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Are capital controls in the foreign exchange market effective?☆

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A B S T R A C T

One of the reasons for governments to employ capital controls is to obtain some degree of monetary independence. In this paper we test whether capital controls can reduce the link between exchange rates fluctuations and cross border interest differentials. Recent capital control proxies are used in order to determine the date of capital account liberalization for a panel of Western European and emerging countries. Results show that capital controls have a very limited effect on observed deviations from interest parities, even when accounting for the political risk associated with capital controls.

1. Introduction

Academic and policy debates about vices and virtues of capital controls exist for a long time and opinions on their usefulness swing like a pendulum. The argument dates back as far as the mercantilists who sought to control flows of bullion. This ideological school was subsequently denounced by Adam Smith in favor of free markets. The 20th century saw a large revival of capital controls, driven by

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the war effort of both world wars. Afterwards the Bretton Woods system combined capital controls with fixing exchange rates. Keynes considered capital controls as an important cornerstone to financial stability during the Bretton Woods system – an idea revived by Tobin (1978). The meltdown of the Gold Exchange Standard induced a liberalization wave that lasted through the 1990s. The aftermath of the Asian crisis made some reconsider the wisdom of the widespread liberalization and the debate was re-oriented once more. Nowadays, a significant fraction of the academic community supports controls in specific circumstances, see e.g. Krugman (1999) or Rodrik (1998), and advocates of a well-thought phasing out of capital controls.

The classic argument in favor of capital controls goes back to the welfare theory of the 'second best': in the presence of market imperfections (incomplete markets, asymmetric information, transaction costs etc.) additional distortions such as capital controls might be welfare enhancing by offsetting some of the previous distortions' negative welfare effects. However, capital controls remain a distortion and as such should only be maintained if the benefits outweigh the costs. And the potential costs are numerous: capital controls have to be regularly revised to close loopholes; time and resources have to be expended to execute the controls; controls limit the potential for portfolio diversification and decrease the amount of risk that can be shared and diversified (Voth, 2003) and increase the cost of capital for local firms (Bekaert and Harvey, 2000); capital controls potentially increase exchange rate volatility (Glick and Hutchison, 2005) and the risk of currency crises (Bordo et al., 2001).

This paper's goal is not to perform a full-fledged welfare analysis to quantify direct and indirect costs and benefits of capital controls: our ambition is rather more modest in that we want to investigate whether capital controls are able to bring more 'monetary freedom' in the foreign exchange market. Loosely speaking, 'monetary freedom' can be thought of as the desire to manage domestic monetary policy in a more independent way from the exchange rate. Monetary freedom constitutes one of the classic motivations for governments to impose capital controls (Magud et al., 2011). The potential gain in monetary freedom allows governments to use the monetary and fiscal instruments more effectively together to steer the domestic economy.

Magud et al. (2011) survey the literature and conclude that inflow controls (but not outflow controls) contributed to increased monetary freedom in several well documented cases like Chile, Colombia, Malaysia and Thailand. In a broader setting, including more countries, this effect cannot be replicated: Montiel and Reinhart (1999) show that capital controls do not improve the ability of monetary policy to change the composition of capital flows (although capital controls themselves may have a direct effect on the composition); Edison and Reinhart (2001) find that capital controls do not affect the co-movement of domestic and foreign interest rates, and Miniane and Rogers (2007) show that the presence of capital controls do not diminish the impact of U.S. monetary policy shocks on the domestic economy.

In this paper we take an alternative route to assess the effect of capital controls on 'monetary freedom'. We investigate to what extent capital controls contribute to deviations from the (covered and uncovered) interest parity conditions for foreign exchange. Given the potential of capital controls to limit arbitrage and speculation, exchange rate parity conditions constitute a natural testing framework for the 'monetary freedom' hypothesis: the well-known Covered and Uncovered Interest Parity relations relate cross-border interest differentials to current and future (expected) price formation in foreign exchange markets in the following way:

$$(f - s)_t = (i - i^*)_t, \tag{1}$$

$$E_t s_{t+1} - s_t = (i - i^*)_t, \tag{2}$$

with E_t the rational expectations operator, s_t and f_t the natural logarithms of the nominal bilateral spot and 1-month forward exchange rate, expressed in domestic currency per unit of foreign currency, and i and i^* domestic and foreign interest rates on monthly deposits, respectively.[1]

[1] The majority of the empirical literature studying deviations from these parity conditions tests these equations using a monthly time horizon. For sake of comparability we therefore opt for the same time horizon and data frequency.

The Covered Interest Parity (CIP) condition (1) reflects that proceeds on foreign currency deposits should equal the proceeds on equal-maturity domestic deposits after converting the foreign proceeds into domestic currency via the forward market. The Uncovered Interest Parity (UIP) relation (2) implies that (risk-neutral) investors are indifferent between equal-maturity domestic and foreign deposits provided the (time t expected) deposit returns (converted in the domestic currency via the time t spot market expectation) are the same.

Empirically there is substantial evidence that CIP holds well, although small but significant deviations are sometimes observed (Clinton, 1988; Fletcher and Taylor, 1996; Akram et al., 2008). Deviations from UIP are much more severe and widespread. A large number of possible explanations for this 'forward discount bias' puzzle have been proposed (and empirically tested) ranging from time varying risk premia (see e.g. Fama, 1984; Cavaglia et al., 1994; Wolff, 1987). Learning (Lewis, 1989), central bank intervention (McCallum, 1994) deviations from rationality (Frankel and Froot, 1987), peso problems (Kaminsky, 1993; Flood and Rose, 1996), or monetary volatility (Moore and Roche, 2012), but all with limited success. Comprehensive surveys on the forward discount bias and its potential causes include Chinn (2006), Engel (1996), and Sarno (2005).

Only a handful of papers empirically investigated whether capital controls distort parity conditions.[2] Francis et al. (2002) show that deviations from UIP can be partly attributed to financial liberalization (although results are highly country specific). Phylaktis (1988, 1990) also finds that capital controls explain part of observed UIP differentials in several Latin American countries. These last two studies explicitly split the effects of capital controls into direct tax effects and indirect political risk effects. The political risk premium can be seen as an indirect effect of capital controls arising from expected shifts in the severity of capital controls. Within the frameworks of the cited articles, both effects exist and can partially explain deviations from parity conditions. Dooley and Isard (1980) give evidence that capital controls significantly impacted offshore-onshore interest differentials in Germany, again both through direct tax effects and indirect political risk effects. Finally, Holmes and Wu (1997) hardly find any evidence that the financial liberalization of Europe in the 1990s affected deviations from CIP.

This paper considers regression equations of the framework outlined in Equations (1) and (2), augmented with proxies for capital controls and political risk premiums. Augmenting the parity regressions with those variables enables one to determine the contributions of these variables to observed deviations from CIP and UIP. Anticipating on our results, we find some evidence that capital controls can effectively distort the covered interest arbitrage condition. However, capital controls seem unable to explain even a fraction of the observed forward discount bias in the UIP relation. This also implies that capital controls are ineffective in creating more monetary freedom for domestic monetary authorities. For some countries, the installment of capital controls even leads to an erosion of monetary freedom. Surprisingly, these results are not fundamentally altered when controlling for political risk premia.

The remainder of the paper is structured as follows. Section 2 provides a detailed discussion on the used regression methodology and how the capital control proxies and resulting political risk premiums are determined. In Section 3 we present and interpret outcomes of the CIP regressions and UIP regressions augmented with proxies for capital controls and other control variables. Finally, Section 4 provides some concluding remarks.

2. Methodology and data

This paper's regression-based approach is reminiscent of earlier empirical work on the validity of parity conditions. The CIP condition (1) is typically tested by regressing the (1-month) forward premiums on (1-month) cross-border interest differentials:

[2] The suggestion itself that capital controls play a role in explaining deviations from CIP and UIP is not new, however. Chinn and Meredith (2005), Dahlquist and Gray (2000) and Frankel and Poonawala (2006), amongst others, mention capital controls as a potential reason for the failure of UIP; and Gibson (1989) and Johnston (1983) argue that capital controls could have an effect on onshore covered interest differentials.

$$(f-s)_t = \alpha + \beta(i-i^*)_t + \varepsilon_t. \tag{3}$$

As for testing (2), we replace the ex ante (1-month) expected spot rate change in (2) by its ex post realized value and regress it on the (1-month) lagged interest differential:

$$s_{t+1} - s_t = \alpha + \beta(i-i^*)_t + \nu_{t+1}. \tag{4}$$

The spot and 1-month forward rates (local currency units per US$) are retrieved from Thomson DataStream. Interest rates are retrieved from the IMF International Financial Statistics (IFS) database. Note that the interest rates used in this study are all onshore rates, unless specified otherwise. We use onshore rates because these are most likely to be affected by capital controls. In contrast offshore rates are generally not affected by domestic capital controls. We will nevertheless also use offshore rates to determine our proxy for the political risk premium.

The bulk of existing empirical evidence points towards a rejection of the joint null hypothesis (H_0:$\alpha = 0;\beta = 1$) for the UIP regressions, and there is some evidence for small deviations from the joint null in the case of CIP. We would like to know to what extent capital controls contribute to these deviations. To this aim, we augment the benchmark regressions with proxies for capital controls (and some other control variables). We allow for a potential capital controls (CC) impact on both the intercept α and the slope β by letting CC enter the regressions in both an additive and a multiplicative way (interaction term). This gives rise to the following pair of testing equations for CIP and UIP, respectively:

$$(f-s)_t = \alpha + \beta(i-i^*)_t + \gamma_1 CC_t + \gamma_2 CC_t(i-i^*)_t + controls + \varepsilon_t, \tag{5}$$

$$s_{t+1} - s_t = \alpha + \beta(i-i^*)_t + \gamma_1 CC_t + \gamma_2 CC_t(i-i^*)_t + controls + \nu_{t+1}. \tag{6}$$

2.1. Capital controls

Measuring capital controls is difficult, because a large array of different types of controls exist. Loosely speaking, capital controls can be divided into administrative controls such as outright bans, and market-based controls such as taxes; controls that aim to curb short-term capital flows versus long-term capital flows; controls that aim residents versus controls on non-residents; or controls on inflows versus controls on outflows. Furthermore each of these types of controls can be applied to any of the many different financial markets that exist in a country. The popular IMF capital control dummy, published in the Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER), has been a traditional choice as a capital control proxy.[3] However, by now this capital control proxy is accepted to be too 'crude' and several attempts have been made to come up with more refined measures. Several studies have succeeded in building refined indices for a particular country such as Chile (Edwards and Rigobon, 2005), or a particular sector such as equities (Edison and Warnock, 2003); however the limited scope of these proxies means they cannot be used to look at the general use of capital controls measured across a range of countries. Therefore this paper focuses on proxies that cover a wide range of countries and captures a wide range of different capital controls. For a survey on these capital control indices see Quinn et al. (2011).

In this paper we will use the Miniane (2004) index for our European sample and the Schindler (2009) index for our emerging sample.[4] Both proxies are based on the new post-1996 AREAER capital control classification. They take on a value between 0 and 1 reflecting the proportion of financial market segments that exhibit capital controls in a given country during a given year. A score of 0 indicates a fully liberalized market; whereas a score of 1 reflects a domestic financial system that is fully

[3] For example Alesina et al. (1994), Chanda (2001), Epstein and Schor (1992), Garret (1995), Grilli and Milesi-Ferretti (1995), Leblang (1995), Milner (1996), Razin and Rose (1994), and Rodrik (1998) all use the IMF dummy in their studies.

[4] The Miniane index is available from http://www.imf.org/external/pubs/ft/staffp/2004/02/miniane.htm, the Schindler index from http://www.palgrave-journals.com/imfsp/journal/v56/n1/suppinfo/imfsp200828s1.html.

insulated from the rest of the world. More details on the construction of these proxies (and more specifically the separate financial market segments for which capital controls are measured) are provided in the Appendix. The *de jure* nature of our capital control proxies implies that countries declare to the IMF how high or low these proxies are but the IMF does not measure or observe them directly, i.e. *de jure* capital control intensities do not necessarily reflect the real effective capital control intensity in the market place (*de facto*). Still these indices are currently the most detailed information sets that are available for a broad range of countries and over relatively long time spans.

The Miniane index has been selected for the European sample, because it spans a longer time horizon, going all the way back to the early 80s. However, there is not enough information available to disentangle controls on capital inflows and outflows from the early 80s onwards, i.e., the Miniane index is only available in aggregate form.

Contrary to the Miniane index, the Schindler index only goes back to 1995. Given that financial liberalization already occurred prior to 1995 in most of our European sample, the Schindler index proxies for European countries (albeit in existence) are unsuited to identify the impact of capital controls on monetary freedom. However, the more detailed nature of the Schindler index makes it a natural choice for the emerging sample: the Schindler index not only covers a wider range of emerging countries than the Miniane index, it also enables one to distinguish between controls on capital inflows and outflows. Notice also that the relatively late starting date of the Schindler index (it starts in 1995) does not constitute a major obstacle to identify the impact of emerging capital controls on parity deviations because financial liberalization in emerging markets is a relatively recent phenomenon.

Figs. 1 and 2 show annual capital control proxies for our European sample and emerging sample, respectively. Those countries are included for which capital control proxies are available over sufficiently long time periods and with sufficient time variation to make the econometric identification of capital control effects possible. The European sample includes Austria, Denmark, Spain, France, Italy, Norway, Portugal, Sweden, Belgium, the Netherlands, and Switzerland. The emerging sample includes the Czech Republic, Mexico, Malaysia, Thailand, Turkey, Argentina, Brazil, Indonesia, Romania, Russia and Korea.

Fig. 1 shows that European countries gradually liberalized their capital accounts in the 80s and 90s on the way towards the creation of a single European currency. The Figure also illustrates that there was substantial cross country heterogeneity in the speed of European capital account liberalization. Fig. 2 shows that, in contrast to the European sample, there does not seem to be a general trend towards liberalization in the selected emerging markets. Most Asian countries (except South Korea) actually sustained or strengthened existing capital controls in the aftermath of the Asian crisis. Remarkable is also the radically opposite approaches towards capital account liberalization in neighboring countries Argentina and Brazil. Finally the Figure shows that countries seem to employ more controls on outflows than controls on inflows. As evidence seems to suggest that inflow controls are generally more effective (Magud et al., 2011), this is slightly surprising.

Throughout our analysis we make the simplifying assumption that the United States (US$ acts as numéraire currency) and the United Kingdom (where euro-currency deposit rates are priced) are both fully liberalized. Stated otherwise, potential direct and indirect effects of US and UK based capital controls are set at zero for sake of convenience. We believe this is a reasonable assumption with an eye towards the value of the Miniane index and the Schindler index in both of these countries. The low average Miniane index values observed for the US (0.26) and the UK (0.08) seem to confirm this. Moreover, the Schindler index reveals that neither country has inflow controls. Outflow controls are also quite small in the US (0.25 on average) and they are even completely absent in the UK.

2.2. Political risk

Some of the earlier exchange rate literature has separated the effects of capital controls into those caused by a 'direct' tax effect on arbitrageurs and speculators and a political risk premium associated with 'prospective' controls, see e.g. Aliber (1973), Dooley and Isard (1980) or Phylaktis (1988) for earlier references. Loosely speaking, the political risk premium reflects the perceived possibility that controls will be imposed on capital flows (or that the intensity of existing capital controls may be altered in the future). As the political risk is unobserved by definition we need to identify it before we can include it in

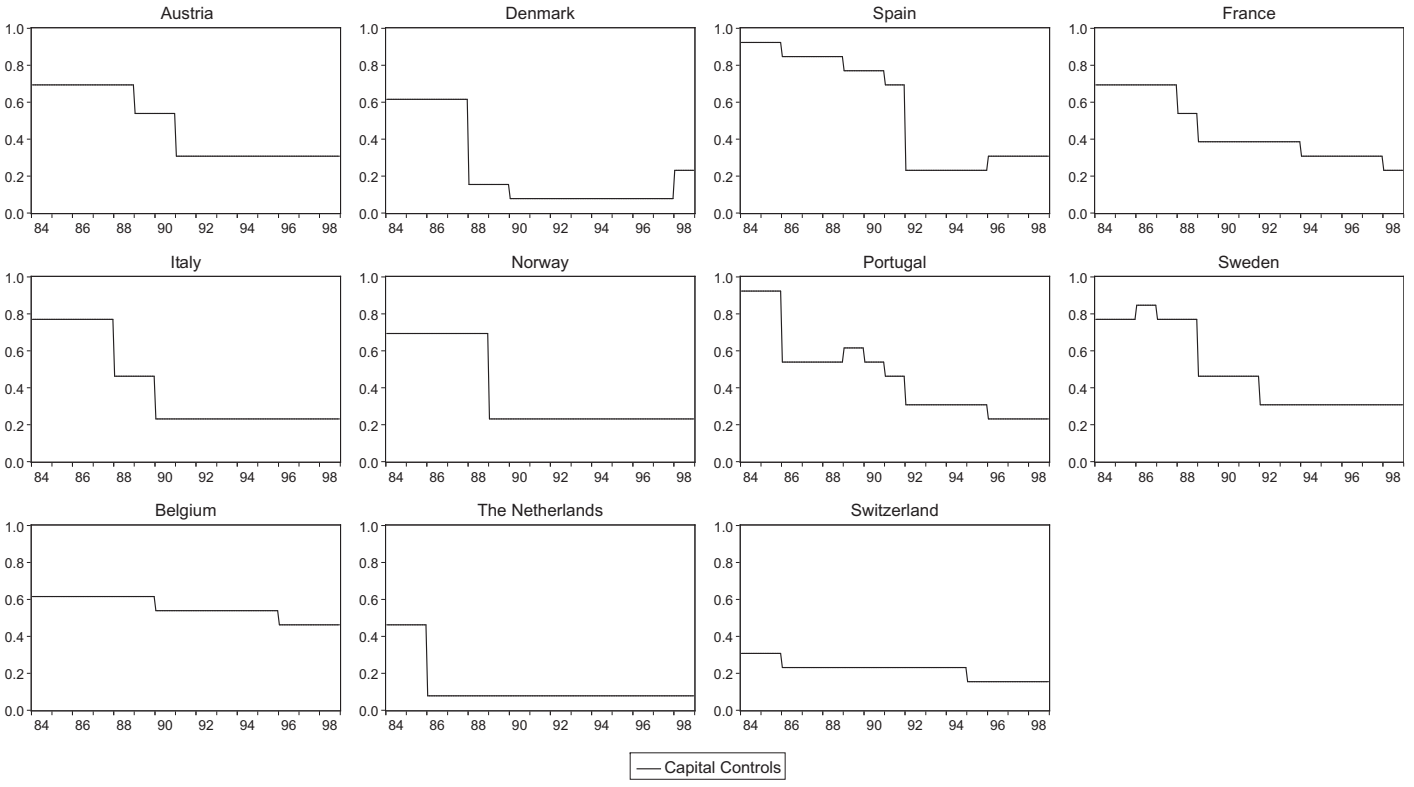

Fig. 1. Capital controls, European countries (Miniane (2004) Index).

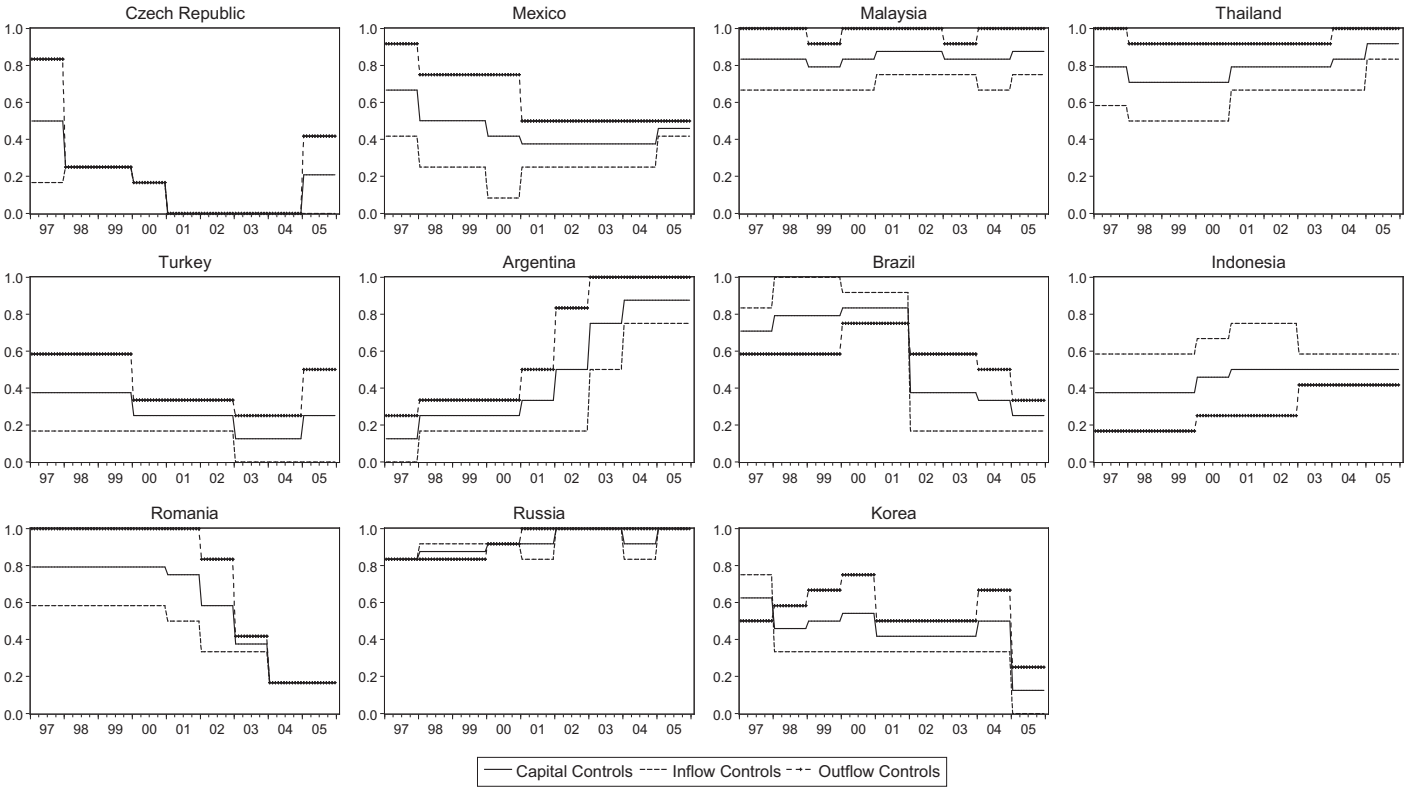

Fig. 2. Capital controls, emerging countries (Schindler (2009) Index).

our UIP testing regression.[5] Dooley and Isard (1980) proposed an identification approach for political risk based on "offshore" and "onshore" interest rates on deposits with similar maturity, currency, and risk-profiles. Offshore deposits, sometimes also called euro-deposits, are deposits offered in the local currency, but held abroad. If capital can move freely and governments are equally credible, there should be no difference in interest rates on euro-deposits and domestically held deposits. We decompose the offshore-onshore interest differential into two parts: the first part is the observed direct effect of capital controls; the second part is attributed to the political risk that is associated with holding deposits in the target country:

$$\left(i_{\text{off}} - i\right)_t = \eta_0 + \eta_1 CC_t + controls + \rho_t. \tag{7}$$

We proxy the political risk associated with country i as part of the offshore-onshore interest differential that cannot be explained by the direct effects of capital controls, i.e. the political risk thus boils down to $PR_t \equiv \eta_0 + \rho_t$. Remember that it is assumed that there are no capital controls effects associated with the off-shore country (the UK). The onshore rates are identical to the interest rates used in the regressions above whereas offshore interest rates are proxied with 1-month eurocurrency interest rates taken from DataStream. As offshore rates were not available for all countries, the political risk regression will be estimated on a reduced sample which includes all countries for which data is available.

The "direct" tax effect in Dooley and Isard's terminology boils down to $\eta_1 CC$ and can be either positive or negative depending on whether the capital control CC refers to inflows or outflows. In order to grasp this point, first assume that $(i_{\text{off}} - i)_t > 0$. In this situation, it is profitable for arbitrageurs to borrow capital in the onshore market and re-invest it into an offshore deposit; the resulting capital outflow would ultimately drive down the difference in the two interest rates in the absence of controls on outflows. (Effective) outflow controls that (partially) prevent this type of arbitrage are expected to widen the offshore-onshore differential ($\eta_1 > 0$). Conversely, when $(i_{\text{off}} - i)_t < 0$, effective inflow controls are expected to widen the offshore-onshore interest differential ($\eta_1 < 0$). As for the aggregate capital control measure, its coefficient sign can either be negative or positive.

Including the political risk premium, *PR*, in both an additive and multiplicative way in Equation (6) leads to the extended testing regression:

$$\Delta s_{t+1} = \alpha + \beta(i - i^*)_t + \gamma_1 CC_t + \gamma_2 CC_t(i - i^*)_t + \delta_1 PR_t + \delta_2 PR_t(i - i^*)_t + \text{controls} + \nu_{t+1}. \tag{8}$$

If (5) and (8) are correctly specified and capital controls are effective, the intercept α and slope β come closer to their theoretical values of 0 and 1 and the parameter vector (γ_1,γ_2,δ_1,δ_2)is expected to be statistically significantly different from zero. Although studying the impact of capital controls and political risk on α is also interesting, we will mainly focus on the impact of capital controls and political risk on β as this directly reflects the change in monetary freedom due to capital controls. Effective capital controls by definition increase monetary freedom (and decrease β) provided γ_1 and δ_2 are negative.

The testing Equations (5), (6) and (8) will be estimated for the European currency panel and the emerging currency panel separately using the Seemingly Unrelated Regression (SUR) estimator. This panel estimation technique takes into account the cross sectional dependence between the regression residuals in order to increase the estimation efficiency as compared to single equation OLS outcomes. Cross sectional correlations in currency panels can be relatively high due to the common numéraire currency (US$) against which all exchange rates are expressed.

2.3. Control variables

To account for other factors that may interact with the effect of capital controls on exchange rate parities we control for exchange rate regimes and currency crises.[6] Multistep exchange rate regime

[5] Interest arbitrage by definition does not involve expectational variables. Political risk premiums are therefore not included in the CIP testing regressions.

[6] We thank the referees for their suggestion to also control for political risk and currency regimes.

Table 1
Covered interest parity: SUR regressions for European countries (January 1984–December 1998, $T = 180$).

Country	CIP base model		CIP regression with capital controls			
	α ($\times 10^{-3}$)	β	α ($\times 10^{-3}$)	β	γ_1 ($\times 10^{-3}$)	γ_2
Austria	−0.031 (0.029)	0.993*** (0.011)	0.206** (0.094)	0.948*** (0.038)	−0.509** (0.234)	0.043 (0.102)
Denmark	−0.399*** (0.073)	1.176***††† (0.022)	−0.155 (0.107)	1.158***††† (0.032)	−0.968** (0.464)	0.059 (0.191)
Spain	1.475*** (0.234)	0.771***††† (0.040)	−2.699*** (0.870)	1.401***††† (0.114)	5.720*** (1.060)	−0.942*** (0.150)
France	0.329*** (0.096)	0.984*** (0.021)	−0.158 (0.291)	1.078*** (0.106)	0.675 (0.438)	−0.219 (0.278)
Italy	−0.262 (0.178)	0.960*** (0.037)	−0.836** (0.353)	1.195***††† (0.070)	2.087** (0.865)	−0.680*** (0.163)
Norway	0.000 (0.072)	0.918***††† (0.017)	−0.467*** (0.149)	0.905***†† (0.039)	1.508*** (0.461)	−0.060 (0.098)
Portugal	3.260*** (0.668)	0.698***††† (0.082)	−4.435** (1.877)	0.773*** (0.176)	13.472*** (3.580)	−0.075 (0.347)
Sweden	0.193* (0.108)	0.895***††† (0.024)	0.052 (0.360)	0.862***†† (0.064)	0.064 (0.572)	0.123 (0.136)
Belgium	0.492*** (0.058)	0.895***††† (0.024)	−2.625*** (0.635)	1.959***†† (0.381)	5.612*** (1.146)	−1.934*** (0.700)
Netherlands	−0.050 (0.031)	0.995*** (0.012)	−0.047 (0.051)	0.993*** (0.020)	−0.011 (0.448)	0.020 (0.157)
Switzerland	0.153* (0.087)	0.846***††† (0.024)	−0.627 (1.175)	0.960*** (0.297)	3.529 (5.170)	−0.503 (1.225)
R^2	0.872		0.894			
Wald					87.913***	66.340***

The left panel contains CIP regression results for the base model: $(f - s)_t = \alpha + \beta(i - i^*)_t + \varepsilon_t$. The right panel contains CIP regression results with capital controls: $(f - s)_t = \alpha + \beta(i - i^*)_t + \gamma_1 CC_t + \gamma_2 CC_t(i - i^*)_t + controls + \varepsilon_t$. Where *controls* contain the currency regime and crisis dummies. Standard errors are reported between brackets below each point estimate and the average goodness of fit in the SUR panel is denoted by R^2. The capital controls proxy used is the Miniane index. The bottom row, *Wald*, reports the Wald test of the cross equation restriction that $\gamma_i = 0$ across countries. Asterisks indicate coefficients that are (statistically) significantly different from zero at the 10% level (*), 5% level (**) or 1% level (***). Statistically significant slope deviations from 1 ($\hat{\beta} - 1$) are denoted at the 10% level (†), 5% level (††) or 1% level (†††).

dummies are based on the Reinhart and Rogoff (2004) classification and reflect 6 possible categories of differing exchange rate fixity, with lower values indicating higher levels of fixity.[7] Financial crises dummies are included for the relevant countries to isolate wild swings in interest rates associated with crisis periods. The following crises are included: the Scandinavian crisis (1992) in the European sample; the currency crises in Asia (1997–1998), Argentina (2001), Brasil (1998), Mexico (1998), Russia (1998) and Turkey (2000–2001) for the emerging sample.

3. Estimation results

First, we discuss the estimation outcomes for the Covered Interest Parity (CIP) testing equation (Section 3.1). Next, we move on to a presentation of the results for the Uncovered Interest Parity (UIP) testing equation (Section 3.2). Both testing equations are augmented with capital control proxies as well as other control variables. As the paper focuses on whether capital controls bring more monetary freedom or not, we will mostly pay attention to the outcomes of the interaction terms involving capital controls and political risk premiums: they potentially impact the slope β which reflects the strength of the relation between interest rate differentials and exchange rates.

3.1. Deviations from Covered Interest Parity

Tables 1 and 2 contain the CIP regression outcomes for the European and the emerging panels, respectively. In order to disentangle the impact of capital controls on CIP deviations, we run regressions

[7] The Reinhart and Rogoff exchange rate classification can be found on http://www.carmenreinhart.com/research/publications-by-topic/exchange-rates-and-dollarization/.

Table 2
Covered interest parity: SUR regressions for emerging countries (January 1997–December 2005, $T = 108$).

Country	CIP base model		CIP regression with capital controls					
	α ($\times 10^{-3}$)	β	α ($\times 10^{-3}$)	β	γ_1^{in}($\times 10^{-3}$)	γ_2^{in}	γ_1^{out}($\times 10^{-3}$)	γ_2^{out}
Czech Republic	−0.008	0.893***†††	−0.569***	1.398***	−0.509	−1.080 **	2.472***	−0.636***
	(0.086)	(0.020)	(0.148)	(0.115)	(0.850)	(0.418)	(0.553)	(0.104)
Malaysia	0.946***	0.616***†††	−3.336	−3.419†	3.767	3.607 *	1.568	1.543
	(0.108)	(0.057)	(3.451)	(2.526)	(2.602)	(2.026)	(2.987)	(1.850)
Mexico	−0.148	1.035***	−14.512	2.516***†	−5.968	0.348	27.957*	−2.536
	(0.924)	(0.098)	(8.992)	(0.913)	(14.796)	(1.732)	(14.676)	(1.592)
Thailand	2.200***	0.513***†††	6.738	0.987	−4.533	−4.153 ***	−2.385	1.875
	(0.287)	(0.040)	(4.828)	(1.175)	(3.448)	(1.587)	(5.585)	(1.879)
Turkey	2.066	1.013***	5.977	0.937***	−41.524**	0.873	−5.094	0.083
	(1.597)	(0.019)	(3.867)	(0.115)	(20.029)	(0.718)	(7.325)	(0.231)
Wald					8.982	17.955***	24.310***	40.888***

The left panel contains CIP regression results for the base model: $(f - s)_t = \alpha + \beta(i - i^*)_t + \varepsilon_t$. The right panel contains CIP regression results with separate controls for inflows and outflows: $f_t - s_t = \alpha + \beta(i - i^*)_t + \gamma_1^{in} CC_t^{in} + \gamma_2^{in} CC_t^{in}(i - i^*)_t + \gamma_1^{out} CC_t^{out} + \gamma_2^{out} CC_t^{out}(i - i^*)_t + controls + \varepsilon_t$. Where *controls* contain the currency regime and crisis dummies. Standard errors are reported between brackets below each point estimate and the average goodness of fit in the SUR panel is denoted by R^2. The capital controls proxy used is the Schindler index. The bottom row, *Wald*, reports the Wald test of the cross equation restriction that $\gamma_i^j = 0$ across countries. Asterisks indicate coefficients that are (statistically) significantly different from zero at the 10% level (*), 5% level (**) or 1% level (***). Statistically significant slope deviations from 1 ($\widehat{\beta} - 1$) are denoted at the 10% level (†), 5% level (††) or 1% level (†††).

both with and without a capital control proxy. The left panels in Tables 1 and 2 correspond with the base regression (3) whereas the right panels in both tables contain regression results for the augmented regression model (5). Capital controls are deemed effective if they bring more monetary independence, i.e. if γ_2 in Table 1 or γ_2^{in} and γ_2^{out} in Table 2 are significantly negative.

The benchmark CIP regression – Equation (3) – renders coefficient estimates of α and β that are often significantly different from their theorized values (0 and 1) in both samples. However, the economic magnitudes of these parity deviations are in most cases relatively small, implying a small economic significance – Portugal, Malaysia and Thailand constitute notable exceptions, exhibiting both large additive and multiplicative deviations from CIP.[8] Also, notice that the regression fit (measured by R^2) exceeds 0.8 for both the European sample and the emerging sample.

Upon inclusion of the capital controls proxy the goodness-of-fit as measured by R^2 rises only marginally, indicating that the marginal explanatory power of capital controls is not very large. The capital control proxies (additive terms as well as interaction terms) are often statistically significant and the cross-equation restriction that $\gamma_1 = 0$ and $\gamma_2 = 0$can be rejected at the 1% level for all but one case (last rows of Tables 1 and 2). Upon looking at the t-statistics of the γ estimates per country, one can see that the rejection of the cross equation restriction $\gamma_2 = 0$ seems to be driven by 3 countries in the European sample. In the emerging sample, it can be seen that both inflow controls and outflow controls have a significant impact on CIP deviations: Mexico shows a significant effect of outflow controls; Malaysia, Thailand, and Turkey have a significant effect of inflow controls; The Czech Republic shows a significant effect of both inflow and outflow controls. Especially clear is the *slope* effect of inflow controls, with the Czech Republic, Malaysia and Thailand all having a significant estimate. The result of the emerging sample is interesting in a couple of ways. The fact that mainly the inflow controls are those that are found to be effective corroborates the conclusion of Magud et al. (2011). Also, we find that for Malaysia, the inflow controls had a significant impact, but the outflow controls did not. This finding contradicts the general belief that Malaysia's outflow controls were particularly effective, more so than outflow controls in most other countries (a finding also reported in Magud et al. (2011)).

[8] The exchange rate data used in the regressions are the mid-point prices calculated as averages of bid and ask quotes. The parity deviations observed are generally smaller than the bid-ask spread prevailing at the time.

Table 3
Uncovered interest parity: SUR regressions for European countries (January 1984–December 1998, $T = 180$).

Country	UIP base model		UIP with capital controls			
	α ($\times 10^{-3}$)	β	α ($\times 10^{-3}$)	β	γ_1 ($\times 10^{-3}$)	γ_2
Austria	−3.022 (2.514)	−0.418**†††(0.230)	−1.389 (3.220)	−0.805† (1.001)	−3.663 (5.391)	0.201 (2.392)
Denmark	−1.805 (2.451)	−0.379††† (0.253)	−1.093 (2.608)	−0.615††† (0.406)	−2.420 (5.085)	0.723 (2.036)
Spain	−3.305 (3.274)	0.491 (0.411)	−3.085 (9.756)	1.260 (1.270)	4.796 (11.663)	−2.356 (1.655)
France	−1.601 (2.609)	−0.578**††† (0.271)	−2.012 (4.060)	1.521 (1.284)	5.570 (5.107)	−6.295* (3.218)
Italy	−2.927 (3.864)	0.495 (0.684)	−4.983 (7.601)	1.198 (1.478)	5.697 (17.846)	−1.783 (3.368)
Norway	0.751 (2.301)	−0.547††† (0.343)	−0.685 (3.573)	−1.045†† (0.823)	6.818 (9.460)	0.162 (2.009)
Portugal	2.595 (2.848)	−0.039††† (0.235)	−8.076 (6.628)	0.017 (0.625)	18.916 (11.930)	−0.184 (1.176)
Sweden	−2.492 (2.676)	0.100† (0.464)	−1.551 (7.261)	0.069 (1.276)	0.821 (11.250)	−0.562 (2.618)
Belgium	−2.710 (2.470)	−0.375††† (0.233)	−2.967 (8.376)	−3.605 (4.546)	0.563 (14.528)	5.574 (8.142)
Netherlands	−3.047 (2.512)	−0.473**††† (0.223)	−2.318 (2.598)	−0.527††† (0.547)	−8.434 (9.278)	−1.941 (3.285)
Switzerland	−3.589 (2.845)	−0.445††† (0.364)	−21.148 (18.408)	−4.647 (4.641)	76.875 (80.093)	17.158 (19.039)
R^2	0.021		0.022			
Wald					6.370	8.846

The left panel contains UIP regression results for the base model: $\Delta s_{t+1} = \alpha + \beta(i - i^*)_t + v_{t+1}$. The right panel contains UIP regression results with capital controls: $\Delta s_{t+1} = \alpha + \beta(i - i^*)_t + \gamma_1 CC_t + \gamma_2 CC_t(i - i^*)_t + controls + v_{t+1}$. Where *controls* contain the currency regime and crisis dummies. Standard errors are reported between brackets below each point estimate and the average goodness of fit in the SUR panel is denoted by R^2. The capital controls proxy used is the Miniane index. The bottom row, *Wald*, reports the Wald test of the cross equation restriction that $\gamma_i = 0$ across countries. Asterisks indicate coefficients that are (statistically) significantly different from zero at the 10% level (*), 5% level (**) or 1% level (***). Statistically significant slope deviations from 1 ($\hat{\beta} - 1$) are denoted at the 10% level (†), 5% level (††) or 1% level (†††).

3.2. Deviations from Uncovered Interest Parity

Tables 3 and 4 report UIP regression results for the European and the emerging panels, respectively. Just as for the CIP condition, we compare different regression specifications with or without capital controls to assess the impact of these variables on observed deviations from UIP. Tables 3 and 4 are similarly structured as Tables 1 and 2. The left panels in Tables 3 and 4 correspond with the base regression (4) whereas the right panels in both tables contain regression results for the augmented regression model (6).

The benchmark UIP regression outcomes for European and emerging countries render coefficient estimates $\widehat{\alpha}$ and $\widehat{\beta}$ that are often statistically significantly different from their theorized values (0 and 1) and the economic magnitudes of these deviations are much larger than for the CIP condition, especially for the slope β. This corroborates with the well-known stylized fact of the 'forward discount' bias puzzle. For the European sample the slope coefficient is significantly less than one for almost all countries, and even negative for the majority of countries. The bias is less for the emerging sample: for instance half the coefficients in the emerging sample are positive.[9] The stylized fact that UIP deviations dominate CIP deviations is further confirmed by the poor regression fit R^2 that is found to be much below the CIP fit. This spectacular difference in regression fit can be understood by the so-called 'news dominance' phenomenon, i.e. the variation in exchange rate returns is much larger than the variation in the cross-border interest differential and the forward premium. The latter two variables exhibit comparable volatility, see e.g. De Vries (1994, p. 355) for a discussion of these empirical stylized facts.

The question arises to what extent the large downward bias in β disappears when including capital controls in the UIP regression (as reported in right panels of Tables 3 and 4). Augmenting the benchmark UIP regression with capital controls hardly increases the regression fit; also the intercept and slope estimates do not seem to come closer to their theoretical values of 0 and 1. In fact, the intercept terms – reflecting additive deviations from UIP – deviate more strongly from 0 when UIP regressions are augmented with the capital control proxy. More importantly, the deviations from

[9] A similar result is reported by Frankel and Poonawala (2006).

Table 4
Uncovered interest parity: SUR regressions for emerging countries (January 1997–December 2005, $T = 108$).

Country	UIP base model		UIP regression with controls on In/Outflows					
	α ($\times 10^{-3}$)	β	α ($\times 10^{-3}$)	β	γ_1^{in} ($\times 10^{-3}$)	γ_2^{in}	γ_1^{out} ($\times 10^{-3}$)	γ_2^{out}
Czech	−2.317	0.448	−1.739	−7.332 (5.621)	49.173	12.652	−1.232	7.798
Republic	(4.021)	(0.924)	(7.315)		(42.551)	(20.494)	(26.810)	(5.009)
Malaysia	1.539	3.904	−240.085	10.656	−71.511	37.598	298.131 **	−33.515
	(5.047)	(2.592)	(161.435)	(118.004)	(121.617)	(94.476)	(139.844)	(86.678)
Mexico	2.693	0.058†	49.645	−8.369**†	8.779	−6.692	−60.511	12.958
	(5.109)	(0.536)	(48.389)	(4.883)	(80.020)	(9.279)	(78.746)	(8.488)
Thailand	−4.752	−0.182	−113.142	−123.543***†††	33.784	20.231	93.820	117.734***
	(5.871)	(0.850)	(82.092)	(19.770)	(59.311)	(26.045)	(94.564)	(30.915)
Turkey	1.525	0.793***	50.087	−2.272***†††	181.683	1.498	−82.886	3.911*
	(14.064)	(0.173)	(34.228)	(1.023)	(178.008)	(6.386)	(64.998)	(2.062)
Argentina	−4.530**	1.074***	2.291	0.040 (1.088)	51.722***	−12.481***	−26.336	4.255***
	(2.241)	(0.169)	(9.245)		(17.643)	(4.273)	(30.081)	(1.272)
Brazil	19.135	−1.093 †††	−146.088	9.279 (10.983)	−153.218 ***	8.197***	491.490**	−28.655
	(17.268)	(0.829)	(136.200)		(49.991)	(2.895)	(237.166)	(18.363)
Indonesia	41.615	−1.943††	−39.425	4.333 (20.899)	186.153	−12.007	11.317	−1.352
	(31.165)	(1.392)	(228.316)		(363.307)	(36.330)	(181.815)	(28.731)
Korea	−4.299	2.048	2.462	3.691 (21.370)	−3.635	8.303	15.018	−11.189
	(7.516)	(1.322)	(19.582)		(52.354)	(9.976)	(44.294)	(33.366)
Romania	6.631	−0.132†††	4.038	−1.631 (2.265)	−56.471	6.160	60.403	−2.214
	(7.573)	(0.211)	(37.130)		(338.276)	(14.917)	(175.241)	(7.799)
Russia	−0.335	0.933***	133.966***	6.930 (5.407)	−1.511	−4.949	−134.995***	−1.855
	(2.124)	(0.270)	(50.774)		(27.144)	(4.471)	(48.109)	(3.541)
R^2	0.327		0.404					
Wald					20.102**	18.002*	21.302**	33.497***

The left panel contains UIP regression results for the base model: $\Delta s_{t+1} = \alpha + \beta(i - i^*)_t + v_{t+1}$. The right panel contains UIP regression results with capital controls: $\Delta s_{t+1} = \alpha + \beta(i - i^*)_t + \gamma_1^{in} CC_t + \gamma_2^{in} CC_t(i - i^*)_t + \gamma_1^{out} CC_t + \gamma_2^{out} CC_t(i - i^*)_t + controls + v_{t+1}$. Where *controls* contain the currency regime and crisis dummies. Standard errors are reported between brackets below each point estimate and the average goodness of fit in the SUR panel is denoted by R^2. The capital controls proxy used is the Schindler index. The bottom row, *Wald*, reports the Wald test of the cross equation restriction that $\gamma_i^j = 0\partial$ across countries. Asterisks indicate coefficients that are (statistically) significantly different from zero at the 10% level (*), 5% level (**) or 1% level (***). Statistically significant slope deviations from 1 ($\widehat{\beta} - 1$) are denoted at the 10% level (†), 5% level (††) or 1% level (†††).

unbiasedness ($\widehat{\beta} - 1$) increase in a majority of cases for the augmented regressions, i.e., capital controls seem counterproductive in that they seem to induce a reduction of monetary independence instead of an increase.

Given the results above, it is not surprising that the statistical significance of the capital control variables – as reflected by the γ-coefficients in the right panels of Tables 3 and 4 – is rather weak. The lack of statistical significance is particularly striking for the European sample: γ_1 and γ_2 are jointly insignificant across the European countries; The slope effect, γ_2, is only individually significant for France. The emerging panel regression results exhibit somewhat more statistical significance. The effect of capital controls on the slope of the UIP regression, γ_2^{in} and γ_2^{out}, is jointly significant. However, the significant values are often positive which would imply even stronger slope biases $\beta < 1$ after liberalization; otherwise stated, capital controls seem counterproductive and do not seem to bring more 'monetary freedom'.

On the basis of Tables 1–4, an intermediate conclusion is that capital controls do have some association with deviations from CIP, but the empirical evidence on capital controls as drivers of UIP deviations is rather weak. Slopes do not move closer to 1 when augmenting the UIP equation with capital controls. Furthermore the slope effects that are statistically significant often exhibit the wrong sign. However, this apparent anomaly could be due to yet another omitted variables bias. Dooley and Isard (1980) and Phylaktis (1988, 1990) have argued that the effect of capital controls on interest rate differentials and exchange rate movements may be twofold: (i) a direct 'tax' effect of current capital controls, as studied above; and (ii) an indirect effect of investors' expectations about future controls, also referred to as the 'political risk premium'. In the methodology section we explained that political risk is by definition an unobserved variable. To proxy it we regress offshore-onshore interest

Table 5
UIP and political risk: SUR regressions for European countries (January 1984–December 1998, $T = 180$).

Country	Political risk regression		UIP regression with capital controls and political risk					
	η_0 ($\times 10^{-3}$)	η_1 ($\times 10^{-3}$)	α ($\times 10^{-3}$)	β	γ_1 ($\times 10^{-3}$)	γ_2	δ_1	δ_2
Denmark	−0.026	0.041	−1.114	−0.380	−3.997	−0.024	0.232	−11.790
	(0.039)	(0.134)	(2.896)	(0.709)	(10.471)	(4.204)	(0.199)	(87.411)
France	−0.361	−0.080	−0.009	2.474†	3.949	−8.246**	−3.345***	304.138
	(0.240)	(0.343)	(4.627)	(1.608)	(6.542)	(3.941)	(1.252)	(321.593)
Italy	−0.052	1.184***	−6.696	1.086	9.760	−1.761	0.184	440.281
	(0.113)	(0.225)	(8.845)	(1.748)	(21.249)	(4.023)	(6.271)	(1107.034)
Belgium	4.606***	−9.427***	0.716	1.180	−5.878	−3.188	0.502	−286.646
	(0.411)	(0.738)	(12.605)	(7.037)	(22.245)	(12.668)	(1.018)	(567.717)
Netherlands	−0.059***	−0.138 *	−1.785	−0.262	−15.713	−4.404	0.882	1657.078
	(0.015)	(0.081)	(2.690)	(0.707)	(11.917)	(3.998)	(5.138)	(2188.480)
Switzerland	1.302***	−8.442***	−18.834	−4.021†	68.005	13.381	0.628	−606.726
	(0.347)	(1.534)	(18.614)	(4.865)	(81.049)	(19.748)	(2.587)	(526.989)
R^2	0.167		0.032					
Wald		267.985***			3.021	6.305	10.886*	3.439

The left panel contains the political risk regression results: $(i^{\text{off}} - i)_t = \alpha + \gamma_1 CC_t + controls + \rho_t$. Where $PR_t = \rho_t$ is the (de-meaned) political risk premium and *controls* contain the currency regime and crisis dummies. The right panel contains UIP regression results with capital controls and political risk: $\Delta s_{t+1} = \alpha + \beta(i - i^*)_t + \gamma_1 CC_t + \gamma_2 CC_t(i - i^*)_t + \delta_1 PR_t + \delta_2 PR_t(i - i_t^*) + controls + v_{t+1}$. Standard errors are reported between brackets below each point estimate and the average goodness of fit in the SUR panel is denoted by R^2. The capital controls proxy used is the Miniane index. The bottom row, *Wald*, reports the Wald test of the cross equation restriction across countries, e.g. $\forall \gamma_i = 0$. Asterisks indicate coefficients that are (statistically) significantly different from zero at the 10% level (*), 5% level (**) or 1% level (***). Statistically significant slope deviations from 1 ($\widehat{\beta} - 1$) are denoted at the 10% level (†), 5% level (††) or 1% level (†††).

differentials on capital control proxies and identify the political risk premium as the intercept plus the regression residual of this 'political risk premium' regression, see Equation (7). Next, we augment the UIP regression with capital controls and political risk in order to disentangle the effects they have on the slope coefficient β, see Equation (8). Regression results are summarized in Tables 5 and 6 for European countries and emerging countries, respectively. The left panels in Tables 5 and 6 report results for the political risk premium regression (7) whereas the right panels contain the results for the UIP regression (8) augmented with capital controls and political risk.

Due to the limited availability of offshore interest rates, the number of countries in Tables 5 and 6 is much smaller than in the previous tables. The political risk premium regressions (left panels) reveal that capital controls have a significant impact on the offshore-onshore differential in 4 out of 6 European countries. *Ex ante* it is expected that the 'direct tax effect' of capital controls is larger in the emerging sample, *vis-a-vis* the European sample, because their financial markets are less developed and have more distortions.

Upon augmenting the UIP regression with both capital controls and political risk premiums (see the right panels in Tables 5 and 6), the results confirm our earlier findings that capital controls have, at most, a limited effect on the deviations from UIP. Moreover, this observation seems robust to letting the capital controls proxy interact with a proxy for political risk. The failure of political risk to explain deviations from UIP in emerging markets is surprising because most observers would probably judge recurrent switches in capital control regimes more likely in emerging markets than in developed countries. Otherwise stated, they expect a larger magnitude of political risk in emerging markets. However, the right panel in Table 6 clearly shows that political risk has no significant effect on either the intercept or the slope of the UIP regressions in any of the three emerging countries investigated. This seems to contradict with Phylaktis (1988) who shows that political risk *did* play an important role in explaining uncovered interest differentials in Argentina.

Finally, the question arises why capital controls are so ineffective in distorting exchange rate parities and in bringing monetary freedom. One possible explanation may be the capital control's inherent 'fungibility', see e.g. Valdes-Prieto and Soto (1998) and De Gregorio et al. (2000). Loosely speaking, the fungibility property of capital controls means that clever and imaginative speculators always find ways to (legally) circumvent capital controls. For example, investors can shift capital into sectors or financial

Table 6
UIP and political risk: SUR regressions for emerging countries (January 1997–December 2005, $T = 108$).

Country	Political risk regression			UIP regression with capital controls and political risk							
	$\eta_0\ (\times 10^{-3})$	$\eta_1^{\text{in}}(\times 10^{-3})$	$\eta_1^{\text{out}}(\times 10^{-3})$	$\alpha\ (\times 10^{-3})$	β	$\gamma_1^{\text{in}}(\times 10^{-3})$	γ_2^{in}	$\gamma_1^{out}(\times 10^{-3})$	γ_2^{out}	δ_1	δ_2
Indonesia	16.027*	−9.697	−13.501	57.091	−5.333 (20.863)	87.872	−1.299	−99.100	0.386	5.661 (3.697)	−6.798 (97.369)
	(9.409)	(13.096)	(10.255)	(229.520)		(362.221)	(36.009)	(178.233)	(28.675)		
Malaysia	−1.780 (2.124)	−0.014 (1.628)	1.672 (1.855)	−188.483	41.301 (120.211)	−140.231	34.358	295.752**	−60.940	−5.328 (9.997)	−3664.343
				(164.535)		(127.714)	(95.923)	(142.311)	(89.584)		(3260.280)
Thailand	−13.155***	−10.602***	18.596***	−144.623	−144.642***†††	27.361	13.607	128.746	143.542***	0.459 (2.591)	106.654
	(4.483)	(2.824)	(4.899)	(97.017)	(23.274)	(73.700)	(34.740)	(116.071)	(38.275)		(370.619)
R^2	0.308			0.368							
Wald		14.546***	16.541***			1.400	0.287	6.105	14.519***	2.633	1.350

The left panel contains the political risk regression results: $(i^{\text{off}} - i)_t = \alpha + \gamma_1^{\text{in}} CC_t + \gamma_1^{\text{out}} CC_t + controls + \rho_t$. Where $PR_t = \rho_t$ is the (de-meaned) political risk premium and *controls* contain the currency regime and crisis dummies. The right panel contains UIP regression results with capital controls and political risk: $\Delta s_{t+1} = \alpha + \beta(i - i^*)_t + \gamma_1^{in} CC_t + \gamma_2^{in} CC_t (i - i^*)_t + + \gamma_1^{out} CC_t + \gamma_2^{out} CC_t (i - i^*)_t \delta_1 PR_t + \delta_2 PR_t (i - i_t^*) + controls + v_{t+1}$. Standard errors are reported between brackets below each point estimate and the average goodness of fit in the SUR panel is denoted by R^2. The capital controls proxy used is the Schindler index. The bottom row, *Wald*, reports the Wald test of the cross equation restriction across countries, e.g. $\forall \gamma_i = 0$. Asterisks indicate coefficients that are (statistically) significantly different from zero at the 10% level (*), 5% level (**) or 1% level (***). Statistically significant slope deviations from 1 $(\beta - 1)$ are denoted at the 10% level (†), 5% level (††) or 1% level (†††).

products that are not taxed, such as derivatives. The use of transfer pricing is another example. By artificially changing the prices charged to subsidiaries, capital flows can be disguised as trade flows and thus shifted from the (controlled) capital account to the liberalized current account. Moreover, it can be expected that investors become more adept at circumventing the controls over time, which decreases capital control effectiveness even further, unless the government continually keeps closing loopholes.

4. Conclusion

Policymakers often cite several arguments for implementing controls on the free flow of capital across borders. Insulating interest rate policies from exchange rate policies (so-called 'monetary freedom') by weakening the link between interest rates and exchange rates is one of the those prime motives for installing capital controls. The question arises to what extent controls are effective in enhancing monetary freedom. In a world of perfect capital mobility, perfect asset substitutability and the presence of arbitrageurs and speculators, exchange rate changes and domestic and foreign interest rates are linked together by parity conditions such as the Covered Interest arbitrage relation (CIP) and the Uncovered Interest Parity condition (UIP). Hence, these two parity conditions constitute a natural framework to measure the capacity of capital controls to driving a wedge between interest rates and exchange rates. In fact, there is abundant empirical evidence of deviations from parities, particularly deviations from UIP. We argue that if capital controls would have the potential to bring more monetary freedom, this should contribute to these parity deviations in the foreign exchange market.

On the one hand capital controls seem to have a small but significant impact on observed deviations from covered interest rate parity, both for European countries as well as emerging countries. Capital controls are also found to have a significant impact on offshore-onshore differentials. These findings are in contrast to the conclusions of Montiel and Reinhart (1999), Miniane and Rogers (2007) and Edison and Reinhart (2001) who find that capital controls do not impact monetary freedom. On the other hand, capital controls do not explain much of the observed deviations from Uncovered interest Parity. The European sample hardly provides evidence that capital controls partly cause deviations from UIP. In the emerging sample, capital controls only have something to say on the deviations from UIP provided the effects of inflow and outflows are separated, but the impacts on UIP deviations – if statistically significant – often appear with the wrong sign. Somewhat surprisingly, the inclusion of a political risk proxy as an additional factor in the UIP regressions hardly alters the results: neither capital controls nor political risk seem important determinants of the observed deviations from UIP. The latter outcome somewhat contrasts with earlier studies by Dooley and Isard (1980) and Phylaktis (1988, 1990) who report an important role for political risk in explaining interest differentials. It cannot be excluded that the different methodologies used for determining political risk proxies as compared to the current paper may play some role in this.

Appendix. Capital Control indices

Miniane Index

The Miniane index reflects the percentage of capital controls present over the 13 asset categories below. The presence (or absence) of a capital control per asset category is represented by a dummy variable. The cross-asset dummy average equals the Miniane index which lies between 0 and 1 by construction. For exact details on how capital controls are measured in each asset category see Miniane (2004).

1. Shares or other securities of a participating nature and bonds and other debt securities with a maturity of more than one year
2. Money market instruments
3. Collective investment securities
4. Commercial credits
5. Financial credits
6. Direct Investment
7. Repatriation or liquidation of direct investments

8. Guarantees and sureties
9. Real estate transactions
10. Derivatives
11. Provisions specific to commercial banks
12. Provisions specific to institutional investors
13. Multiple exchange rate arrangements

Schindler Index

The Schindler index reflects the percentage of capital controls present over the 12 asset categories below. The presence (or absence) of a capital control per asset category is represented by a dummy variable. The cross-asset dummy average equals the Schindler index which lies between 0 and 1 by construction. For exact details, see Schindler (2009).

1. Shares or other securities of a participating nature
   (a) Purchase locally by nonresidents
   (b) Sale or issue abroad by residents
   (c) Purchase abroad by residents
   (d) Sale or issue locally be nonresidents
2. Bonds or other debt securities
   (a) Purchase locally by nonresidents
   (b) Sale or issue abroad by residents
   (c) Purchase abroad by residents
   (d) Sale or issue locally be nonresidents
3. Money market instruments
   (a) Purchase locally by nonresidents
   (b) Sale or issue abroad by residents
   (c) Purchase abroad by residents
   (d) Sale or issue locally be nonresidents
4. Collective investments
   (a) By residents to nonresidents
   (b) By nonresidents to residents
5. Financial Credits
   (a) By residents to nonresidents
   (b) By nonresidents to residents
6. Direct Investment
   (a) Outward investment
   (b) Inward investment
   (c) Liquidation of direct investment

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