 0, X_i]$ for the non-treated observations. One matching method, called covariate matching, pairs the treated observations with the non-treated observations with similar observed values of covariates *X*. This method, however, is difficult to implement when the set of covariates *X* is large.

To overcome the high-dimensionality problem, Rosenbaum and Rubin (1983) propose matching based on their propensity scores, which are the probabilities of treatment assignment conditional on *X* and can be estimated using logit or probit models. This matching is called the "propensity score matching" method. It applies the conditional independence assumption on the propensity score ($Y_i(0), Y_i(1) \perp T_i \mid P(X_i)$) and matches the treated observations to the non-treated observations on the basis of the one-dimensional variable $P(X_i)$, instead of the multidimensional vector *X*. Using propensity score matching, the ATT can then be estimated as

$$ATT = E[Y_i(1) \mid T_i = 1, P(X_i)] - E[Y_i(0) \mid T_i = 0, P(X_i)]. \quad (5)$$

By pairing each treated observation to non-treated observations with similar propensity scores, this methodology can eliminate sample selection bias to identify a credible counterfactual in the estimation of the ATT (Dehejia and Wahba, 2002; Caliendo and Kopeinig, 2008).

Caliendo and Kopeinig (2008) suggest that the probability of adopting a treatment should be estimated by using observed pre-treatment variables (pre-treatment characteristics). To obtain the probability of adopting a treatment at time *t*, we thus use a set of covariates *X* at time *t*-1.

This means that equation (5) can be represented (by replacing subscript *i* with as a time subscript):

$$ATT = E[Y_t(1) \mid T_t = 1, P(X_{t-1})] - E[Y_t(0) \mid T_t = 0, P(X_{t-1})]. \quad (6)$$

In what follows, we proceed in the following sequence. First, we estimate the probability of an intensive capital control by using a probit model, where the dependent variable takes the value of one (when the control is intensified) or zero (otherwise). Second, we estimate the average treatment effect of intensive capital controls on FYD flows by calculating the mean difference in outcomes between the treated and the non-treated observations based on propensity score matching.

In finding the right non-treated matches for the treated observations, we employ nearest-neighbor and radius matching methods. The nearest-neighbor method (with replacement) pairs each treated observation with a non-treated observation with the closest propensity score. In contrast, the radius method pairs a treated observation with all non-treated observations with estimated propensity scores falling within a specified radius *r* (Dehejia and Wahba 2002): *r*=0.005, *r*=0.01, *r*=0.05 in our case. Use of four matching methodologies is intended as a robustness check, as no matching method is ideal for all situations (Imbens 2004; Caliendo and Kopeinig 2008). The common support condition is imposed, however, in the application of both types of matching methods, namely, we drop the non-treated observations whose estimated propensity scores are lower than the lowest score among the treated observations or higher than the highest score among the treated observations (Becker and Ichino 2002).

A probit model of the probability of an intensive capital control was estimated, with the following explanatory variables: the 3-month moving average monthly rate of change in FYD at time *t-1*, the 3-month moving average exchange market pressure index (EMPI) at time *t-1*, the 3-

month moving average Japan-U.S. interest rate differential at time *t-1*, the 3-month moving average rate of increase in industrial production at time *t-1*, and a dummy variable for the post-Smithsonian floating exchange rate period (i.e., after February 1973).[12] EMPI is as defined previously. To ensure robustness, we estimated four different specifications of the model by the maximum likelihood method, namely, the full model (denoted as [1] in the table), a specification in which the average lagged rate of change in FYD is removed ([2]), one that excludes May 1972-November 1973 from the sample ([3]), and one that excludes the first episode (of administrative controls, August-November 1971), in order to focus on the impact of market-based controls only (Table 5). The estimated coefficients of most explanatory variables are significant at conventional significant levels, though not all have the expected signs. The probit model fits the data quite well (pseudo R2 ranging from 0.30 to 0.52), with the accuracy of predictions ranging from 77 to 88 percent of the time; the null hypothesis that all slopes are zero is rejected at the 1 percent level.

In all four specifications, the coefficients of EMPI is negative and significant, suggesting that as the yen faced appreciation pressure the probability of introducing an intensive control rose. Except for the third specification, the coefficient of industrial production is positive and significant, implying that as the economy expanded the probability of imposing a capital control rose, presumably to mitigate pressure on the exchange rate. Likewise, except for the third specification, the coefficient of the floating regime dummy is negative and significant. This means that the authorities were more prone to use capital controls when the exchange rate was fixed (this result does not obtain in the third specification because it excludes the fixed exchange

[12] We have verified that no substantive results, including the ultimate assessment of the effectiveness of controls, change even if we alternatively use the one-period lags in place of the 3-month moving averages. The use of 3-month averages, however, yields a better fit for the probit model.

rate period of May 1972-November 1973). On the other hand, the coefficient of the interest rate differential is negative and significant. While this may be counterintuitive (if it means that an increase in the relative rate of return on domestic assets led to a decline in the volume of inflows), the overall results in all specifications are found to satisfy the critical balancing property (Becker and Ichino 2002), namely that the distributional properties of the covariates are similar between the treated and non-treated observations.[13]

The ATT of intensive capital controls on FYD flows can then be estimated on the basis of the matched samples (Table 6). Matching was carried out separately based on the estimated propensity scores from each version of the probit model, as reported in Table 5 ([1][2][3][4]), utilizing four matching methodologies: nearest-neighbor matching with replacement,[14] as well as radius matching with the choice of r=0.005, r=0.01, r=0.05 (denoted in the table as A, B, C, D, respectively).[15] Hence, there are 16 combinations ([1A] through [4D]). Except in one case ([2B], the ATT is estimated to be negative. This means that, with the application of propensity score matching, the overall results become more favorable to the effectiveness of capital controls (in Table 4, the ATT obtained without propensity score matching was 1.575 and nearly significant at the 10 percent level). Even in the least favorable case ([2B]), ATT (0.49) is smaller.

None of the ATTs have turned out to be significant at conventional levels, however. The numerically highest t-statistic is 1.42 ([3D]), which obtains when the radius matching method of 0.05 was used (hence allowing farthest apart observations to be included in the non-treated sample) and the period May 1972-November 1973 (during which the earlier event study shows

[13] Lin and Ye (2007) and Caliendo and Kopeinig (2008) suggest that the purpose of the probit estimation is not to explain accurately the probability of treatment choice but to control effectively for the covariates that affect the treatment choice and the outcome.

[14] In the case of matching with replacement, each non-treated observation can be matched more than once. Matching with replacement increases the average quality of matching by increasing the set of possible matches (Caliendo and Kopeinig 2008).

[15] This was done by using the Stata ado-file of Becker and Ichino (2002).

that the controls were not effective) was excluded. Focusing on the bottom of the table where the estimates from model [4] are reported, we find no evidence that the results are sensitive to the exclusion or inclusion of the first episode of administrative controls from the sample. On this basis, we conclude that while the market-based controls had only a marginal, if any, impact on the volume of short-term capital flows. This may mean that the price elasticity of short-term capital inflow is small, so that market-based controls may need to become prohibitive, perhaps accompanied by administrative measures, to be effective in a meaningful way.

V. CONCLUSION

We have tested the effectiveness of marginal reserve requirements used by Japan in the 1970s to influence short-term capital flows through free-yen deposits. Capital controls, market-based ones in particular, are receiving increasing respectability in the policy making community as a legitimate tool of protecting domestic financial markets from the vicissitude of cross-border capital flows, but the case for them almost entirely relies on the mixed evidence from the experience of Chile with unremunerated reserve requirements in the 1990s. We contribute to the ongoing debate on the use of capital controls by providing evidence from the experience of Japan in the 1970s when the capital account was closed except for short-term capital flows through FYDs.

Testing for the effectiveness of a capital control on the volume of inflows is notoriously difficult because of the endogeneity of such a measure, which is typically imposed or intensified precisely when inflows surge. Thus, unless we address this problem, any assessment of effectiveness is biased against concluding that the measure was effective. We applied the method of propensity score matching to account for the endogeneity, and found that an increase in marginal reserve requirements only modestly reduced the volume of short-term capital inflows

through non-resident free-yen accounts. This was in contrast to the results that obtained when the endogeneity was not properly controlled for. Even so, the impact was not statistically significant, implying that the price elasticity of short-term capital flows was small. We conclude that market-based controls must be nearly prohibitive, perhaps combined with administrative measures, to be effective in a meaningful way.

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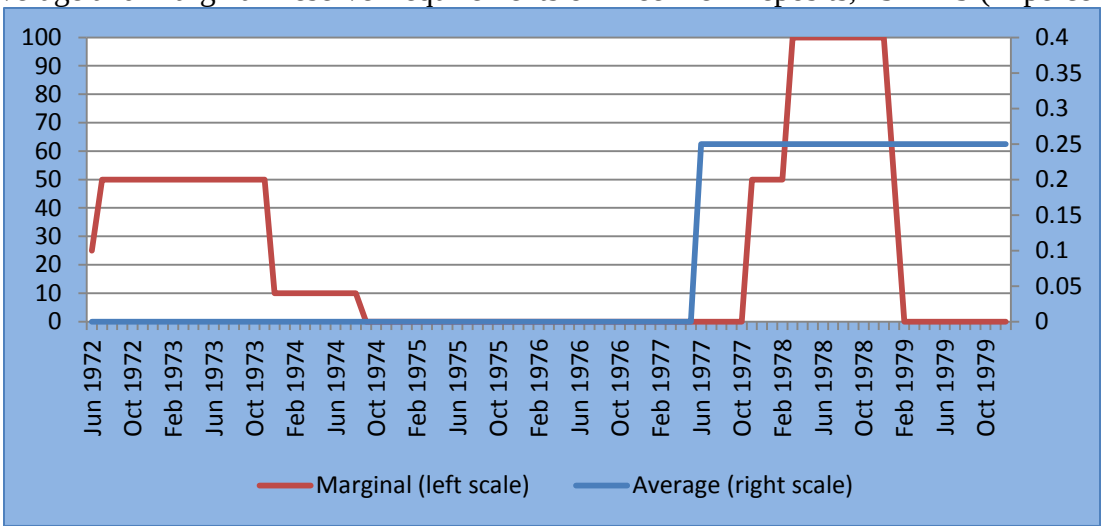
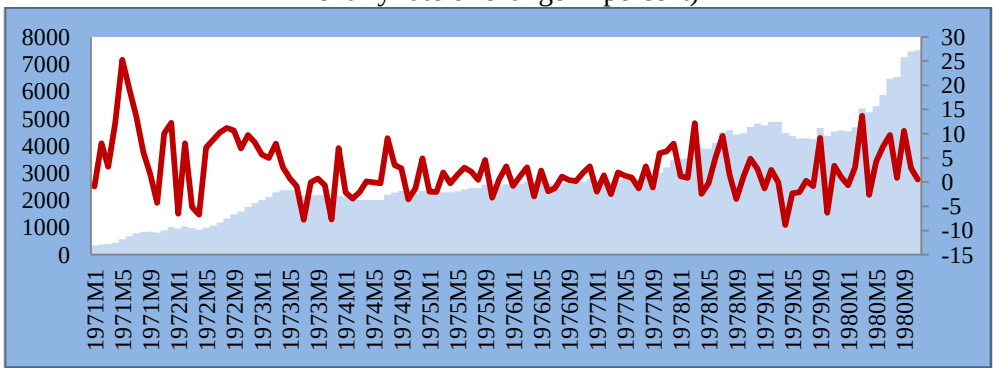
Figure 1
Average and Marginal Reserve Requirements on Free-Yen Deposits, 1972-79 (in percent)



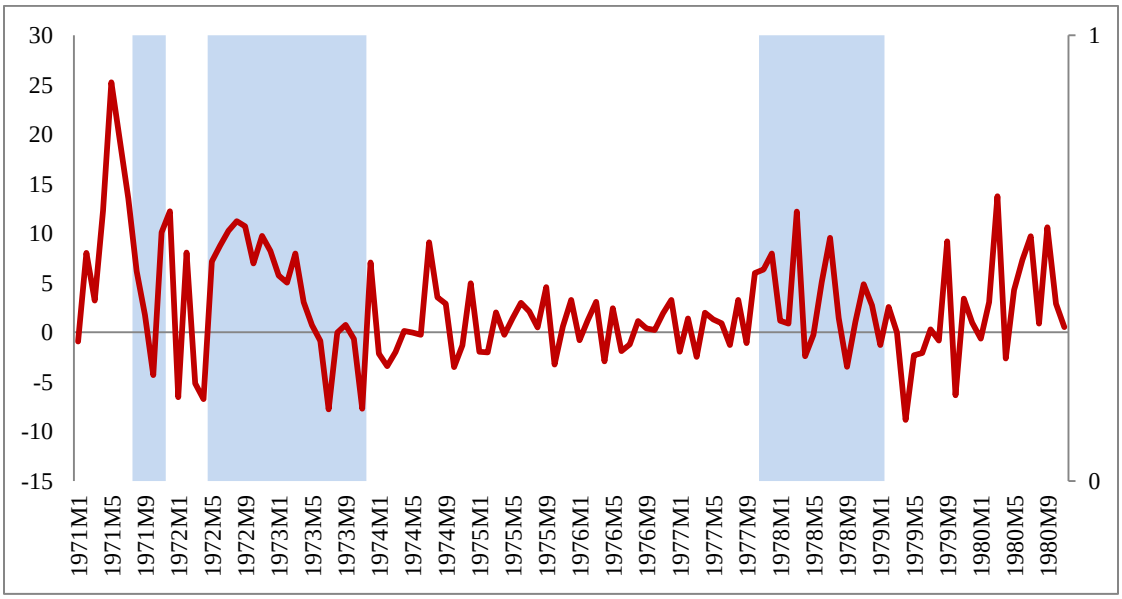
Source: Bank of Japan, *Economic Statistics Monthly*, March 1986

Figure 2
Free-yen Deposits, 1971-1980 (Left scale: outstanding monthly balance in billions of yen; Right scale: monthly rate of change in percent)



Note: Non-resident free-yen deposits in all authorized foreign exchange banks.
Sources: Bank of Japan, *Economic Statistics Monthly* and *Economic Statistics Annual*, various issues.

Figure 3
Monthly Rate of Change in Free-yen Deposits during Intensive Control Periods, 1971-1980
(In percent)



Notes: Non-resident free-yen deposits in all authorized foreign exchange banks; the shaded areas indicate the months in which controls were intensified.
Sources: Bank of Japan, *Economic Statistics Monthly* and *Economic Statistics Annual*, various issues: authors' estimates.

Table 1:
Measures to Control Short-Term Capital Flows through Free-yen Accounts, 1971-1980

Date	Measures, as applied to authorized foreign exchange banks
1. Tightening, August-September 1971	Requirements introduced to keep the outstanding balance of free-yen deposits at or below 18 August balance (19 August 1971); the base changed to 27 August balance (28 August 1971); the application of the "yen conversion limit" changed from monthly average balance to daily balance basis (7 September 1971)
2. Easing, December 1971-January 1972	The application of the "yen conversion limit" reverted back to average monthly basis (21 December 1971); restrictions introduced in August 1971 abolished on the maximum balance of free-yen deposits (6 January 1972)
3. Tightening, May-July 1972	The application of the yen conversion limit changed from monthly to daily (May 1972); 25% marginal reserve requirements imposed on free-yen deposits on the basis of 21 April-20 May 1972 balance (1 June 1972); marginal reserve requirements raised to 50% on the basis of 21 May-20 June 1972 balance (1 July 1972)
4. Easing, December 1973	Marginal reserve requirements lowered from 50% to 10% on the basis of 21 May-20 June 1972 balance (10 December 1973); the yen conversion limit abolished for free-yen deposits, with the exception of head office/branch accounts (17 December 1973)
5. Easing, September 1974	Marginal reserve requirement lowered from 10% to zero (12 September 1974)
6. Tightening, June 1977	0.25% reserve requirements introduced on the outstanding balances of free-yen deposits (1 June 1977).
7. Tightening, November 1977	50% marginal reserve requirements introduced on the basis of 1-31 October 1977 balance (22 November 1977)
8. Tightening, March 1978	Marginal reserve requirements raised from 50% to 100%, on the basis of 1-28 February balance (18 March 1978)
9. Easing, January-February1979	Marginal reserve requirements lowered from 100% to 50%, on the basis of 1-28 February 1978 balance (17 January 1979); marginal reserve requirements further reduced to zero percent (10 February 1989)

Sources: Bank of Japan (1988); Fukao (1990); and Inuta (2000).

Table 2
An Event Study: The Impact of Tightening or Easing Controls on Free-yen Deposits, 1971-1980

	Date (number of measures)	Tightening or easing	One-month change (%)	Direction of change relative to the counter-factual of 2.75	Two-month change (%)	Direction of change relative to the counter-factual of 5.29
1	August-September 1971 (2)	Tightening	-3.45	Successful	6.63	Not successful
2	December 1971-January 1972 (2)	Easing	4.71	Successful	-0.44	Not successful
3	May-July 1972 (3)	Tightening	20.67	Not successful	31.36	Not successful
4	December 1973 (2)	Easing	-2.14	Not successful	-5.55	Not successful
5	September 1974 (1)	Easing	-3.53	Not successful	-4.85	Not successful
6	June 1977 (1)	Tightening	-1.29	Successful	1.98	Successful
7	November 1977 (1)	Tightening	7.94	Not successful	9.12	Not successful
8	March 1978 (1)	Tightening	-2.42	Successful	-2.66	Successful
9	January-February 1979 (2)	Easing	1.24	Not successful	-7.59	Not successful

Note: The counterfactuals are simply calculated as the average one-month and two-month changes in the sample that excludes the event month and subsequent three months.

Table 3
An Event Study: The Impact of Tightening or Easing Controls on the Total Balance of Free-yen Deposits, 1971-1980

	Date (number of measures)	Tightening or easing	One-month change (%)	Counter-factual: predicted change (%)	Direction of change relative to counter-factual	Two-month change (%)	Counter-factual: predicted change (%)	Direction of change relative to counter-factual
1	August-September 1971 (2)	Tightening	-3.45	9.99	Successful	6.63	18.09	Successful
2	December 1971-January 1972 (2)	Easing	4.71	5.42	Not successful	-0.44	9.86	Not successful
3	May-July 1972 (3)	Tightening	20.67	3.02	Not successful	31.36	5.57	Not successful
4	December 1973 (2)	Easing	-2.14	-0.35	Not successful	-5.55	-0.25	Not successful
5	September 1974 (1)	Easing	-3.53	3.31	Not successful	-4.85	6.15	Not successful
6	June 1977 (1)	Tightening	-1.29	2.51	Successful	1.98	4.81	Successful
7	November 1977 (1)	Tightening	7.94	4.93	Not successful	9.12	9.28	Successful
8	March 1978 (1)	Tightening	-2.42	5.45	Successful	-2.66	10.21	Successful
9	January-February 1979 (2)	Easing	1.24	1.32	Not successful	-7.59	2.99	Not success

Note: The counterfactuals are calculated as the predicted one-month and two-month changes from a simple linear prediction model.

Table 4
The Average Treatment Effect on the Treated (ATT):
Months of Intensive Controls versus Non-Treated Months

	Number of months	Average monthly rate of change in free-yen deposits (%)	ATT (t-statistic)
"Treated" months: intensive controls (T=1)	38	3.648	1.575 (1.457)
All "non-treated" months (T=0)	81	2.074	
Memorandum: all months	119	2.576	

Note: The index (T) takes the value of one for: August-November 1971; May 1972-November 1973; and November 1977-January 1979.

Table 5
Probit Estimates of Intensive Capital Controls

	[1]	[2]	[3]	[4]
3-month moving average monthly rate of change in free-yen deposits (t-1) (%)	0.045 (1.04)		0.070 (1.44)	0.033 (0.68)
3-month moving average rate of change in EMPI (t-1)	-0.233*** (-3.89)	-0.257*** (-4.29)	-0.403*** (-4.27)	-0.224*** (-3.18)
3-month moving average interest rate differential (t-1) (%)	-0.165** (-2.35)	-0.181*** (-2.61)	-0.239*** (-2.87)	-0.163*** (-2.26)
3-month moving average rate of increase in industrial production (t-1) (%; year-on-year)	0.174*** (4.04)	0.172*** (4.03)	0.006 (0.33)	0.175*** (4.09)
Dummy for the floating exchange rate period	-0.929* (-1.90)	-1.160*** (-2.82)	0.925* (1.80)	-0.901* (-1.73)
Constant	-1.647*** (-3.02)	-1.389*** (-3.29)	-5.540*** (-3.42)	-1.649*** (-3.05)
Number of observations	119	119	100	115
Log pseudo likelihood	-50.0	-50.6	-23.6	-49.2
Pseudo R2	0.33	0.32	0.52	0.30
H0: All slopes=0, Wald chi2 [p-value]	32.31 [0.00]	29.06 [0.00]	25.64 [0.00]	25.44 [0.00]
AIC	112.0	111.2	59.1	110.4
BIC	128.7	125.1	74.8	126.8
Correctly classified (%)	79.0	76.5	88.0	79.1

Notes: The numbers in parentheses are z-statistics, which are calculated using heteroskedasticity-robust standard errors; ***, **, * indicate statistical significance at the 1, 5, 10 % levels, respectively.

Table 6
Propensity Score Matching Methods:
The Effect of Intensive Capital Controls on Free-yen Deposits

Matching method	Nearest neighbor	Radius (r=0.01)	Radius (r=0.025)	Radius (r=0.05)
Model (covariates)	Probit model [1]			
	[1A]	[1B]	[1C]	[1D]
Average treatment effect on treated (ATT)	-0.93	-0.97	-1.66	-1.51
Analytical standard errors	2.60	2.63	2.05	1.66
T-statistics	-0.36	-0.37	-0.81	-0.91
Treatment/non-treatment	38/17	16/17	27/25	30/38
Common support condition satisfied	Yes	Yes	Yes	Yes

Model (covariates)	Probit model [2]			
	[2A]	[2B]	[2C]	[2D]
Average treatment effect on treated (ATT)	-2.91	0.49	-0.40	-0.38
Analytical standard errors	2.83	2.10	1.58	1.47
T-statistics	-1.03	0.23	-0.26	-0.26
Treatment/non-treatment	38/18	19/26	28/40	35/40
Common support condition satisfied	Yes	Yes	Yes	Yes

Model (covariates)	Probit model [3]			
	[3A]	[3B]	[3C]	[3D]
Average treatment effect on treated (ATT)	-2.95	-2.38	-3.04	-3.47
Analytical standard errors	2.35	3.90	2.97	2.44
T-statistics	-1.26	-0.61	-1.02	-1.42
Treatment/non-treatment	19/8	4/5	11/10	13/14
Common support condition satisfied	Yes	Yes	Yes	Yes

Model (covariates)	Probit model [4]: excluding administrative controls			
	[4A]	[4B]	[4C]	[4D]
Average treatment effect on treated (ATT)	-2.65	-3.81	-1.91	-1.16
Analytical standard errors	2.09	3.05	1.92	1.68
T-statistics	-1.27	-1.25	-0.99	-0.69
Treatment/non-treatment	34/17	15/13	25/32	30/39
Common support condition satisfied	Yes	Yes	Yes	Yes

Note: ATT is the mean difference between the treated (with intensive capital controls) and the non-treated (without such controls) observations in terms of monthly changes in free-yen deposits. We employ propensity score matching based on the estimated probit model of Table 5 to choose the non-treated observations against which the treated observations are compared.