Does Capital Control Policy Affect Real Exchange Rate Volatility?
A Novel Approach Using Propensity Score Matching

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Honors Thesis submitted in partial fulfillment of the requirements for Graduation with Distinction in the Department of Economics at Trinity College of Duke University.

Duke University
Durham, North Carolina
2008

T A B L E   O F   C O N T E N T S

A C K N O W L E D G E M E N T S

A B S T R A C T

Propensity score matching is a statistical technique recently introduced in the field of economics, which researchers use to assess the treatment effect of policy initiatives. In this study I use propensity score matching to analyze the treatment effect of capital control policy on real exchange rate volatility. I find the treatment effect of adopting relatively liberal capital controls is a decrease in real exchange rate volatility. This is the first empirical study to provide insight into the causal relationship between capital controls and real exchange rates, which may be crucial to macroeconomic policy decisions for emerging economies such as China.

K E Y W O R D S

Capital controls, real exchange rate, treatment effects, propensity score matching.

Many in China fear that the removal of capital controls that restrict the ability of domestic investors to invest abroad or to sell and purchase foreign currency could cause a destabilization of the whole system.

- Alan Greenspan
The Wall Street Journal, March 2, 2004

1. I N T R O D U C T I O N

Until recently it would have been impractical to empirically explore the validity of China's fears that the liberalization of capital controls would cause a "destabilization of the whole system," and in particular an increase in the volatility of the real exchange rate.[1] Because the degree of capital controls is endogenously determined, traditional econometric methods such as linear regression are unreliable in assessing the causal relationship between capital control liberalization and real exchange rate volatility. Moreover, an experiment in which countries are randomly assigned to liberalize or maintain their level of capital controls is simply infeasible.

In this paper I apply Leuven and Sianesi's (2003) method of propensity score matching to determine the treatment effect of capital control policy on real exchange rate volatility. The traditional approach to dealing with the endogenaiety problem in empirical macroeconomics is to use an instrumental variables method. However, one problem with such methods is the difficulty of finding instruments that are both valid and relevant. As an alternative to these methods, Leuven and Sianesi (2003) developed propensity score matching, a novel econometric procedure that takes a different approach to solving the endogenaiety problem. Essentially, their Stata module *PSMATCH2* creates 'couples' from

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[1] Real exchange rate volatility is one of many macroeconomic indicators of stability that could have been measured in this study. It is nonetheless of particular concern for influential export-based emerging countries such as China given that past declines in exports have been formally attributed to periods of increased real exchange rate volatility (Rose 2000; Iwatsubo and Karikomi 2006). In practical terms, high real exchange rate volatility had directly affected the South Korean steel and chemical industries in the 1990s.

data points by matching 'male' data points (countries that adopted a particular policy) with 'female' data points (countries that did not adopt the policy), based on their similar *propensity* to make a change in policy. In my case, I study the decision to liberalize capital controls. The treatment effect of capital control liberalization is, therefore, the average of the differences in real exchange rate volatilities between the 'male' and 'female' data points of each 'couple'.

I find that the treatment effect of capital control *liberalization* is a *decrease* in real exchange rate volatility. This result is reinforced in my second finding that capital control *tightening* causes an *increase* in real exchange rate volatility. Although it is impossible to achieve statistical significance at the 95% level in most propensity score matching tests with *PSMATCH2*, I can be confident of these results at the 90% level. Moreover, correlations and additional statistical tests examining long-run real exchange rate volatility following changes in capital control policy are consistent with the results discovered using the propensity score matching technique.

The causal relationship discovered in this study is especially pertinent to emerging economies such as China. Despite the desire to self-insure against macroeconomic instability and massive capital outflows characteristic of the 1997 Asian financial crisis (Bartolini and Drazen 1997), emerging economies with tight capital control policies are expected to eventually follow the global trend of increasingly liberalized capital controls that began in the 1980s. On the basis that capital controls inhibit the most efficient use of economic resources, Edwards (1999) notes that "at the practical policy level the debate has centered not so much on whether capital controls should be eliminated, but on *when* and *how fast* this should be done." Therefore if the results of this study are indeed valid where the treatment effect of capital control liberalization is an increase in macroeconomic stability (and not less

stability as per China's hypothesis) then the policy implication of this study is large enough to warrant more careful empirical and theoretical consideration in the field.

Beyond the important policy implications, my research is novel in its use of the propensity score matching technique and Chinn and Ito's (2007) intensity-modified ordinal index of *de jure* capital controls, *KAOPEN*. Although there has been ample research in the past decade with regard to the advantages and effects of capital controls, the literature is largely empirical[2] and limited to a collection of case studies on characteristic countries[3] concerned most often with the impact of such controls on growth, inflation and capital flows. Edwards (1999) explains that economists have been reluctant to incorporate indices of capital controls into their research because of the difficulty in documenting subtle differences between *de jure* capital controls across multiple countries and a significant period of time. Nevertheless, Chinn and Ito's (2007) ordinal index of capital controls is the first to enable accurate and simultaneous empirical measurements of relatively subtle changes in capital control policy across 161 countries in a time period of 35 years. As one of the first studies to incorporate Chinn and Ito's (2007) novel measure of financial openness, this research is unique not only in the methodology it uses to discuss treatment effects, but also in its comparative study of capital control policy across multiple countries and a broad period of time.

The remainder of this paper is organized as follows: Section 2 presents the relevant literature on capital control indices preceding the creation of Chinn and Ito's (2007) *KAOPEN* measure of financial openness, as well as an explanation of the intuition behind Leuven and Sianesi's (2003) propensity score matching tool for Stata, *PSMATCH2*. Section

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[2] As documented in Frenkel (2001) and Edwards (1999).
[3] For example: Chile, Mexico, Uruguay, Colombia by Edwards (1999) and New Zealand, Portugal, Ireland, Israel by Alfaro and Kanczuk (2004).

3 describes the data to be used herein. Section 4 explores the methodology, results, and some interpretation. Section 5 concludes.

2.   L I T E R A T U R E   R E V I E W

Although this is the first study to directly explore the treatment effect of capital control policy on real exchange rate volatility, there are three elements in particular which require further elaboration in the literature review. First I describe the function of capital controls, and discuss previous research that establishes a relationship between capital controls and real exchange rates. Second, I provide a brief history of capital control indices leading up to Chinn and Ito's (2007) novel measure of financial openness. In so doing, I establish why Chinn and Ito's (2007) *KAOPEN* variable is the first to enable accurate and simultaneous empirical measurements of relatively subtle changes in *de jure* capital control policy across multiple countries. Third, I explain the intuition behind Leuven and Sianesi's (2003) propensity score matching tool *PSMATCH2,* which I use to study the treatment effect of capital control policy changes on real exchange rate volatility. Additionally, I describe two recent studies that also use *PSMATCH2* as their central empirical method.

2.1   The Relationship Between Capital Controls and Real Exchange Rates

Capital controls are financial market policies designed to limit or redirect international transactions of investment-related financial instruments. These controls have generally been used for a variety of reasons: to support government attempts to broaden the tax base for a capital levy, sustain fixed or managed exchange rate policy, prevent capital outflows by making them cost-prohibitive, or promote macroeconomic stability. Bartolini and Drazen (1997) note that some form of capital control policy was used in 3 of 24 OECD

countries[4] and in as many as 126 of 158 developing countries in 1995. Two particular examples of capital control policy are the Chilean *Encaje* and the U.S. *Interest Equalization Tax.*

Between 1991 and 1998, the Chilean *Encaje* was put in place to restrict capital inflows for three reasons. First, the *Encaje* attempted to promote macroeconomic stability by limiting potentially volatile inflows that could be drained in a crisis. Second, it attempted to reduce destabilization by avoiding capital that could distort incentives in financial markets. Third, it attempted to reduce monetary expansion, which in turn decelerates domestic inflation, and inhibits rapid appreciation of the real exchange rate.

The rationale for capital controls in the U.S. was very different than in Chile. The U.S. *Interest Equalization Tax* between 1963 and 1974 targeted capital outflows to correct a balance of payments deficit. The objective of the U.S. *Interest Equalization Tax* was to reduce the demand for foreign assets, without having to either use contractionary monetary policy or devalue the local currency, thus allowing for inflation to be higher.

The Chilean *Encaje* and the U.S. *Interest Equalization Tax* suggest that capital controls can vary considerably in both their objective and implementation. Because capital controls can take many forms – from taxes to restrictions and outright prohibitions on the cross-border trade of assets – it can be difficult to model the effects of these policies. Nevertheless, economists broadly agree that while capital controls may be useful under certain circumstances they are still fundamentally taxes on the movement of capital, which are analogous to tariffs on goods in that they both detract from economic efficiency. Specifically, capital controls can be detrimental when they prevent financial resources from being used where they are needed most (Neely 1999). Basic theory and empirical studies follow this intuition.

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[4] Greece, Norway and Turkey.

Barro (1997) explains as per the neoclassical model, that in a closed economy the capital stock is expected to grow according to a concave function, very slowly approaching a long-run steady state. As illustrated in Figure 1, if the capital account were opened the capital stock would rapidly approach the steady state, net of adjustment costs.[5] Rapid adjustment of the capital stock to the long-run steady state promotes a more efficient allocation of global capital because of the higher rate of return in the previously closed economy. These capital inflows are beneficial because they presumably increase growth, employment opportunities, and living standards in the liberalizing country. However, the potential consequences of this adjustment may also deter certain countries from liberalizing their capital controls.

Fundamentals suggest that a rapid increase in capital inflows to the steady state would also generate an increase in aggregate expenditure, which would in turn give rise to increased pressure on domestic prices, cause an appreciation of the real exchange rate, and thus imply a loss of international competitiveness. Edwards' (1999) Latin American case studies provide empirical evidence that an increase in capital flows is associated with an appreciation of the real exchange rate. Moreover, Edwards (1999) uses a Granger causality test to show that it is not possible to reject the null hypothesis that increased capital flows *cause* real exchange rate movements.

Although these theories suggest that there are immediate benefits (e.g. an increase in available capital) and costs (e.g. macroeconomic overheating) to capital control liberalization, they nonetheless provide no explanation of how changes in policy affect macroeconomic stability, or more specifically real exchange rate volatility. Edwards (1999) and Alfaro and Kanczuk (2004) have commented in Latin American case studies that a short period of real

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[5] Bartolini and Drazen (1997) extend the basic model where liberal policy allowing capital outflows sends a favorable signal to investors that intrinsically triggers *additional* capital inflows, thereby increasing the long-run steady state. Bartolini and Drazen's (1997) model is consistent with the experiences of several Latin American and European countries that have liberalized their capital controls.

exchange rate volatility tends to follow the liberalization of capital controls in most countries, presumably until capital flows stabilize at a steady state.[6] Nevertheless, both studies suggest that beyond the initial period of capital adjustment, differences in real exchange rate volatility before and after the liberalization are largely unclear. Edwards (1999) also notes that even if there had been a greater increase in post-policy real exchange rate volatility, it would still be difficult to attribute the increase in volatility to the change in policy because volatility is on average much higher in Latin America than in the rest of the world. Therefore, in order to gain a better understanding of how capital controls affect macroeconomic stability, it may be necessary to look beyond case studies, and use an index of capital controls to simultaneously study subtle policy changes across a broad range of countries and time.

2.2   Capital Control Indices

Edwards (1999) and Rogoff (1999) argue that our understanding of the effects of capital control policy is limited by our ability to construct an accurate index of capital controls. In effect, our basic notion of capital controls is largely the product of careful case studies on characteristic countries such as Chile, Colombia, Germany, Malaysia, Mexico, and Portugal.[7] Although capital control indices could allow the simultaneous study of capital control policy across a broad range of countries and time, researchers have been reluctant to use indices for a few reasons. Specifically, past indices have failed to account for the *intensity* of capital controls, the *subtlety* regarding the direction of the controls, and most importantly

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[6] Edwards (1999) proposes that relaxing the external credit constraint may have two implications: a long-run increase in the sustainable volume of capital flows and a short-run overshooting of capital into the economy. Although the long-run effect is dependent on the stock demand for the country's securities by foreigners, the real rate of growth, and the world interest rate, a short-run overshooting may occur because capital inflows can exceed long-run equilibrium volume while the new capital is dispersed into the economy. Although the theory is admittedly circumstantial, the author suggests this short-run overshooting of capital may have caused the increase in real exchange rate volatility following capital control liberalization.
[7] Edwards (1999), Neely (1999), and Alfaro and Kanczuk (2004),

the *efficacy* with respect to discerning between de facto and de jure controls. In this subsection, I discuss the evolution of capital control indices. I also explain the construction of Chinn and Ito's (2007) *KAOPEN* measure of financial openness, which I use in this study.

2.2.1   Binary Indices Preceding Chinn and Ito (2007)

The primary source from which capital control indices are constructed is the International Monetary Fund's *Annual Report of Exchange Arrangements and Exchange Restrictions* (*AREAER*). Published annually since 1967, the *AERAER* also offers a summary table with binary indicators for four types of de facto controls: (a) multiple exchange rates, (b) restrictions on current account transactions, (c) restrictions on capital account transactions, and (d) regulatory requirements on the surrender of export proceeds. Eichengreen (1998) was among the first to use a capital control index in his research, combining the binary indicators in the *AREAER* summary tables to create a four-point scale of capital controls.

Eichengreen (1998) used the index to suggest that contrary to the general consensus in the field, there was no trend of capital control liberalization over time. This result was received with much skepticism. Edwards (1999) among others used Eichengreen's (1998) study to illustrate the danger of generalizing complex capital controls in a simple four-point scale. He argued that the results of Eichengreen's (1998) study were misleading because the index failed to capture differences in the intensity of capital controls. He also reasoned that the index did not account for whether the de facto policies recorded in the *AREAER* effectively restricted capital flows, or if the controls were regularly circumvented. For example, Edwards (1999) explains that according to the four *AREAER* binary indicators, Chile, Mexico and Brazil were subject to the same degree of capital controls between 1992 and 1994. In reality, not only did capital control policy undergo important changes within

these countries, but also the controls between these countries were extremely different. Brazil employed an arcane list of restrictions on inflows and outflows, while Chile directed its *Encaje* principally at short-term inflows, and Mexico in practice had free capital mobility.

In 1998 the *AREAER* expanded the four subcategories in its summary table and now offers fourteen binary indicators for de facto controls on: capital market securities, collective investment instruments, commercial credits, foreign direct investment, and real estate transactions among others. Johnston and Tamirisa (1998) extrapolate these fourteen disaggregated binary indicators back in time through 1996 to create a new capital control index. Miniane (2004) uses the same approach as Johnston and Tamirisa (1998), expanding the index to include data through 1983, but for only 34 countries. Although more accurate than the four-point scale of capital controls, Johnston and Tamirisa (1998) and Miniane's (2004) indices are still unadjusted for intensity and efficacy. Moreover, they are either severely limited by the number of years (Johnston and Tamirisa 1998) or number of countries (Miniane 2004) they exclude. In summary, these indices are both imperfect as they still do not allow the accurate and simultaneous study of capital control policy worldwide.

Chinn and Ito (2007) create the first intensity-modified measure of financial openness. They also expand the range of data, providing capital control measures for 181 countries from 1970 through 2005. Moreover, unlike the original four-point measures of capital controls, the *KAOPEN* variable correctly demonstrates the gradual liberalization of capital controls across all countries in each decade since 1970.[8] Because of these improvements, the *KAOPEN* variable should provide a relatively accurate representation of capital controls in this study. In the next section, I discuss the construction of *KAOPEN*.

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[8] *KAOPEN* demonstrates the gradual liberalization of capital controls in each decade (1970-79, 1980-89, 1990-99, 2000-05) for the aggregate of countries as well as for subgroups of (a) Central/Eastern European (ex-planning) countries, (b) South Asian/Middle Eastern/African countries, (c) emerging countries, (d) less-developed countries, and (e) industrial countries. Asia-Pacific countries demonstrate liberalization in each period except from 2000-05. However, this apparent tightening of capital controls is consistent with policy initiatives that followed the Asian Financial Crisis beginning in 1997. See Figure 3.

2.2.2   Construction of Chinn and Ito's (2007) *KAOPEN* Variable

The following section on the construction of the KAOPEN variable closely follows Chinn and Ito's (2007) original discussion.

Chinn and Ito (2007) derive their measure of financial openness by assessing the intensity of each of the four categories of controls listed in the original *AREAER* summary tables: (k_1) multiple exchange rates, (k_2) restrictions on current account transactions, (k_3) restrictions on capital account transactions, and (k_4) regulatory requirements on the surrender of export proceeds. Because the index focuses on *financial openness* rather than the degree of *restriction*, the highest-intensity controls are assigned the *minimum* value, while non-existent controls are assigned the *maximum* value. Furthermore, in order to reflect the delay (latency period) for a new policy to realize its full effect, Chinn and Ito (2007) use a five-year moving average for restrictions on capital account transactions (k_3). In other words, instead of using k_3 at time t to construct $KAOPEN_t$, they instead use $SHAREk_3$.

(1) $SHAREk_3$ = $(k_{3,t} + k_{3,t-1} + k_{3,t-2} + k_{3,t-3} + k_{3,t-4}) / 5.$

The authors use principal components analysis to reduce the multidimensional data set [$k_{1,t}$, $k_{2,t}$, $SHAREk_{3,t}$, and $k_{4,t}$] to a single dimension $KAOPEN_t$. In principal components analysis each attribute of the data set is first mean centered. Then an orthogonal linear transformation converts the data set to a new coordinate system so that the greatest variance in any attribute comes to lie on the first coordinate, or the first principal component. By definition, the aggregate measure of financial openness $KAOPEN_t$ including all years and countries has a mean of zero. Its value also *increases* when capital controls are *more* liberal.

Chinn and Ito (2007) use the first eigenvector for *KAOPEN* to demonstrate that their measure of financial openness is not merely driven by changes in the moving average of restrictions on capital account transactions ($SHAREk_3$). As per the mathematical definition

of an eigenvector, each component of the eigenvector represents the weight of that particular attribute in determining *KAOPEN*. Because the first eigenvector for *KAOPEN* is $(k_{1,t}, k_{2,t}, SHAREk_{3,t}, k_{4,t}) = (0.25, 0.52, 0.57, 0.58)$, it is reasonable to deduce that the inclusion of $k_{1,t}, k_{2,t},$ and $k_{4,t}$ in the index allows it to more accurately represent the intensity of the capital controls. Chinn and Ito (2007) argue that the existence of different types of restrictions (k_1, k_2, or k_4) alongside k_3 signals more intense capital controls. For example, a county might support its capital account controls (k_3) by imposing restrictions on current account transactions (k_2) to prevent the private sector from bypassing the capital controls.

In summary, Chinn and Ito's (2007) measure of financial openness is arguably an improvement over previous binary indices because of its vast coverage of countries and time. More importantly it considers the intensity and not simply the existence of capital controls. Therefore, I am reasonably confident that *KAOPEN* is the measure of capital controls that most accurately reports their existence and intensity worldwide and across time.

2.3   Propensity Score Matching Using Leuven and Sianesi's (2003) *PSMATCH2*

The ultimate goal of this study is to evaluate the treatment effect of changes in capital control policy on real exchange rate volatility. If changes in capital control policy were exogenous, it would be possible to estimate the average treatment effect on the treated (ATT) simply by subtracting the average volatility of non-treated countries from the average volatility of treated countries.

(2) ATT   = $E[P_{i1} \mid C_i{=}1] - E[P_{i0} \mid C_i{=}1]$
      C =   dummy for country of observation
      $P_{i1} \mid C_i{=}1$ =   volatility given a change of policy in country *1*
      $P_{i0} \mid C_i{=}1$ =   volatility in country *1* if there had not been a change in policy

The above construction is analogous to a clinical experiment where the left-hand term ($P_{i1} \mid C_i{=}1$) represents the group of subjects randomly assigned to take the test drug (*change in*

capital controls), and the right-hand term (P_{i0} | C_i=1) represents the group of subjects randomly assigned to the placebo (*no change in capital controls*). However, because changes in capital control policy are nonrandom it is impossible to determine what the volatility would have been in a given country, had that country *not* changed its capital control policy. Moreover, it is implausible in the macroeconomy to design an experiment to test this treatment effect by randomly assigning countries to either change or maintain their capital control policy.

Given that capital control policy is endogenous to other macroeconomic factors, we would generate biased estimates by simply subtracting the average volatility of countries that changed their capital controls from the average volatility of countries that maintained their policy. In other words, because capital control policy is systematically correlated with a set of macroeconomic factors that also affect volatility, we are confronted with the problem of *selection on observables*. In this study I use Leuven and Sianesi's (2003) propensity score matching tool for Stata (*PSMATCH2*) to address the issue of self-selection in policy adoption.

The intuition for propensity score matching is to recreate *Equation 2* with a mock control group to simulate a randomized experiment. If we assume that conditional on *attributes* X_i the outcomes are independent of the particular country C_i, then it is also possible to observe the average treatment effect with the following equation:

(3)   ATT   = $E[P_{i1}$ | C_i=1, $X_i]$ - $E[P_{i0}$ | C_i=0, $X_i]$
       P_{i1} | C_i=1, X_i =   volatility in country *1* which changed its policy, under
                                         conditions X_i
       P_{i0} | C_i=0, X_i =   volatility in country *0* which maintained its policy,
                                         under the same conditions X_i

Using the mock experiment in *Equation 3*, it is possible to estimate the average treatment effect by creating 'couples' from data points by matching 'male' data points (countries that liberalized capital controls) with 'female' data points (countries that maintained capital

controls) conditional on having the same attribute values (X_i) to eliminate the selection bias. The average treatment effect is the average difference in volatility between the 'male' and 'female' data points of each 'couple'.

In order to facilitate the matching process as the number of matching variables in X_i increases, Leuven and Sianesi (2003) transform the values for the matching variables (attributes) in X_i into a propensity score.[9] A *propensity score* is simply the probability of a change in capital control policy given country C_i's set of attribute values X_i. In *PSMATCH2*, the propensity to make a change in capital control policy is estimated using a logit model. Matching the 'couples' using propensity scores instead of with raw attributes changes *Equation 3* to the following:

(4)   ATT   = $E[P_{i1} \mid C_i=1, \ pr(X_i)]$ - $E[P_{i0} \mid C_i=0, pr(X_i)]$
$P_{i1} \mid C_i=1, X_i$ =   volatility in country *1* which changed its policy, given the propensity to change its policy $pr(X_i)$
$P_{i0} \mid C_i=0, X_i$ =   volatility in country *0* which maintained its policy, given the same propensity to change its policy $pr(X_i)$

Following the above discussion on the intuition for propensity score matching, there are three basic steps in Leuven and Sianesi's (2003) *PSMATCH2* tool. First, *PSMATCH2* uses a logistic regression to predict the propensity to make a change in capital controls. Second, *PSMATCH2* matches data 'couples' based on their propensity scores and a given *caliper*, which is the maximum allowable difference in propensity between the 'male' and 'female' components of each 'couple'. Third, *PSMATCH2* determines the treatment effect by averaging the difference in volatility between the 'male' (treated) and 'female' (untreated) data points of each 'couple'.

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[9] Rosenbaum and Rubin (1983) first proposed that treated units and control units could be matched with propensity scores instead of using a set of basic attributes X_i. However, Leuven and Sianesi (2003) constructed the first working model for propensity score matching in Stata.

Recent papers that use Leuven and Sianesei's (2003) propensity score matching tool include Lin and Ye (2007 forthcoming) and Guo et al. (2005). Lin and Ye (2007) use *PSMATCH2* to show that inflation targeting does not significantly affect either inflation or inflation variability. Guo et al. (2005a) use *PSMATCH2* to evaluate the effectiveness of substance abuse services for child welfare clients. Guo (2005b) also provides a guide to the Stata code for *PSMATCH2*.

3. DATA DESCRIPTION

In this section I describe the three principal elements of data used in this study. First I define real exchange rates, and explain the calculation of real exchange rate volatility. Second, I present summary statistics for the central explanatory variable: Chinn and Ito's (2007) measure of financial openness. Note that the construction of *KAOPEN* is discussed in detail in Section 2.2.2. Third, I describe the matching variables used in the logistic regression to determine the propensity to change capital control policy.

3.1 Real Exchange Rate Volatility

The central dependent variable in this study is real exchange rate volatility. I begin with normalized and trade-weighted monthly real effective exchange rates ($RER_{i,t}$) based on relative consumer prices from the International Monetary Fund Statistical Database. The IMF defines $RER_{i,t}$ as follows.

$$(5)^{10} \quad \delta_{ij,t} = \Delta \ln(e_{ij,t})$$
$$(6)^{11} \quad \pi_{i,t} = \Delta \ln(P_{i,t})$$
$$(7)^{12} \quad \varepsilon_{ij,t} = \delta_{ij,t} + \pi_{j,t} - \pi_{i,t}$$
$$(8)^{13} \quad \varphi_{i,t} = \Sigma \, J_{ij} \cdot \varepsilon_{ij,t}$$

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[10] *e* is exchange rate between country *i* and its trading partners *j*.

[11] $\pi_{i,t}$ is inflation in country *i* at time *t*. *P* is the price level.

[12] $\varepsilon_{ij,t}$ represents $\Delta \ln(RER)$ between countries *i, j* at time *t*.

[13] *J* is the trade weight for country *i* of country *j*. Note that $J_{ij} \neq J_{ji}$.

(9) $\varphi_{i,t}$ $=$ $\ln(RER_{i,t}) - \ln(RER_{i,t-1})$

(10) $RER_{i,t} =$ $RER_{i,t-1} \cdot \exp(\varphi_{i,t})$

Although IMF data is available through 1970, it is extremely limited in the number of countries through 1975. Therefore, I only consider data in the period from 1975-2005. For this period data is offered for 91 countries. Note that although the IMF collects data for more than 180 countries, many choose not to disclose the necessary information to determine historical monthly real effective exchange rates.

I convert monthly values of $RER_{i,t}$ into annual measures of real exchange rate volatility ($RERvol_{i,t}$) by taking the standard deviation of the natural logarithm of the monthly $RER_{i,t}$ values within each year. In other words, my measure of real exchange rate volatility is the percentage deviation from the average real exchange rate in any given year. The mathematical formula for $RERvol_{i,t}$ is as follows.

(11) $\mu_{i,t}$ $=$ $(^1/_{12}) \Sigma \ln(RER_{i,t})$

(12) $\sigma_{i,t}$ $=$ $\sqrt{[(^1/_{12-1}) \Sigma (\ln(RER_{i,t}) - \mu)^2]}$

$\sigma_{i,t}$ $=$ $RERvol_{i,t}$

Finally, I also create a de-trended measure of real exchange rate volatility ($RER\hat{vol}_{i,t}$) by removing the time and country-specific fixed effects according to the following formula.

(13) $RER\hat{vol}_{i,t} =$ $RERvol_{i,t} - \hat{Country}_i - \hat{Year}_t$

By defining real exchange rate volatility ($RERvol_{i,t}$) as the percentage deviation from the average in each sub-period, I am implicitly removing the long-term trend from my calculation of volatility. Although Clark et al. (2004) note that there is no consensus in the field on how to define volatility; the convention is nonetheless to measure deviations from long-term trends. Rose (2000) justifies that the negative macroeconomic effects associated with volatility are characteristic of deviations from long-term trends rather than changes

which are consistent with the trend. Therefore, I am confident that my definition of real exchange rate volatility accurately captures macroeconomic instability.

Summary statistics for real exchange rate volatility ($RERvol_{i,t}$) are presented in Table 1. Additionally, real exchange rate volatility versus time is plotted in Figure 2.

3.2 Financial Openness

The central explanatory variable is Chinn and Ito's (2007) *KAOPEN* measure of financial openness. By construction, *KAOPEN* is positive when capital controls are relatively liberal, and negative when capital controls are tight. Table 1 includes summary statistics for the subset of 91 countries from 1975-2005 used in this study (limited by IMF data on real exchange rates) alongside the summary statistics for the complete 35-year index of all 181 countries. Although I use less than half of Chinn and Ito's (2007) original data (2422 of 5102 values) I am confident from the summary statistics that I have a representative sample.

I graph average financial openness versus time in Figure 3 to emphasize the accuracy of the *KAOPEN* variable in reflecting de jure capital controls. Recall from Section 2.2.1 that Chinn and Ito's (2007) index is unique in that it correctly demonstrates the global liberalization of capital controls since the 1970s.

3.3 Matching Variables Used to Determine the Propensity Score

I use nine additional matching variables (covariates) in a logistic regression to determine the propensity for country i at time t to make a change in capital controls. These variables are: (a) the natural logarithm of real GDP per capita, (b) the natural logarithm of population, (c) openness to cross-border trade, (d) capital inflows, (e) months of foreign reserves, (f) debt as a fraction of GDP, (g) CPI inflation, (h) a binary variable for floating exchange rate mechanism, and (i) a binary variable for whether a currency crisis is taking

place. I discuss the construction, sources and rationale for including each of these variables below. I also provide summary statistics for these variables in Table 1.

3.3.1 Natural Logarithm of Real GDP Per Capita (lnGDPn$_{i,t}$)

The source for real GDP per capita data is the Penn World Table. I calculate the natural logarithm of real GDP per capita from the raw data before including it in the logistic regression to calculate the propensity for changes in capital control policy. I include this measure in the propensity calculation because of the tendency for developed countries to have more liberal capital controls. For example, Bartolini and Drazen (1997) note that only 3 of 24 OECD countries had in place some form of capital control policy in 1995, while 126 of 158 developing countries used capital controls that same year.

3.3.2 Natural Logarithm of Population (lnPOP$_{i,t}$)

I calculate the natural logarithm of population from raw population data available in the Penn World Table. I include population because extremely small countries may be more reliant on foreign capital, and would therefore be more likely adopt liberal policies with respect to capital flows.

3.3.3 Openness to Cross-Border Trade (OPENTRD$_{i,t}$)

$$(14) \quad \text{OPENTRD}_{i,t} \quad = \quad \text{(Imports + Exports)} \; / \; \text{GDP}$$

Openness to cross-border trade is defined above as the sum of imports plus exports, divided by GDP. Data for trade openness is available from the World Bank World Tables. Hau (2002) presents a model in which smaller real exchange rate movements follow supply-side shocks if the economy is more open to international trade. Also, Quinn (1997) notes that openness to cross-border trade is moderately correlated with capital account openness. Therefore, it is worthwhile to consider trade openness in the propensity calculation.

3.3.4 Capital Inflows (CAPF$_{i,t}$)

I use data from Rose (2000) to determine capital inflows. The measure includes foreign direct investment, portfolio investments, and loans. I include a variable for capital flows in the propensity calculation because the volume and volatility of capital flows are important factors in the determination of capital control policy (Neely 1999)

3.3.5 Months of Foreign Reserves (RESmo$_{i,t}$)

Months of foreign reserves are defined as the total value of foreign reserves divided by the value of imports per month. This data is available in Rose (2000). I include this variable because low reserves may signal a currency crisis and may also lead to a tightening of capital control policy as a defensive mechanism.

3.3.6 Debt as a Fraction of GDP (DEBT$_{i,t}$)

The measure of debt divided by GDP is also available in Rose (2000). I include this data in the propensity calculation for the same reason as GDP per capita. That is, more developed countries with less debt may be more likely to have liberal capital control policies.

3.3.7 CPI Inflation (CPIinf$_{i,t}$)

Data for CPI inflation is available from the World Bank World Tables. I include this data because inflation is an important signal of macroeconomic stability. Moreover, Edwards (1993) notes that market-distorting policies such as capital controls are often related to high levels of inflation.

3.3.8 Binary Variable for Floating Exchange Rate Mechanism (EXRbin$_{i,t}$)

I use a binary variable for de jure floating exchange rate mechanisms available in Rose (2000). The variable takes on a value of *1* if the exchange rate is floating, and *0* if the

exchange rate is fixed, managed, or intermediate. This measure is based on data from the IMF Annual Report of Exchange Arrangements and Exchange Restrictions. I include the exchange rate mechanism because the exact way in which capital inflows cause a real exchange rate appreciation is dependent on the nature of the exchange rate system. With a fixed exchange rate, the increased availability of foreign resources will result in an accumulation of reserves, monetary expansion, and increased inflation. These factors will in turn, eventually cause the real exchange rate to appreciate. Conversely, under a floating exchange rate system, nominal and real exchange rate appreciations occur simultaneously (Frenkel et al. 2001). Moreover, Quinn (1997) notes that the use of fixed exchange rates is often coupled with the use of capital controls.

3.3.9   Binary Variable for Currency Crises (CCRbin$_{i,t}$)

I use Rose's (2000) binary variable for currency crises which takes on a value of *1* if a currency crisis is occurring and *0* if not. The variable is constructed from journalistic and academic episodes of past currency crises. I include this variable because currency crises are an important determining factor of capital control policy, as evidenced after the Asian Financial Crisis when Malaysia, Thailand, and Indonesia among others tightened their controls (as per Palma 2000 and also reflected in Chinn and Ito's *KAOPEN* index).

4.   E M P I R I C A L   S P E C I F I C A T I O N

In this section I discuss two sets of tests to explore the relationship between real exchange rate volatility and capital control policy. In Section 4.1, I present the results of preliminary tests, including the correlation of financial openness and real exchange rate volatility. I also calculate a measure of long-term volatility following different types of changes in capital control policy. In Section 4.2, I explore whether the changes in capital

controls *caused* the changes in volatility that I observed in the preliminary tests. In order to discuss the treatment effect of capital control policy on real exchange rate volatility, I apply Leuven & Sianesi's (2003) propensity score matching technique to the data. Specifically, I use propensity score matching to answer two questions: (a) what is the treatment effect of *changing* (liberalizing or tightening) capital control policy on real exchange rate volatility; and (b) what is the treatment effect of *using* tight or liberal capital controls on real exchange rate volatility?

4.1   Capital Control Policy Changes and Real Exchange Rate Volatility

In order to understand the basic relationship between capital controls and real exchange rate volatility, I begin by exploring the correlation between relative financial openness ($KAOPEN_{i,t}$) and de-trended[14] real exchange rate volatility ($RERv\hat{o}l_{i,t}$) in Figure 4. It is important to emphasize that financial openness is a relative measure because Chinn and Ito's *KAOPEN* index is ordinal rather than cardinal. The correlation coefficient is -0.181, suggesting a weak (yet still statistically significant[15]) negative relationship between the relative degree of capital controls and real exchange rate volatility. Note that the statistically significant negative relationship is still preserved (ρ=-0.176) when outliers with de-trended volatilities greater than 20% are removed from the calculation. The implication from this test is that countries with more liberal capital controls are associated with less real exchange rate volatility. Nevertheless, because financial openness and real exchange rate volatility are both endogenous, it is impossible to infer causality from this correlation alone.

In a second preliminary test, I examine the long-term change in volatility for countries that make considerable changes in their capital control policy. I define long-term

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[14] I remove the time and country-specific fixed effects from real exchange rate volatility ($RERvol_{i,t}$) to calculate de-trended real exchange rate volatility ($RERv\hat{o}l_{i,t}$). Please refer to Section 3.1 for more details.
[15] $p < 0.05$

change in real exchange rate volatility ($\Delta RERvolLT_{i,t}$) as the difference between the average

volatility for the three years prior to the policy change and the average volatility for the three

years following the policy change. The mathematical formula is as follows.

$$(15) \quad \Delta RERvolLT_{i,t} = \ [(RERvol_{i,t-3} + RERvol_{i,t-2} + RERvol_{i,t-1}) \ / \ 3]$$
$$- [(RERvol_{i,t} + RERvol_{i,t+1} + RERvol_{i,t+2}) \ / \ 3]$$

According to the construction of the long-term change in volatility variable, *positive* values of

ΔRERvolLT$_{i,t}$ signify a long-term *decrease* in volatility following the policy change, while

negative values signify an increase in volatility. In Figure 5a, I display the long-term change in

volatility at year *t* for all countries that significantly *liberalized* their capital controls during year

t by at least +0.5 standard deviations of the *KAOPEN* index.[16] In Figure 5b, I display the

change in volatility for all countries that *tightened* their capital controls by at least -0.5SD of

KAOPEN.

I find that there is a statistically significant[17] decrease in long-term real exchange rate

volatility of approximately 1% for countries that liberalized their capital controls in Figure

5a. Countries that tightened their capital controls in Figure 5b also experienced an average

decrease in long-term volatility of approximately 0.1%, but this result is not statistically

significant at the 95% level. Although it makes sense that countries in Figure 5a experience a

decrease in long-term volatility after a market-promoting policy change, it is more difficult to

interpret the positive result for countries that made a market-interfering policy change in

Figure 5b. Nevertheless, even if long-term real exchange rate volatility were to improve on

average after a country tightens its capital controls, it is still impossible to determine from

this test whether the decision to tighten capital controls *causes* the decrease in volatility. In

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[16] The standard deviation of the *KAOPEN* index for the countries and years used in this study is 1.533. Therefore, in order to examine changes greater than +0.5SD of *KAOPEN*, I consider data points where $KAOPEN_{i,t} - KAOPEN_{i,t-1} > 0.767$.
[17] $p < 0.05$.

order to determine causality, I use Leuven and Sianesi's (2003) propensity score matching technique.

4.2   Treatment Effects of Capital Control Policy Changes

I use propensity score matching to reconsider the question of whether changes in capital control policy cause changes in de-trended real exchange rate volatility. To simplify the propensity score matching process, I consider capital control liberalization and tightening separately.

In order to examine the treatment effect of capital control liberalization, I start a new database, which only includes countries that maintained or significantly liberalized their capital controls by more than 0.25SD of *KAOPEN*.[18] According to the three basic steps in Leuven and Sianesi's (2003) *PSMATCH2* tool, I first, use a logistic regression with the matching variables listed in Section 3.3[19] to predict the propensity for countries to liberalize their capital controls. Results for the logistic regression step are listed in Table 2. Next, I repeat the data-matching step several times, varying the caliper (maximum allowable difference in propensity scores between treated and untreated data points in each couple) from 0.01 to 0.005, or approximately 0.1 standard deviations of the propensity score. Smaller calipers ensure smaller differences in the propensity to liberalize between treated and untreated data points for each couple. In the third step, *PSMATCH2* determines the treatment effect by averaging the differences in volatility between the treated and untreated data points of each couple created in the second step. I repeat this process to determine the

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[18] In order to include only countries that maintained or liberalized their capital controls, I temporarily discard data points where $KAOPEN_{i,t} - KAOPEN_{i,t-1} < 0$. I define significant liberalizers or *treated* units as all data where $KAOPEN_{i,t} - KAOPEN_{i,t-1} \geq 0.25$SD of *KAOPEN* (0.383). I define countries which maintained their capital controls or *untreated* units as the remaining data where $0 \leq KAOPEN_{i,t} - KAOPEN_{i,t-1} < 0.25$SD of *KAOPEN* (0.383).

[19] The matching variables are: (a) the natural logarithm of real GDP per capita, (b) the natural logarithm of population, (c) openness to cross-border trade, (d) net capital flows, (e) months of foreign reserves, (f) debt as a fraction of GDP, (g) CPI inflation, (h) a binary variable for floating exchange rate mechanism, and (i) a binary variable for whether a currency crisis is taking place. Details on the source and construction of these variables is available in Section 3.3

treatment effect for tightening, using a new database that only includes countries that tightened or maintained their capital controls. I list the results for these propensity score matching tests in Table 3.

The average treatment effect for countries that tightened their capital controls is an increase in real exchange rate volatility of approximately 2.5% in the year following the policy change. Results are statistically significant at the 95% level for all calipers. The results for liberalizers are more ambiguous. Although the average treatment effect for all calipers is a decrease in real exchange rate volatility of approximately 0.1%, the effect is statistically insignificant even at the 90% level. Therefore, while I can be certain that tightening capital controls *causes* an increase in real exchange rate volatility, I cannot be certain that there is a non-zero effect for the liberalization of capital controls.

In the second propensity score matching exercise, I explore the treatment effect of simply *having* liberal, moderate, or tight capital controls. I define liberal capital controls as the top third (with the greatest financial openness) of the *KAOPEN* index, moderate capital controls as the middle third, and tight capital controls as the bottom third. This second exercise is important because it explains whether the negative correlation between financial openness and real exchange rate volatility (Figure 4) is *caused* by a country's choice in capital controls, or instead due to other macroeconomic factors.

I illustrate the difference between the first and second tests in Figure 6. The large outlined diamonds depict the first experiment in which I determine the treatment effect for capital control liberalization by calculating the difference in real exchange rate volatility between countries that liberalized and countries that maintained their capital controls, given the same propensity to liberalize their capital controls. The large solid diamonds depict the second experiment in which I determine the treatment effect for using tight capital control

policy by calculating the difference in volatility between countries that use tight and countries that use moderate controls, given the same propensity to use tight controls.

In order to determine the treatment effect of simply having in place particular capital control policies, I repeat the process described above for the first exercise. However, in this variation of the test, I use separate data sets to explore the treatment effect for choosing liberal instead of moderate capital controls, and for using tight instead of moderate controls. I list the results for these tests in Table 2.

For all calipers, the treatment effect for using liberal instead of moderate capital controls is an average decrease in real exchange rate volatility of approximately 1%. This effect is significant at the 90% confidence level in all trials. Although the effect is more ambiguous when a small caliper is used for countries that use tight capital controls, I can be relatively confident from the trials with large and medium calipers that the use of tight capital control policy causes countries to have higher real exchange rate volatility. Specifically, the treatment effect for using tight instead of moderate capital controls is an average increase in real exchange rate volatility of approximately 1%.

Considering the results of the two propensity score matching experiments together, I am reasonably confident that using a relatively liberal capital control policy causes a country to have lower real exchange volatility. Moreover, the treatment effect of further liberalization is likely a decrease in real exchange rate volatility. I also find that choosing tight capital controls as well as the act of tightening capital controls both cause increases in real exchange rate volatility. The magnitude of the average treatment effect for these tests is a change in real exchange rate volatility of approximately 1%. These results are consistent with the neoclassical expectation that market-promoting policy is associated with better macroeconomic stability.

5. CONCLUDING REMARKS

In this study I use a novel statistical approach to determine the treatment effect of capital control policy choices on real exchange rate volatility. I find that liberalizing capital controls and maintaining a liberal policy are both associated with decreases in real exchange rate volatility. Conversely, tightening capital controls and maintaining a tight policy are both associated with increases in real exchange rate volatility. Although few tests are statistically significant at the 95% level, the majority of tests show that that these results are significant at the 90% level. In making a first attempt to understand the treatment effect of capital control policy on real exchange rate volatility, this study will hopefully provide a directive for future empirical and theoretical research to describe the mechanism by which the choice of capital control policy affects the stability of the macroeconomy.

T A B L E S & F I G U R E S

Table 1.
Summary Statistics for all Data

VARIABLE	OBS.	MIN.	25TH %ILE	MEDIAN	75TH %ILE	MAX.	MEAN	STANDARD DEV.
$KAOPEN_{i,t}$*	5102	-1.767	-1.216	-0.724	1.501	2.602	0.000	1.510
$KAOPEN_{i,t}$	2398	-1.767	-1.105	-0.062	1.560	2.603	0.099	1.533
$RERvol_{i,t}$	2398	0.002	0.013	0.021	0.037	0.896	0.035	0.054
$lnGDPn_{i,t}$	2314	5.657	7.804	8.657	9.415	10.585	8.533	1.095
$lnPOP_{i,t}$	2356	10.564	14.541	15.823	16.875	20.982	15.510	2.025
$OPENTRD_{i,t}$	2314	6.320	48.094	69.594	104.683	427.857	81.657	50.905
$CAPF_{i,t}$	1267	0	1.48×10^7	7.07×10^7	3.62×10^8	5.49×10^{10}	8.85×10^8	4.22×10^9
$RESmo_{i,t}$	1299	0.027	1.554	2.719	4.430	15.442	3.365	2.536
$DEBT_{i,t}$	1239	0	35.117	54.287	83.817	414.945	66.799	48.493
$CPIinf_{i,t}$	2356	-17.785	2.719	6.715	14.047	244.551	13.208	22.857
$EXRbin_{i,t}$	1825	0	*Binary Variable*		1		0.203	0.402
$CCRbin_{i,t}$	1617	0	*Binary Variable*		1		0.048	0.213

Description of variables:

$KAOPEN_{i,t}$*   Chinn and Ito (2007) index of financial openness (complete 35-year index of 181 countries)
$KAOPEN_{i,t}$ Chinn and Ito (2007) index of financial openness for countries and years used in study
$RERvol_{i,t}$ Real exchange rate volatility, (standard dev. of natural log of monthly real exchange rates)
$lnGDPn_{i,t}$ Natural logarithm of GDP per capita, in billions
$lnPOP_{i,t}$ Natural logarithm of population, in millions
$OPENTRD_{i,t}$ Openness to cross-border trade: (Imports + Exports) / GDP, in %
$CAPF_{i,t}$ Capital inflows
$RESmo_{i,t}$ Months of foreign reserves: (Total Reserves / Monthly Imports), in months
$DEBT_{i,t}$ Debt as a percentage of GDP: (Total Debt / GDP), in %
$CPIinf_{i,t}$ Consumer price index inflation, in %
$EXRbin_{i,t}$ Binary variable: *1* for floating exchange rate, *0* for fixed or managed exchange rate
$CCRbin_{i,t}$ Binary variable: *1* indicates that a currency crisis is taking place, *0* indicates no currency crisis

For more information on the construction of the above variables, please refer to Section 3.

Table 2.
Logistic Regressions Predicting the Propensity to Change Capital Control Policy
(Step 1 of Propensity Score Matching Process)

MATCHING VARIABLE	(A) TREATMENT EFFECT FOR <u>CHANGING</u> POLICY		(B) TREATMENT EFFECT FOR <u>HAVING</u> POLICY	
	1. Propensity to <u>Liberalize</u> vs. No Change	2. Propensity to <u>Tighten</u> vs. No Change	1. Propensity to Use <u>Liberal</u> vs. Moderate Control	2. Propensity to Use <u>Tight</u> vs. Moderate Control
$lnGDPn_{i,t}$	0.394* (0.126)	0.459* (0.169)	0.193 (0.199)	-0.549* (0.139)
$lnPOP_{i,t}$	-0.323* (0.146)	-0.476* (0.186)	-0.224* (0.096	0.213* (0.075)
$OPENTRD_{i,t}$	0.013* (0.003)	0.003 (0.004)	0.014* (0.004)	-0.016* (0.003)
$CAPF_{i,t}$	-1.58×10^{-11} (2.77×10^{-11})	-5.14×10^{-11} (6.68×10^{-11})	1.93×10^{-10} (1.52×10^{-10})	1.87×10^{-10} (1.26×10^{-10})
$RESmo_{i,t}$	0.108* (0.038)	0.108* (0.049)	0.172* (0.045)	-0.069 (0.037)
$DEBT_{i,t}$	-0.005* (0.002)	0.005 (0.003)	-0.005 (0.003)	0.003 (0.002)
$CPIinf_{i,t}$	0.012* (0.003)	0.000 (0.005)	0.010* (0.004)	-0.008 (0.003)
$EXRbin_{i,t}$	1.191* (0.232)	0.332 (0.327)	0.219 (0.315)	-0.843* (0.230)
Sample Size	739	646	702	875

In set (A) I define countries that liberalized or tightened their capital controls as countries that experienced a change in *KAOPEN* of more than ±0.25SD (0.383). In set (B) I define countries with liberal controls as the top third and most financially open data points of the *KAOPEN* index, moderate controls as the middle third, and tight controls as the bottom third. For more detail, please refer to Section 4.2.

The sample size refers to the number of *treated* units (e.g. countries liberalized capital controls) plus *untreated* units (e.g. countries that maintained capital controls) assigned *propensity scores* (e.g. likelihood to liberalize capital controls) in that particular test. Note that only the minority of data points assigned propensity scores in this process is eventually matched into pairs in Step 2. For example, in Column A1 if the caliper is 0.1 (maximum allowable distance between propensities of treated and untreated units), then 159 pairs are created. That is to say that only 318 (159 treated units + 159 untreated units) of 739 data points are used in the eventual calculation of the treatment effect in Step 3.

* Indicates statistical significance at the 95% level.

Table 3.
Treatment Effects
(Step 3 / Results for Propensity Score Matching Process)

| CALIPER SIZE	(A) TREATMENT EFFECT FOR <u>CHANGING</u> POLICY		(B) TREATMENT EFFECT FOR <u>HAVING</u> POLICY	
	1. Treatment Effect for <u>Liberalize</u> vs. No Change	2. Treatment Effect for <u>Tighten</u> vs. No Change	1. Treatment Effect for <u>Liberal</u> vs. Moderate Control	2. Treatment Effect for <u>Tight</u> vs. Moderate Control
0.1 Large Caliper	-0.001 (0.006) *159 Pairs*	0.024** (0.009) *81 Pairs*	-0.010* (0.006) *74 Pairs*	0.010* (0.008) *186 Pairs*
0.05 Medium Caliper	-0.002 (0.007) *157 Pairs*	0.024** (0.009) *81 Pairs*	-0.010* (0.006) *73 Pairs*	0.012** (0.005) *172 Pairs*
0.1SD of Propensity Score (~0.005) Small Caliper	0.000 (0.006) *151 Pairs*	0.025** (0.009) *78 Pairs*	-0.009* (0.006) *61 Pairs*	0.004 (0.007) *168 Pairs*

Treatment effect is the average difference in volatility between the treated (liberalized or tightened) component and the untreated (no change or moderate control) component of each pair. *Positive* values indicate an *increase* in volatility when treated, while *negative* values indicate a *decrease* in volatility when treated. For more detail, please refer to Section 4.2.

Note that a large caliper (maximum allowable difference in propensity score between treated and untreated data points in each couple) includes a greater number of less-accurately matched pairs, while a small caliper ensures a greater degree of similarity within pairs but uses less data.

* Indicates statistical significance at the 90% level.
** Indicates statistical significance at the 95% level.

Figure 1.
Neoclassical Model for Capital Account Liberalization
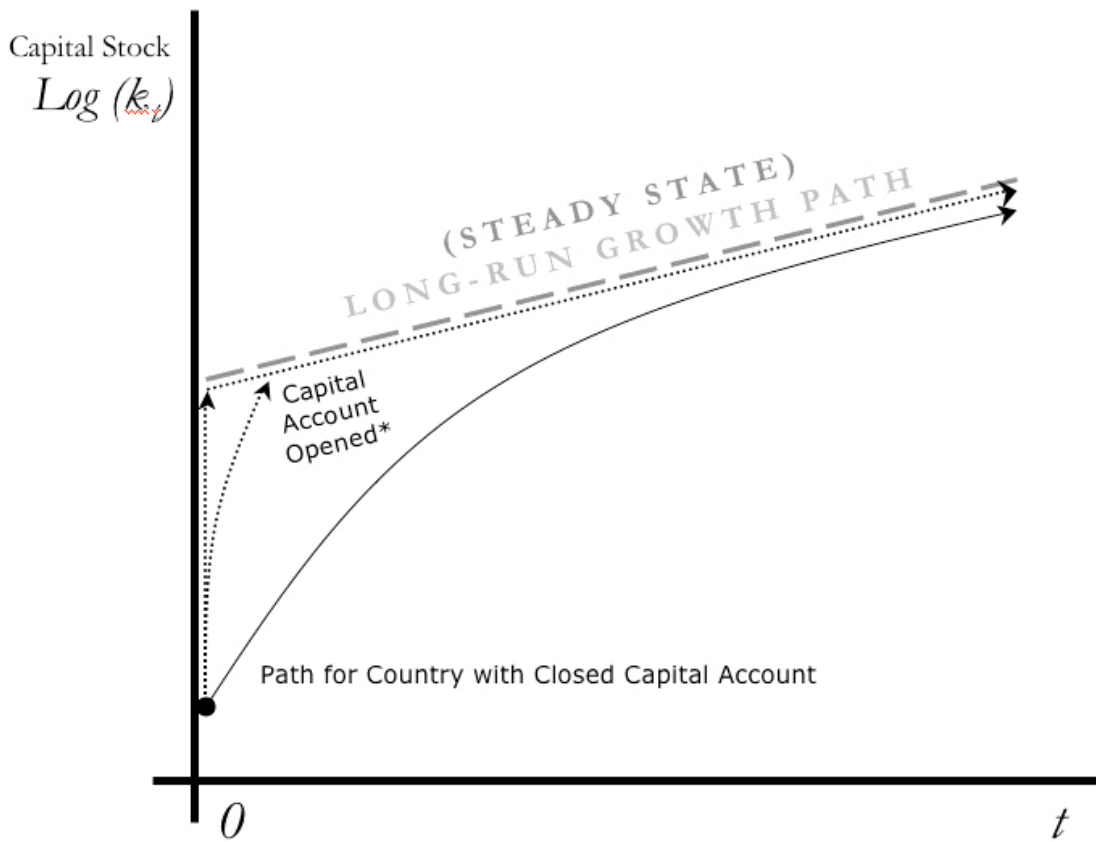
*If the capital account is opened and there are no adjustment costs, the capital stock follows the dotted path on the left, and immediately increases to the steady state. If there are adjustment costs, when the capital account is opened the capital stock will adjust rapidly but not immediately to the steady state, following the dotted path on the right.
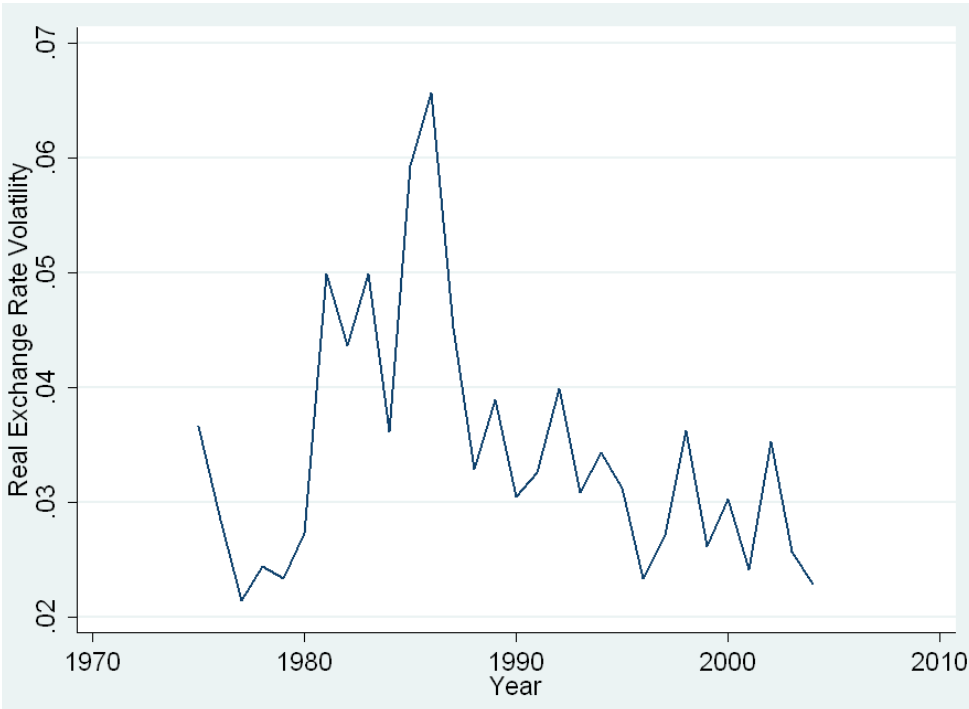
Figure 2.
Real Exchange Rate Volatility Versus Time

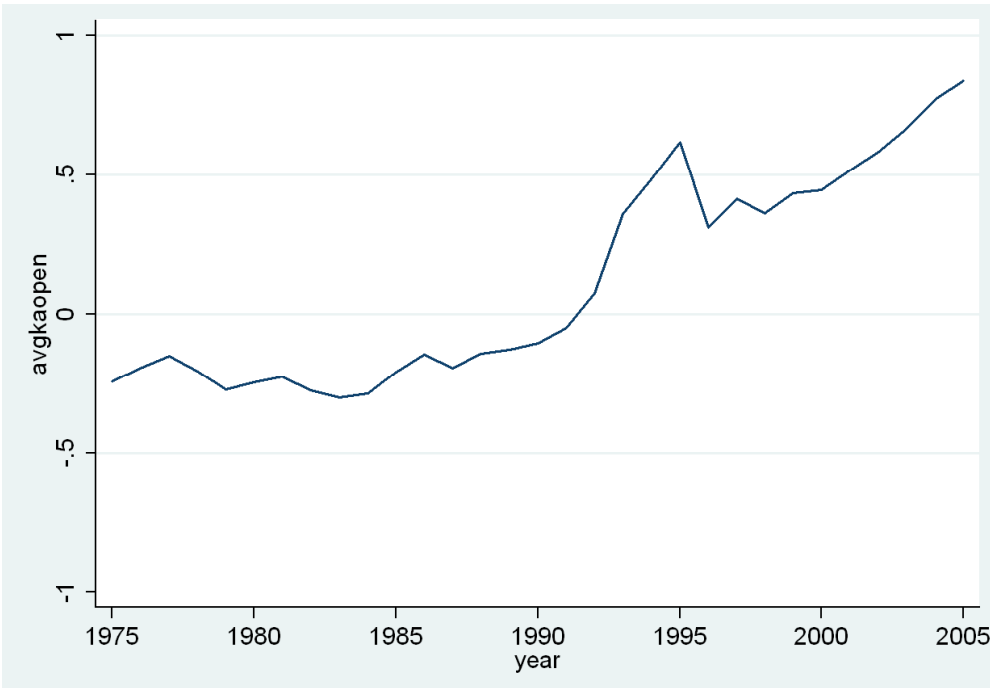**Figure 3.**
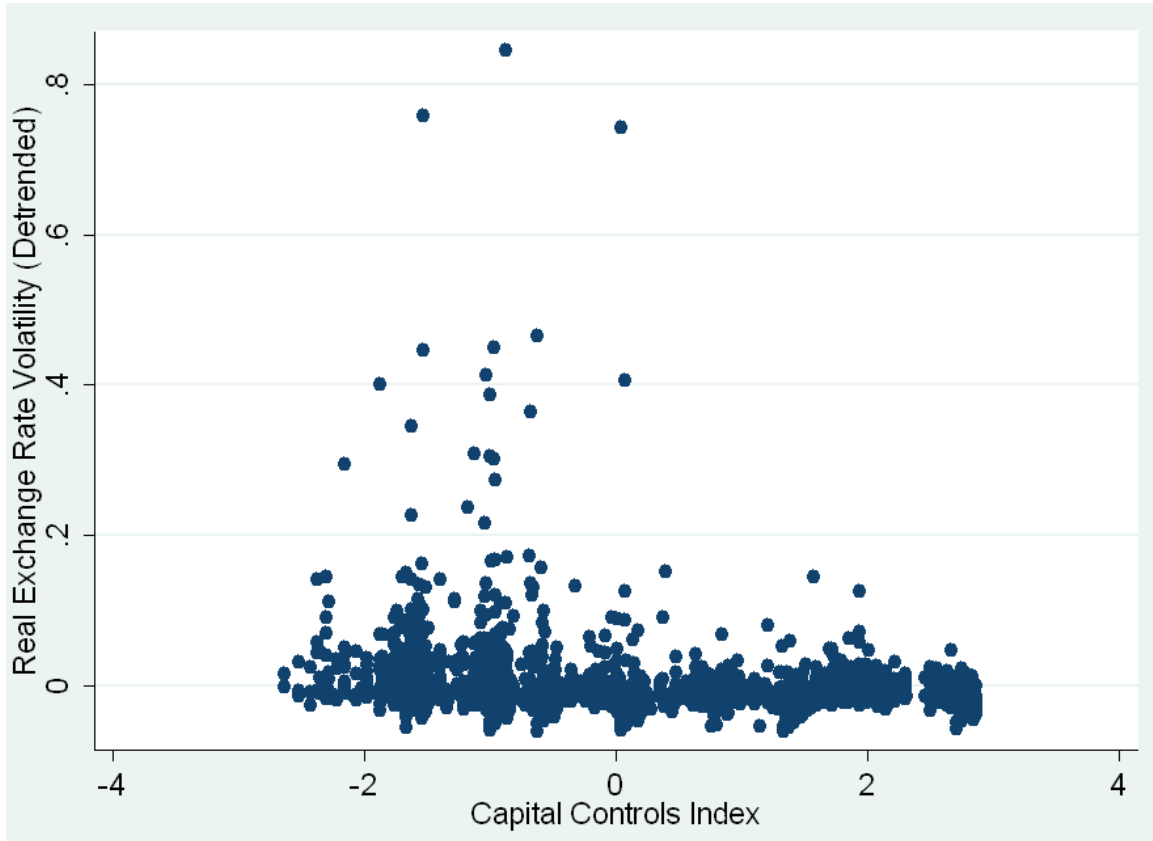Average Financial Openness (*KAOPEN*) Versus Time

Figure 4.
Correlation Between De-trended Real Exchange Rate Volatility and Financial Openness


The correlation coefficient (ρ) is -0.181, suggesting a weak (yet still statistically significant[20]) negative relationship between capital controls and real exchange rate volatility. Note that the statistically significant negative relationship is still preserved (ρ=-0.176) when outliers with de-trended volatilities greater than 20% are removed from the calculation.

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[20] $p < 0.05$

Figure 5.
Long-Term Changes in Volatility for Countries that Liberalized or Tightened
Capital Control Controls
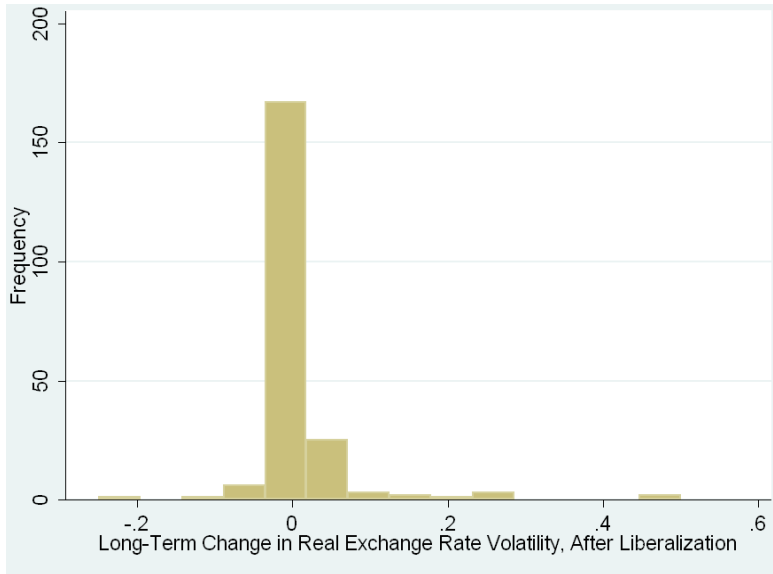
$\Delta RERvolLT_{i,t}$ Change in Long-Term Volatility Following Capital Control **Liberalization**	
Mean	0.011
Standard Error	0.004
95% Conf. Interval	[0.002, 0.020]
Number of Observations	211
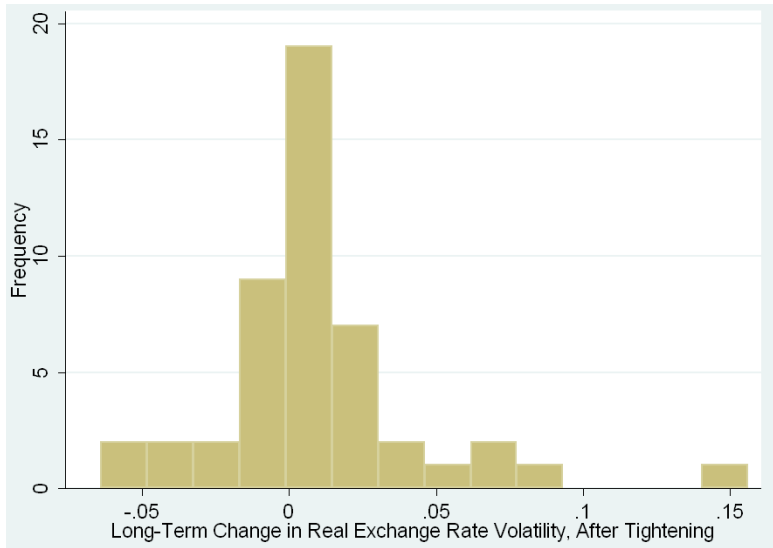
$\Delta RERvolLT_{i,t}$ Change in Long-Term Volatility Following Capital Control **Tightening**	
Mean	0.009
Standard Error	0.005
95% Conf. Interval	[-0.002, 0.019]
Number of Observations	48

I define long-term change in real exchange rate volatility ($\Delta RERvolLT_{i,t}$) as the difference between the average volatility for the three years prior to the policy change and the average volatility for the three years following the policy change. According to the construction of the long-term change in volatility variable, *positive* values of $\Delta RERvolLT_{i,t}$ signify a long-term *decrease* in volatility following the policy change, while *negative* values signify an increase in volatility. See Equation 15 in Section 4.1 for more details.

Liberalization or tightening of capital controls for this test is defined as changes in the *KAOPEN* index between year *t* and *t-1* of at least 0.5SD (0.767)

Figure 6.
Diagram of Propensity Score Matching Tests
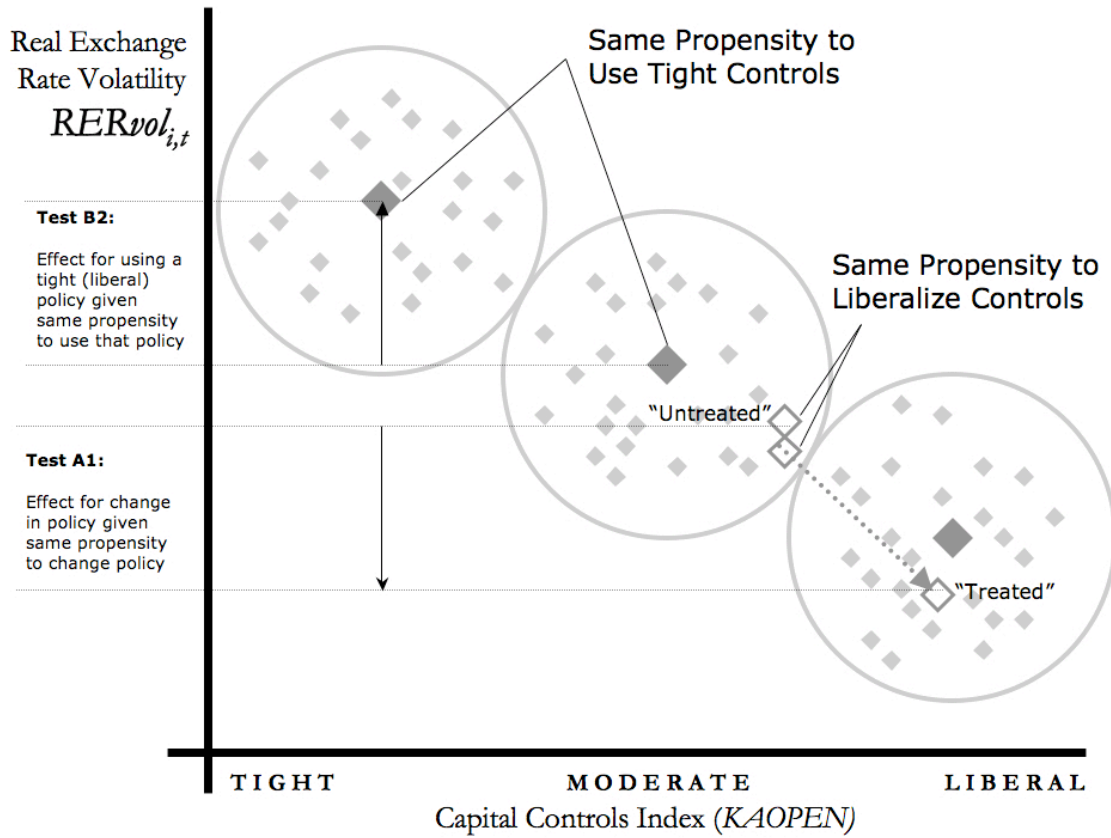
The large outlined diamonds depict the first experiment in which I determine the treatment effect for capital control liberalization by calculating the difference in real exchange rate volatility between countries that liberalized and countries that maintained their capital controls, given the same propensity to liberalize their capital controls.

The large solid diamonds depict the second experiment in which I determine the treatment effect for using tight capital control policy by calculating the difference in volatility between countries that use tight and countries that use moderate controls, given the same propensity to use tight controls.

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