Are consistent pegs really more prone to currency crises?

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Abstract

This paper empirically evaluates the treatment effect of consistent pegs (i.e., the policy that countries claim to have pegged regimes and actually adopt the announced pegged regimes) on the occurrence of currency crises to examine whether consistent pegs are indeed more prone to currency crises than other regimes. To estimate the treatment effect of consistent pegs properly, we must carefully control for the self-selection problem of regime adoption because a country's exchange rate regime choice is non-random. We thus use matching estimators as a control for the self-selection problem. We find interesting and robust evidence that consistent pegs significantly decrease the probability of currency crises compared with other exchange rate policies.

JEL classification: F31; F33

Keywords: Exchange rate regimes; Currency crises; Consistent pegs; Self-selection bias; Matching estimators

1. Introduction

It is well known that several countries' actual (*de facto*) exchange rate regimes are inconsistent with their official (*de jure*) exchange rate regimes (Calvo and Reinhart, 2002; Reinhart and Rogoff, 2004; Levy-Yeyati and Sturzenegger, 2005). For example, Calvo and Reinhart (2002) suggest that, in reality, many countries that claim to have floating regimes actively manage their exchange rate. According to Alesina and Wagner (2006), because a large depreciation (or devaluation) of an exchange rate makes market participants recognize that such countries are vulnerable in terms of monetary and exchange rate regimes, many countries try to actively manage the exchange rate to avoid such situations even if they claim to have floating regimes. Therefore, many countries strategically (or are forced to) follow regimes that differ from their announced regimes.

It is often claimed that announcing the adoption of pegged regimes increases the risk of currency crises because official pegs may become targets of speculative attacks (e.g., Levy-Yeyati and Sturzenegger, 2005; Genberg and Swoboda, 2005). However, countries with consistent pegs (i.e., the policy that countries claim to have pegged regimes and actually adopt the announced pegged regimes) may avoid speculative attacks because they can enhance the credibility of their currencies by following through on their commitments to adopt pegged regimes. In a world with increasingly integrated capital markets, are consistent pegs really prone to speculative attacks and currency crises, as is commonly assumed? The purpose of this paper is to answer this question.

According to Levy-Yeyati and Sturzenegger (2005), to avoid speculative attacks, many countries adopt pegged regimes but they do not claim to have pegged regimes when they want to stabilize their currencies. Alesina and Wagner (2006) call this behavior "fear of announcing a peg." [1] Is this policy effective? Are there significant differences in the probability of currency crises between the policy of adopting regimes different from the announced regimes and that of actually adopting the announced regimes? [2] These questions must be addressed to evaluate the actual exchange rate policies. However, there is little empirical literature investigating the links between actual regimes, announced regimes and the occurrence of currency crises.[3] The focus of this paper is

[1] While Levy-Yeyati and Sturzenegger (2005) call this behavior "fear of pegging," Alesina and Wagner (2006) call it "fear of announcing a peg." In this paper, following Alesina and Wagner (2006), we call it "fear of announcing a peg."

[2] The links between deviations of actual exchange rate regimes from announced ones and economic performance have been analyzed, for example, by Carrera and Vuletin (2003) (real effective exchange rate volatility), Bastourre and Garrera (2004) (volatility of economic growth), and Dubas et al. (2005) (economic growth).

[3] In a related paper, Genberg and Swoboda (2005) compare the distribution of monthly exchange rate changes for countries that adopt a "fear of announcing a peg" policy versus countries with consistent pegs from 1974 to 2001 using data from the IMF classification and the *de facto* Reinhart and Rogoff (2004) classification. According to their analysis, average exchange rate changes are lower for countries with consistent pegs than for countries with a "fear of announcing a peg" policy, but the standard deviation is higher for countries with consistent pegs than for countries with a "fear of announcing a peg" policy.

addressing this research gap.

Some empirical studies have investigated the links between exchange rate regimes and currency crises using various datasets and methods (e.g., Ghosh et al., 2003; Bubula and Ötker-Robe, 2003; Husain et al., 2005; Haile and Pozo, 2006; Coulibaly, 2009; Esaka, 2010). However, these previous studies provide a mixed view of the effect of exchange rate regimes on the occurrence of currency crises. Therefore, it is very useful to determine which types of exchange rate regimes are more susceptible to speculative attacks and currency crises and which exchange rate regimes can avoid currency crises. As Levy-Yeyati and Sturzenegger (2005) point out, if many countries strategically follow actual regimes different from their announced regimes to avoid speculative attacks, an examination of whether deviations of actual exchange rate regimes from announced regimes affect the occurrence of currency crises will provide useful information.

Accordingly, this paper empirically evaluates the effect of consistent pegs on the occurrence of currency crises to examine whether countries with consistent pegs have significantly higher or lower probabilities of currency crises compared with other exchange rate policies. In doing so, we investigate whether the deviations of actual exchange rate regimes from the announced regimes affect the occurrence of currency crises. We use the relationship between the *de jure* IMF classification and the *de facto* Reinhart and Rogoff (2004) classification as an indicator of discrepancy or consistency between announced and actual regimes.

To properly estimate the effect of exchange rate regimes on the incidence of currency crises, we must carefully control for the self-selection problem of regime adoption. Previous studies (cited above) do not explicitly address this problem. In the estimation of these studies, self-selection bias can arise because a country's exchange rate regime choice is non-random (i.e., there are systematic differences between countries that do and do not adopt a specific regime). This issue suggests that previous studies may provide an inaccurate picture of the effect of exchange rate regimes on the occurrence of currency crises.

In this paper, we employ the bias-corrected matching estimator of Abadie and Imbens (2006) to address the self-selection problem of regime adoption. Abadie and Imbens (2006) show that the simple matching estimator will be biased in finite samples because matching is not exact when the matching variables are continuous. To remove this bias, they propose the bias-corrected matching estimator, which adjusts for differences in covariate values within the matches. Abadie and Imbens (2011) find that, in the simulation study, the bias-corrected matching estimator performs well compared with both simple matching estimators without the bias-adjustment and regression-based estimators in terms of bias and mean-squared error. In addition, we apply propensity score matching to test the robustness of the results from the matching method of Abadie and Imbens (2006). To our

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Moreover, countries with consistent pegs often experience extreme changes in their exchange rates. Therefore, Genberg and Swoboda (2005) suggest that committing to pegged regimes increases the risk of speculative attacks. However, they did not conduct a formal statistical analysis.

knowledge, no other paper investigates the effect of consistent pegs on the occurrence of currency crises using matching methods.[4]

The central concept of matching methods is matching each treated unit with control units that have similar observed characteristics and then comparing the outcomes between the treated and the control units. The advantage of matching methods is that they can formally control for the non-random selection problem and avoid the specification of the functional form because they are nonparametric techniques (Dehejia and Wahba, 2002; Imbens, 2004; Imbens and Wooldridge, 2009). The matching techniques can avoid selection bias and provide unbiased estimates of treatment effects (Imbens, 2004; Abadie and Imbens, 2006; Imbens and Wooldridge, 2009).

Using matching estimators, we estimate the average treatment effect of consistent pegs on the likelihood of currency crises to examine whether consistent pegs are actually more vulnerable to currency crises than other exchange rate policies. We find interesting evidence that consistent pegs significantly decrease the likelihood of currency crises compared with other exchange rate policies. This result is robust to a wide variety of matching methods.

The paper is organized as follows. Section 2 presents the data of currency crises and *de jure* and *de facto* exchange rate regimes. Section 3 explains significant exchange rate policies that indicate discrepancies between announced regimes and actual regimes. Section 4 presents an empirical methodology for matching methods. Section 5 estimates the average treatment effect of consistent pegs on the risk of currency crises using matching methods. Section 6 empirically examines our hypotheses. Finally, Section 7 presents a summary and concluding remarks.

2. Data

This section presents the data on currency crises and the classifications of *de jure* and *de facto* exchange rate regimes used in this paper. Our sample consists of 84 countries from 1980-1998.[5]

2.1. Currency crisis data

The data on currency crisis episodes are obtained from Esaka (2010).[6] Following previous studies of currency crises (e.g., Kaminsky and Reinhart, 1999; Glick and Hutchison, 2005; Hong and Tornell, 2005), the exchange market pressure index (EMPI) is employed to identify currency crisis episodes. The EMPI is constructed from a weighted average of real exchange rate changes and

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[4] To our knowledge, no other paper investigates the links between deviations between actual and announced exchange rate regimes and economic performances using matching methods.

[5] We excluded the following from the sample: some small countries, Middle Eastern countries, transition economies, and other countries with incomplete data. Moreover, we excluded the United States and Germany because these countries act as references for most of the countries in the sample.

[6] For additional details regarding the method of identifying currency crises, see Esaka (2010).

foreign reserves changes.[7] Both successful and unsuccessful speculative attacks on the currencies can be captured by applying this index. Esaka (2010) defines a currency crisis as having occurred when the EMPI for a country meets either of the following two conditions: (1) the EMPI is above the mean plus two times the standard deviation and the EMPI is above 10% or (2) the EMPI is above 50%.[8] Here, we construct the currency crisis dummy variable, which takes a value of one if the country experiences a currency crisis in a particular year and zero otherwise. Following the treatment effect literature, the currency crisis dummy is defined as the outcome variable.

Following Glick and Hutchison (2005), we use a two-year window following the onset of a crisis and eliminate any observations within two years of each crisis from our dataset to reduce the chances of capturing the continuation of the same currency crisis. Using a two-year window allows us to investigate the causality from exchange rate regimes to currency crises, because the reverse causality from crises to exchange rate regimes can be mitigated by eliminating two-year observations following the onset of a crisis from our dataset. The selected currency crisis episodes are shown in Appendix A. The 84 countries in our sample experienced 154 currency crises from 1980 to 1998.[9]

2.2. Classifications of exchange rate regimes

In this paper, we use the *de jure* IMF classification as the announced (or official) exchange rate regimes and the *de facto* Reinhart and Rogoff (2004) classification as the actual exchange rate regimes to examine whether consistent pegs are really more prone to currency crises than other regimes.

2.2.1. The de jure IMF classification

Before 1998, the IMF's official classification system classified IMF member countries on the basis of their own official statements regarding the degree of exchange rate flexibility. Regimes were mainly classified into three categories: (1) peg (i.e., pegged to a single currency or a composite of currencies), (2) limited flexibility, and (3) more flexible, which included managed floating and independently floating. However, it is well known that the actual, *de facto* exchange rate regimes of several countries were inconsistent with their official, *de jure* exchange rate regimes (Calvo and Reinhart, 2002; Reinhart and Rogoff, 2004; Levy-Yeyati and Sturzenegger, 2005).

To correct this shortcoming, the IMF has adopted a new classification system based on the IMF

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[7] As in Hong and Tornell (2005), Esaka (2010) uses the real exchange rate to avoid counting continuous high inflation rates as crises.

[8] As for previous studies of currency crises (e.g., Glick and Hutchison, 2005), the threshold values of the EMPI are also adjusted by employing some means.

[9] We compare these crisis episodes with those of previous studies and do not find any crucial differences.

members' *de facto* regimes since January 1999.[10] The new IMF system classifies member countries into eight categories: (1) exchange arrangements with no separate legal tender, including formal dollarization and currency unions, (2) currency boards, (3) other conventional fixed pegs, (4) pegged exchange rates within horizontal bands, (5) crawling pegs, (6) exchange rates within crawling bands, (7) managed floating with no predetermined path for the exchange rate, and (8) independently floating. The data from the old and new IMF classifications can be obtained from the IMF's *Annual Report on Exchange Arrangements and Exchange Restrictions* (AREAER).

For the purpose of this paper, we classify *de jure* exchange rate regimes into two categories: (1) "pegs" and (2) "floats." Following Genberg and Swoboda (2005), we thus define the first and second old IMF categories as (1) "pegs" and the third old IMF category as (2) "floats." Because the IMF has adopted a new classification system based on IMF members' *de facto* regimes since 1999, we cannot treat the IMF's classification data after 1999 as announced exchange rate regimes. Therefore, we use the years from 1980 to 1998.

2.2.2. The de facto Reinhart and Rogoff (2004) classification

In this paper, we use the *de facto* classification of Reinhart and Rogoff (2004) as data indicating the actual exchange rate regimes. Reinhart and Rogoff (2004) construct a historical database of the *de facto* exchange rate regimes for 153 countries from 1946 to 2001. They classify exchange rate regimes by applying detailed country chronologies and a broad variety of descriptive statistics on official and market-determined (dual and parallel) exchange rates.

Reinhart and Rogoff (2004) classify countries into fourteen categories: (1) no separate legal tender, (2) pre-announced peg or currency board arrangement, (3) pre-announced horizontal band that is narrower than or equal to +/-2%, (4) *de facto* peg, (5) pre-announced crawling peg, (6) pre-announced crawling band that is narrower than or equal to +/-2%, (7) *de facto* crawling peg, (8) pre-announced crawling band that is wider than or equal to +/-2%, (9) *de facto* crawling band that is narrower than or equal to +/-2%, (10) *de facto* crawling band that is narrower than or equal to +/-5%, (11) moving band that is narrower than or equal to +/-2%, (12) managed floating, (13) freely floating, and (14) freely falling.[11]

The first feature of Reinhart and Rogoff's classification is the creation of a new separate category for a country with a twelve-month rate of inflation above 40%. Such a country is classified as "freely falling." Thus, it is possible to precisely capture the relationship between exchange rate regimes and performances using the "freely falling" category.

For the purposes of this paper, we classify *de facto* exchange rate regimes into three

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[10] For more details of the IMF new classification system, see Bubula and Ötker-Robe (2002).

[11] The Reinhart and Rogoff (2004) classification data were obtained from *http://www.puaf.umd.edu/faculty/papers/reinhart/papers.htm.*

categories: (1) "pegs," (2) "floats," and (3) "freely falling."[12] Following Genberg and Swoboda (2005), we thus define the first through ninth Reinhart and Rogoff (2004) categories as "pegs," the tenth through thirteenth categories as "floats," and the fourteenth category as "freely falling." Note that in Reinhart and Rogoff's (2004) framework, categories (1) through (9) correspond to a peg (i.e., pegged to a single currency or a composite of currencies) and thus to "limited flexibility" in terms of the IMF classification, while categories (10) through (13) correspond to "more flexible" (i.e., managed floating and independently floating) in terms of the IMF classification.

As in Reinhart and Rogoff (2004), Bubula and Ötker-Robe (2002) and Levy-Yeyati and Sturzenegger (2005) provide data on *de facto* exchange rate regimes. These classifications have their own merits, but the Reinhart and Rogoff (2004) classification is more appropriate for our purposes. First, we use the Reinhart and Rogoff (2004) data because they construct the database of *de facto* regimes for many countries over a long period. Given that Bubula and Ötker-Robe (2002) construct a database on *de facto* regimes from 1990 to 2001 following the new IMF classification system, they do not provide useable data before 1990. Although Levy-Yeyati and Sturzenegger (2005) classify exchange rate regimes from 1974 to 2000, their sample contains many cases in which *de facto* regimes are not identified for any year, especially in developing countries.

Second, because Reinhart and Rogoff (2004) treat high-inflation countries as another category (freely falling), we can use their data to separately consider the situation in which high-inflation countries have a high probability of currency crises. Thus, we can precisely capture the probability that a country that adopts a specific exchange rate regime will experience a currency crisis. Third, because Reinhart and Rogoff (2004) provide a relatively longer duration of exchange rate regime as compared with other *de facto* data (Husain et al., 2005), we can use their data to examine the impact of relatively long-lived exchange rate regimes on currency crises.

3. Deviations of actual regimes from announced regimes and currency crises

In this section, we first explain important aspects of exchange rate policies that indicate the relationship between announced regimes and actual regimes. Next, we formulate some hypotheses regarding how deviations of actual regimes from announced ones affect the occurrence of currency crises.

3.1. Exchange rate policies and the difference between announced regimes and actual regimes

Figure 1 shows the differences and similarities between announced regimes and actual regimes, including the following exchange rate policies. First, the "fear of announcing a peg" policy (i.e.,

[12] When we attempted to further subdivide exchange rate regimes, the resulting categories did not have a sufficient number of observations to statistically examine the link between the deviations between actual and announced regimes and currency crises.

Regime 2) is defined as a policy in which countries actually adopt pegged regimes but do not claim to have pegged regimes (Levy-Yeyati and Sturzenegger, 2005; Alesina and Wagner, 2006). Second, the "fear of pegging" policy is defined as a policy in which countries actually adopt floating regimes but claim to have pegged regimes, and the "inability of pegging" situation is defined as a situation in which countries cannot maintain announced pegged regimes (Alesina and Wagner, 2006). Regime 3 is treated as the "fear of pegging" or "inability of pegging" because these situations cannot be clearly distinguished from the data. Third, the "consistent" policy is defined as a policy in which countries actually adopt the announced regimes (Carrera and Vuletin, 2003). For example, the "consistent pegs" policy (i.e., Regime 1) is defined as a policy that consistently maintains announced pegged regimes, while the "consistent floats" policy (i.e., Regime 4) is defined as a policy that consistently maintains announced floating regimes.[13]

[Insert Figure 1 approximately here]

3.2. Our hypotheses

Let us now develop the following hypotheses to examine the link between the above-mentioned exchange rate policies and currency crises.

3.2.1. Consistent pegs vs. other exchange rate policies

It is often claimed that actually adopting announced pegged regimes encourages capital inflows that are more excessive than those of other policies and that this policy is susceptible to sharp capital flow reversals induced by shocks. Moreover, the exchange rate policy may become a target of speculative attacks. Therefore, we hypothesize that countries with consistent pegs have a significantly higher risk of currency crises than countries with other policies.

However, countries that consistently maintain a commitment to pegged regimes can enhance greater credibility in their currencies by requiring increased macroeconomic policy discipline and maintaining stricter rules for exchange policies compared with other polices. Therefore, we hypothesize that countries with consistent pegs have a significantly lower risk of currency crises than those with other policies due to this policy's capacity to enhance credibility. To determine which hypothesis is correct, we statistically examine whether a policy of consistent pegs significantly increases or decreases the probability of currency crises compared with other policies.

[13] For example, before the European currency crisis in 1992-1993, Italy, Spain, and Sweden adopted consistent pegs, while Norway adopted a "fear of pegging" policy (i.e., Regime 3). Before the Mexican currency crisis in 1994-1995, Mexico adopted a "fear of announcing a peg" policy (i.e., Regime 2). Before the Asian currency crisis in 1997-1998, Indonesia, Korea, and Malaysia had a "fear of announcing a peg" policy (i.e., Regime 2), while Thailand had consistent pegs. For Australia, which adopted consistent floats, the currency crisis occurred in 1985.

3.2.2. Consistent pegs vs. fear of announcing a peg

Levy-Yeyati and Sturzenegger (2005) point out that many countries try to avoid explicit commitments to pegged regimes even if they actually adopt pegged regimes because official pegs may become targets of speculative attacks. Therefore, many countries that actually adopt pegged regimes do not announce these regimes to avoid speculative attacks. If this policy is indeed appropriate, countries with a "fear of announcing a peg" policy (i.e., Regime 2) will have a significantly lower probability of currency crises than countries with consistent pegs (i.e., Regime 1).

As discussed by Alesina and Wagner (2006), because deviations of actual regimes from announced regimes are likely to imply increased uncertainty in the exchange rate policy, these situations essentially affect the credibility of an exchange rate regime. Therefore, countries that actually adopt announced pegged regimes may avoid speculative attacks because they can enhance credibility in their currencies by upholding their commitments to adopt pegged regimes. If this policy is indeed effective, countries with consistent pegs (i.e., Regime 1) will have a significantly lower probability of currency crises than countries with a "fear of announcing a peg" policy (i.e., Regime 2). The question is which of these dynamics is actually observed in the data. Thus, we statistically verify whether there is a significant difference between the probability of crises for countries with a "fear of announcing a peg" policy and countries that adopt consistent pegs.

3.2.3. Consistent pegs vs. fear of pegging (or inability of pegging)

Because situations in which countries fear pegging or are unable to peg allow market participants to perceive vulnerabilities in monetary and exchange rate regimes, such countries lose market credibility in their currencies (Alesina and Wagner, 2006). Therefore, we hypothesize that countries that cannot peg or fear pegging (i.e., Regime 3) have a substantially higher probability of currency crises than countries with consistent pegs (i.e., Regime 1).

3.2.4. Consistent pegs vs. consistent floats

As mentioned above, it is often claimed that one of the major ingredients of the environment leading to currency crises is consistent pegs. Consistent pegs may promote excess capital inflows by minimizing the exchange rate risk for international investors. Therefore, we hypothesize that countries with consistent pegs have a significantly higher risk of currency crises than countries with consistent floats.

However, consistent pegs may enhance credibility in their currencies by requiring increased macroeconomic policy discipline and maintaining stricter, clearer rules for exchange policies compared with consistent floats. Therefore, we hypothesize that countries with consistent pegs have a significantly lower risk of currency crises than consistent floats. To determine which hypothesis is correct, we statistically examine whether a policy of consistent pegs significantly increases or decreases the probability of currency crises compared with consistent floats.

4. Empirical methodology

This paper formally controls for the self-selection problem of regime adoption to properly estimate the effect of consistent pegs on the occurrence of currency crises. Following the treatment effect literature, we refer to the adoption of consistent pegs as "treatment," the country (observation) with consistent pegs (i.e., Regime 1) as the "treatment group," and the country (observation) with other regimes (i.e., Regime 2, Regime 3, and Regime 4) as the "control group." We also define the occurrence of currency crises (i.e., currency crisis dummy) as the "outcome" variable.[14]

To address the self-selection problem of regime adoption, we employ the matching methods recently developed in the microeconometric evaluation studies.[15] Matching methods have been widely used in labor and health economics (Imbens and Wooldridge, 2009). In the fields of macroeconomics and international economics, matching methods have recently been used to conduct policy evaluation. For example, using propensity score matching methods, Persson (2001) examines the effect of currency unions on international trade; Lin and Ye (2007) conduct a policy evaluation of inflation targeting; Glick et al. (2006) investigate the effect of capital account liberalization on the frequency of currency crises; and Lin and Ye (2010) evaluate the effect of dollarization on international trade. Using covariate matching methods, Baier and Bergstrand (2009) examine the effect of free trade agreements on international trade. The advantage of the matching methods is that they can eliminate sample selection bias by formally controlling for the non-random selection problem and avoid the specification of the functional form because they are nonparametric techniques (Imbens, 2004; Imbens and Wooldridge, 2009).

Following the treatment effect literature,[16] we estimate the average treatment effect on the treated (ATT) to examine the effect of consistent pegs on currency crises. We thus consider the following equation:

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

[14] Both continuous and discrete scalar variables can be applied as the outcome variable in the estimation of the average treatment effect (Abadie et al., 2004).
[15] We note that instrumental variable method is not appropriate for addressing the self-selection bias in this paper because it is very difficult to find suitable instrumental variables for regime choice that are not also correlated with the occurrence of currency crises and instrumental variable method requires the specification of the functional form.
[16] The presentation in the rest of this section is partly based on the theory and empirical application of matching methods by Heckman et al. (1998), Imbens (2004), Abadie and Imbens (2006), Lin and Ye (2007), Caliendo and Kopeinig (2008), and Imbens and Wooldridge (2009).

where T is the dummy variable of the treatment ($T=1$ when treated and $T=0$ when not treated); $Y_i(1)$ and $Y_i(0)$ denote the potential outcomes: $Y_i(1)$ is the outcome when unit i adopts the treatment (e.g., consistent pegs) and $Y_i(0)$ is the outcome when unit i does not adopt the treatment.[17] The fundamental problem in estimating the ATT is that the second term on the right-hand side ($Y_i(0) \mid T_i = 1$) is not observable, while the first term ($Y_i(1) \mid T_i = 1$) is observable. That is, the key point is identifying the counterfactual for $E[Y_i(0) \mid T_i = 1]$.

If the treatment decision (i.e., the regime choice) is random, the ATT can be estimated using the sample mean of the outcome for the control group ($E[Y_i(0) \mid T_i = 0]$). However, this method can generate biased estimates because the regime choice is not random in this analysis. In the estimation of the ATT, self-selection bias arises when the treatment decision is systematically correlated with a set of observable covariates that also affect the outcome (Imbens, 2004; Caliendo and Kopeinig, 2008; Imbens and Wooldridge, 2009).

To identify the second term on the right-hand side of equation (1), first, the conditional independence assumption (unconfoundedness assumption) is needed as

$$Y_i(0), Y_i(1) \perp T_i \mid X_i. \tag{2}$$

This assumption implies that given pre-treatment variables or covariates X, the treatment assignment is independent of the potential outcomes. Therefore, for units with similar values of X, the treatment assignment is random with respect to the potential outcomes. Second, the common support condition (overlap condition) is needed as[18]

$$0 < P(T_i = 1 \mid X_i) < 1, \tag{3}$$

where $P(T_i = 1 \mid X_i)$ is the conditional probability of adopting the treatment given observed covariates X. This probability is the so-called "propensity score." This condition requires the existence of some comparable control units for each treatment unit.

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

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

[17] In this analysis, only one of the potential outcomes is observed for each unit i, and the other is unobserved or missing. The unobserved outcome is called the counterfactual outcome. The matching estimators impute the missing potential outcome using average outcomes for units with similar values of covariates (Abadie et al., 2004).

[18] According to Heckman et al. (1998), we need only 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.

where $E[Y_i(0) | T_i = 1, X_i]$ is replaced with $E[Y_i(0) | T_i = 0, X_i]$ for observed control units. The covariate matching method matches treatment units to control units with similar observed values of covariates X. The matching can establish a credible counterfactual to properly estimate the average treatment effect by matching each treatment unit to control units with similar covariates (Imbens and Wooldridge, 2009).

Abadie and Imbens (2006) show that the simple covariate matching estimator will be biased in finite samples because matching is not exact when the matching variables are continuous. To remove the bias, they develop the bias-corrected matching estimator, which adjusts for difference within the matches for the differences in covariate values. This estimator combines matching, which compares each treated observation with control observations with similar values of covariates, and regression, which reduces remaining biases due to covariate imbalances (Abadie and Imbens, 2011). For details of the bias-corrected matching estimator used in this paper, see Appendix C. Abadie and Imbens (2011) find that, in the simulation study, the bias-corrected matching estimator of Abadie and Imbens (2006) performs well in terms of bias, root-mean-squared error, and coverage rates compared with both simple matching estimators and regression estimators.

We thus apply the bias-corrected matching estimator proposed by Abadie and Imbens (2006) to precisely estimate the average treatment effect of consistent pegs on currency crises.[19] Following Abadie et al. (2004), we use the weighted Euclidean vector norm to measure the distance between different values of the matching variables. Abadie et al. (2004) suggest that, in simulations, using four matches performs well in terms of mean-squared error for the matching estimator of Abadie and Imbens (2006). Following the recommendation of Abadie et al. (2004), we use four matches in this analysis.

In this paper, we examine the link between exchange rate policies at time t-1 and currency crises at time t, taking into account the causality of time series. According to Imbens (2004) and Caliendo and Kopeinig (2008), observed pre-treatment variables (pre-treatment characteristics) should be used as matching variables. For the treatment at time t-1, we thus use a set of covariates at time t-2.

According to Imbens (2004) and Caliendo and Kopeinig (2008), observed pre-treatment variables (covariates) should be chosen based on economic theory and previous empirical findings. Referring to previous studies of the determinants of the exchange rate regime choice (e.g., Juhn and Mauro, 2002; Alesina and Wagner, 2006)[20] and currency crises (e.g., Kaminsky and Reinhart, 1999;

[19] Abadie and Imbens (2006) employ matching with replacement, allowing each control unit to be used as a match more than once. Matching with replacement increases the average quality of matching by increasing the set of possible matches (Abadie and Imbens, 2006).

[20] Juhn and Mauro (2002) review previous empirical studies for the determinants of exchange rate regime choice. They suggest that the sign and the significance of the coefficients of explanatory variables

Glick and Hutchison, 2005), we first choose the following matching variables: the log ratio of broad money to foreign reserves, domestic credit growth, the current account-to-GDP ratio, real exchange rate overvaluation,[21] the ratio of foreign liabilities to money, and real GDP growth.[22] We then estimate the average treatment effect of consistent pegs on the frequency of currency crises by calculating the mean difference in outcomes between the treatment and the control observations using the bias-corrected matching estimators.

Imbens and Wooldridge (2009) and Crump et al. (2009) point out that the estimation of the average treatment effect is often hampered by a lack of covariate overlap (overlap condition). This lack of overlap can lead to imprecise estimates. To address the lack of overlap, Crump et al. (2009) accordingly propose the simple rule of thumb of discarding all units (observations) with estimated propensity scores outside the range [0.1, 0.9]. In numerical simulations, they prove that the average treatment effect can be estimated most precisely using this technique. Following Crump et al. (2009), we first trim the sample by discarding observations with the estimated propensity scores (i.e., the estimated probability of adopting consistent pegs from probit models) outside the range [0.1, 0.9] and then estimate the average treatment effects of consistent pegs on currency crises using the covariate matching of Abadie and Imbens (2006).

5. The treatment effect of consistent pegs on currency crises

In this section, we estimate the average treatment effect of consistent pegs on the incidence of currency crises using the bias-corrected matching estimator of Abadie and Imbens (2006) to examine whether consistent pegs significantly increase or decrease the probability of currency crises compared with other regimes. Before proceeding, we present the frequency of currency crises at *t* by each exchange rate regime at *t-1*. Table 1 shows that the probability of currency crises is lower for consistent pegs (7.9 %) than for other regimes (10.9 %). It is also observed that the probability of currency crises is lower for consistent pegs than for a "fear of announcing a peg" policy (8.7%), fear pegging or are unable to peg (12.2%), and consistent floats (13.2%). Therefore, we find that consistent pegs are likely to be less vulnerable to currency crises than other regimes.

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substantially change depending on the sample of countries, the sample period, the methodology, and the model specification. Therefore, we do not present a detailed argument regarding the sign and the significance of covariates in this paper.

[21] Following previous studies, real exchange rate overvaluation was calculated as follows. First, we estimated a simple linear regression of the real exchange rate on a constant term and a time trend. Next, we defined the difference between the estimated fitted value and the real exchange rate as the real exchange rate overvaluation.

[22] At the same time, considering the balancing property, we choose the above-mentioned matching variables. These variables are constructed using the data from the IMF, *International Financial Statistics* CD-ROM and the World Bank, *World Development Indicators* CD-ROM.

[Insert Table 1 approximately here]

5.1. Results of the bias-corrected matching estimator: Benchmark

Table 2 presents the results from estimating the average treatment effects of consistent pegs on the occurrence of currency crises using the bias-corrected matching estimator of Abadie and Imbens (2006).[23] For matching methods, the common support condition (overlap condition) is imposed and the freely falling observations are excluded. The numbers in parentheses are heteroskedasticity-consistent standard errors.[24]

From the benchmark in column (1) of Table 2, we note that the average treatment effect is significantly negative at the 1% level, indicating that consistent pegs significantly decrease the incidence of currency crises compared with other regimes.[25] The estimated average treatment effect of consistent pegs is -0.067, suggesting that consistent pegs are associated with a 6.7% decrease in the likelihood of currency crises relative to other regimes. According to the probability of currency crises in Table 1, consistent pegs have a 3.0% (=10.9-7.9) lower probability of currency crises compared with other regimes. Therefore, the effect of consistent pegs on currency crises for the matching technique is larger, on the order of 6.7%. The treatment effect of consistent pegs is strongly significant and economically meaningful for covariate matching methods.

Therefore, after controlling for self-selection bias, consistent pegs have a significant economic effect on lowering the occurrence of currency crises. That is, when two countries have similar values of covariates, and when one country adopts consistent pegs and the other does not, the country with consistent pegs is substantially less prone to currency crises.

Our finding is in contrast to the conventional policy wisdom that consistent pegs are prone to currency crises. However, consistent pegs may enhance the credibility of their currencies by sustaining their commitment to pegged regimes and requiring increased macroeconomic policy discipline. Therefore, it can be surmised that, although consistently following a commitment to pegged regimes may become targets of speculative attacks, countries that maintain announced pegged regimes are substantially less prone to speculative attacks compared with other regimes.

[Insert Table 2 approximately here]

5.2. Robustness: Additional matching variables

We conduct several robustness checks. First, we estimate the average treatment effects using

[23] Matching estimates are obtained using the Stata ado-file of Abadie et al. (2004).
[24] Abadie and Imbens (2008) show that, in the simulation study, the analytic standard errors of Abadie and Imbens (2006) work well even in fairly small samples.
[25] Many variables (e.g., domestic credit growth, the current account-to-GDP ratio, and real GDP growth) that may influence both the regime choice and the occurrence of currency crises are included as matching variables in the benchmark estimations.

additional matching variables. Von Hagen and Zhou (2005) show that capital account liberalization affects the choice of exchange rate regime. Glick and Hutchison (2005) and Glick et al. (2006) find that countries with capital controls have a significantly higher likelihood of currency crises than countries with no capital controls. Therefore, capital account liberalization may influence both regime choice and the occurrence of currency crises. We thus add the dummy for capital account liberalization[26]to the baseline matching variables in the case of "adding 1 variable" in Table 2.

The political system and level of development may also affect the choice of exchange rate regime (Juhn and Mauro, 2002). We thus add the typology of the political system by Beck et al. (2001) and the logarithm of GDP per capita to the matching variables. In the case of "adding 3 variables" in Table 2, these three variables (including the dummy for capital account liberalization) are added to the baseline matching variables. From columns (2) and (3), we note that the average treatment effects of consistent pegs are significantly negative, indicating that consistent pegs have a significantly lower likelihood of currency crises compared with other regimes. Therefore, we obtain substantially the same result as the benchmark.

5.3. Robustness: Alternative common support condition

Second, we estimate the average treatment effects with and without imposing the common support condition. We use the alternative common support condition of Becker and Ichino (2002), while the common support condition of Crump et al. (2009) is applied in the benchmark. Following the implementation of Becker and Ichino (2002), we exclude control observations whose estimated probability of adopting consistent pegs (from probit models) are lower than the lowest probability among the treatment observations or higher than the highest probability among the treatment observations. From the case of the common support condition of Becker and Ichino (2002) in columns (4)-(6) and no common support condition in columns (7)-(9), we note that the average treatment effects of consistent pegs are significantly negative for all cases. Therefore, even after using the alternative common support condition, we obtain essentially the same result as the benchmark.

5.4. Robustness: Alternative number of matches

Third, we use the alternative number of matches to estimate the average treatment effects of consistent pegs, while we use four matches in the benchmark following the recommendation of

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[26] Following Glick and Hutchison (2005), we construct the dummy for capital account liberalization on the basis of the IMF's AREAER. As a robustness check, we also estimate the average treatment effects by imposing the exact matching of the dummy for capital account liberalization. This analysis forcibly matches each treatment observation with control observations within the same condition of capital account liberalization. The average treatment effects are significantly negative at the 1% level, while the detailed results are omitted to save space. Therefore, we obtain substantially the same results as in columns (2) and (3) of Table 2.

Abadie et al. (2004). We use one match in columns (10)-(12) and eight matches in column (13)-(15). From columns (10)-(15) of Table 2, we note that the average treatment effects of consistent pegs are significantly negative for all case. Therefore, our results remain essentially the same as the benchmark despite changing the number of matches.

5.5. Robustness: Comparisons within the same year

Fourth, we estimate the average treatment effects by including year dummies as matching variables. In this analysis, we perform exact matching on year dummies to force that the comparisons of the treated observations (i.e., consistent pegs) and the control observations (i.e., other regimes) are done within the same year period.[27] From columns (16)-(18) of Table 2, we note that the average treatment effects of consistent pegs are significantly negative for all case. Therefore, our qualitative results remain essentially the same even when comparing consistent pegs with other regimes within the same year period.

In addition, we apply propensity score matching methods to test the robustness of the results from the matching method of Abadie and Imbens (2006). For more details of the propensity score matching method, see Appendix B. From columns (1)-(3) of Appendix Table B, we note that the average treatment effects of consistent pegs are significantly negative, indicating that consistent pegs significantly decrease the incidence of currency crises compared with other regimes. Therefore, for propensity score matching, we obtain substantially the same results as the matching estimator of Abadie and Imbens (2006).

6. Empirical examination of our hypotheses

To examine our hypotheses presented in sub-section 3.2, we select the following sub-samples: (1) *de facto* pegs, (2) *de jure* pegs, and (3) consistent pegs and consistent floats.

6.1. Consistent pegs vs. fear of announcing a peg

We first select the dataset of *de facto* pegs to examine whether countries with consistent pegs have a significantly higher or lower probability of currency crises compared with countries with a "fear of announcing a peg" policy. Table 3 shows the estimated average treatment effects using the bias-corrected matching estimator when the dataset of *de facto* pegs is used. We refer to the observation with consistent pegs as the treatment group and to the observation with "fear of announcing a peg" as the control group.

From the benchmark in column (1) of Table 3, we note that the average treatment effect is

[27] In addition to the above-mentioned, to control for the contagious effects of currency crises and the common shocks in the world, we include year dummies as matching variables.

significantly negative, indicating that consistent pegs significantly decrease the incidence of currency crises compared with a "fear of announcing a peg" policy. The estimated average treatment effect of consistent pegs is -0.047, suggesting that consistent pegs lower the likelihood of currency crises by 4.7% relative to "fear of announcing a peg."

We conduct several robustness checks, as in Section 5. From Table 3, we note that the average treatment effects of consistent pegs are significantly negative for all cases. Therefore, we obtain qualitatively the same results as the benchmark estimation. Although Levy-Yeyati and Sturzenegger (2005) report that many countries may actually adopt pegged regimes but not announce pegged regimes to avoid speculative attacks, a policy of consistently maintaining announced pegged regimes substantially decreases the risk of speculative attacks compared with a "fear of announcing a peg" policy.

[Insert Table 3 approximately here]

6.2. Consistent pegs vs. fear of pegging (or inability of pegging)

Second, we select the dataset of *de jure* pegs to examine whether countries that cannot peg or fear pegging have a substantially higher probability of currency crises than countries with consistent pegs. Table 4 shows the estimated average treatment effects using the bias-corrected matching estimator when the dataset of *de jure* pegs is used. We refer to the observation with consistent pegs as the treatment group and to the observation with fear of pegging as the control group.

From the benchmark in column (1) of Table 4, we note that the average treatment effect of consistent pegs is significantly negative, indicating that a fear or inability of pegging significantly increases the risk of currency crises compared with consistent pegs. The estimated average treatment effect is -0.080, suggesting that consistent pegs lower the likelihood of currency crises by 8.0% relative to a fear of pegging (an inability of pegging). As in Section 5, we conduct several robustness checks. From Table 4, we note that the average treatment effects of consistent pegs are significantly negative for all cases.[28] Therefore, we obtain qualitatively the same results as the benchmark estimation.

[Insert Table 4 approximately here]

6.3. Consistent pegs vs. consistent floats

Third, we select the dataset of consistent pegs and consistent floats to examine whether

[28] From the case of the common support condition of Becker and Ichino (2002) in columns (4)-(6) and no common support condition in columns (7)-(9), we observe that the average treatment effects change depending on the set of covariates. This result may occur because the alternative common support condition is inappropriate in this analysis.

consistent pegs significantly increase or decrease the probability of currency crises compared with consistent floats alone. Table 5 presents the results from estimating the average treatment effects using the dataset of consistent pegs and consistent floats. We refer to the observation with consistent pegs as the treatment group and to the observation with consistent floats as the control group.

From the benchmark in column (1) of Table 5, we note that the average treatment effect is significantly negative, indicating that consistent pegs significantly decrease the incidence of currency crises compared with consistent floats. The estimated average treatment effect is -0.118, suggesting that consistent pegs lower the likelihood of currency crises by 11.8% relative to consistent floats. We conduct several robustness tests to verify the robustness of the result of the benchmark. From Table 5, we note that the average treatment effects of consistent pegs are significantly negative for all cases. Therefore, we obtain qualitatively the same results as the benchmark.

Overall, while we examined our hypotheses and executed sensitive analyses when selecting datasets, we find that consistent pegs significantly decrease the probability of currency crises compared with other exchange rate policies.

[Insert Table 5 approximately here]

7. Summary and concluding remarks

In this paper, we formally evaluate the treatment effect of consistent pegs on the occurrence of currency crises in 84 countries from 1980 to 1998. We carefully address the self-selection problem of regime choice that previous studies have not explicitly addressed. To address the self-selection problem, we employ the bias-corrected matching estimators of Abadie and Imbens (2006).

Using matching estimators, we estimate the average treatment effect of consistent pegs on the incidence of currency crises. After controlling for self-selection bias, we find several interesting results. First, countries with consistent pegs have a significantly lower probability of currency crises than countries with other exchange rate policies. Second, countries with consistent pegs have a significantly lower probability of currency crises than those with a "fear of announcing a peg" policy. Third, countries with consistent pegs have a significantly lower probability of currency crises than those with a fear of pegging. Fourth, countries with consistent pegs have a significantly lower probability of currency crises than those with consistent floats. These results are robust to a wide variety of matching methods.

Therefore, we statistically confirm that deviations of actual exchange rate regimes from announced regimes significantly affect the occurrence of currency crises. We can reasonably conclude that countries that consistently maintain announced pegged regimes are least prone to speculative attacks and currency crises because they can enhance credibility in their currencies by sustaining their commitment to pegged regimes.

Appendix A. Currency crises, 1980-1998

Appendix Table A shows various currency crisis episodes from 1980 to 1998. The 84 countries experienced 154 currency crises from 1980 to 1998.

[Insert Appendix Table A "Currency crises, 1980-1998" approximately here]

Appendix B. Propensity score matching

To test the robustness of the results from the matching method of Abadie and Imbens (2006), we employ some propensity score matching methods.[29] Following the implementation of propensity score matching (e.g., Dehejia and Wahba, 2002; Imbens, 2004; Caliendo and Kopeinig, 2008), we perform the following procedure. In the first stage, we estimate the probability of adopting consistent pegs (propensity scores) using multinomial probit models. The dependent variable is the index of exchange rate regimes (Regime 1=1, Regime 2=2, Regime 3=3, and Regime 4=4). We use the same matching variables (the explanatory variables in probit models) as the benchmark of the bias-corrected matching estimators in Sections 5 and 6.

In the second stage, we estimate the average treatment effect of consistent pegs on the frequency of currency crises by calculating the mean difference in outcomes between the treated and control observations using propensity score matching. In this paper, we employ nearest-neighbor and radius matching methods as propensity score matching methods. The nearest-neighbor matching technique (with replacement) matches each treated observation with the control observation that has the closest propensity score. The radius matching technique matches a treated observation to control observations with estimated propensity scores falling within specified radius r. We employ two radius matching estimators: r=0.01 and r=0.05. Following Crump et al. (2009), we use the observations of the interval [0.1, 0.9] for the estimated propensity scores as the common support condition (overlapping condition).

Appendix Table B presents the estimated average treatment effects of consistent pegs when

[29] Propensity score matching methods were introduced and developed by Rosenbaum and Rubin (1983) and have recently become very popular in the program evaluation literature. The advantage of propensity score matching is that this technique can overcome the high-dimension problem because matching is performed based on only the estimated propensity score from the probit model of several observables (Dehejia and Wahba, 2002). However, the reduction in dimensions may cause less precise matching (Imbens, 2004).

matching is carried out using the estimated propensity scores from multinomial probit models.[30] The dataset of *de facto* pegs is selected in columns (4)-(6), the dataset of *de jure* pegs is selected in columns (7)-(9), and the dataset of consistent pegs and consistent floats is selected in columns (10)-(12). From Appendix Table B, we note that the average treatment effects of consistent pegs are significantly negative for all cases, except for column (4). This result indicates that consistent pegs significantly decrease the incidence of currency crises compared with other regimes. Therefore, for propensity score matching, we obtain substantially the same results as the matching method of Abadie and Imbens (2006). Overall, our results are robust to various matching techniques.

[Insert Appendix Table B "Propensity score matching" approximately here]

Appendix C. Bias-corrected matching estimator

Following the notation and terminology of Abadie et al. (2004) and Abadie and Imbens (2006, 2011), Appendix C presents the bias-corrected matching estimator for the average treatment effect on treated (ATT) in this paper. Let T_i be a binary variable that indicates exposure of unit *i* to treatment, so $T_i = 1$ if unit *i* was exposed to treatment and $T_i = 0$ otherwise. For unit *i*, $i = 1, \cdots, N$, let $Y_i(1)$ and $Y_i(0)$ denote the potential outcomes: $Y_i(1)$ is the outcome of unit *i* when exposed to the treatment and $Y_i(0)$ is the outcome of unit *i* when not exposed to the treatment. There are N_1 treated units and N_0 control units, $N = N_0 + N_1$. We are interested in estimating the ATT:

$$\tau^t = E[Y_i(1) - Y_i(0) \mid T_i = 1].$$

To identify and estimate the above ATT, we assume that assignment to treatment is independent of the potential outcomes, conditional on the covariates, and that the probability of assignment is bounded away from zero and one. That is, (1) the conditional independence assumption (unconfoundedness) and (2) the overlap condition (common support condition) are needed, as noted by Section 4.

Abadie and Imbens (2006) consider the case of matching with replacement, allowing each control unit to be used as a match more than once. Matching with replacement increases the average quality of matching by increasing the set of possible matches. To measure the distance between different values of the covariates *X*, Abadie et al. (2004) use the weighted Euclidean vector norm (for $x \in X$, let $\| x \|_V = (x' Vx)^{1/2}$ be the vector norm with the positive definite weight matrix V being the

[30] Matching estimates are obtained using the Stata ado-file of Becker and Ichino (2002). Because Abadie and Imbens (2008) show that standard errors obtained from bootstrapping are not valid for matching estimators, we thus calculate analytical standard errors of the average treatment effect.

diagonal matrix constructed from the inverses of the variances of each element of X_i).[31]

Let $j_m(i)$ be the index of the m-th match to unit i. That is, among the units in the opposite treatment group to unit i, unit $j_m(i)$ is the m-th closest unit to unit i in terms of the covariates, measured by the Euclidean distance between the two vectors. Let $J_M(i)=\{j_i(1),\cdots,j_M(i)\}$ denote the set of indices for the first M matches for unit i. Following Abadie et al. (2004), we use the four nearest neighbors ($M=4$) in this paper. Let $K_M(i)$ denote the number of times unit i is used as a match if M matches are performed per unit, $K_M(i)=\sum_{l=1}^{N} 1\{i\in J_M(l)\}$, where $1\{\cdot\}$ is the indicator function.

The simple matching estimator for the ATT imputes the missing potential outcomes $Y_i(0)$ (i.e., counterfactual) when $T_i=1$ using the average of the outcome of the nearest neighbors of the control group:

$$\hat{Y}_i(0)=\begin{cases} Y_i & \text{if } T_i=0, \\ \dfrac{1}{M}\sum\limits_{j\in J_M(i)} Y_j & \text{if } T_i=1. \end{cases}$$

For unit i in the treated sample, the expression in the second line of the above equation gives the average value of Y_j for the matches to unit i in the control sample. Abadie and Imbens (2006) write the simple matching estimator for the ATT that uses M matches per unit with replacement as

$$\hat{\tau}_M^{m,t}=\frac{1}{N_1}\sum_{T_i=1}\left(Y_i-\hat{Y}_i(0)\right),$$

where N_1 is the number of treated units in the sample.

Abadie and Imbens (2006) point out that the simple covariate matching estimator will be biased in finite samples because matching is not exact when the matching variables are continuous. They show that this matching estimator is not $N^{1/2}$-consistent in general because it has a bias of order $O_p(N^{-1/k})$ due to matching discrepancies, where k is the number of continuous covariates.

To remove the bias, they propose a bias-corrected matching estimator, which adjusts for difference within the matches for the differences in covariate values. The adjustment is based on an estimate of the regression function $\mu_t(x)=E[Y(t)\mid X=x]$ for the control sample because we are interested in

[31] Abadie et al. (2004) define $\| z-x \|_V$ as the distance between the vector z and x, with V being the diagonal matrix constructed from the inverses of the variances of each element of the matching variables.

estimating the ATT.[32] Given the estimated regression function for the controls, we predict the missing potential outcomes $Y_i(0)$ as

$$\tilde{Y}_i(0) = \begin{cases} Y_i & \text{if } T_i = 0, \\ \frac{1}{M} \sum_{j \in J_M(i)} (Y_j + \hat{\mu}_0(X_i) - \hat{\mu}_0(X_j)) & \text{if } T_i = 1, \end{cases}$$

where $\hat{\mu}_0$ is a consistent estimator of $\mu_0(x) = E[Y(t) \mid X = x]$. The term $\hat{\mu}_0(X_i) - \hat{\mu}_0(X_j)$ in the second line of the above equation is used to adjust the counterfactual estimates to account for differences in the matching variables for each treatment observation (X_i) and its matched control observations (X_j). The bias-corrected matching estimator for the ATT that uses M matches per unit with replacement is calculated as

$$\hat{\tau}_M^{bcm,t} = \frac{1}{N_1} \sum_{T_i = 1} \left(Y_i - \tilde{Y}_i(0)\right).$$

Abadie and Imbens (2006, 2011) show that the bias-corrected matching estimator is $N^{1/2}$-consistent and asymptotically normal regardless of the number of covariates. Abadie and Imbens (2011) find that, in the simulation study, the bias-corrected matching estimator performs well in terms of bias, root-mean-squared error, and coverage rates compared to both simple matching estimators and regression estimators. In this paper, we apply the bias-corrected matching estimator of Abadie and Imbens (2006) using the Stata ado-file of Abadie et al. (2004) to estimate the average treatment effect of consistent pegs on the occurrence of currency crises.

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[32] The bias-corrected estimator combines matching with regression estimates of Y_j on covariates X_j, weighted by the number of times unit i is used as a match ($K_M(i)$), using the control sample.

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Table 1
Exchange rate regimes and currency crises, 1980-1998

Case	Groups	Regimes	Regimes (t-1)	Crises (t)	Probability
			Number	Number	(%)
Case 1	Treatment	Regime 1 (consistent pegs)	596	47	7.9%
	Control	Other regimes (except for freely falling)	686	75	10.9%
	Exclusion	Freely falling	143	32	22.4%
		Total	1425	154	10.8%
Case 2	Treatment	Regime 1 (consistent pegs)	596	47	7.9%
	Control	Regime 2 (fear of announcing a peg)	300	26	8.7%
	Control	Regime 3 (fear of pegging)	197	24	12.2%
	Control	Regime 4 (consistent floats)	189	25	13.2%
	Exclusion	Freely falling	143	32	22.4%
		Total	1425	154	10.8%

Table 2
Bias-corrected matching estimators: consistent pegs vs. other regimes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Case	Benchmark	Robustness: adding variables		Robustness: overlap condition of Becker and Ichino (2002)			Robustness: no overlap condition		
Covariates	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables
Matching estimators	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected
ATT	-0.0665***	-0.0947***	-0.0755***	-0.0662***	-0.0913***	-0.0762***	-0.0729***	-0.0983***	-0.0761***
SE	(0.0229)	(0.0239)	(0.0223)	(0.0220)	(0.0229)	(0.0215)	(0.0223)	(0.0236)	(0.0207)
Z-statistics	-2.91	-3.96	-3.39	-3.01	-3.98	-3.54	-3.28	-4.16	-3.67
Observations	869	869	858	901	901	879	926	926	909
Number of matches	4	4	4	4	4	4	4	4	4
Common support condition	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.0314, 0.9643]	[0.0314, 0.9642]	[0.0459, 0.9630]	No	No	No

	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Case	Robustness: number of matches=1			Robustness: number of matches=8			Robustness: including year dummies		
Covariates	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables
Matching estimators	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected
ATT	-0.0846***	-0.1078***	-0.1163***	-0.0653***	-0.0785***	-0.0625***	-0.0692***	-0.0768***	-0.0604***
SE	(0.0254)	(0.0277)	(0.0341)	(0.0214)	(0.0220)	(0.0198)	(0.0226)	(0.0238)	(0.0215)
Z-statistics	-3.32	-3.89	-3.42	-3.05	-3.57	-3.15	-3.06	-3.22	-2.80
Observations	869	869	858	869	869	858	869	868	850
Number of matches	1	1	1	8	8	8	4	4	4
Common support condition	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]

Note: We refer to the observation with Regime 1 (i.e., consistent pegs) as the "treatment group" and to the observation with other regimes (except for freely falling) as the "control group." We employ the nearest-neighbor bias-corrected matching estimators of Abadie and Imbens (2006). Matching and bias-adjusted variables are the log ratio of broad money to foreign reserves, domestic credit growth, the ratio of foreign liabilities to money, the current account-to-GDP ratio, real GDP growth, and real exchange rate overvaluation. In the cases of "adding 1 variable," the dummy for the capital account liberalization is added to the baseline matching variables. In the cases of "adding 3 variables," the dummy for the capital account liberalization, the typology of the political system, and the logarithm of GDP per capita are added to the baseline matching variables. When including year dummies as matching variables in columns (16)-(18), we force that the comparisons of the treated observations and the control observations are done within the same year period. The freely falling observations are excluded. ATT is the average treatment effect on the treated. The numbers in parentheses are heteroskedasticity-consistent standard errors.
*** Significant at the 1 % level.

Table 3

Bias-corrected matching estimators: consistent pegs vs. fear of announcing a peg

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Case	Benchmark	Robustness: adding variables		Robustness: overlap condition of Becker and Ichino (2002)			Robustness: no overlap condition		
Covariates	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables
Matching estimators	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected
ATT	-0.0472**	-0.0673**	-0.0494*	-0.0398*	-0.0578*	-0.0459*	-0.0507**	-0.0578*	-0.0503*
SE	(0.0226)	(0.0297)	(0.0268)	(0.0231)	(0.0317)	(0.0274)	(0.0247)	(0.0323)	(0.0283)
Z-statistics	-2.09	-2.26	-1.84	-1.73	-1.82	-1.67	-2.06	-1.79	-1.78
Observations	638	631	622	655	655	642	660	660	648
Number of matches	4	4	4	4	4	4	4	4	4
Common support condition	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.0668, 0.9909]	[0.0781, 0.9912]	[0.1015, 0.9896]	No	No	No

	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Case	Robustness: number of matches=1			Robustness: number of matches=8			Robustness: including year dummies		
Covariates	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables
Matching estimators	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected
ATT	-0.0587**	-0.0651**	-0.0662*	-0.0425*	-0.0656**	-0.0458*	-0.0584*	-0.0709**	-0.0453*
SE	(0.0276)	(0.0318)	(0.0380)	(0.0246)	(0.0308)	(0.0261)	(0.0305)	(0.0298)	(0.0257)
Z-statistics	-2.13	-2.05	-1.74	-1.73	-2.13	-1.75	-1.92	-2.38	-1.77
Observations	638	631	622	638	631	622	612	610	603
Number of matches	1	1	1	8	8	8	4	4	4
Common support condition	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]

Note: See Table 2. We refer to the observation with Regime 1 (i.e., consistent pegs) as the "treatment group" and to the observation with Regime 2 (i.e., "fear of announcing a peg") as the "control group." We estimate the average treatment effect of consistent pegs on the occurrence of currency crises using the dataset of *de facto* pegs.

** Significant at the 5 % level.

* Significant at the 10 % level.

Table 4
Bias-corrected matching estimators: consistent pegs vs. fear of pegging

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Case	Benchmark	Robustness: adding variables		Robustness: overlap condition of Becker and Ichino (2002)			Robustness: no overlap condition		
Covariates	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables
Matching estimators	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected
ATT	-0.0798*	-0.0918**	-0.1028**	-0.0826*	-0.1742***	-0.2126***	-0.0815*	-0.1721***	-0.2125***
SE	(0.0467)	(0.0437)	(0.0499)	(0.0451)	(0.0449)	(0.0641)	(0.0441)	(0.0441)	(0.0632)
Z-statistics	-1.71	-2.10	-2.06	-1.83	-3.88	-3.31	-1.85	-3.90	-3.36
Observations	522	428	408	547	548	534	554	554	543
Number of matches	4	4	4	4	4	4	4	4	4
Common support condition	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1620, 0.9970]	[0.1164, 0.9990]	[0.1292, 0.9992]	No	No	No

	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Case	Robustness: number of matches=1			Robustness: number of matches=8			Robustness: including year dummies		
Covariates	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables
Matching estimators	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected
ATT	-0.0868**	-0.1062**	-0.1341***	-0.0756**	-0.0786**	-0.0971**	-0.0939**	-0.1216***	-0.1184***
SE	(0.0381)	(0.0441)	(0.0446)	(0.0379)	(0.0382)	(0.0404)	(0.0400)	(0.0373)	(0.0395)
Z-statistics	-2.28	-2.41	-3.01	-1.99	-2.06	-2.40	-2.35	-3.26	-3.00
Observations	522	428	408	522	428	408	425	400	372
Number of matches	1	1	1	8	8	8	4	4	4
Common support condition	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]

Note: See Table 2. We refer to the observation with Regime 1 (i.e., consistent pegs) as the "treatment group" and to the observation with Regime 3 (i.e., fear of pegging or inability of pegging) as the "control group." We estimate the average treatment effect of consistent pegs on the occurrence of currency crises using the dataset of *de jure* pegs.

*** Significant at the 1 % level.

** Significant at the 5 % level.

* Significant at the 10 % level.

Table 5
Bias-corrected matching estimators: consistent pegs vs. consistent floats

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Case	Benchmark	Robustness: adding variables		Robustness: overlap condition of Becker and Ichino (2002)			Robustness: no overlap condition		
Covariates	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables
Matching estimators	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected
ATT	-0.1178**	-0.1472**	-0.1433***	-0.1197**	-0.1349**	-0.1447***	-0.1206**	-0.1412**	-0.1465***
SE	(0.0501)	(0.0589)	(0.0460)	(0.0489)	(0.0582)	(0.0519)	(0.0481)	(0.0554)	(0.0490)
Z-statistics	-2.35	-2.50	-3.12	-2.45	-2.32	-2.79	-2.51	-2.55	-2.99
Observations	526	519	493	534	534	529	552	552	546
Number of matches	4	4	4	4	4	4	4	4	4
Common support condition	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1849, 0.9938]	[0.1903, 0.9951]	[0.1992, 0.9954]	No	No	No

	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Case	Robustness: number of matches=1			Robustness: number of matches=8			Robustness: including year dummies		
Covariates	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables	Baseline	Adding 1 variable	Adding 3 variables
Matching estimators	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected	Bias-corrected
ATT	-0.1649**	-0.1937**	-0.2046***	-0.1116***	-0.1426***	-0.1329***	-0.0784*	-0.1024**	-0.0897**
SE	(0.0752)	(0.0817)	(0.0786)	(0.0433)	(0.0496)	(0.0422)	(0.0435)	(0.0431)	(0.0418)
Z-statistics	-2.19	-2.37	-2.60	-2.58	-2.87	-3.15	-1.81	-2.38	-2.14
Observations	526	519	493	526	519	493	421	421	415
Number of matches	1	1	1	8	8	8	4	4	4
Common support condition	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]

Note: See Table 2. We refer to the observation with Regime 1 (i.e., consistent pegs) as the "treatment group" and to the observation with Regime 4 (i.e., consistent floats) as the "control group." We estimate the average treatment effect of consistent pegs on the occurrence of currency crises using the dataset of Regime 1 and Regime 4.

*** Significant at the 1 % level.

** Significant at the 5 % level.

* Significant at the 10 % level.

Appendix A

Appendix Table A. Currency crises, 1980-1998

Country	2-year window	Country	2-year window	Country	2-year window	Country	2-year window
Australia	81, 85	Ecuador	82, 86, 88	Central African Republic	94	Swaziland	82, 85
Austria		Egypt	81, 89, 91	Costa Rica	81	Syrian Arab Republic	82, 88, 91
Belgium	82	India	88, 90, 93	Cyprus	95	Tanzania	86
Canada		Indonesia	82, 86, 97	Dominican Republic	85	Togo	94
Denmark		Jordan	88	El Salvador	86, 90	Tunisia	81, 92
Finland	92	Korea	97	Gabon	89, 94	Uganda	83, 85, 87, 89
France	82	Malaysia	97	Ghana	83, 87, 90	Uruguay	82, 84
Greece	83, 85	Mexico	82, 85, 94	Grenada		Zambia	86, 94
Iceland	80, 82	Morocco	81	Guatemala	86	Zimbabwe	91, 97
Ireland	86	Nigeria	82, 86, 92, 94	Guyana	82, 87, 89, 91		
Italy	92, 95	Pakistan	82, 96	Haiti	82		
Japan	89	Panama		Honduras	90		
Netherlands		Peru	87, 90	Jamaica	83, 91		
New Zealand	82, 84	Philippines	84, 97	Kenya	81, 95, 97		
Norway	92	Singapore	97	Lesotho			
Portugal	82	South Africa	84	Madagascar	81, 87, 94		
Spain	82, 92	Sri Lanka	89, 98	Malawi	82, 92, 94, 98		
Sweden	82, 89, 92	Thailand	80, 82, 97	Mali	94		
Switzerland	81	Turkey	91, 94	Malta	85, 95		
United Kingdom	81, 92	Venezuela	84, 86, 89, 94	Mauritania	85, 91		
Argentina	81, 83, 89, 91	Algeria	90	Mauritius	81, 83, 92		
Brazil	83, 90	Bolivia	81, 83, 85, 88, 90	Nepal	84, 93		
Chile	82, 85	Botswana	82, 84	Niger	94		
China, P.R.: Hong Kong		Burundi	83, 88, 98	Paraguay	84, 86, 89, 92		
Colombia	83	Cameroon	84, 94	Senegal	94		

Source: Esaka (2010).

Appendix B

Appendix Table B. Propensity score matching

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment groups	Regime 1 (consistent pegs)			Regime 1 (consistent pegs)		
Control groups	Other regimes			Regime 2 (fear of announcing a peg)		
Matching methods	Nearest neighbor	Radius (*r*=0.01)	Radius (*r*=0.05)	Nearest neighbor	Radius (*r*=0.01)	Radius (*r*=0.05)
ATT	-0.063**	-0.070***	-0.070***	-0.034	-0.052**	-0.044*
SE	(0.027)	(0.021)	(0.021)	(0.029)	(0.025)	(0.024)
T-statistics	-2.303	-3.247	-3.377	-1.157	-2.044	-1.787
Treatment observations	415	409	415	415	404	411
Matched control group observations	222	447	454	154	217	218
Common support condition	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]

	(7)	(8)	(9)	(10)	(11)	(12)
Treatment groups	Regime 1 (consistent pegs)			Regime 1 (consistent pegs)		
Control groups	Regime 3 (fear of pegging or inability of pegging)			Regime 4 (consistent floats)		
Matching methods	Nearest neighbor	Radius (*r*=0.01)	Radius (*r*=0.05)	Nearest neighbor	Radius (*r*=0.01)	Radius (*r*=0.05)
ATT	-0.099*	-0.114***	-0.119***	-0.108**	-0.076*	-0.078*
SE	(0.058)	(0.040)	(0.039)	(0.050)	(0.045)	(0.043)
T-statistics	-1.691	-2.853	-3.055	-2.171	-1.686	-1.799
Treatment observations	415	388	407	415	396	415
Matched control group observations	97	120	124	90	110	112
Common support condition	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]	[0.1, 0.9]

Note: In the first stage, we estimate the probability of adopting Regime 1 using multinomial probit models. The dependent variable is the index of exchange rate regimes. We use the same covariates (pre-treatment variables) as the benchmark of the bias-corrected matching estimators in Tables 2-5. In the second stage, we estimate the average treatment effect of Regime 1 on the frequency of currency crises by calculating the mean difference in outcomes between the treated and the control observations using propensity score matching. In this analysis, we employ nearest-neighbor and radius matching methods as propensity score matching methods. We employ two radius matching estimators: *r*=0.01 and *r*=0.05. The freely falling observations are excluded. Following Crump et al. (2009), we use the observations of the interval [0.1, 0.9] for the estimated propensity scores as the common support condition. ATT is the average treatment effect on treated. The numbers in parentheses are analytical standard errors of the treatment effect.

*** Significant at the 1 % level.

** Significant at the 5 % level.

* Significant at the 10 % level.

Figure 1
***De jure* and *de facto* exchange rate regimes**

Exchange rate regimes		*De facto* classification (Reinhart and Rogoff, 2004)		
		1	2	3
IMF classification		Pegs	Floats	Freely falling
1	Pegs	**Regime 1** *De jure* pegs and *de facto* pegs **Consistent pegs**	**Regime 3** *De jure* pegs and *de facto* floats **Fear of pegging** or **Inability of pegging**	**Freely falling** High inflation
2	Floats	**Regime 2** *De jure* floats and *de facto* pegs **Fear of announcing a peg**	**Regime 4** *De jure* floats and *de facto* floats **Consistent floats**	

Sources: Levy-Yeyati and Sturzenegger (2005), Genberg and Swoboda (2005), and Alesina and Wagner (2006).