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DEPARTMENT OF ECONOMICS

Working Paper

Capital controls and the real exchange rate: Do controls promote disequilibria?

by

Juan Antonio Montecino

Working Paper 2015-02



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Capital controls and the real exchange rate: Do controls promote disequilibria?*

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Abstract

The consensus view is that capital controls can effectively lengthen the maturity composition of capital inflows and increase the independence of monetary policy but are not generally effective at reducing net inflows and influencing the real exchange rate. This paper presents empirical evidence that although capital controls may not directly affect the long-run equilibrium level of the real exchange rate, they may enable disequilibria to persist for an extended period of time relative to the absence of controls. Allowing the speed of adjustment to vary according to the intensity of restrictions on capital flows, it is shown that the real exchange rate converges to its long-run level at significantly slower rates in countries with capital controls. This result holds whether permanent or episodic controls are considered. The benchmark estimated half-lives for the speed of adjustment are around 3.5 years for countries with strict capital controls but as low as 2 years in countries with no restrictions on international capital flows. The paper also presents a stylized two-sector dynamic investment model with constraints on externally-funded investment to illustrate potential theoretical channels.

JEL classification: F2, F31, F36, F41

Keywords: Capital Controls, Real Exchange Rates, Undervaluation

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1 Introduction

Once considered heretical to the tenets of prudent macroeconomic policy, in recent years capital controls have regained respectability in official policy circles and received fresh attention among academics as potential macro-prudential tools (e.g. Jeanne and Korinek, 2010). In the wake of the global financial crisis and mounting evidence of the destabilizing effects of unregulated international capital flows, the International Monetary Fund (IMF), previously the champion of capital account liberalization, reversed decades of official policy recommendations and declared that capital controls should once again be included in a country’s “policy toolkit.”¹ At the same time, as expansionary monetary policy in industrial nations has flooded emerging markets with foreign funds, a number of countries have imposed restrictions on capital inflows, specifically citing a concern with excessive exchange rate appreciation and a desire to preserve export sector competitiveness. This shift in opinion regarding the use of capital controls has taken place along with a growing recognition that some rapidly industrializing nations, in particular China, have benefitted from so-called “neo-mercantilist” policies and have used capital controls to deliberately maintain an undervalued real exchange rate.

These calls for the greater use of capital controls to manage the real exchange rate stands at odds with the empirical literature on the effectiveness of controls, which has not found clear evidence that controls can influence this variable.² The consensus from empirical studies is that while capital controls are quite effective at increasing the autonomy of monetary policy and lengthening the maturity composition of capital flows, they are not generally successful at reducing net capital inflows or influencing the real exchange rate. However, with very few exceptions, previous studies examining the impact of capital controls on the real exchange rate have overlooked the latter’s fundamental determinants and long-run equilibrium.

An extensive literature argues that in the long-run the real exchange rate is pinned down by monetary and real fundamentals. According to the theory of purchasing power parity (PPP), international goods arbitrage and the law of one price imply that in the long-run the real value of two countries’ currencies should be equal.³ Alternatively, if PPP should fail to hold, a large body of work has asserted that real exchange rates are driven by real fundamentals such as the relative productivity of the tradable sector (the Balassa-Samuelson effect), net foreign assets, or the terms of trade. However, there exists considerable uncertainty regarding the real exchange rate’s adjustment dynamics towards its long-run equilibrium. As Rogoff (1996) noted when stating the famous “PPP puzzle”, the consensus estimates for the speed of adjustment, with half-lives ranging from between 3 to 5 years, are too slow to be consistent with the theory of PPP or other equilibrium models of real exchange rate determination. In addition to the PPP puzzle, the question is further confounded by recent evidence of substantial cross-country and regional heterogeneity in real exchange rate dynamics.

This paper presents new empirical evidence on the adjustment dynamics of the real exchange rate towards its long-run equilibrium in the presence of capital controls. Using a large panel of developed and developing countries, this paper shows that while capital controls may not affect the equilibrium level of the real exchange rate, controls can substantially slow its speed of adjustment towards this long-run level, causing disequilibria to persist for extended periods of time. Specifically, this paper uses panel dynamic ordinary least-squares (DOLS) to estimate the long-run cointegrating relationship between the real exchange rate and a set of fundamentals. This equilibrium relationship is used to calculate the extent of real under or overvaluations – that is, of disequilibria – which are then imposed on an error-correction model to study the short-run adjustment dynamics towards equilibrium. Although a number of recent studies acknowledge and allow for heterogeneity in both the cointegrating relationship as well as the speed of adjustment, few studies have sought to explicitly explain this heterogeneity. Thus, a novel contribution of the present study is to give content to variations in the error-correction mechanism by explicitly modeling it as a function of restrictions on capital mobility.

The empirical results are consistent with the hypothesis that capital controls slow the speed of adjustment

¹Examples of work by IMF staff articulating this change in opinion are Ostry et al. (2010), Ostry et al. (2011b), and Ostry et al. (2011a). These new perspectives on the role of capital controls became part of the IMF’s “institutional view” late in 2012 (IMF, 2012).

²A detailed survey of this literature is presented below.

³For comprehensive accounts of the PPP debate see Froot and Rogoff (1995) and Taylor (2006).

towards the long-run equilibrium and therefore allow real exchange rate disequilibria to persist for longer periods of time relative to the absence of controls. The point estimates from the baseline model imply half-lives for the adjustment of disequilibria of roughly 3.5 years in countries with stringent restrictions on international financial transactions but as short as 2 years in countries with completely open capital accounts. These results therefore imply considerable differences in real exchange rate adjustment dynamics between countries depending on the intensity of capital controls. Moreover, these findings are not sensitive to whether permanent or temporary capital controls are considered.

This paper is also related to the growing literature on the positive development effects of real exchange rate undervaluation. A number of studies have shown that there exists a robust relationship between an undervalued real exchange rate and faster economic growth (see, for example, Rodrik (2008) and Rapetti et al. (2012)). These positive growth effects have been explained through a variety of channels: sectoral misallocation of capital due to government and market failures (Rodrik, 2008); hidden unemployment in an underdeveloped dual economy (Razmi et al., 2012); or learning by doing externalities in the tradables sector (Korinek and Serven, 2010). What all these models have in common, however, is the importance for long-run growth of the tradable sector and the potential to use undervaluation as a development tool. But how exactly should policymakers wield this new tool? It is poorly understood how a persistent undervaluation can actually be achieved and whether restrictions on capital mobility can play a role.⁴ Another contribution of this paper is therefore to help fill this gap. The empirical results presented below suggest that capital controls are capable of promoting real exchange rate undervaluation for extended periods of time, and are therefore compatible with a broader development strategy based on the promotion of tradable goods.

The remainder of the paper proceeds as follows. Section 2 provides a literature review of the debate on long-run PPP, the equilibrium real exchange rate literature, as well as studies on the macroeconomic effects of capital controls. Section 3 provides a stylized two-sector model of real exchange rate dynamics and capital controls. Section 4 describes the dataset and econometric methodology used in the empirical analysis. Section 5 presents the main results while the final section provides some concluding remarks.

2 Related literature

2.1 The long-run PPP debate

Purchasing power parity (PPP) is the proposition that after accounting for the domestic prices of goods and nominal exchange rates, all national currencies should have the same purchasing power. This proposition is derived from the Law of One Price (LOP), which states that in the absence of frictions such as transaction costs or other barriers to trade, international goods trade should cause all identical goods to trade for the same price across markets after converting into a common currency. Otherwise, it would be possible to profit through arbitrage and thus prices would eventually equalize across countries. Similarly, the price of a basket of comparable goods across countries should also be equal when expressed in the same currency.⁵

In its simplest form, PPP states that the price levels of two countries should be exactly equal after multiplying by the bilateral exchange rate and this parity should hold continuously. If P is the domestic price level, P^* the foreign price level, and s the bilateral exchange rate, PPP holds that:

$$P = sP^* \tag{1}$$

This is sometimes referred to as *absolute* PPP and implies that the real exchange rate between two countries, defined as the nominal exchange rate deflated by the relative price level, should always equal exactly one. That is,

$$RER \equiv \frac{P}{sP^*} = 1 \tag{2}$$

⁴A notable contribution is Jeanne (2012).

⁵The modern formulation of PPP is due to the Swedish economist Gustav Cassel in the early 20th century but elements of the doctrine can be traced as far back to the Salamanca school in 16th century Spain.

should hold continuously. Obviously, as anyone familiar with international time-series data can attest, the real exchange rate is almost never equal to one. This is compatible with the PPP hypothesis if one considers that the baskets of goods used to construct consumer prices indexes (CPI) differ across countries. If these differences in the composition of goods included in each country’s CPI are roughly constant across time, one obtains what is often referred to as *relative* PPP:

$$\frac{P}{sP^*} = c \tag{3}$$

where c is a constant. Taking logs and differentiating with respect to time, this yields the testable hypothesis:

$$\hat{P} - \hat{s} - \hat{P}^* = 0 \tag{4}$$

where a “hat” over a variable denotes its rate of change. Equation (4) predicts that changes in the domestic price level are exactly offset by changes in the nominal exchange rate and the foreign price level. As Froot and Rogoff (1995) note, most early tests based on (4) rejected the PPP hypothesis with the exception of some studies based on hyperinflationary episodes.

Although both the absolute and relative versions of the PPP hypothesis do not find empirical support as continuous propositions, PPP may nevertheless hold *in the long-run*. That is, the real exchange rate may be subject to large variations in the short-run, but in the long-run it could still converge to a constant mean. Referred to as *long-run PPP*, this has often been interpreted as the testable hypothesis that the real exchange rate is stationary or mean-reverting. Thus, the literature has focused on tests of the form:

$$\Delta RER_t = (\rho - 1)RER_{t-1} + e_t \tag{5}$$

where the null hypothesis is that the autoregressive coefficient $\rho = 1$. Under the null hypothesis the real exchange rate contains a unit root and thus follows a random walk. In other words, PPP does not hold in the long-run since shocks to the real exchange rate do not fade out over time.

Early studies during the 1980s on long-run PPP generally failed to reject the null hypothesis that the real exchange rate contains a unit root. For example, the famous study by Meese and Rogoff (1983) was not able to reject the nonstationarity null and found that a random walk model outperformed structural models of real exchange rate determination in out of sample forecasting. However, it was often argued at the time that this failure to reject the null could be due to a lack of power of existing unit root tests. If ρ is sufficiently close – but not actually equal – to one, then in practice it may be very difficult to reject the unit root null even if the real exchange rate is in reality stationary.

This issue of the low power of standard unit root tests has received a great deal of attention. Lothian and Taylor (1997) used simulated data with sample moments similar to available real exchange rate data to examine the power of standard unit root tests. They found that “standard tests for mean reversion... have extremely poor power characteristics” and that “the rejection frequency does not improve significantly with a sample corresponding to 100 years.” Faced with this low power problem, a number of studies emerged using much longer historical series on real exchange rates. For instance, Frankel (1986) used an 116 year dataset on the dollar/pound real exchange rate and was able to reject the unit root null. Another response was to increase the power of tests by pooling observations across countries and testing the unit root null jointly. Two early such studies are Oh (1996) and Wu (1996), both of which rejected the unit root null in a panel of countries.

However, although power issues have been correctly emphasized in the debate, the potential size problems of unit root and cointegration tests have received comparatively less attention. This issue was studied by Engel (2000) using disaggregated price data on tradables and nontradables as well as Monte Carlo methods. Engel decomposes the real exchange rate movements into a tradables component, which according to theory should be stationary, and nontradables component, which is potentially non-stationary. He argues that if the innovational variance of the nontradable component is sufficiently small relative to the tradable component, it will be extremely difficult for unit root tests to distinguish between the non-stationary null and the stationary alternative. He concludes that “the size bias in unit root (and cointegration) tests is large, even when the unit root component accounts for a large proportion of the conditional variance of real exchange rate at the

100-year horizon. . . it appears that unit root tests might routinely reject the null hypothesis even when there is a large permanent component based on the relative price of non-traded goods.”

Studies during the late 1980s and early 1990s continued to offer mixed support for long-run PPP, although a number of influential studies were able to reject the unit root hypothesis. But even if the unit root hypothesis could sometimes be rejected, the estimated speed of mean reversion was far too slow to be consistent with the PPP hypothesis. This led Rogoff (1996), in summarizing the state of the debate as it stood in 1996, to formulate what he referred to as the “PPP puzzle”:

At long last, a number of recent studies have weighed in with fairly persuasive evidence that real exchange rates (nominal exchange rates adjusted for differences in national price levels) tend toward purchasing power parity in the very long run. Consensus estimates suggest, however, that the speed of convergence to PPP is extremely slow; deviations appear to damp out at a rate of roughly 15 percent per year. . . The purchasing power parity puzzle then is this: How can one reconcile the enormous short-term volatility of real exchange rates with the extremely slow rate at which shocks appear to damp out? (Rogoff, 1996)

Since the original formulation of the PPP puzzle, the literature on long-run PPP has failed to provide a clear answer to the question of mean reversion and has largely evolved in parallel to advances in time-series econometrics. For example, tests for cointegration between price levels and nominal exchange rate have been employed extensively (the existence of a cointegrating relationship would imply that the residual – the real exchange rate – is stationary and hence long-run PPP holds). Other notable advances include unit root tests allowing for known and unknown structural breaks (e.g. Papell, 2002) and panel studies allowing for heterogeneity in real exchange rate dynamics along the panel dimension.

Heterogeneity across countries offers a powerful explanation for the literature’s mixed results: it may well be the case that mean reversion accurately describes the behavior of the real exchange rate but only for a subset of countries. This raises the related issue of so-called “survivorship bias,” whereby studies using long historical datasets are skewed towards industrialized countries for which real exchange rate data is available. Although purportedly solving the power problem, historical studies may have inadvertently introduced a sample bias problem if industrialized countries are more likely to exhibit mean reversion than less developed countries. For instance, Froot and Rogoff (1995) failed to reject the unit root null for 75 years of real exchange rate data from Argentina. Cheung and Lai (2000) uncover substantial amounts of heterogeneity in exchange rate dynamics across a sample of 94 countries. In particular, Cheung and Lai find that it is much harder to reject the unit root null in Latin America and Europe. Curiously, their results also suggest that mean reversion is generally *harder* to detect in industrialized countries than in developing ones.

Similarly, Breitung and Candelon (2005) study long-run PPP across Asia and Latin America. They point out that frequent currency crises could be leading to incorrect failures to reject the unit root null and therefore propose testing for unit roots in a panel setting allowing for multiple structural breaks. In line with Cheung and Lai (2000), their results provide some evidence of long-run PPP in Asian countries but not for Latin America.

Another recent study allowing for heterogeneity across countries and structural breaks is Westerlund and Edgerton (2008). They develop new panel cointegration test that allows for multiple endogenously determined structural breaks as well as panel-specific time trends, heteroskedasticity, and serial and cross-sectional correlation. Using quarterly data on bilateral exchange rates and price levels for 17 industrialized countries over 1973-1998, they show that the individual series are nonstationary but *not cointegrated*. In other words, their results do not support the long-run PPP hypothesis.

2.2 The equilibrium real exchange rate and its determinants

If PPP does not hold and the real exchange rate does not converge to a time invariant mean, what can we say about its long-run determinants? An alternative literature has stressed that in the long-run the real exchange rate is determined by fundamentals, including the relative productivity of the tradable and non-tradable sectors, the net foreign assets position, the terms of trade, as well as aggregate demand factors – most

notably fiscal policy.⁶ Although this literature is diverse, the unifying theme is to treat the real exchange rate as nonstationary and use cointegration techniques, emphasizing explicit equilibrium relationships. Indeed, this has led many authors to informally refer to these similar studies as the “equilibrium real exchange rate approach.”

Perhaps the most classic explanation for the failure of PPP is the relative productivity channel, which can be traced to Balassa (1964) and Samuelson (1964). This is the so-called Balassa-Samuelson effect, which in its simplest form predicts that countries with higher productivity in the tradable goods sector will tend to have more appreciated real exchange rates.⁷ Empirical tests of the Balassa-Samuelson effect usually make the simplifying assumptions that non-tradable productivity growth is zero. This makes it possible to test the hypothesis using data on the real exchange rate as the dependent variable and productivity in the tradable sector, typically proxied as real per capita income, as the independent variable. A positive coefficient is then taken as evidence of the Balassa-Samuelson effect.

The Balassa-Samuelson effect has proven remarkably robust since its first test by Balassa (1964). Two examples of recent empirical confirmation of the Balassa-Samuelson effect are Lothian and Taylor (2008) and Chong et al. (2012). Employing a new semi-parametric approach, Chong et al. estimate the cointegrating relationship between the real exchange rate and productivity in a panel of 21 OECD countries at a quarterly frequency. Their novel local projection approach makes it possible to purge the effects of short-run shocks and frictions and yields strong confirmation of the Balassa-Samuelson effect. Notably, the estimated half-life for the speed of adjustment is significantly shorter than in the standard literature: around 1.5 to 2 years compared to between 3 and 5 years in previous studies. Lothian and Taylor, use nearly two hundred years of data for the US, UK, and France to test the presence of the Balassa-Samuelson effect in an explicitly nonlinear framework that allows volatility shifts in the nominal exchange rate across monetary regimes. Their results suggest that the Balassa-Samuelson effect explains nearly 40 percent of variations in the sterling-dollar real exchange rate over the whole sample.⁸

Additional recent confirmation of the Balassa-Samuelson effect is provided by Bordo et al. (2014), who use historical data for 14 countries covering four distinct monetary regimes: the classical gold standard, the war and interwar years, Bretton Woods, and the post-Bretton Woods managed floats. They show that the traditional Balassa-Samuelson model cannot explain the small empirical effect of productivity on the real exchange rate or the substantial heterogeneity in its magnitude across monetary regimes. Modern versions of the model, including those that allow a role for product differentiation and terms of trade channels, fit the data much better. In particular, plausible shifts in structural parameters due to changes in monetary regimes can explain the historical variations in the Balassa-Samuelson effect and help reconcile discrepancies in estimates across countries. Bordo et al. conclude: “although the Balassa-Samuelson effect tends to vary across regimes, the evidence suggests that it is present, and in the long-run the real exchange rate is not constant but conditioned on relative income levels.”

Another standard long-run determinant of the real exchange rate is the net foreign asset position. Interest in the impact of net foreign asset holdings on international relative prices dates back at least to the time of Keynes during the 1920’s debate on the so-called *transfer problem*. Also, contemporary textbook models of an open economy predict a positive relationship between stocks of foreign assets and the relative price of non-tradable goods (e.g. Vegh, 2013, ch. 4). Since foreign assets represent a claim on tradable goods, an exogenous increase in foreign assets raises the supply tradables and should lead to an increase in the relative price of non-tradables. Early empirical evidence of a positive association between net foreign asset stocks and the real exchange rate is provided by Gagnon (1996) and Lane and Milesi-Ferretti (2004). More recent studies that find a positive and significant effect include Ricci et al. (2013) and IMF (2013).

⁶Of course, this small list does not exhaust the full of range of explanations for why PPP may not hold in the long-run. A full review is beyond the scope of this paper.

⁷Some authors prefer to refer to this as the “Harrod-Balassa-Samuelson” effect due to early insights from Harrod (1933).

⁸Naturally, the Balassa-Samuelson hypothesis is not without its skeptics and a number of recent studies have offered qualifications or questioned its validity. On the theoretical front, if home and foreign goods are imperfect substitutes, changes in productivity will have an effect on the term of trade, which will run counter to the traditional Balassa-Samuelson effect and hence diminish (or even reverse) its empirical magnitude. See Choudhri and Schembri (2010) for a formal model of this mechanism.

Changes in the terms of trade can also affect the real exchange rate and may help explain the long-run failure of PPP. In his 1930 *A Treatise on Money*, Keynes noted that a major problem with the theory of purchasing power parity is its neglect of the influence of the terms of trade on the real exchange rate, which “not only upsets the validity of [its] conclusions over the long period, but renders them even more deceptive over the short period...”⁹ It is well understood in standard open economy macroeconomic models that improvements in the terms of trade can lead to a real appreciation of the exchange rate.

A large volume of empirical studies support the proposition that the terms of trade have significant effects on the real exchange rate (for example: Edwards (1986), De Gregorio and Wolf (1994), and Ricci et al. (2013)). Nevertheless, empirical estimates vary considerably, from very large effects in so-called “commodity currency” countries, to economically negligible in others.¹⁰ This diversity of empirical results could be due to substantial cross-country heterogeneity. In other words, it may well be the case that the terms of trade are an important determinant of the real exchange rate but only in a subset of countries. Case studies of countries where, for instance, commodities are important will tend to support the hypothesis. Moreover, pooling multiple countries and imposing a homogenous relationship between the terms of trade and the real exchange rate will yield misleading results.

One simple way to address the problem of heterogeneity is to estimate the cointegrating relationship for each country separately. Of course, this is only feasible if the sample size is sufficiently large within each panel. Cashin et al. (2004) examine the long-run relationship between the terms of trade and the real exchange rate with monthly data on the prices of 44 commodities and 58 countries over the period 1980-2002. Testing for cointegration allowing for unknown structural shifts in the intercept, Cashin et al. find significant cointegrating relationships between the real exchange rate and the terms of trade but only in around one third of the countries in the sample. This suggests that the long-run equilibrium exchange rate is only driven by the terms of trade in so-called “commodity currency” countries. However, for these commodity currencies, movements in the terms of trade explain a remarkably large amount of the variation in the real exchange rate. Their estimates imply that nearly 85% of real exchange rate variations are due to the terms of trade.

Other potentially important determinants of the real exchange rate include government expenditure and demographic factors, most notably population growth. Government expenditure is expected to influence the real exchange rate through its effect on aggregate demand and the price level. It may also produce a real appreciation since public spending tends to be more concentrated on non-tradable goods and services (see, for example, De Gregorio and Wolf, 1994; Arellano and Larrain, 1996; Chinn, 1997). Although demographic factors have not received much attention in the equilibrium real exchange rate literature, higher fertility may appreciate the real exchange rate by raising consumption associated with child-rearing, which mainly consists of non-tradables. Rose et al. (2009) present a formal model and empirical evidence of this channel.

Taking stock, the empirical evidence on long-run PPP is at best mixed and there is sufficient contrary evidence to suggest that the real exchange rate may in fact have a unit root. Moreover, theoretical models provide ample explanations for why the real exchange rate may exhibit a high degree of persistence. On the other hand, a large number of studies have shown the existence of long-run relationships between the real exchange rate, productivity, net foreign assets, the terms of trade, as well as a range of other factors. One upshot of this equilibrium real exchange rate literature is that it provides a partial explanation of the PPP puzzle: if in the long-run the real exchange rate converges to a time-varying level instead of a constant mean, as the PPP hypothesis implies, the estimated decay half-life for the deviations from equilibrium are considerably shorter. Therefore, estimating the extent of real exchange rate disequilibria needs to take into account that the equilibrium level is likely time-varying and driven by fundamentals in the long-run.

2.3 What are capital controls and how are they measured?

In the broadest sense, capital controls refer to any administrative or market-based restriction on cross-border financial flows. These can range from outright prohibitions on the ownership of domestic assets by foreigners,

⁹Originally cited by Cashin et al. (2004).

¹⁰Moreover, there is no clear consensus on whether or not the terms of trade should be treated as stationary or nonstationary and if cointegration techniques should be used to assess their effects.

to simple taxes on foreign exchange transactions or international borrowing. Although some ambiguity exists in the usage of the term across different studies, capital controls can either be permanent, reflecting long-term structural aspects of an economy that, for instance, has not liberalized international capital flows; or temporary and imposed to cope with short-term, often cyclical, factors such as a surge in capital inflows. The ambiguity arises from the fact that prior to the current era of financial globalization, most countries in the world would be considered as having “capital controls.” Therefore, from a semantic perspective, it is reasonable to limit the usage of the term to refer to intermediate cases where countries have some degree of capital mobility but restrict capital flows in certain ways to achieve domestic policy objectives. Capital controls may also be imposed either on a small subset of specific assets categories, or across the board, restricting or otherwise regulating international transactions in all types of financial instruments. A further distinction can be made between controls on capital inflows – that is, when foreigners acquire domestic assets – and controls on capital outflows – when domestic residents increase their holdings of foreign assets.

In addition to these distinctions, capital controls can also cover a wider and more subtle range of regulations governing capital inflows. For example, domestic monetary authorities may require firms to deposit a fraction of funds borrowed abroad in non-interest bearing accounts for a specified period of time. These “unremunerated reserve requirements” or URR, as they have come to be known, have been used most famously in Chile during the 1990s and in Colombia during the 2000s. Countries may also enforce so-called “minimum stay” requirements on foreign direct investment, barring the entry of short-term and potentially speculative investments.

Measures of financial openness fall into two broad categories: so-called de jure and de facto indexes. De jure indexes attempt to measure legal or regulatory barriers to international financial transactions. De facto measures, on the other hand, capture the actually existing level of financial integration in a given country independently of its laws and regulations. The vast majority of de jure-type indexes are based on information contained in the IMF’s Annual Report on Exchange Arrangements and Restrictions (AREAR), a yearly publication documenting changes in IMF member country laws and regulations governing international financial transactions. The AREAR provides detailed textual descriptions of changes in laws and regulations each year as well as standardized tables that distinguish between restrictions on a large number of disaggregated transaction categories. As pointed out by the thorough account of Quinn et al. (2011), de jure measures that use the AREAR can be further sub-divided into those based on the simple binary information contained in the standardized tables, and measures based on textual analysis of each country’s annual update. The latter measures, though more difficult to construct, are richer in information and tend to exhibit more variation across time and across countries.

De facto measures of capital mobility take observable macroeconomic variables as their starting point and often combine them with predictions from theoretical models to infer the extent of actual mobility. One such simple de facto measure, which has a close parallel in standard measures of trade openness, is the ratio gross capital flows (inflows plus outflows) to GDP. This has the advantage of being extremely easy to construct and is based on relatively readily available balance of payments data. The de facto extent of capital mobility or financial integration can also be inferred from a country’s correlation between saving and investment. This method dates back to Feldstein and Horioka (1980) and is based on the idea that savings and investment should be highly correlated in non-financially integrated countries while countries with a high degree of mobility should have lower correlations.

A major problem with de facto measure of capital controls, however, is that they in general are potentially as much an outcome variable as they are an indicator of restrictions on capital flows. Using the gross flows to GDP ratio, for instance, it is not obvious if an increase in the measured mobility is due to internal or external factors. In other words, an increase in the mobility index could be due entirely to an external surge in capital flows that is completely independent of changes in domestic policies. Therefore, de facto indexes are poorly suited to empirical studies where the aim is to ascertain the effect of a policy change since they do not actually measure changes in the *intention* to restrict flows.

For this reason, I prefer de jure measures of capital controls in the empirical analysis described below. In particular, I will primarily use the de jure index constructed by Schindler (2009), the so-called “Schindler index”, which is based on detailed textual analysis of information contained in the IMF’s AREAR. As noted

by Quinn et al. (2011) in a thorough assessment of the most common measures of capital controls, the Schindler index is by far the most granular, covering a large range of disaggregated financial instruments and distinguishing between controls on inflows and outflows. The Schindler index has recently been updated to cover a larger number of countries and years by Fernández et al. (2014).

2.4 Rationale and policy objectives of capital controls

The rationale for imposing capital controls can be understood through the lens of the open economy policy “trilemma,” which states that it is impossible for a small open economy to simultaneously have perfect capital mobility, an independent monetary policy, as well as a managed exchange rate. For example, countries that wish to maintain a competitive exchange rate must either give up their monetary policy autonomy or restrict movement on capital flows. Similarly, consider a country that faces a surge in capital inflows and fears that these may lead to the formation of destabilizing asset bubbles or may lead to an undesirable exchange rate appreciation. The monetary authority could attempt to discourage the inflows by lowering the domestic interest rate. By taking this route, however, it forfeits using the policy rate to influence price stability and may inadvertently stimulate a domestic investment boom.

If this hypothetical country were to instead impose temporary controls on capital inflows – in the form of, say, a tax on short-term flows – it could in principle decrease net capital flows, prevent the excessive appreciation, and limit its exposure to speculative short-term flows while taking advantage of international capital markets. Capital controls, in this context, provide an intermediate solution to the policy trilemma and can increase the autonomy of domestic macroeconomic policy.

In recent years a growing literature has stressed the desirability of short-term or “episodic” controls from a macro-prudential perspective. Within this literature, perhaps the most influential approach is the so-called “externality view” of capital flows associated with Korinek (2011) and Jeanne and Korinek (2010). According to this view, capital inflows impose negative externalities through their effect on the real exchange rate and the value of domestic collateral. These models typically feature a foreign borrowing constraint which is a function of the economy’s aggregate balance sheet. Decentralized agents take the real exchange rate as given when choosing how much to borrow abroad and thus fail to internalize their contribution to systemic risk. In other words, the decentralized equilibrium can have too much foreign debt and is Pareto inefficient. Capital controls, in this light, can be seen as a Pigouvian tax that induces decentralized agents to internalize the externality and achieves a welfare improvement.

Two innovative papers, Fernández et al. (2014) and Gallego et al. (2002) take a different approach and ask if the observed imposition of controls is consistent with the stated objectives of policymakers. Gallego et al. examine Chile’s capital controls and test if the effective tax on capital inflows was driven by (i) the difference between inflation and the Central Bank’s target, (ii) the volume of net capital inflows, (iii) the share of long-term inflows, and (iv) the rate of depreciation of the nominal exchange rate. All four of these variables were found to have statistically significant effects on the effective tax on capital inflows – for instance, Chile’s Central Bank appears to have tightened restrictions whenever inflation exceeded its target and loosened restrictions when the maturity composition of inflows became more favorable. Fernández et al. ask the related question: are capital controls actually prudential in practice? To answer this, they look at how capital controls move over the business cycle and if there is any evidence that controls behave counter cyclically. Surprisingly, especially considering the recent call for prudential controls, they conclude: “The central result of our analysis is that capital controls are virtually flat during macroeconomic booms or busts. This is the case regardless of whether the indicator used to identify booms and busts is output, the current account, or the real exchange rate.” In other words, policymakers as a general rule do not appear to tighten controls during booms nor loosen them during busts. This raises important questions about what conclusions can be drawn from cross-country studies: if controls are not imposed for the reasons typically assumed by empirical studies, can they really be said to be effective in a meaningful sense?

But how exactly can capital controls influence the *real* exchange rate? Jeanne (2012) presents an infinite horizon intertemporal model of a small open endowment economy with both tradable and non tradable goods. With a completely open capital account the model predicts that a relative increase in the endowment of tradable goods will result in a real appreciation, defined as a fall in the relative price of tradable goods.

Jeanne uses this setup to explore the implications of “Chinese style” capital controls: a closed capital account where only the public sector can trade in foreign assets. This effectively allows the public sector to set the desired path of reserves and hence the net foreign asset position. As a result, consumption of tradable goods must adjust to satisfy the level of reserve accumulation set by policy and therefore the government can target any real exchange rate level it chooses. However, the model also predicts that once controls are removed the real exchange rate will “over appreciate” as the foreign reserves accumulated while the government was leaning against the wind are now available for private consumption. Intuitively, capital controls and reserve accumulation amount to “forced saving” relative to the optimal consumption path chosen by the forward looking agent.¹¹

Another straightforward way to think about the influence of capital controls is to think of them as introducing a wedge between the domestic and foreign interest rate. Suppose that capital controls are binding and the effective interest rate firms face when choosing how much capital to employ is $r^e = r + \tau$, where the subscript e denotes “effective” and τ is the effective tax on borrowing produced by the imposition of controls. In the simple Balassa-Samuelson model described above, an increase in the effective interest rate will lower the capital labor ratio in the tradable sector and push down wages. If the tradable sector is relatively more capital intensive than the non-tradable sector, this will lead to a fall in the relative price of non-tradable goods; that is, a real depreciation. Therefore, by introducing a wedge between the marginal product of capital and the foreign interest rate, capital controls can increase the effective interest rate firms face when maximizing profits and hence affect the equilibrium real exchange rate.

However, it may not be realistic to assume that controls remain equally binding or effective indefinitely. “Sophisticated” financial markets may, over time, find regulatory loopholes or other ways to evade the controls. A simple way to model this is to assume that the effective tax on borrowing is now time-varying and fades out after controls are imposed. Firms in the tradable sector will now choose the capital-labor ratio by equating the marginal product of capital to a time-varying effective interest rate. If $r^e \rightarrow r$ as $t \rightarrow \infty$, the capital labor ratio will converge to the equilibrium level that would have prevailed in the absence of controls. Along the adjustment path as the capital labor ratio converges to its equilibrium level, the real exchange rate will also deviate and under or overvaluations will be gradually eliminated. In other words, through this mechanism, controls that tax capital inflows can lead to deviations from the equilibrium real exchange rate and these deviations will in general be more persistent the longer it takes financial market participants to learn how to evade the controls.

Capital controls could also impose adjustment costs on investment, especially in industries heavily reliant on external financing. As is well known from dynamic investment models, if adjustment costs are convex the capital stock will not adjust instantaneously to equate the marginal product of capital to the exogenous interest rate and the greater the adjustment costs, the slower the speed of convergence to the steady state equilibrium. Capital controls may increase such adjustment costs – by imposing regulatory barriers to foreign investment or slowing the pace at which firms can use foreign funds (e.g. as does a URR) – or place binding constraints on the level of investment. Therefore, in general, we should expect capital controls to lead to slower convergence towards the steady state capital stock and observe real exchange rate disequilibria along the adjustment path. This simple channel is formalized below in Section 3.

2.5 Effects and effectiveness of controls

The general consensus from the large empirical literature on the macroeconomic effects of capital controls is that controls are most effective at altering the maturity composition of flows and preserving monetary policy autonomy. The evidence is much more ambiguous on the issues of stemming the volume of flows and influencing the real exchange rate, although studies have generally found more support for the former. This characterization is supported Magud and Reinhart (2006), who carry out a meta analysis of 30 empirical studies taking into account the methodological and econometric rigor of each study.

¹¹Jeanne points out that excess appreciation could be avoided if the extra reserves are destroyed once the capital account is liberalized. It is worth noting that capital controls in this model reduce welfare. This is because there are no externalities and hence the decentralized equilibrium is efficient. Deviations from the optimal path chosen by consumers are therefore, tautologically, welfare reducing.

Chile’s experience with capital controls during the 1990s has by far received the most attention and is typically considered a model example of successfully implemented controls. It is therefore instructive to briefly consider Chile’s macroeconomic context as well as its experience with capital controls. During the early 1990s Chile experienced a surge in capital inflows caused by a confluence of push and pull factors.¹² The surge in capital inflows threatened to derail its macroeconomic framework and development strategy, which was primarily export oriented. As such, authorities determined it was desirable to impose restrictions on capital inflows in order to maintain the competitiveness of the exchange rate and keep inflation in check. Domestic authorities were also concerned with the destabilizing properties of what were perceived as temporary and volatile capital flows. Chile introduced an “unremunerated reserve requirement” (URR) for foreign borrowing.¹³ As the name suggests, this required firms borrowing abroad to deposit a portion of these funds in a non-interest bearing account at the Central Bank for a pre-specified period of time. This effectively acted as a tax on capital inflows. However, unlike an ordinary tax, the URR has the additional properties that its effective cost is higher for flows with shorter maturities and for longer deposit periods.¹⁴ Therefore, the URR can potentially not only discourage capital inflows, but also discriminate in favor of inflows of longer maturities.

An influential early study on the effects of Chile’s URR is De Gregorio et al. (2000). De Gregorio et al. use monthly data and a variety of econometric techniques, including a VAR, to test if the URR led to significant increases in monetary policy independence, altered the composition of flows, and depreciated the real exchange rate. The benchmark model consists of an index of the power of the URR, the domestic real interest rate, both short-term and long-term capital flows, expected nominal depreciation, as well as depreciation of the real exchange rate. Including short and long-term capital flows as separate variables makes it possible to examine the impacts of the URR on the maturity composition of flows. The results suggest that Chile’s controls led to a lengthening of the average maturity of flows, a temporary increase in the domestic interest rate, and a very small but statistically significant real depreciation.

More evidence on the effectiveness of Chile’s controls is provided by Gallego et al. (2002), which is methodologically similar to the empirical evidence presented in the present paper below. Gallego et al. construct a *de jure* measure of the intensity of Chile’s URR and use cointegration methods to estimate the equilibrium real exchange rate. The cointegrating relationship is then imposed in an error-correction model that includes short-run determinants of real exchange rate movements, including real differentials between domestic and foreign interest rates. The results suggest that while the URR does not have an effect on the equilibrium level of the real exchange rate, it may have temporarily led to a depreciation.

It is worth keeping in mind that a vast majority of the empirical evidence is based on studies of capital controls imposed during the 1990s. Moreover, these have been disproportionately dominated by studies of Chile’s URR. An exception is Baba and Kokenyne (2011), which looks at the effects of capital controls on emerging markets during the 2000s. Baba and Kokenyne focus on three capital control episodes – the foreign exchange tax in Brazil (2008), and the URRs in Colombia (2007-08) and Thailand (2006-08) – and one episode of capital outflow liberalization – South Korea (2005-08). Rather than pooling the four episodes and conducting a cross-country study, the authors argue that it is preferable to evaluate the controls in each country separately and thus avoid potential comparability problems. They build *de jure* indices of the intensity of the controls in each country and, as has become standard in the literature, they look at

¹²External push factors included loose monetary policy in industrialized countries, which led to large interest rate differentials between Chile and international financial centers. As far as domestic pull factors are concerned, Chile had a favorable international reputation for free-markets and prudent macroeconomic policy. Moreover, the return of democracy after General Augusto Pinochet’s defeat in the 1989 plebiscite was associated with a fall in the country’s risk premium, increasing the availability of external financing.

¹³See Edwards (1999) for more details on Chile’s URR.

¹⁴As shown by Valdés-Prieto and Soto (1998), the effective cost of the URR can be expressed as

$$\tau(k) = \left(\frac{r^* \lambda}{1 - \lambda} \right) \left(\frac{\rho}{k} \right)$$

where k is the number of months the foreign funds stay in the country (the maturity), r^* is the international interest rate, λ is the share of the funds that have to be deposited at the Central Bank, and ρ is the number of months the reserves must remain deposited.

the effectiveness of capital controls on (i) lowering net inflows, (ii) altering the maturity composition, (iii) increasing monetary autonomy, and (iv) reducing exchange rate pressure using both system GMM and VAR estimators. Their results are consistent with the literature: controls during the 2000s appear to have successfully altered the maturity composition and lowered the overall volume of flows in Colombia and Thailand. Controls also appear to have successfully preserved monetary policy independence in Brazil and Colombia, albeit temporarily. Of most relevance to the present study, their results provide no evidence that controls in any country were able to successfully influence the real exchange rate.

Brazil's recent experience with capital controls has also been studied by Alfaro et al. (2014), who use firm-level data to investigate the impact of controls on stock returns and investment. Alfaro et al. carry out an event study around major capital control announcement dates in Brazil between 2008-09. Consistent with increasing costs of capital, they find large and statistically significant falls in stock returns around announcement dates. Alfaro et al. also show that investment fell after the imposition of controls and that firms heavily dependent on external funding were more affected.

Another notable cross-country study is Klein (2012), which emphasizes the difference between countries with permanent controls – “walls” – and temporary controls – “gates”. This distinction is potentially important for two reasons: first, episodic controls, imposed temporarily to deal with cyclical fluctuations in capital inflows or other transitory external factors, are closer in spirit to the contemporary policy discussion pertaining to the macroprudential benefits of controls. Second, as Klein writes, “one problem with gates, however, is that they might not shut tightly. Episodic controls are likely to be less efficacious than long-standing controls because evasion is easier in a country that already has experience in international capital markets than in one that does not.” Using data for 44 developed and developing countries over 1995-2010, Klein shows that there is some correlation between permanent controls and lower growth rates of financial variables associated with instability, slower real appreciations, and faster real GDP growth. These associations, however, are absent for episodic controls. Moreover, Klein points out that most countries with permanent controls tend to be poorer on average and hence the results could suffer from omitted variable bias. When differences in levels of development are controlled for, neither permanent nor episodic controls appear to have significant effects on financial vulnerabilities, GDP growth, or real exchange rate appreciation.

This symmetry between the failure to find robust significant effects on the level of flows and the real exchange rate is suggestive because in most textbook models both variables are endogenous and simultaneously determined. In other words, it is difficult to conceive of potential channels through which capital controls would affect the level of the real exchange rate without affecting inflows. Another way to explain this failure is that it is naive to simply include the level or intensity of capital controls as an explanatory variable for the level or rate of change of the real exchange rate and expect to find a significant coefficient. A perhaps better approach it is to consider capital controls as *structural* changes in the economic environment that may affect the relationship between underlying fundamentals or give domestic authorities greater policy space to influence macroeconomic variables. In other words, there is no *a priori* reason to expect a change in the regulatory environment to itself directly affect the real exchange rate except through its influence on the the behavior of market participants or, for instance, the effectiveness of policy interventions such as foreign reserve accumulation.

Viewed through this lens, it is perhaps not surprising that the IMF's new external balance assessment (EBA) methodology (IMF, 2013), did not find statistically significant level effects of capital controls on the real exchange rate but that these instead appeared to increase the influence of reserve accumulation. In the baseline EBA specification, reserve accumulation is interacted with a measure of capital account openness, and changes in reserves do not appear to have a significant effect on the real exchange rate unless the extent of capital mobility is taken into account. Specifically, the results imply that reserve accumulation has a greater effect on the real exchange rate in countries with more closed capital accounts.

It is also worth considering that although controls may appear restrictive on paper they may be less so in practice. This complicates the interpretation of empirical evidence on the effectiveness of controls and is therefore worth considering briefly. It is often argued that capital controls may not be effective because they can be easily evaded or are simply not binding due to a lack of adequate enforcement by domestic authorities. This may reflect a lack of administrative capacity by local authorities, especially in a developing

country. It could also result from the difficulty of regulating international capital flows in the presence of sophisticated financial actors. Edwards (1999) points out that capital controls could be simply evaded by misinvoicing exports and imports or by mislabeling the type of capital flow. And as noted by Garber (1998), derivatives markets can be used to bypass controls that discriminate between types of flows.¹⁵

The potential for and implications of evasion, however, are likely overstated. Even if determined financial market participants are able to successfully evade capital controls, they must incur costs to do so and therefore a wedge between domestic and international markets is created. Empirical evidence that controls are much harder to evade or are much more binding than is sometimes asserted is presented by Levy-Yeyati et al. (2008). Using data on internationally listed stocks, Levy-Yeyati et al. examine the differences in the prices of domestically listed stocks and depository receipts (DR) traded in major financial sectors. According to the law of one price, absent frictions or restrictions on international financial markets, arbitrage should cause the prices of domestically listed stocks and DRs for the same firm to equalize. The authors show that a tightening of capital controls leads to an increase in the “cross-market premium” on securities traded in more than one market. This suggests that capital controls prevent perfect arbitrage from taking place and therefore are not easily evaded.

To summarize the preceding discussion, the apparent consensus in the literature on capital controls is that controls are quite effective at altering the composition of capital inflows and increasing the independence of monetary policy, even when the potential for evasion is considered. Recent theoretical work on capital inflows and systemic risk provide clear rationale for imposing temporary capital controls as part of a macro prudential framework and suggest that controls can improve welfare. The empirical evidence on the effectiveness of controls on the volume of flows and the real exchange rate is much more mixed. If any consensus is discernible at all, it is that controls are not effective at influencing either of these variables. However, it remains unclear if this is due simply to misspecification or if the effects do not actually exist. Below I will provide empirical evidence that capital controls do appear to impact the behavior of the real exchange rate, albeit not in a way previously considered by empirical studies. Specifically, the evidence below suggests that capital controls alter the disequilibrium dynamics of the real exchange rate, slowing its adjustment speed and allowing over or undervaluations to persist for longer periods of time.

3 A toy model of capital controls and RER dynamics

In this section I will briefly present a toy model formalizing the mechanisms through which capital controls may impact the real exchange rate and slow its speed of adjustment towards its long-run equilibrium. The discussion is largely informal and proofs of the existence and uniqueness of equilibrium are omitted. The key insight from this model is that capital controls may constrain investment in firms reliant on external credit and this may prevent the capital stock from adjusting to its steady state level as fast as it would have in the absence of controls. Since the capital stock affects factor allocations across sectors, along the adjustment path the real exchange rate will also deviate from its long-run steady state level. This simple mechanism is by no means exhaustive but is presented with the hope of aiding intuition.

The model is set in continuous time and consists of a small open economy with two sectors: tradables and non-tradables. Purchasing power parity is assumed to hold for tradable goods but not for non-tradables. The price of tradables goods is therefore exogenous and normalized to unity. The relative price of non-tradables in terms of tradables – the real exchange rate – will be denoted as q , where an increase denotes a real appreciation.¹⁶ For simplicity, I assume the existence of a representative household that maximizes utility over an infinite horizon and supplies labor inelastically. The household’s intertemporal optimization

¹⁵It is not clear if the potential for evasion is a valid argument against the use of capital controls per se. Like any regulation or policy, capital controls need to be properly implemented before the desirability of its effects can be assessed. If anything, the potential for evasion merely suggests that regulators must take care to close legal loopholes and actively monitor international transactions while controls are in place.

¹⁶For notational ease I have omitted time indexes.

problem is to choose a path of consumption bundles and foreign assets $\{c^T, c^N, b\}_0^\infty$ in order to maximize

$$U = \int_{t=0}^{\infty} e^{-\rho t} [a \ln c^T + (1-a) \ln c^N] dt \quad (6)$$

subject to the household's flow constraint and transversality condition

$$\lim_{\tau \rightarrow \infty} e^{-r[\tau-t]} b = 0$$

The flow constraint governs the accumulation of foreign assets b , denominated in terms of tradables, and is given by:

$$\dot{b} = rb + w - c^T - qc^N \quad (7)$$

where r is the exogenously given world interest rate and w is the wage rate. The optimization problem's first order conditions yield the familiar Euler equation:

$$\hat{c}^T = r - \rho \quad (8)$$

where a "hat" over a variable denotes a growth rate. For the remainder I will assume that $r = \rho$, which leads the household to prefer a constant path of consumption of tradable goods, denoted by \bar{c}^T .

Tradable goods are produced using both capital and labor with the constant returns to scale technology $A^T F(k, l^T)$, where k is the stock of capital, l^T is labor in the tradable sector, and A^T is a sector-specific productivity shifter. The production function $F(\cdot)$ is strictly concave. Non-tradable output is produced using only labor with the constant returns to scale technology $A^N l^N$. Labor is assumed to be perfectly mobile within the economy but not across borders, ensuring that the wage is equal between sectors. In addition, the labor market is assumed to clear. To solve the model it will first prove convenient to find the economy's static equilibrium ignoring the dynamic investment decisions of firms in the tradable sector. Profit maximization in the tradable sector yields the following demand for labor:

$$l^T = F_l^{-1}(w, A^T, k) \quad (9)$$

where F_l^{-1} is the inverse of the partial derivative of F with respect to labor. $l^T(w, A^T, k)$ is decreasing in the wage and increasing in both k and A^T . The first order condition for non-tradable firms equates the marginal product of labor to the wage rate. The wage is thus pinned down by the real exchange rate and productivity in the non-tradable sector:

$$w = qA^N \quad (10)$$

Combining equations (9) and (10) and making use of the full employment condition $\bar{L} = l^T + l^N$, the supply function for non-tradable sector firms is given by:

$$y^N = A^N (\bar{L} - l^T(q, A^N, A^T, k)) \quad (11)$$

The supply of non-tradable goods, $y^N(q, A^N, A^T, k)$, is thus increasing in the real exchange rate (since $l_q^T < 0$) and decreasing in the capital stock (since $l_k^T > 0$).

To find the equilibrium consumption of tradable goods it is necessary to impose the equilibrium condition for the non-tradable sector: $y^N = c^N$. The economy's aggregate flow constraint is given by:

$$\dot{b} = rb + y^T - c^T - C(I, k) \quad (12)$$

where $C(I, k)$ denotes tradable-sector costs associated with investment, which will be specified below. Integrating forward, applying the transversality condition, and recalling that \bar{c}^T is a constant, the equilibrium consumption of tradable goods is given by:

$$\bar{c}^T = rb_0 + r \int_{\tau=t}^{\infty} e^{r[\tau-t]} [y^T(q, k, A^T, A^N) - C(I, k)] d\tau \quad (13)$$

The equilibrium real exchange rate can now be obtained by using the first order conditions from the household's optimization problem. Specifically, it is given by the marginal rate of substitution between tradable and non-tradable goods. The real exchange rate is therefore defined implicitly at each instant by:

$$q = \left(\frac{1-a}{a} \right) \left[\frac{rb_0 + r \int_{\tau=t}^{\infty} e^{r[\tau-t]} [y^T(q, k, A^T, A^N) - C(I, k)] d\tau}{y^N(q, k, A^N, A^T)} \right] \quad (14)$$

Provided q^* exists and is unique, we can use the implicit function theorem to find the effect of a change in k on q . Assuming the change in k is anticipated, the equilibrium consumption of tradables will not be affected and the change in the real exchange rate will come about purely through changes in the supply of non-tradable goods. Formally, the effect on q for a change in k at time t is given by:

$$\frac{dq}{dk} = - \frac{q \frac{\partial y^N}{\partial k}}{y^N + q \frac{\partial y^N}{\partial q}} > 0 \quad (15)$$

Expression (15) is unambiguously positive since $\frac{\partial y^N}{\partial k} < 0$ and $\frac{\partial y^N}{\partial q} > 0$. It will be convenient in what follows to denote the instantaneous equilibrium real exchange rate as a function of k :

$$q^* = q(k) \quad (16)$$

All that remains is to characterize the dynamic investment behavior of firms in the tradable sector. Firms are assumed to adjust their capital stock in order to maximize the present value of future profits and face convex adjustment costs $C(I, k) = I + (\phi/2)(I^2/k)$. In addition, firms face controls on capital inflows which may or may not be binding depending on the level of desired investment. Specifically, I assume that investment I may not exceed a certain threshold Ψ , which can be interpreted as a quantitative restriction on foreign borrowing. The firm's investment problem can be written as:

$$\max_{\{I, k\}_{\tau}^{\infty}} \Pi = \int_{t=\tau}^{\infty} e^{-r[\tau-t]} \left(\pi(\tilde{k})k - I - \frac{\phi I^2}{2k} \right) d\tau \quad \text{subject to } \dot{k} = I, \quad I \leq \Psi \quad (17)$$

where $\pi(\tilde{k})k$ is each firm's instantaneous profit function as a function of the aggregate capital stock \tilde{k} , which is taken as given by the individual firm, and each individual firm's capital k . The Lagrangian for this problem can be written as:

$$\mathcal{L} = \left(\pi(\tilde{k})k - I - \frac{\phi I^2}{2k} + QI \right) + \lambda(\Psi - I) \quad (18)$$

where the first term in parentheses is the current value Hamiltonian, Q is the co-state variable, and λ is the multiplier on the inequality constraint. The co-state variable Q is the famous "Tobin's Q " and measures the shadow price of capital. This problem has the following first order conditions:

$$FOC[I]: \quad -1 - \phi \frac{I}{k} + Q - \lambda = 0 \quad (19)$$

$$FOC[k]: \quad \pi(\tilde{k}) + \frac{\phi}{2} \left(\frac{I}{k} \right)^2 - rQ + \dot{Q} = 0 \quad (20)$$

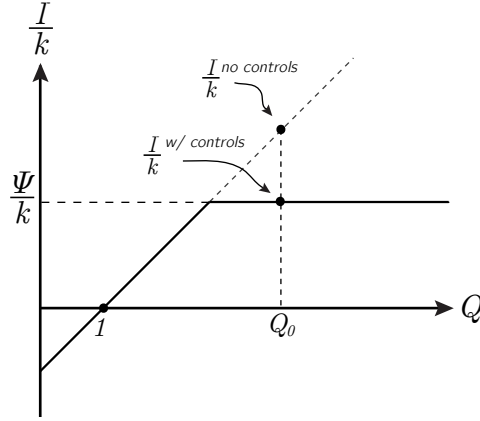
$$FOC[Q]: \quad \dot{k} - I = 0 \quad (21)$$

$$FOC[\lambda]: \quad \Psi - I = 0 \quad (22)$$

As is well known in investment models with convex adjustment costs, this model will exhibit saddle path stability assuming the following transversality condition holds:

$$\lim_{\tau \rightarrow \infty} e^{-r[\tau-t]} Q = 0 \quad (23)$$

Figure 1: Investment reaction function



Integrating (20) forward and imposing (23), the shadow price of capital at time t is given by:

$$Q(t) = \int_{\tau=t}^{\infty} e^{-r[\tau-t]} \left(\pi(\tilde{k}) - \frac{\phi}{2} \left(\frac{I}{k} \right)^2 \right) d\tau \quad (24)$$

Equation (19) defines investment as a function of Q and needs to be analyzed when the inequality constraint is binding and when it is slack. If the constraint is not binding ($I < \Psi$) the multiplier on the inequality constraint is equal to zero ($\lambda = 0$). Conversely, if the constraint is binding and $I = \Psi$, then $\lambda > 0$. Putting these pieces together, the optimal investment reaction function of tradable firms is a piecewise linear function of Q :

$$\frac{I}{k} = \begin{cases} \frac{Q-1}{\phi} & \text{if } Q < \phi\Psi + 1 \\ \frac{\Psi}{k} & \text{if } Q \geq \phi\Psi + 1 \end{cases} \quad (25)$$

Investment will be positive whenever $Q > 1$ and negative if $Q < 1$. Because of the presence of capital controls, investment will react slower to sufficiently large Q . This is shown in Figure 1. Suppose the shadow price of capital is initially $Q_0 > 1$. As shown in Figure 1, the level of investment given Q_0 is smaller than the level that would have prevailed if controls were absent. This implies that the capital stock will take longer to converge to its long-run steady state value, which is defined implicitly by:

$$r = \pi(\tilde{k}^{SS}) \quad (26)$$

where \tilde{k}^{SS} is the steady state capital stock. Similarly, along this adjustment path the real exchange rate will appreciate as \tilde{k} approaches the steady state. This in turn implies that the real exchange rate will take longer to converge to the long-run steady state when capital controls are binding. Formally, differentiate (16) with respect to time, recalling that $\partial q^*/\partial k > 0$ and $\dot{k} = I$:

$$\dot{q} = \begin{cases} \frac{\partial q^*}{\partial k} \left(\frac{Q-1}{\phi} \right) k & \text{if } Q < \phi\Psi + 1 \\ \frac{\partial q^*}{\partial k} \Psi & \text{if } Q \geq \phi\Psi + 1 \end{cases} \quad (27)$$

It is now easy to verify that when capital controls are binding ($I = \Psi$), ceteris paribus tighter controls will cause the real exchange rate to appreciate at a slower rate than under looser controls. In particular, suppose controls are loosened and Ψ increases. Clearly,

$$\frac{\partial \dot{q}}{\partial \Psi} = \frac{\partial q^*}{\partial k} > 0 \quad (28)$$

This expression is clearly positive, which implies that capital controls will slow the appreciation of the real exchange rate and slow its convergence towards its steady state level. Intuitively, capital controls constrain investment financed through external funding. This slows the growth rate of the capital stock, delaying its convergence towards its steady state level. Since the real exchange rate depends on the instantaneous allocation of labor across sectors, which is itself a function of the capital stock, we will observe a slower appreciation along the adjustment path.¹⁷

4 Data and empirical framework

The dataset consists of a large panel of countries observed at a yearly frequency over the period 1980-2011. Most of the variables come from the IMF's International Financial Statistics (IFS) and the World Bank's World Development Indicators (WDI). The dependent variable of interest is the natural logarithm of the real effective exchange rate (REER), which is an index constructed on the basis of a weighted average of each country's bilateral exchange rates vis-à-vis its trading partners deflated by its relative price level, where the weights reflect the importance of trade with each partner. The long-run variables included in the cointegrating relationship are the following: log PPP GDP per capital ($LN Y$), and net foreign assets divided by total imports (NFA). The short-run determinants of the real exchange rate are: log commodity terms of trade (TOT), government expenditure to GDP (GOV), annual population growth (POP), and a dummy variable for the advent of currency crises ($CRISIS$). Finally, seven different measures of capital controls are considered in the benchmark regressions. The first five are the Schindler index for overall restrictions on international capital flows (SCH) and several subindexes for restrictions on inflows (SCH_{IN}), outflows (SCH_{OUT}), equity transactions (SCH_{EQ}), and collective investments (SCH_{CI}). In addition, I also consider episodic capital controls ($KLEIN$) as defined by Klein (2012). As a robustness exercise, I also use the Chinn-Ito index of financial liberalization from Chinn and Ito (2008). Summary statistics are presented in Table 1.

The order of integration of each variable was determined using the panel unit root tests proposed by Pesaran (2007), Im et al. (2003), and Levin et al. (2002). In all three tests the null hypothesis is that the series have a unit root. The results for all three tests on all the main variables are reported in Table 2. All three tests fail to reject the null hypothesis that the level of the real exchange rate is non-stationary but easily reject the null for its first difference. This indicates, consistent with the literature discussed above, that the real exchange rate is likely $I(1)$ and therefore it will be treated as such in the empirical analysis that follows. The three tests also suggest that $LN Y$, NFA and TOT are first-difference stationary. As such, these are also treated as $I(1)$. The results are slightly ambiguous for population growth and government expenditure. Although Pesaran's CADF test fails to reject the null for POP , the IPS and LLC tests do reject the unit root null hypothesis. The results for GOV are similarly ambiguous: the unit root null is rejected by the IPS test but not by the CADF or the LLC tests.

The variables were tested for cointegration using the panel error-correction tests proposed by Westerlund (2007) and implemented by Westerlund and Edgerton (2008). These tests are derived from a panel error-correction model that allows for heterogeneity in the error-correction dynamics, including panel-specific intercepts, trends, and slopes. The test statistics are based on the idea that if the series are cointegrated, the coefficient on the error-correction term should be significantly negative. Westerlund develops four alternative statistics, two of which are constructed by averaging the estimated coefficients (G_α) and t-statistics (G_t) from each panel-specific error-correction term. The latter two are calculated by pooling observations across panels and estimating the error-correction term (P_α) and t-statistic (P_t).

Test results are shown in Table 3. Three of the four test statistics reject the null hypothesis of no cointegration for the model including $REER$, $LN Y$, and NFA . Results for the model including the log terms of trade are inconclusive: only one of the three test statistics rejects the null of no cointegration. This

¹⁷It is worth noting that this stylized model is also consistent with the two theoretical long-run determinants of the real exchange rate discussed above. It is easy to verify from (14) that an increase in the productivity of tradable goods ($\uparrow A^T$) will be associated with a real appreciation. Similarly, an increase in the net foreign asset position ($\uparrow b_0$) will also cause q to appreciate. However, since this model only features two types of goods it is silent on the effects of the terms of trade.

Table 1: Summary statistics

	Mean	Std. Dev.	Min	Max
<i>Long run variables</i>				
Log Real Effective Exchange Rate (<i>REER</i>)	4.626	0.371	2.278	7.685
Log PPP GDP per capita (<i>LNy</i>)	8.588	1.270	4.621	11.723
Net Foreign Assets / Imports (<i>NFA</i>)	-1.082	3.121	-41.475	30.253
<i>Short run variables</i>				
Log Commodity Terms of Trade (<i>TOT</i>)	4.732	0.374	2.675	6.421
Government Expenditure / GDP (<i>GOV</i>)	0.163	0.065	0.020	0.762
Population Growth (<i>POP</i>)	0.017	0.016	-0.181	0.175
Currency Crisis Dummy (<i>CRISIS</i>)	0.034	0.182	0.000	1.000
<i>Capital control indices</i>				
Schindler Index – Overall (<i>SCH</i>)	0.315	0.350	0.000	1.000
Schindler Index – Inflow (<i>SCH_{IN}</i>)	0.285	0.331	0.000	1.000
Schindler Index – Outflow (<i>SCH_{OUT}</i>)	0.345	0.397	0.000	1.000
Schindler Index – Equity (<i>SCH_{EQ}</i>)	0.313	0.365	0.000	1.000
Schindler Index – Collective Investment (<i>SCH_{CI}</i>)	0.300	0.372	0.000	1.000
Klein Episodic Controls (<i>KLEIN</i>)	0.011	0.075	0.000	1.000
Chinn-Ito Financial Openness Index (<i>CHITO</i>)	0.098	1.549	-1.864	2.439

Note: Each variable was obtained from the following sources. *REER*: IMF International Financial Statistics. *LNy*: World Development Indicators. *NFA*: External Wealth of Nations Database (Lane and Milesi-Ferretti, 2007). *TOT*: IMF World Economic Outlook Database. *GOV*: World Development Indicators. *POP*: World Development Indicators. *CRISIS*: Broner et al. (2013). *SCH_j*: Fernández et al. (2014). *KLEIN*: Klein (2012). *CHITO*: Chinn and Ito (2008).

Table 2: Panel unit root tests.

	CADF		IPS		Levin-Lin-Chu	
	Z_{t-bar}	p-value	Z_{t-bar}	p-value	Adj. t^*	p-value
<i>REER</i>	-1.192	0.117	-1.250	0.106	0.511	0.695
$\Delta REER$	-8.299	0.000	-24.711	0.000	-16.236	0.000
<i>LNy</i>	3.477	1.000	5.023	1.000	1.960	0.975
ΔLNy	29.403	0.000	-23.104	0.000	-15.529	0.000
<i>NFA</i>	5.141	1.000	-0.782	0.217	0.115	0.546
ΔNFA	-8.211	0.000	-26.294	0.000	-18.860	0.000
<i>TOT</i>	-1.129	0.130	-0.279	0.390	-0.432	0.333
ΔTOT	-11.098	0.000	-34.173	0.000	-24.462	0.000
<i>POP</i>	4.488	1.000	-35.720	0.000	-27.552	0.000
ΔPOP	-6.420	0.000	-33.785	0.000	-28.940	0.000
<i>GOV</i>	0.904	0.817	-2.065	0.020	-0.700	0.242
ΔGOV	-10.406	0.000	-34.596	0.000	-24.774	0.000

Note: Pesaran’s CADF test is implemented in Stata by Lewandowski (2006). The CADF test considers the case with 2 lags, a constant, and cross-sectional demeaning. Both the Im-Pesaran-Shin and Levin-Lin-Chu tests include a time trend and common AR coefficient. The panel-specific lag-orders were chosen using the BIC.

Table 3: Panel cointegration tests

	Panel-Specific		Pooled	
	G_t	G_α	P_t	P_α
<i>REER, LNY, NFA</i>	-1.536*	-4.890	-13.080***	-3.388**
<i>REER, LNY, NFA, TOT</i>	-1.631	-5.365	-14.475**	-4.618
<i>REER, LNY, NFA, SCH</i>	-1.803	-2.050	-6.415	-1.504
<i>REER, LNY, NFA, CHITO</i>	-1.831	-5.777	-12.221	-3.710

Note: This table reports the Z-values from the Westerlund (2007) panel cointegration tests. The null hypothesis is no cointegration. All tests consider the case with one lag and panel specific intercepts. These tests were implemented in Stata by Westerlund and Edgerton (2008). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

is consistent with Cashin et al. (2004), who showed, as discussed above, that the real exchange rate may only be cointegrated with the terms of trade in so-called commodity currency countries. Given the inconclusive evidence of a cointegrating relationship, *TOT* is treated as a short-run determinant of the real exchange rate and omitted from the baseline specification of the long-run level. Finally, Table 3 also reports results for tests of a long-run relationship between *REER*, *NFA*, and two complementary measures of capital controls: *SCH* and *CHITO*. All four test statistics fail to reject the no cointegration null. This suggests that capital controls do not have a long-run effect on the equilibrium real exchange rate. However, as we shall see below, this does not rule out significant effects on the short-run disequilibrium dynamics of the real exchange rate.

The cointegrating relationship is estimated using the method of dynamic ordinary least-squares (DOLS) proposed by Saikkonen (1991). As Saikkonen shows, the cointegrating relationship can be consistently and efficiently estimated by OLS adding leads and lags of the first differenced cointegrated variables with Newey-West heteroskedasticity and autocorrelation consistent (HAC) standard errors. Because $REER_{i,t}$ is an index and does not contain information about the relative level of the real exchange rate, the model includes country fixed effects. The inclusion of country fixed effects also addresses potential omitted variable bias. Year dummies are also included to control for common time factors. The estimated long-run equilibrium equation is given by

$$REER_{i,t} = \gamma_i + \alpha_t + \beta x_{i,t} + \sum_{j=-\rho}^{\rho} \eta \Delta x_{i,t-j} + e_{i,t} \quad (29)$$

where γ_i and α_t are vectors of country and year fixed effects, respectively, $x_{i,t}$ is a vector of $I(1)$ variables cointegrated with $REER_{i,t}$, and the fourth term on the right hand side is the set of leads and lags of $\Delta x_{i,t}$. The error term $e_{i,t}$ captures short-run deviations from the long-run relationship and can be interpreted as the extent of real exchange rate disequilibria. A positive $e_{i,t}$ implies the real exchange rate is overvalued while a negative value implies an undervaluation. To estimate how fast deviations from the long-run equilibrium are eliminated, the estimated residuals, $\hat{e}_{i,t}$ are imposed on the error-correction model (ECM) in equation (30):

$$\Delta REER_{i,t} = \Theta_{i,t} \hat{e}_{i,t-1} + \alpha \Delta x_{i,t} + \beta z_{i,t} + u_{i,t} \quad (30)$$

where

$$\Theta_{i,t} = \theta_1 + \theta_2 SCH_{i,t} \quad (31)$$

The ECM is augmented with a vector of short-run stationary variables $z_{i,t}$. These include the annual change in the government expenditure to GDP ratio (ΔGOV), population growth (*POP*), the log growth of the commodity terms of trade (ΔTOT), and a dummy for currency crises (*CRISIS*). The coefficient $\Theta_{i,t}$ measures the speed of adjustment towards the long-run equilibrium and varies across both countries and years. Consistency between equations (29) and (30) requires $\Theta_{i,t} < 0$. Otherwise, $e_{i,t}$ would be non-stationary and therefore $REER_{i,t}$ and $x_{i,t}$ cannot be cointegrated. Rather than allowing unlimited heterogeneity, the speed of adjustment is modeled as a function of a constant base-rate θ_1 and an additional term that depends on the intensity of capital controls. Hence, the speed of adjustment is captured by the marginal effect of $\hat{e}_{i,t}$ on

$\Delta REER_{i,t}$:

$$\frac{\partial \Delta REER_{i,t}}{\partial \hat{e}_{i,t-1}} = \begin{cases} \theta_1 & \text{if } SCH_{i,t} = 0 \text{ (no capital controls)} \\ \theta_1 + \theta_2 & \text{if } SCH_{i,t} = 1 \text{ (full capital controls)} \end{cases} \quad (32)$$

If capital controls slow the speed of adjustment and cause disequilibria to persist for longer periods of time, then $\Theta_{i,t}$ should be smaller in absolute value when controls are present. This requires $\theta_1 < 0$, $\theta_2 > 0$, and $|\theta_2| \leq |\theta_1|$. The latter restriction ensures that the system is stable and that disequilibria are not explosive.

Putting the pieces together, the empirical strategy is to estimate the long-run equilibrium relationship (29) and use the residuals to estimate the ECM in (30). To estimate the effect of differences in capital controls on the speed of adjustment, the different measures of capital control intensity are interacted with the lagged residuals. Therefore, a positive and statistically significant coefficient on the interaction term would confirm the hypothesis. The ECM is augmented with a lagged dependent variable to account for potential persistence in short-run real exchange rate movements and a full set of country and time dummies to deal with unobservable short-run time-invariant and country-invariant factors. Since the introduction of a lagged dependent variable in a fixed-effects framework introduces dynamic panel bias (Nickell bias), the ECM is estimated using two-step GMM.

5 Empirical results

The results for the benchmark equilibrium real exchange rate level regressions are presented in Table 4. I consider a variety of specifications for the long-run relationship, including a simple model where the long-run real exchange rate only depends on log GDP per capita. These results appear in column (1). The coefficient is positive, indicating that an increase in productivity leads to a real appreciation, and statistically significant at the one percent level. Its magnitude is also economically significant: a one percent increase in *LN**Y* leads to roughly a quarter of a percent increase in the real exchange rate. Column (2) considers another stripped down model where the equilibrium *REER* depends solely on *NFA*. Consistent with the literature, a higher net foreign assets position has a statistically significant positive effect on the real exchange rate. In particular, a one standard deviation increase of *NFA* leads to a six percent real appreciation. Next, column (3) considers the log commodity terms of trade which, as expected, has a positive coefficient. However, the estimate is not statistically significant. The specification in column (4), which will serve as the baseline for the error-correction models estimated below, includes both *LN**Y* and *NFA* simultaneously. Both coefficients have the expected signs and are significant at standard significance levels.

To compare with the results below, in each level specification I report the error-correction term for a simple ECM with homogenous adjustment dynamics. The speed of adjustment ranges from a low of 0.19 to a high of 0.21, indicating that roughly a fifth of the disequilibria are eliminated each year. These estimates are consistent with previous studies and, in particular, are very close to those reported by Ricci et al. (2013), who report an adjustment speed of 0.2. As a reference, these estimated adjustment speeds imply half-lives on average of roughly 3 years.

The results for the ECM with heterogenous adjustment dynamics in (30) are shown in Table 5. The lagged residual $\hat{e}_{i,t-1}$ corresponds to the baseline level specification in column (4) of Table 4. As described above, the ECM is augmented with a lagged dependent variable and a full set of country and time dummies. Because the combination of a lagged dependent variable and fixed country effects introduces Nickell bias, the model is estimated using two-step GMM. The baseline ECM specification is reported in column (1), which includes an interaction term between the $\hat{e}_{i,t-1}$ and the Schindler index of capital control intensity (*SCH*). The first thing to note is that the results appear to support the hypothesis that capital controls slow the speed of adjustment towards long-run equilibrium. Specifically, the interaction term has a positive and significant coefficient that is smaller in absolute value than the coefficient on $\hat{e}_{i,t-1}$. The Hansen J statistic test for the over identifying restrictions fails to reject the null hypothesis while the Kleibergen-Paap statistic for testing for weak instruments does reject the null. This indicates that the model is well specified and that the lagged levels of the real exchange rate are good instruments for $\Delta REER_{i,t-1}$.

Table 4: Long-run cointegrating relationship

Dependent Variable: REER					
	(1)	(2)	(3)	(4)	(5)
Log PPP GDP Per Capita (<i>LN</i> <i>Y</i>)	0.254*** (0.051)			0.226*** (0.048)	0.266*** (0.066)
Net Foreign Assets / Imports (<i>NFA</i>)		0.019* (0.010)		0.023** (0.010)	0.022** (0.010)
Log Commodity Terms of Trade (<i>TOT</i>)			0.091 (0.057)		0.070 (0.054)
Error-Correction Term					
\hat{e}_{t-1}	-0.210*** (0.034)	-0.186*** (0.033)	-0.204*** (0.033)	-0.192*** (0.034)	-0.186*** (0.033)
Observations	2,191	2,191	2,173	2,191	2,173
Countries	88	88	88	88	88
R-squared	0.585	0.572	0.556	0.598	0.601
RMSE	0.193	0.196	0.199	0.190	0.189
Country FE?	Yes	Yes	Yes	Yes	Yes
Year FE?	Yes	Yes	Yes	Yes	Yes

Note: The benchmark DOLS specification includes one lead and two lags of the differenced long-run explanatory variables. Results are robust to different lag lengths. The coefficient and standard error estimates for the leads and lags are not reported. Full results are available upon request. Robust HAC standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

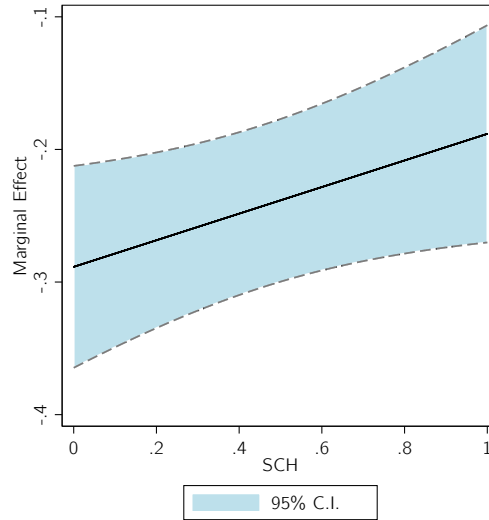
As discussed above, the speed of adjustment is measured by the marginal effect of $\hat{e}_{i,t-1}$ on $\Delta REER_{i,t}$. The baseline results are consistent with the hypothesis that the marginal effect increases with the intensity of capital controls – that is, the speed of adjustment is slower the higher the intensity of controls. This is depicted graphically in Figure 2, along with the 95 percent confidence interval. The different adjustment dynamics based on the intensity of capital controls can also be depicted graphically as a phase-diagram in $(e_{i,t-1}, \Delta REER_{i,t})$ -space. A dynamically stable equilibrium relationship requires a downward sloping curve, where steeper slopes correspond to faster adjustment dynamics. This is shown in Figure 3 for two cases: no capital controls ($SCH = 0$) and full capital controls ($SCH = 1$). The “phase arrows” portray the dynamic directions of motion. Specifically, whenever $e_{i,t-1} < 0$ and the real exchange rate is undervalued, $\Delta REER_{i,t} > 0$ and thus the undervaluation is gradually eliminated. As can be seen in Panel (a), when controls are absent the real exchange rate rapidly adjusts to eliminate disequilibria (a steeper adjustment curve). However, when controls are set at their full intensity in Panel (b), the adjustment curve is flatter.

The remaining columns in Table 5 report results from using alternative measures of capital controls. The specifications in columns (2) and (3) consider the effects of controls exclusively on capital inflows and on capital outflows, respectively. These results are not very different from the baseline specification, although the point estimates for the interaction term with SCH_{IN} and SCH_{OUT} are slightly smaller. Taking full advantage of the granularity of the Schindler index, I also examine if controls on some types of financial instruments are more effective than others. Columns (4) and (5) report results using the SCH subindexes for equities and collective investments, respectively. These results are very similar to the baseline estimates in column (1), although the point estimate for controls on equity transactions is somewhat larger than for the average index and for collective investments.

Zooming into further detail, Tables 7 and 8 report ECM estimates using even finer instrument subcategories for capital inflows and outflows, respectively. In general terms, the results imply substantially different adjustment speeds depending on the type of restriction imposed. For instance, there is no evidence that restricting cross-border bond transactions has a statistically significant impact on the speed of adjustment.¹⁸

¹⁸Estimates considering controls on inward and outward direct investment, as well as for financial credits (not reported) are

Figure 2: Error correction speed as a function of capital controls intensity



Note: This figure shows the speed of adjustment as a function of the level of capital controls ($\theta_1 + \theta_2 SCH_{it}$). The estimated coefficients and standard errors correspond to specification (1) from Table 5.

The results are further nuanced within instrument categories: restrictions on selling or issuing equities abroad appear effective (Table 7, column (3)) but restrictions on the local purchase of equities by non-residents do not (Table 7, column (2)). Inflow restrictions on collective investments, on the other hand, appear to be unambiguously effective, as are outflow restrictions on equity transactions.

As discussed above, it is potentially important to distinguish between permanent and episodic capital controls. Thus, as an additional robustness exercise, I consider the impact of episodic capital controls as defined by Klein (2012).¹⁹ Specifically, I use an index of the average intensity of episodic controls, *KLEIN*. This index is simply an episodic counterpart of *SCH* that, in line with Klein’s work, excludes permanent restrictions on capital flows. The key takeaway from this robustness exercise is that episodic capital controls also appear to slow the speed of adjustment, as indicated by the positive and significant coefficients on the interaction term. Moreover, the point estimate for the *KLEIN* interaction term is substantially larger than for any other measure of capital controls. This suggests that temporary capital controls are not only an effective means of slowing *REER* adjustment, but may also be more effective than their permanent counterparts.

This result, however, should be interpreted with care. First, the large adjustment slowdown observed with episodic controls may arise because these are often imposed in conjunction with other policy measures designed to lean against the wind. In other words, the estimated impact of temporary controls may be picking up the effects of other complementary policy interventions. Nevertheless, these types of endogeneity concerns are lessened by Fernández et al. (2014)’s findings that controls are generally unresponsive to cyclical factors, including changes in the real exchange rate. Second, as discussed above, it is highly likely that controls may lose their efficacy over time as financial markets learn how to evade them and exploit legal loopholes. Therefore, the larger point estimate for the *KLEIN* interaction term may reflect that the controls in question have not remained in place long enough to lose their efficacy.

As a final robustness exercise, column (7) of Table 5 reports results using Chinn and Ito (2008)’s index of international financial liberalization. Unlike *SCH* and *KLEIN*, *CHITO* ranges from -1.86 (most closed) to 2.44 (most liberalized). As such, a *negative* coefficient on the interaction term would now constitute evidence in favor of the hypothesis that less open capital accounts slow the adjustment speed of the real

similarly insignificant. Full results are available upon request.

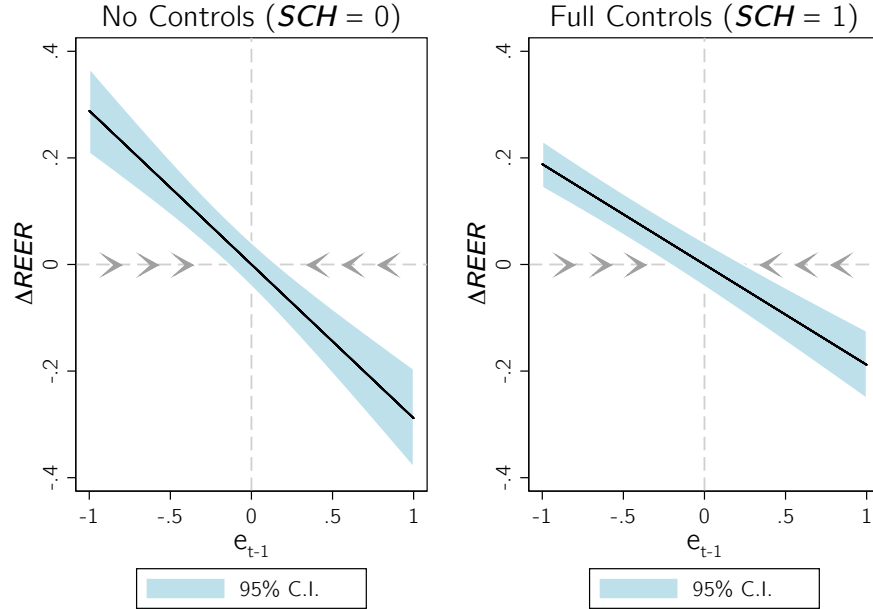
¹⁹The episode dates and instruments covered were taken from Table A.1 in Klein (2012).

Table 5: Error-Correction Models

Dependent Variable: $\Delta REER$							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\hat{\epsilon}_{t-1}$	-0.289*** (0.039)	-0.274*** (0.037)	-0.288*** (0.038)	-0.300*** (0.037)	-0.285*** (0.036)	-0.253*** (0.031)	-0.222*** (0.034)
$\hat{\epsilon}_{t-1} \cdot SCH$	0.100** (0.051)						
$\hat{\epsilon}_{t-1} \cdot SCH_{IN}$		0.091* (0.051)					
$\hat{\epsilon}_{t-1} \cdot SCH_{OUT}$			0.079* (0.042)				
$\hat{\epsilon}_{t-1} \cdot SCH_{EQ}$				0.122** (0.049)			
$\hat{\epsilon}_{t-1} \cdot SCH_{CI}$					0.100** (0.043)		
$\hat{\epsilon}_{t-1} \cdot KLEIN$						0.154** (0.069)	
$\hat{\epsilon}_{t-1} \cdot CHITO$							-0.022** (0.010)
$\Delta REER_{t-1}$	0.150*** (0.050)	0.148*** (0.050)	0.153*** (0.050)	0.156*** (0.050)	0.149*** (0.050)	0.145*** (0.050)	0.176*** (0.063)
$CRISIS$	-0.220*** (0.039)	-0.221*** (0.039)	-0.220*** (0.039)	-0.221*** (0.039)	-0.221*** (0.039)	-0.221*** (0.039)	-0.148*** (0.025)
ΔGOV	0.707** (0.326)	0.710** (0.326)	0.706** (0.326)	0.702** (0.336)	0.733** (0.317)	0.721** (0.322)	0.880*** (0.265)
POP	1.761** (0.786)	1.806** (0.792)	1.734** (0.789)	1.781** (0.787)	1.781** (0.781)	1.951** (0.823)	2.626** (1.114)
ΔTOT	-0.020 (0.028)	-0.020 (0.028)	-0.021 (0.028)	-0.021 (0.028)	-0.018 (0.028)	-0.024 (0.028)	-0.043 (0.036)
Observations	707	707	707	707	707	707	1,380
Countries	48	48	48	48	48	48	59
R-squared	0.414	0.413	0.414	0.417	0.417	0.414	0.319
RMSE	0.056	0.056	0.056	0.055	0.055	0.056	0.086
Hansen J Stat (p-val)	0.755	0.726	0.786	0.744	0.740	0.691	0.267
Underindent. test (p-val)	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Note: Each ECM is estimated with two-step GMM using the residuals from the DOLS regression in Table 4. All specifications include lagged differences of the long-run variables LNY and NFA . The single lag-order was chosen using the AIC and BIC. SCH refers to the overall Schindler index. SCH_j refers to the Schindler sub indexes j , where IN , OUT , EQ , and CI denote, respectively, average restrictions on capital inflows, outflows, equities, and collective investments. $KLEIN$ is an index for the intensity of Klein (2012)'s episodic capital controls. $CHITO$ refers to the Chinn-Ito index of financial liberalization (Chinn and Ito, 2008). Robust HAC standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure 3: Adjustment Dynamics



Note: The adjustment dynamics correspond to specification (1) from Table 5. The arrows show the direction of motion. The left panel was calculated setting capital control intensity to zero ($SCH = 0$). The right panel considers the case with capital controls set to the highest intensity ($SCH = 1$).

exchange rate.²⁰ As expected, the coefficient on the interaction term is negative and statistically significant. Moreover, these estimates are remarkably similar to the benchmark results: the error-correction speed when *CHITO* is set to its minimum is roughly -0.18. Similarly, the estimates imply a speed of adjustment of -0.28 in a fully liberalized country.

These estimates imply significant heterogeneity in the speed of adjustment across both countries and time. To illustrate these differences in speed, Table 6 reports the estimated half-lives for the persistence of disequilibria. The real exchange rate converges to its equilibrium level at a very high speed in countries with relatively low control intensities. For instance, in the baseline estimate it only takes 2 years for half of a deviation to be eliminated in countries with no controls. This estimated half-life is significantly smaller than those reported in most of the literature. On the other hand, the half-life is as high as 3.3 years with a full set of controls and 3.5 years in countries with strict controls on equity transactions. The differences are even starker when episodic controls are imposed: the half-life for countries with strict episodic controls is nearly 7 years.

6 Concluding remarks

This paper has examined the relationship between capital controls and the real exchange rate. In surveying the extensive literature on the determinants of the real exchange rate, it was determined that ample evidence and theory support the proposition that in the long-run the real exchange is non-stationary and driven by fundamentals. The consensus among empirical studies on the effects of capital controls is that these enable domestic authorities to maintain an independent monetary policy and shield countries from short-term, speculative flows. The evidence is far less conclusive when it comes to limiting the overall volume of flows and influencing the real exchange rate.

²⁰*CHITO* has the advantage that it covers more countries and years than the *SCH* data.

Table 6: Estimated Half-Lives (years) from Error-Correction Model

	<i>SCH</i>	<i>SCH_{IN}</i>	<i>SCH_{OUT}</i>	<i>SCH_{EQ}</i>	<i>SCH_{CI}</i>	<i>KLEIN</i>	<i>CHITO</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
No Controls	2.036	2.163	2.037	1.943	2.065	2.378	2.144
Average Controls	2.318	2.443	2.254	2.267	2.354	2.992	2.728
Full Controls	3.323	3.418	2.946	3.536	3.392	6.629	3.475

Note: This table reports the number of years it takes the real exchange rate to eliminate half of its disequilibrium from its long-run equilibrium level. The half-lives correspond to the specifications in columns (1) through (7) in Table 5. In the case of the Schindler indexes and Klein’s episodic index, the half-lives are calculated evaluating the capital controls at zero (no capital controls), the sample average for each control measure (average controls), and at one (full controls). For the Chinn-Ito index of capital account liberalization, the half-lives are calculated setting $CHITO=2.4$ (complete liberalization), $CHITO=0.1$ (sample average), and $CHITO=-1.9$ (completely closed).

Previous studies, however, have overlooked the long-run determinants of the real exchange rate and are therefore misspecified. Taking the determinants of the real exchange rate seriously, it was shown that capital controls may have very dramatic effects on real exchange rate dynamics, especially if controls are sufficiently strict. Specifically, controls appear to enable real undervaluations or overvaluations to persist for significantly longer periods compared to countries without controls.

These findings are consistent with a variety of theoretical channels and it is not this paper’s aim to stress one channel over another. Nevertheless, one interpretation is that these findings are consistent with the stylized dynamic investment model presented above. According to this model, the quantitative restrictions on external funding are binding and constrain investment in the tradable sector. The capital stock therefore responds more sluggishly to the shadow price of capital and along the adjustment path the real exchange rate deviates from its long-run level. Another interpretation is that capital controls introduce a wedge between the domestic and international interest rate, distorting factor allocations between sectors and producing real exchange rate disequilibria. If controls do not influence the long-run equilibrium $REER$ but slow its adjustment speed, then this wedge must fade out over time, possibly due to evasion by sophisticated financial markets.

Future work should examine the role of error-correction non-linearities and in particular potential differences between the speed of adjustment of overvaluations and undervaluations. Moreover, it may prove fruitful to examine if these non-linearities are also compounded by different types of capital controls. For instance, controls on inflows may slow the correction of undervaluations but *increase* the speed of adjustment when the real exchange rate is overvalued. Conversely, tighter controls on outflows may cause undervaluations to be eliminated more quickly while allowing overvaluations to persist for longer or become more severe.

The broader lesson to take from this study is that capital controls are an effective policy tool for managing the real exchange rate. In other words, controls can help achieve policy objectives in addition to the macro-prudential concerns stressed by the recent literature. In particular, countries seeking to promote the growth of the domestic manufacturing sector may fruitfully employ capital controls to help achieve a real undervaluation. Nevertheless, strictly speaking, the empirical results presented above do not explain how an undervaluation is initially achieved but rather suggest that the real exchange rate, once already undervalued, will take longer to converge to its long-run level. How the undervaluation is originally achieved and how this affects the real exchange rate’s short-run dynamics requires further research.

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Table 7: ECM with controls on capital inflows, by instrument.

Instrument:	Dependent Variable: $\Delta REER$											
	Equities			Bonds			Money Market			Collective Investments		
	<i>IN</i>	<i>PLBN</i>	<i>SIAR</i>	<i>IN</i>	<i>PLBN</i>	<i>SIAR</i>	<i>IN</i>	<i>PLBN</i>	<i>SIAR</i>	<i>IN</i>	<i>PLBN</i>	<i>SIAR</i>
Control Type <i>j</i> :	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\hat{\epsilon}_{t-1}$	-0.265*** (0.034)	-0.251*** (0.033)	-0.267*** (0.033)	-0.324*** (0.031)	-0.316*** (0.031)	-0.329*** (0.033)	-0.261*** (0.035)	-0.261*** (0.033)	-0.252*** (0.034)	-0.271*** (0.035)	-0.259*** (0.032)	-0.270*** (0.035)
$\hat{\epsilon}_{t-1} \cdot SCH_j$	0.058 (0.038)	0.019 (0.035)	0.049* (0.030)	0.028 (0.045)	0.005 (0.031)	0.029 (0.040)	0.055 (0.041)	0.064* (0.034)	0.017 (0.033)	0.096** (0.039)	0.079*** (0.030)	0.065** (0.033)
Observations	707	707	707	619	619	619	707	707	707	707	707	707
Countries	48	48	48	48	48	48	48	48	48	48	48	48
R-squared	0.412	0.41	0.413	0.44	0.439	0.441	0.412	0.414	0.41	0.418	0.416	0.416
RMSE	0.056	0.056	0.056	0.054	0.054	0.054	0.056	0.056	0.056	0.055	0.056	0.056

Note: *IN*: average controls on capital inflows. *PLBN*: purchase locally by non-residents. *SIAR*: sale or issue abroad by residents. Short-run control variables and lagged first-differences of long-run variables not reported. Robust HAC standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: ECM with controls on capital outflows, by instrument.

Instrument:	Dependent Variable: $\Delta REER$											
	Equities			Bonds			Money Market			Collective Investments		
	<i>OUT</i>	<i>SILN</i>	<i>PABR</i>	<i>OUT</i>	<i>SILN</i>	<i>PABR</i>	<i>OUT</i>	<i>SILN</i>	<i>PABR</i>	<i>OUT</i>	<i>SILN</i>	<i>PABR</i>
Control Type <i>j</i> :	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\hat{\epsilon}_{t-1}$	-0.305*** (0.037)	-0.285*** (0.032)	-0.286*** (0.038)	-0.348*** (0.039)	-0.344*** (0.035)	-0.330*** (0.036)	-0.272*** (0.037)	-0.248*** (0.034)	-0.283*** (0.038)	-0.277*** (0.035)	-0.262*** (0.033)	-0.276*** (0.036)
$\hat{\epsilon}_{t-1} \cdot SCH_j$	0.105*** (0.037)	0.071** (0.028)	0.070** (0.033)	0.06 (0.049)	0.05 (0.038)	0.03 (0.036)	0.053 (0.042)	0.003 (0.034)	0.074* (0.039)	0.06 (0.037)	0.026 (0.028)	0.065* (0.037)
Observations	707	707	706	619	619	619	707	707	707	707	707	707
Countries	48	48	48	48	48	48	48	48	48	48	48	48
R-squared	0.419	0.416	0.415	0.442	0.442	0.441	0.412	0.41	0.415	0.413	0.411	0.414
RMSE	0.055	0.056	0.056	0.053	0.054	0.054	0.056	0.056	0.056	0.056	0.056	0.056

Note: *OUT*: average controls on capital outflows. *SILN*: sale or issue locally by non-residents. *PABR*: purchase abroad by residents. Short-run control variables and lagged first-differences of long-run variables not reported. Robust HAC standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.