Banco Central de Chile
Documentos de Trabajo

Central Bank of Chile
Working Papers

N° 182

Septiembre 2002

ARE DEVALUATIONS REALLY CONTRACTIONARY?

I. Igal Magendzo

Documentos de Trabajo del Banco Central de Chile
Working Papers of the Central Bank of Chile
Huérfanos 1175, primer piso.
Teléfono: (56-2) 6702475, Fax: (56-2) 6702231

Documento de Trabajo
N° 182

Working Paper
N° 182

ARE DEVALUATIONS REALLY CONTRACTIONARY?

I. Igal Magendzo
Economista Senior
Gerencia de Análisis Macroeconómico
Banco Central de Chile

Resumen

Dado que no existe un consenso teórico acerca del efecto que tienen las devaluaciones sobre el producto, la evidencia empírica ha jugado un rol fundamental. Los hallazgos empíricos han favorecido la visión de que las devaluaciones son contractivas. En el presente trabajo el autor sugiere que lo que está detrás de estos resultados es un sesgo de selección. En teoría, las mismas variables que determinan la probabilidad de una devaluación determinan la tasa de crecimiento del producto. El autor controla por el sesgo de selección utilizando "matching estimators" (estimadores apariados) y datos para 155 países para el período 1970-1999 que incluyen 264 episodios de devaluaciones. Al no controlar por sesgo de selección las devaluaciones parecen ser contractivas, en línea con hallazgos previos de literatura empírica. Sin embargo, cuando el autor controla por el sesgo de selección el efecto contractivo desaparece. El trabajo incluye un extensivo análisis de sensibilidad.

Abstract

Given the theoretical disagreement on the effect of a devaluation on output, empirical evidence plays an fundamental role. Empirical findings have favored the view that devaluations are contractionary. In this paper the author argues that what stands behind these results is selection bias. In theory, the same variables that determine the probability of a devaluation determine the rate of growth of output. The author controls for selection bias using matching estimators and extensive dataset of 155 countries for the period 1970-1999 that includes as many as 264 devaluation episodes. Not controlling for selection bias, devaluations appear to be contractionary, in line with previous findings. Nevertheless, when the author controls for this bias, the contractionary effect vanishes. Extensive sensitivity analysis is provided.

This paper is based on chapter four of my PhD dissertation at UCLA. I thank Sebastian Edwards, who has been a tireless source of knowledge and inspiration. I acknowledge the helpful comments of José De Gregorio, Al Harberger, Joseph Hotz, Ed Leamer, Rossen Valkanov and Klaus Schmidt-Hebbel.
E-mail: imagendz@bcentral.cl.

I Introduction

More often than not, theoretical models have ambiguous results on the e®ect of a devaluation on output. Whether a devaluation is contractionary or expansionary depends on the particular model as well as on the particular parameters of the economy. On the other hand, the experience of a number of countries seems to validate the view that devalautions are rather contractionary. During the 1990's Mexico, Indonesia, Malaysia, South Korea, the Philippines, Thailand, Russia, and Turkey and more recently Argentina experienced a dramatic devaluation of the local currency accompanied by recession rather than recovery. In the present paper I put this view to test.

According to traditional textbook theories, devaluations should have an expansionary e®ect on the economy. The devaluation is supposed to have a switching e®ect, increasing the production of tradables and improving the external position of the country. Nevertheless, there is growing literature claiming that devaluations are likely to be contractionary rather than expansionary[1]. Given the theoretical disagreement, empirical evidence plays an important role. Possibly the earliest paper to study the issue from an empirical perspective is Cooper [1971b] that ¯nds devaluations to be \often" contractionary. Since then, a number of studies have found similar evidence using sophisticated econometric techniques and an increasing number of cross-country observations, while other papers have found the contrary to be true.

The empirical literature so far has overlooked an important factor: countries that experience a sharp devaluation are not randomly chosen among all countries. It is plausible that the same variables that a®ect the likelyhood of a devaluation also determine output growth. This is known in labor literature as \selection bias". Traditional econometric methods are unable to properly account for this problem, and so produce biased estimates. The purpose of the present paper is to provide robust and unbiased evidence on whether devaluations are contractionary in the short run.

In the present paper, I further investigate the short run e®ects of devaluations on growth. In particular, and following the seminal paper by Cooper [1971b] I focus on episodes of \sudden" discrete devaluations[2]. The main reason given by Cooper [1971, p.1] to focus on this type of events is \because of the associated trauma, which arises because so many economic adjustments to a discrete change in the exchange rate are crowded into a relatively short period".

In comparison to the rest of the literature on the issue, this paper uses the largest data set, with the largest number of countries, observations and devaluation episodes. Moreover, in order to tackle the problem of selection bias, the present study is the ¯rst to apply matching estimators to the question at hand. Matching is a non-parametric technique widely used in labor economics to evaluate the e®ect of training and other programs on income. In general, the idea is to compare episodes of large-devaluation with countries that are as similar as possible but that did not devalue. This is a way to mimic or replicate a natural experiment setting.

In this paper, estimates of the e®ect of sharp devaluations that do not control for selection bias show that these events are associated with considerable lower growth rates. This is in line with previous ¯ndings using unconditional comparisons and regression analyses. Nevertheless, after taking into account the fact that variables that induce a sharp devaluation may also a®ect growth, sharp devaluations appear not to have any signi¯cant e®ect on output growth. This result is robust to the de¯nition of sharp devaluation as well as to the variables that are used as control variables. I conjecture that the di®erent channels through which devaluations are supposed to a®ect growth cancel each other.

The rest of the paper is organized as follows. The next section reviews the main theoretical arguments on how a devalaution a®ects output. Based on the existing theoretical work I argue that not accounting for selection bias is the main de¯ciency of previous empirical studies. Section three reviews previous empirical work. I put particular emphasis on the more sophisticated and in°uential part of this extensive literature. I show that, even though results point in every direction, i.e. that devaluations are contractionary, expansionary or neutral, there is growing support for the view that devaluations are rather contractionary. Section four outlines the empirical approach. In order to tackle the problem of selection bias, I use matching estimators. Matching estimators are explained in detail in that section. Section ¯ve concentrates on the results and provides sensitivity analyses, particularly on the methodology as well as on the de¯nition of devaluation. I show that indeed selection bias is what stands behind the fact that the empirical literature so far tends to favor the view that devaluations are contractionary. After controlling for selection bias, the contractionary e®ect of devaluations tends to vanish. Section six concludes.

II Devaluation, Growth and Selection Bias: An Analytical Framework

If there is one thing that characterizes the theories on the shoet run e®ect of devaluation on growth is that almost always the sign of this e®ect is theoretically ambiguous. Even though more traditional views supported the idea that devaluations are expansionary, this argument not only has been apparently rejected by the data, but has also been theoretically challenged in many ways. Most of the challenging arguments have been finely summarized and analyzed by Edwards [1989] as well as by Lizondo and Montiel [1989]. The more typical arguments on the effects of a devaluation on output are:

- According to the traditional view (Dornbusch [1988]), if there is iddle capacity, for a given level of real income, a devaluation will shift demand towards the non-tradable sector, stimulating growth. This is consistent with the view that the total (domestic and foreign) demand for domestically-produced tradable goods is perfectly elastic and not affected by relative price changes.
- A real devaluation affects both real income and the demand for domestically produced goods. If the share of tradable goods in total output is higher (lower) than the share of tradable goods in consumption, then the effect is expansionary (contractionary). The real devaluation increases the real value of the tradable output but also increases the real cost of consumption of tredables. The net effect on income will depend on which is higher: the share of tradable goods in consumption or in income.
- A real devaluation has redistributing effects. This resdistribution of income can be between wages and profits (see Diaz Alejandro [1963], Cooper [1971a, 1971b] and Krugman and Taylor [1978]) or between factors in the tradable and non-tradable sectors. Depending on the specific model's assumptions and particularly on the assumed differences in the marginal propensity to consume, the devaluation can increase or decrease demand for domestically-produced output.
- A devaluation is likeley to have a negative effect through the real cash balance. A devaluation is likely to increase the price level and thereby to reduce the real stock of money. As individuals try to restore the real value of their stock of money, they consume less (see Gylfson and Schmid [1983], Gylfason and Radetzki [1985] and Edwards [1989]). Montiel and Lizondo [1989] argue that this effect can be reverted if individuals hold other types of assets whose nominal value increase with a devaluation, e.g. foreign currency.
- A devaluation rises the domestic price of imported inputs, having a negative effect on aggregate supply. Models including this effect are Gyfalson and Schmid [1983], van Wijnbergen [1986] and Edwards [1989].
- A devaluation rises the real value of the foreign debt service, reducing national income. This contractionary effect has been emphasized by Gylfason and Risager [1984] and Edwards [1989].

A more recent line of thought puts the emphasis on the risk premium (see for example Krugman [1999]). This view is finely summarized in a model presented by Céspedes et al. [2000], to which I will refer as the CCV thereafter. This is a dynamic general equilibrium open-economy model where (Céspedes et al., 2000 p.2): "the real exchange rate plays a central role in the adjustment process, wages are sticky in terms of a domestic currency [...], liabilities are "dollarized", and the country-risk premium is endogenously determined by the net worth of domestic entrepreneurs." The authors use this model to investigate the insulating properties of different exchange rate regimes. Nevertheless, the model has important implications for the effects of a devaluation on output. The CCV model clearly shows that the relevant comparison is not between an episode of nominal devaluation and an episode of stable nominal exchange rate in general. When an adverse shock calls for a real depreciation, the relevant comparison is between letting the nominal exchange rate depreciate or some alternative policy, e.g. having deflation. Even though the devaluation might be partially responsible for a fall in output, to avoid the devaluation (and allow for deflation) might have even worse consequences. Regarding the effect of a devaluation on output, the main lessons from the CCV model are:

- An adverse shock such as an increase in the world interest rate affects output negatively and also calls for a real devaluation. This is also recognized by Kamin and Klau [1997]. This means that even though we might see a fall in output and a nominal devaluation occuring at the same time, that does not necessarily imply that the devaluation caused the fall in output. A devaluation can be neither expansionary nor contractionary and we can still observe a negative correlation between these two variables.

- If a necessary real devaluation is achieved through deflation rather than through a nominal devaluation and wages are sticky there would be a temporary increase in real wages with an adverse effect on output. In that sense, the nominal devaluation is actually expansionary when compared to the alternative scenario. The devaluation isolates the labor markets from the external shock.

- A nominal devaluation has a negative effect on net worth since debt is denominated in dollars. Nevertheless, according to the CCV model, if the needed real depreciation is achieved through deflation, the effect on net worth is even more negative. The initial deflation depreciates the real exchange rate by less, and thus has a smaller negative effect on net worth. But there is a fall in output (as explained above) that, as Céspedes et. al [2000] show, more than compensates for this difference. The total effect is always less negative with a devaluation of the nominal exchange rate.

² Following Bernanke et al. [1998], in the CCV model the risk premium depends on the ratio of investment to ¯rms' net worth. Compared to de°ation, a devaluation implies both higher net worth and higher domestic value of investment. The net e®ect could go either way.

The CCV model also shows that the magnitude of the e®ects of a nominal devaluation or de°ation depend crucially on the parameters of the economy. Of particular interest is the ¯nancial vulnerability of the economy. Finanacial vulnerability acts as a multiplier through the risk premium. In the CCV model, ¯nancial vulnerability is measured through the elasticity of the risk premium with respect to a change in the real exchange rate. This in turn depends on the share of investment demand in total non-consumption demand for home goods, the steady state ratio of debt to net worth, the elasticity of the risk premium function, and monitoring costs in the ¯nancial markets. The more vulnerable the economy is, the stronger the e®ect of real devaluation will be on output. In addition, all e®ects are non-linear. C¶spedes et al. [2000] solve a linearized version of the model. As we deviate from the steady state non-linearities become more and more important. The non-linearities in the CCV model come mostly from information asymmetries in the ¯nancial markets. Finally, given the non-linearities of the CCV model, large devaluations may have e®ects that are very di®erent in magnitude from small devaluations. Krugman [1999] argues that balance sheet e®ects may dominate only for large devaluations.

The implications of the theoretical literature for the appropriate empirical approach are not trivial. First, given the theoretical disagreement on the net e®ect of a devaluation on output, the empirical literature plays an important role. Second, when one asseses the e®ect of devaluation on output one has to take into account that the same variables that a®ect output might also a®ect the exchange rate. One has to control for the relevant variables that are correlated both with output and the exchange rate. These control variables have to include not only the relevant shocks but also other characteristics of the economy that determine how important the e®ects are, such as ¯nancial vulnerability. Third, one has to compare similar economies. The relevant question is what would happen to the output of a country that experiences a nominal devaluation compared to the same country under the same circumstances had it not devalued. In other words, one has to avoid selection bias in the sense of comparing a country that went through a sharp devaluation with another that never needed such a devaluation in the ¯rst place, either because it did not face similar shocks or because it is less vulnerable. Most of the arguments above point in the direction that economies with di®erent structures are likely to respond di®erently to a nominal devaluation. This is a central point and a serious complication that no empirical work on the issue has tackled so far. An important contribution of the present paper is to control for selection bias. Fourth, the non-linear aspect of the problem at hand must be taken into account. To impose a linear structure may be an inadequate way to proceed. Instead, it seems better to use techniques that are more robust to the speci¯cation of the problem. Finally, it seems reasonable and even desirable to concentrate on some non-linear measure of devaluations.

III Previous Findings

Possibly the earliest study on the e®ect of devaluation on economic performance is Cooper [1971b]. That paper studies 24 cases of \large" devaluations in LDCs during the period 1953-66 and ¯nds devaluations to be \often" contractionary. In response, Krueger [1978] analyzed output behavior for 22 cases of \large" devaluation in LDC for the period 1951-70 rejecting for most of the cases the hypothesis that devaluations are contractionary.

Since this early discussion a few papers have found devaluations to be expansionary. Some of them, such as Gylfason and Schmidt [1983], Gylfason and Risager [1984] and Gylfason and Radetzky [1985], have been based on the simulation of small macro models that incorporate elements such as intermediate inputs or debt service. Nevertheless, most of the empirical evidence that favors the view that devaluations are expansionary comes from studies that use VAR techniques. Almost all of these studies, ¯nely summarized in Kamin and Klau [1997], are not focused directly on the e®ects of exchange rates on output per se and analyze the experience of a single country. Also Kamin and Klau [1997] provide time series evidence that for 27 countries and the period 1970-1996 real exchange rate devaluations are expansionary (although the results are shown not to be robust by the same authors). Other (rather simple) empirical studies have found mixed evidence, ¯nding either di®erent results for di®erent countries, or no statistically signi¯cant e®ects (Connolly [1983], Upadhyaya [1999] and Upadhyaya and Upadhyaya [1999]).

However, the most sophisticated piece of econometric work, and the most in°uential, have found devaluations to be contractionary. The ¯rst extensive regression analysis that explicitly takes into account the e®ect of other variables is Edwards [1986]. The study looks at the e®ect of changes in the real exchange rate on output, but controls for the e®ect on output of government spending, monetary shocks, the terms of trade and a country speci¯c time trend. It uses data for 12 developing countries for 1965-80. The author concludes that depreciations of the real exchange rate have a contemporaneous negative e®ect on real output, but this e®ect is reverted after one year.

In a follow-up study, Edwards [1989] extends the analysis of Edwards [1986] to include more detailed analysis of the data, more countries, a longer coverage period, and basically the same theoretical framework (made explicit through a model) and regression techniques. In this paper the author contrasts the growth rate of 39 devaluation episodes for the period 1962-82 with a control group consisting of 24 developing nations that maintained a fixed exchange rate for at least ten years. This comparison shows that the average growth rate of the large-devaluation years is about 2 percentage points lower than that of the control group. The regression analysis, performed for 12 developing countries for the period 1965-84 confirms the above results, showing a short run negative effect of devaluations[3].

Morley [1992] studies 28 large-devaluation cases covering 26 countries and the period 1973-83. This study focuses on the effect of real devaluations on capacity utilization and uses a pooled regression, with no fixed or random effects, except for a dummy for African countries. The regression controls for standard variables such as monetary and fiscal policy and terms of trade, but also controls for some less standard variables such as the rate of growth of exports and imports. The regression yields a coefficient for the real exchange rate that is negative and significantly different from zero. In a more sophisticated econometric work, Kamin and Klau [1997] run a fixed effects error-correction model for 27 countries for the period 1970-1996. They control for monetary, fiscal and external variables as well as for the investment ratio. Also according to this specification real devaluations appear to be contractionary in the short run.

Barro [2001] also focuses on five countries that experienced a large devaluation during the financial crisis of 1997-98. He contrasts the performance of those countries to the performance of other four Asian countries that did not experience a large devaluation during the same period. The author runs a pooled regression for 67 countries for the averages of each five years from 1965-2000. No random or fixed effects are included. In other words, this is a cross section where the units of observation are the pairs country-quinquennium. The control variables are initial per capita GDP, male upper-level schooling, life expectancy, fertility rate, government consumption, a rule-of-law index, a measure of openness, the inflation rate, the investment ratio and the growth rate of the terms of trade. Dummies for the two groups of Asian countries are added to this basic regression. The results show that countries that experienced a large-devaluation grew about four percentage points less than otherwise predicted, while the other four countries grew in a way that is not significantly different from predicted. Unfortunately, the use of five-year averages does not allow us to know if the lower growth happened before, during or after the crisis.

IV Empirical Approach: Matching Estimators

As the above analysis shows, a problem with erxisitng empirical studies is that they do not consider that devaluations are not the result of a random experiment. Consequently, the estimated mean di®erences across devaluations and normal periods are likely to be a biased estimator of the \true" di®erence. The same covariates that a®ect economic performance may induce the devaluation, possibly in a non-liner fashion. For example, if countries are more likely to devalue and to grow less when faced with negative terms of trade shocks, the parameter that captures the e®ect of a devaluation will be correlated with the omitted variables producing a downward bias on the estimated e®ect of the devaluation. Similarly, as the CCV model shows, countries that are ¯nancially more vulnerable are likely to grow less because of a higher risk premium and lower investment and at the same time are more likely to experience a sharp devaluation when phased with a negative external shock.

One way to avoid these problems is to calibrate and simulate a structural model. This is done by C¶spedes et. al [2000], Gylfason and Schmidt [1983] and Gylfason and Risager [1984], among others. In contrast, I will follow an econometric approach in order to estimate the mean (and median) e®ect of devaluations on output as implied by historical data.

In order to avoid the selection bias, it is necessary to de¯ne how the nominal exchange rate reacts to the relevant variables, given that we want to compare countries under similar circumstances. One way to proceed would be to have a complete econometric model of the nominal exchange rate. This is a very di±cult task in face of the particularities of each country and the non-linearities inherent to the problem. Also, the magnitudes are not necessarily comparable across countries. What can be a very dramatic devaluation for one country can be a very common event for another. An alternative is to concentrate on events of sharp devaluations or exchange rate crises. It is a very common practice in international ¯nance to concentrate on crises rather than on the magnitude of the crises[4]. This is obviously a consequence of the di±culty of the problem at hand.

Call g^0_{it} the growth rate of country i in period t if it does not devalue. Similarly, call g^1_{it} the growth rate of country i in period t if it does devalue. De¯ne a set of variables T that a®ect the growth rate, such that:

$$\begin{aligned} g^0_{it} &= h^0(T_{it}) + U^0_{it} \qquad (1) \\ g^1_{it} &= h^1(T_{it}) + U^1_{it}; \end{aligned}$$

where U are additively separable unobservable variables. So far, researchers have assumed that devaluation is a random event, sometimes controlling for T in a linear fashion. This allows to compare directly the outcomes of countries that have devalued and countries that have not devalued. In contrast, I assume here that devaluation responds to a set of variables Z, such that:

$$D = \begin{cases} 1 & \text{if } \psi(Z) - v \geq 0 \\ 0 & \text{otherwise,} \end{cases} \tag{2}$$

where Z may or may not contain some or all the variables in T. The parameter of interest is $\mu = E(g^0_{it} - g^1_{it}|Z; T; D = 1)$. In words, we are interested in the mean effect of a sharp devaluation on a country's economic growth. The analyst's problem, however, is that he does not have data to estimate $E(g^0_{it}|Z; T; D = 1)$, the growth rate of countries that devalued, had they not devalued. An appropriate control group will allow us to compare observations with identical covariates. Unfortunately, we have no such control group. I address the problem using matching estimators, developed in training evaluation labor literature[5]. Matching estimators use the existing data to construct an appropriate sample counterpart for the missing information. I follow this non-parametric approach because: a) it generates an adequate control group on which to perform the comparison of outcomes. This is done by pairing each large-devaluation episode with "similar" episodes from the control group. b) It is a general method in the sense that no particular specification of the underlying model has to be assumed. These two points are relevant to the problem at hand, as explained in the previous section.

The basic assumption of matching estimators is that the selection of the treated is based on observable (to the researcher) variables. In particular it assumes that the outcomes of the un-treated are independent of the participation variable once one controls for the observables: $y_0 \perp D|X$, where X contains the variables in Z and T. In terms of the problem at hand, the devaluation event is totally determined by Z and a random factor. This implies that after controlling for Z, the outcomes of the un-treated (no large-devaluation) is exactly what the outcome of the treated (large-devaluation) would have been without the treatment. The fact that some of these identical country-periods (in terms of Z) had a large-devaluation and others did not is assumed to be a random event.

If the sample is large enough, we might find for each treated observation at least one un-treated observation with the same characteristics, and this would constitute the required counterfactual. In order to guarantee that all treated agents have such a counterpart in the population (not necessarily in the sample) we also need to assume that $0 < Pr(D = 1|Z) < 1$.

The problem is that in most samples it is not possible to find an exact match. The matching method focuses on estimating an average version of the parameter of interest. This average version is given by:

$$M(S) = \frac{\int_S E(y_1 - y_0 | X; D = 1) dF(X | D = 1)}{\int_S F(X | D = 1)}; \qquad (3)$$

where S is a subset of the support of X given D = 1, and X contains all the variables in T and Z. I estimate the difference in outcome as an average of the differences with respect to "similar" -rather than identical -untreated outcomes. Matching estimators in general can be written as:

$$\hat{M}(S) = \sum_{i \in T} w_i (y_i - \sum_{j \in C} W_{ij} y_j) \qquad (4)$$

where $\hat{M}$ stands for the matching estimator, T and C are respectively the sets of treated and untreated periods, W_{ij} are weights attached to each untreated observation j for comparison with treated country i, and w_i are the weights that allow us to reconstruct the outcome distribution for the treated sample. In order to check the robustness of the results I follow different approaches to calculate this average. The different methods differ on the definition of W_{ij}, that is, on the weights assigned to each untreated observation.

The matching method requires that we compare the characteristics of different countries. This can be an extremely painful, time consuming and even impossible high-dimensionality problem. Rosenbaum and Rubin [1983] show that a simplifying way to perform this comparison is to use the propensity score. The propensity score is defined as the probability of participation: $P(Z) = Pr(D = 1|Z)$. In our case, this is the probability of a large-devaluation. This reduces a multi-dimensional problem to a one-dimensional problem, provided that we can estimate $P(Z)$. Instead of matching countries directly on their characteristics, we can compare countries with similar probability of experiencing a large-devaluation.

I use a simple average nearest neighbor estimator. For each treated observation I select among the untreated observations a pre-determined number of nearest neighbor(s). The nearest neighbors of a particular treated observation i are defined as those untreated observations that have the smallest difference in propensity score with respect to i. This method attaches the simple average of the outcomes of the nearest neighbors as the relevant comparison observation. If we choose to have nn nearest neighbors, we set for the nn nearest neighbors (closer propensity) $W_{ij} = 1$ and $W_{ij} = 0$ otherwise, and to set $w_i = \frac{1}{nn}$.

The above method can be applied by varying the number of nearest neighbors. Also it can be performed with and without replacement. In the former case the same untreated observation can be the nearest neighbor of several treated observations. In the latter, each untreated observation can be the nearest neighbor of only one treated observation. This last option demands more data points and increases the likelihood of including observations that are too distant, but reduces the risk of relying on too few comparison observations.

V Results

Large Devaluations: A Definition

As stated above, the present study concentrates on the effect of sharp devaluations on growth. I define sharp devaluations as exchange rate increases that are big from a country's historical perspective. Devaluation episodes are calculated from monthly data on the exchange rate of 155 non OECD countries including Korea and Mexico and for the period ranging from 1970 to 1999. Following Levy-Yeyati and Sturtzeneger [2002] for each country I use the exchange rate with respect to "the most relevant" country[6]. For example, the reference exchange rate for Poland is the zlotych-mark exchange rate, but for Chad (and a number of other African countries) it is the exchange rate with respect to the French franc. Exchange rates with respect to the US are obtained from the IFS CD-ROM provided by the IMF and are used to construct the other bilateral exchange rates when needed. Not all countries have information for all years. There is an average of about 27 years of data per country. The exclusion of OECD countries is common practice in crises literature, somewhat arbitrary, but a safe measure to avoid including countries that are too different at least in the sense that those are mostly net creditor countries.

A high devaluation is defined as an episode in which the monthly exchange rate devaluation deviates from the country's average rate of devaluation over all available months by more than 2.5 times the standard deviation:

$$D = \begin{cases} 1 & \text{if } \varepsilon_{i,t} \geq \mu_i + 2.5 \cdot \sigma_i(\varepsilon) \text{ and } \varepsilon_{i,t} \geq 5\% \\ 0 & \text{otherwise,} \end{cases} \tag{5}$$

where $\varepsilon_{i,t}$ is the rate of devaluation of the currency of country i in month t (using end of month exchange rates), μ_i is the average rate of devaluation for country i over all the available months and $\sigma_i(\varepsilon)$ is the standard deviation of the rate of devaluation for country i over all the available months. I also restrict the definition to contain only episodes in which the rate of devaluation is more than 5%. This is not to include small devaluations from countries that experience very low exchange rate °uctuations. A very similar approach is used by Hutchison [2001].

It is also necessary to take into account very high in°ation episodes. As posed by Kaminsky and Reinhart [1999]: \A single index for the countries that had hyperin°ation episodes would miss sizable devaluations [...] in the moderate in°ation periods, since the historic mean is distorted by the high-in°ation episode." In order to avoid this problem I follow Kaminsky and Reinhart [1999] and treat as a separate unit (a separate country) a period of in°ation higher than 100%, including a window of six month before and after.

Thresholds have been used to de¯ne crisis by an increasing number of studies (Eichengreen et al. [1996], Sachs et al. [1996] and Kaminsky and Reinhart [1999]), but most authors include the variation of reserves (and of interest rates when available) in their de¯nitions, in order to capture events where the currency has been under attack, but no devaluation took place. The object of interest in the present paper are nominal devaluations, so I do not include variations in reserves. From the monthly data I obtain 885 episodes that satisfy the above criteria. The average monthly devaluation rate during these episodes is 95%, with a minimum of 5% (by de¯nition), a maximum of 14,186% and a median of 17%.

For all statistical calculations I use yearly rather than monthly data. A year is said to be a high devaluation year if there is one or more high devaluation months within the year. If there two or more consecutive high-devaluation years, I consider only the ¯rst of them. In addition, a window of two years after each devaluation episode (for the country that exhibits the devaluation) is excluded from the sample. This is not to include into the control group or the treated group observations that might be contaminated by the e®ects of recent devaluations. The resulting large devaluation episodes are listed in Table I, where I include only those observations that have data for per capita GDP growth. There are 264 devaluation episodes left. Some countries, such as Botswana, Burundi, Papua New Guinea, Trinidad and Tobago, and Turkey have had as many as ¯ve devaluation episodes in the period under analysis. The 1990s is the decade with the largest number of episodes (125) and 1994 the year with the largest number of episodes (27).

Unconditional Comparison and Linear Regression: A Benchmark

As a ¯rst approach, I compare the growth rates of large-devaluation periods with the rate of growth of \normal" periods for all country-years. The results are presented in Table II. Periods classi¯ed as \devaluers" exhibit a considerably lower growth rate than do control periods. The mean di®erence in annual growth rate is as much as 2.1 percentage points in favor of the control group. The median di®erence is -1.5. Latin America and the Caribean is the region of the world[7] with the largest di®erence between devaluers and non-devaluers. The mean di®erence in annual per capita growth rate goes to 4.2 percentage points in favor of non-devaluers. Nevertheless, this region does not account for the whole story and without it there is a statistically signi¯cative di®erence in per capita growth of 1.4 percentage points. In addition, Table II shows that the 1990's are not di®erent. Even though average growth rates are lower for both groups, the mean di®erence does not change substantially with respect to the whole sample. The median di®erence is about one percentage point more negative.

The above results are in line with previous ¯ndings. For example, Edwards [1989] ¯nds that the median growth rate at the year of the devaluation is 2.2 percentage points lower than that of the control group. A similar magnitude is provided by Barro [2001].

Of course, unconditional comparison of growth rates is likely to be a biased estimate of the true e®ect. Most studies use regression techniques to control for variables that might a®ect GDP growth and bias the e®ect of the devaluation dummy. Table III presents results using regression analysis. The dependent variable is the logarithmic change in annual per capita GDP[8]. To evaluate the robustness of the results I ¯t six di®erent models. I assume that \potential" per capita output ($\hat{y}$) is determined by per capita capital stock (K) and a Solow residual (A). On the other hand, current per capita growth rate (g) is determined by the growth rate of per capita potential output ($\hat{g}$), a convergence factor that brings actual GDP towards potential GDP (y ¡ $\hat{y}$) and a number of domestic as well as external shocks. For a country i in a particular period t:

$$\hat{y}_{it} = A_{it} ¤ F(K_{it}) \tag{6}$$

$$g_{it} = \hat{g}_{it} + \mu(y_{it} ¡ \hat{y}_{it}) + \text{shocks}_{it} + ´_i + "; \tag{7}$$

where $´_i$ is country speci¯c random e®ect and " is a normally distributed zero mean residual. The choice of variables to explain potential GDP growth comes from the growth literature[9]. I also control for variables that approximate the relevant shocks. The source and construction of all variables are described in detail in the Appendix. The devaluation dummy variable is also included.

The ¯rst equation excludes less traditional (to the empirical growth literature) variables such as the output gap, monetary shocks and a proxy for ¯nancial system fragility. It does include a measure of initial per capita GDP to capture the familiar conditional convergence effect. Also included are two measures of human capital, namely average years of school attainment and life expectancy at birth. There are also four variables that capture some aspects of government policies and institutions: a measure of government size, a measure of macroeconomic stability (inflation), a measure of distortions in the exchange rate market, and a measure of openness to international trade. The next three variables are dummies that capture some other relevant aspects of a country, such as having a civil war during a particular year, being a tropical country and being an exporter of non fuel primary products. Finally, two variables capture external conditions: the growth rate of industrialized countries and the growth rate of the terms of trade.

In addition to the variables in the first equation, I have three variables less traditional in empirical growth literature included in the second equation. First, the output gap to capture some "out of equilibrium" dynamics. Second, and following Edwards [1989], I include a measure of monetary shocks. Third, and following Beck et al. [1999] I include a measure of financial development. In the third equation I included regional dummies, while in the fourth equation I included a dummy for each year. The fifth equation looks only at Latin American countries while the last equation looks at all countries but only for the 1990's. The number of observations varies depending on data availability, with a maximum of 1,269 observations coming from 91 countries. The parameters are estimated using Maximum Likelihood and a Gauss-Hermite quadrature approximation.

It is worth noting that education, measured by average years of schooling, does not seem to have any effect on output growth, except for equation four, where the coefficient is only marginally significant at the 5% level[10]. The same is true for the growth rate of industrialized countries, for which the parameter is not only non-significant but it even changes sign from one equation to another. Also the openness proxy does not show a significant effect on output growth, except for the Latin American countries, where it is negatively correlated with growth. Finally, it is interesting to point out that terms of trade looses its ability to explain growth when controlling for monetary shocks.

With respect to the variable of interest, i.e. the devaluation dummy, the GLS random-effects estimation results show that a devaluation reduces growth by 1.6 to 2.3 percentage points. The effect is the strongest for Latin American countries, with a contractionary effect of 3.1 percentage points of annual per capita GDP growth. Nevertheless, the results are not totally driven by Latin American countries. With Latin America excluded, the contractionary effect reported here is still positive and strongly significant. These results are similar to those obtained from the unconditional comparison. The results are also similar to what other authors have obtained. In particular, Barro [2001] ¯nds that the currency crises experienced by ¯ve Asian countries during the period 1997-1998 is associated with a per capita growth rate that is on average 1.3 percentage points lower than otherwise predicted. Hutchinson [2001] ¯nds balance of payment crises to reduce output growth by 1.5 percentage points, but that study includes reserve losses as part of the crisis indicator.

Matching Estimators

The ¯rst of the matching method is to de¯ne the propensity score. The propensity score corresponds to the ¯tted probability of devaluation, were all variables a®ecting the dependent variable (growth) as well as the probability of a high-devaluation should be included[11]. All estimations presented below were performed by ¯tting a random e®ects probit model.

In order to check the robustness of the results, I ¯tted four di®erent speci¯cations for the probability of currency crisis; the results are reported in Table V. The reported equations were obtained after some speci¯cation search where other variables such as government surplus, current account de¯cit, and past exchange rate regime were discarded[12]. Details about the source and construction of each series are shown in the Appendix. The ¯rst model (column 1) includes variables that are traditionally associated with the probability of an exchange crisis[13]. The ¯rst three variables capture the ¯nancial vulnerability of the country. First, the (lag of the) rate of growth of exports. Second, the (lag of the) ratio of external debt to annual exports. Third, a measure of the ratio of external debt to GDP relative to \normal" or usual values. Some measure of the level of reserves is normally included in other studies. Nevertheless, variations of that variable were not signi¯cant and, in the process of speci¯cation search, it was left out. Two variables capture the e®ects of the real exchange rate on the probability of a nominal devaluation. The ¯rst looks at the contemporaneous level of the real exchange rate, where higher values are expected to be accompanied by nominal devaluations. The second, following Goldfajn and Vald¶es [1997], captures (lagged) real exchange rate misalignments. Similarly, the black market premium is there to capture distortions in the exchange rate markets that might call for a nominal devaluation (see Edwards, 1989). Also included are two measures of how open is the country to international trade. The ¯rst captures actual openness, while the second captures trend openness, smoothing out extraordinary open or closed periods. Finally, there are two variables to capture the importance of regional conditions: following Edwards [2001] I include the number of crises in the region during the same period[14], and the total capital °ows to the region as a fraction of the region's GDP.

Equation 2 includes all the variables in equation 1 and the lag of the output gap. This variable is included to capture factor utilization in the economy, where a larger output gap is supposed to be related to larger unemployment rates, possibly increasing the probability of a devaluation. Equation 4 includes regional dummies, while equation 5 includes yearly dummies.

The results of Table V show that for the variables included in the first equation the parameters have the expected sign and are relatively robust to the different specifications. Some of the results are interesting by themselves. Both the level of the real exchange rate and (the lag of) the measure of real exchange rate overvaluation are positive and significant in explaining the probability of a large devaluation. As expected, when a currency is overvalued it is more likely that the nominal exchange rate will suffer a devaluation. But, also the level of the real exchange rate matters. Based on the CCV model presented above, the (equilibrium) real exchange rate changes in response to foreign and domestic shocks. An increase in the real exchange rate is associated with a larger probability of devaluation as the alternative policy is to allow for wage deflation.

It is also interesting to notice that while openness is positively associated with the probability of devaluation, the smoothed version of the variable has a negative parameter but similar in absolute value. This means that the probability of a large devaluation increases when the country is “more open than usual". This is consistent with the idea that during international liberalization periods, countries are more exposed to balance of payments problems[15]. Additionally, while the debt to exports ratio is marginally significant in two of the five equations, debt to GDP above the historical trend has a positive and significant parameter. This is a measure of financial vulnerability (recall the importance of this variable in the CCV model) and controls for an “excessive debt effect". The smoothed version of the variable proxies the “normal" or “acceptable" level of indebtedness, while deviations from this trend (from above) measures “over-indebtedness" with the associated increase in the risk premium.

Two additional variables have been included in the analysis in order to capture regional financial conditions. First is a variable containing the number of large devaluations detected on other countries of the same region during the same year. This is to capture regional contagion[16], and the parameter is positive and significant. The second is total capital flows to the region as a percentage of the region's GDP. Changes in both these variables are measures of external shocks that demand internal adjustments to the real exchange rate.

The purpose of matching estimators is to compare similar countries to avoid selection bias. The propensity scores calculated above (the fitted probabilities) do a good job in bringing the samples closer together. For example, exports growth rate for devaluers is on average 2.81%, which contrasts with a 10.81% average for the rest of the observations. If we take the average of the control observations derived from the nearest neighbor without replacement using the propensity score implied by equation 3[17] the average growth rate of exports is reduced to 3.4%[18]. Similarly, the average number of crisis in the region during a particular year goes up from 4.4 for all non-devaluer observations, to 6.1 for matching observations, which is the same as the average of devaluers. Also, I report in Table IV the nearest neighbor implied by equation three in Table V for each treated observation, with and without replacement. For some observations, the nearest neighbor seems reasonable at first sight. Nevertheless, for other observations the nearest neighbor might look a little awkward. In order to be more robust to the choice of neighbors I perform also matching using the five nearest neighbors with an equal weight each.

Results for the matching estimates are reported in Table VI. Each column corresponds to the respective equation of Table V, where the fitted probabilities were used as the propensity score to perform the matching. For each equation I calculated the mean and median difference of growth rate of the treated observation (devaluer) with respect to the nearest neighbors. I use a single nearest neighbor without replacement and five nearest neighbors with replacement. If we allow for replacement in the single nearest neighbor estimator, we are left with too few comparison observations and the results are more difficult to trust. On the other hand, if no replacement is allowed for the five nearest neighbors, too many observations are used approaching the results of unconditional comparison.

The results of the nearest neighbor without replacement show that devaluations do not have any discernable effect on per capita output growth. Similarly, not even one of the estimates coming from five nearest neighbors with replacement, which increases considerably the number of control observations, is significantly different from zero; neither is the mean effects nor the median effects.

If we take the point estimates at face value, they range from 0.7 to -0.9 for the means and from 1.1 to -0.2 for the medians. In other words, both mean and median effects do not even present a systematic sign. The median effects, if anything, point more in the direction of an expansionary effect. Nevertheless, from these results we can conclude that it is far-fetched to say that devaluations have any systematic effect on output growth.

Robustness

The Matching Estimator

First, I look at the nearest neighbor with replacement. The results are reported in Table VII. As one can see, the number of control observations drops. The results show that the mean effect is not significantly different from zero in most cases. The exception is equation 4, where the mean effect is positive, but only marginally significant. The median of the difference in per capita growth is positive in four of the five estimates, and two of them turn out to be significantly different from zero. This estimator clearly points to a positive effect of devaluations on output. Nevertheless, these results should be taken carefully because of the small number of control observations.

On the other hand, if the five nearest neighbors without replacement estimator is used, the number of control observations increases considerably, approaching the whole control sample. That is the reason for the mean estimates to become negative and significant, corroborating the need to control for selection bias. In any case, although negative, the median effects are not statistically different from zero.

A matching method I use to check the robustness of the results is the local linear regression (LLR) matching estimator, proposed by Heckman et al. [1997]. LLR is a non-parametric technique similar to traditional kernel regression, but Fan [1992, 1993] shows that it has some important advantages over the kernel regression[19]. In this estimator the assigned weights are:

$$W_{ij} = \frac{K_{ij}\sum_{k\in C} K_{ik}(P_k - P_i)^2 - K_{ik}(P_k - P_j)\left(\sum_{k\in C} K_{ik}(P_k - P_i)\right)}{\sum_{j\in C} K_{ij}\sum_{k\in C} K_{ik}(P_k - P_i)^2 - \left(\sum_{k\in C} K_{ik}(P_k - P_i)\right)^2}; \tag{8}$$

where K_{ij} is a kernel weighting function and P_i is the value of the propensity score for observation i. An additional, but equivalent way to apply LLR is the following: for each treated observation, we run a weighted least square regression of the outcomes on the difference of the propensity score, where less weight is assigned to those observations that have a more different propensity score. Under the assumptions we have made, the resulting constant should be a good estimate of the expected outcome of the treated if it had not received the treatment, i.e. $E(y_{1i}|x; D = 0)$. The weights can be chosen using any standard kernel function and selecting an adequate bandwidth. The LLR estimator can be interpreted as solving:

$$\min_{a;b} \sum (y_{0j} - a - b(P(x_i) - P(x_j)))^2 K\left(\frac{P(x_i) - P(x_j)}{h_n}\right); \tag{9}$$

where a and b are parameters, j indexes untreated observations and i indexes treated observations. I choose a standard Gaussian weighting kernel, with an also standard Silverman's rule of thumb bandwidth.

Using this technique I obtain the results reported in Table VII in the "Local Linear Regression" lines. These results confirm what we concluded above: on average, devaluations do not have a statistically significant effect on per capita output growth. This is true for both the means and the medians.

Heckman et al. [1997] suggest that exclusion restrictions can be used to improve the efficiency of the matching estimators. Given that Z in equation (2) does not include all the variables in T in equation (1), Heckman et al. [1997] show that matching estimators can be performed on the residuals of equation (1) rather than the per capita growth rate itself. Assuming that $h(\epsilon)$ is linear on T, I use the residuals from the first two equations presented in Table V (that do not include regional or yearly dummies), but excluding the large-devaluation dummy variable. I then perform matching estimator to get the effect of devaluation on the residuals. The results are presented in Table VIII. In this case, if anything, we see some evidence of devaluations being expansionary. Three of the four models used to generate the propensity score show a positive and significative median effect in panel a of Table VIII (corresponding to five nearest neighbors with replacement).

Re-Defining Devaluation

The definition of devaluation used so far is to some extent arbitrary. It is important to analyze whether the results are robust to some aspects of the definition. First, I increase the standard deviations needed to qualify as a devaluation from 2.5 to 3.0 in equation (5) and I also reduce it to 2.0. Increasing the threshold to 3.0 standard deviations reduces the number of large devaluation years from 264 to 224, while increasing this threshold to 2.0 increases the number of events to 318. The mean difference in per capita growth reported in panel A of Table II remains practically unchanged, with a -2.01 for the 2.0 standard deviations and -2.14 for the case of 3.0 standard deviations. The effect according to the regression analysis changes, but only marginally. For instance, the dummy effect reported on column (2) of Table III goes from -0.019 to -0.017 for 2.0 standard deviations and to -0.024 for 3.0 standard deviations. Finally, for the matching estimators, the results varying the threshold are reported in Table IX, panels a and b. All the results are not significantly different from zero, confirming the previous results.

In addition, I follow a more traditional definition of large devaluations, based only on the percentage rate of change of the exchange rate. Edwards [1989] as well as Morley [1992] define a large devaluation episode as an episode where the monthly devaluation is above 15%. The disadvantage of this definition, as pointed above, is that the absolute devaluation rate is likely to have very di®erent impacts depending on how volatile is the exchange rate in general. With this de¯nition the number of devaluation episodes is reduced from 264 to 237, but the two de¯nitions share only 183 episodes. Using the 15% threshold the unconditional comparison shows devaluations to be more contractionary. The mean di®erence of per capita growth rates goes from -2.11 to -3.35. The same, though in a less dramatic way, occurs for the regression analysis, where the parameter in front of the dummy variable in model (2) of Table III goes from -0.019 to -0.025. Finally, the results from matching estimators, reported in panel c of Table IX, shows that the signs of mean and median e®ects are systematically negative, but the di®erences are still not signi¯cantly di®erent from zero.

Other

I check the robustness of the results with respect to two additional modi¯cations. First, I include investment rates everywhere in the analysis. In the regression analysis the investment to GDP ratio comes out with a positive and signi¯cative parameter, as expected. The e®ect on the parameter of the dummy variable is negligible. In particular, if the investment to GDP ratio is included in equation 2 of Table III, the parameter of the dummy variable goes from -0.0213 to -0.0209. On the other hand, if the investment to GDP ratio is included in the probit equations the parameter is systematically positive (both contemporaneous and lagged), implying that more investment is associated with a larger probability of devaluation. However, the mean and median e®ects for the matching results reported in Table VI do not change in any signi¯cant way. The reason to exclude this variable from our analysis is to avoid problems of endogeneity.

Finally, I change the de¯nition of high in°ation used to obtain the large devaluation episodes. Recall that high in°ation periods, with a window of six months before and after, were treated as a di®erent country in equation (5). Increasing the high in°ation de¯nition from 100% to 150% reduces the number of monthly events from 885 to 835 and to 834 if high in°ation is de¯ne as above 200%. Accordingly, the number of large devaluation years drops from 264 to 253 and 250, respectively[20].

The unconditional di®erence of means is almost unaltered when changing the in°ation threshold: -2.00 with a threshold of 150% and -1.99 with a threshold of 200%, compared to -2.11 with a threshold of 100%. Also the results from the regression analysis are practically unaltered. Finally, the results from matching estimators show no change in sign and no relevant change in the signi¯cance of the parameters.

VI Conclusions

If devaluations are contractionary {as argued by a growing number of studies both empirical and theoretical{ the policy implication is that countries, in general, should try to avoid those events. Nevertheless, the empirical literature seldom asks what is the alternative to a nominal devaluation when a real exchange accommodation is needed. Traditionally, this literature compares the growth rates during devaluation to the growth rate of a control group. At most, using regression analysis, that literature controls for the steady state growth rate and/or the level of external and internal shocks. But, the control group has to be properly de¯ned in order to answer the question at hand: are devaluations contractionary given the current conditions? What would have been the growth rate of a particular country that devalued the nominal exchange rate had it not devalued? In other words, the control group has to be properly de¯ned to avoid selection bias.

In order to control for selection bias I use matching estimators. This technique builds an appropriate control group based on the propensity score or the probability of a large devaluation. Additionally, this is a semi-parametric technique that is robust to the speci¯cation of the outcome equation. Without controlling for selection bias I ¯nd devaluations to be associated with a growth rate that is 2 percentage points lower than otherwise predicted. This result is consistent with other studies. However, after controlling for selection bias, the contractionary e®ect of devaluation vanishes. I implement a number of variants of matching estimators. I also show sensitivity analysis with respect to the control variables as well as the de¯nition of devaluation. The results are robust: devaluations show no statistically signi¯cant e®ect on output growth.

The above result opens a series of questions with respect to when to allow for sharp devaluations. According to many, Argentina should have devalued its exchange rate years before it actually did in 2000. According to traditional empirical methods Argentina's per capita GDP would have grown about 2 percentage points less than it actually did had it followed this recommendation. The present paper puts a big question mark on this last result. Sharp devaluations should not be seen as a bad thing that should be avoided even at a high price. Even though mean and median e®ects, after controlling for a number of variables, are not statistically di®erent from zero at standard levels of con¯dence, there is a need to look further into the distribution of the e®ects and their interaction with other variables. For which countries under what circumstances are devaluations more likely to be expansionary rather than contractionary? Is there a di®erence for countries that have di®erent kinds of exchange rate regimes? Those are open questions that need to be addressed and that are beyond the scope of the present study.

Appendix: Data Sources and Construction

- GDP PER CAPITA ($\frac{GDP}{POP}$): GDP per capita in dollars of 1995. Obtained from the 2001 World Bank World Developing Indicators CD, code NY.GDP.PCAP.KD. This data was complemented with: i) Penn World Tables (PWT) 6.0 real per capita GDP chain method (1996 prices), and ii) GDP data in 1990 constant dollars obtained from the United Nations Statistical Division divided by Population data that was obtained from the US Department of Commerce. The implied rates of growth of these two additional sources were used to complete the WDI dataset. This variable is also used to construct the initial per capita GDP (explained below), the per capita growth rates (as logarithmic difference), the growth rate of the industrialized countries (all OECD countries except Korea and Mexico, GIND) and the output gap (as deviations from a trend obtained using a standard HP filter, GAP).
- NOMINAL EXCHANGE RATES: End of month exchange rates with respect to the US are obtained from the 2001 IFS CD-ROM provided by the IMF.
- INITIAL PER CAPITA GDP ($Log(\frac{GDP}{POP})$): Corresponds to the five years moving average of the per capita GDP. For a given period t the moving average ranges from $t - 5$ to $t - 9$.
- LIFE EXPECTANCY (LIFE): Corresponds to the historical life expectancy at birth. In other words, it indicates the number of years a newborn infant would live if prevailing patterns of mortality at the time of its birth were to stay the same throughout its life. Data provided by 2001 WDI CD-ROM. Missing data was filled with linear inter and extra-polation when possible.
- AVERAGE YEARS OF SCHOOLING (AYOS): Corresponds to the average year of school attainment of male 25 and over at the secondary and higher levels as reported by Barro and Lee [2000]. Missing data was filled with linear inter and extra-polation when possible.
- GOVERNMENT SPENDING (G): Obtained from WPT6.0 as the share of government spending to GDP (constant prices). This data was complemented with data from the 2001 WDI on Government Expenditure to GDP ratio.
- INFLATION (π): The annual rate of inflation of consumer prices use obtained from the 2001 WDI CD-ROM and complemented with annual variations in the WPT6.0 consumption price level. Monthly data was obtained from the IFS (annual data form the IFS and the WDI are the same, but the WDI data set is more complete).
- BLACK MARKET PREMIUM (BLACKMP): Corresponds to the official exchange rate to parallel exchange rate ratio in the 2001 WDI CD, code PA.NUS.FCRF.XR. It measures the premium people must pay, relative to the official exchange rate, to exchange the domestic currency for dollars in the black market.
- OPENNESS (OPEN): This variables corresponds to the WPT6.0 openness at current prices, measured as the ratio of the sum of exports and imports to GDP. The data was complemented with data from the WDI. Using this variable I also calculated the historical trend (OPENHP) using a standard HP filter.
- CIVIL WAR (CWAR): This is a dummy for years in which a country is undergoing a civil war. The variable is obtained from Dolley and Sambanis [2000].
- TROPICAL: This is a dummy variable for tropical countries as classified by the world bank in the Global Development Network Growth Database.

- EXPORTERS OF PRIMARY PRODUCTS (PRIMARYX): A dummy variable that takes the value of one for exporters of nonfuel primary products. This dummy is found in the Global Development Network Growth Database.

- TERMS OF TRADE (TOT): Terms of trade (goods and services, 1995 = 100) from the Global Development Finance (GDF), a World Bank publication. The data for the logarithmic change in this variable was complemented with the logarithmic change in the terms of trade reported by the IMF in the IFS 2001 CD.

- MONETARY SHOCK (MSHOCK): This is the residual from a Cagan type of demand for money, estimated using a 20 year window rolling regression. For each country I run $Log(M2)_t = \alpha_0 Log(GDP)_t + \alpha_0 \pi_t + Log(M2)_{t-1}$ where M2 is money and quasi money in current local currency from the 2001 WDI CD (code FM.LBL.MQMY.CN), GDP is the per capita GDP defined above multiplied by the total population and π is the inflation rate as defined above. The regression is run for the period 1960-1979 to generate the residuals for the period 1970-1979. Then I move the window a period forward (1961-1980) to generate the residual for the period 1980, and so forth. This series of residuals is the monetary shock variable. The rolling window is used to smooth out changes in the parameters.

- FINANCIAL FRAGILITY: Following Beck et al. [2000] I measure the robustness of the financial sector using the (log of) credit by deposit money banks and other financial institutions to the private sector divided by GDP. The credit to the private sector corresponds to lines 22d and 42d in the IFS. The variable is instrumented using legal origin as instruments. The legal origin can be found in the World Bank Global Development Network Growth Database, also reported by La Porta et al. [2000].

- REGIONS: Regions were classified according to the World Bank classification. According to the WB the globe is divided in 7 geographic regions: East Asia and Pacific, Europe and Central Asia, Latin America and the Caribbee, Middle East and North Africa, North America, South Asia, and Sub-Saharan Africa. Mexico was re-allocated from North America to the Latin America and the Caribbee classification.

- INVESTMENT (INV): Gross domestic fixed investment to GDP ratio is obtained from the WPT6.0 (calculated from constant price values).

- EXPORTS (X): Exports of goods and services in current US$ obtained from the WDI, code BX.GSR.GNFS.CD.

- EXTERNAL DEBT (DEBT): Total external debt in current US$ obtained from the WDI, code DT.DOD.DECT.CD. This variable was used to calculate both the debt to exports ratio and the level of over-indebtedness (OVERDEBT). The latter corresponds to deviation of the external debt to GDP ratio from its historical trend. The trend was calculated using a standard HP filter.

- REAL EXCHANGE RATE (RER): This index (1990 = 100) was constructed using annual average exchange rates, reference country's CPI and domestic CPI. The exchange rate data is described above. The CPIs were obtained from the WDI.

- REAL EXCHANGE RATE MISALIGNMENT (RERMIS): This is the deviation of the real exchange rate from its trend. The trend was calculated using a standard HP filter. Following Goldfajn and Valdés [1997] I calculated other more structural versions of the equilibrium real exchange rate, but the results are practically unchanged. In light of that I report the results using the simplest and more standard calculation, i.e. using the HP filter.

- REGIONAL CRISIS (DREG): Following Edwards [2001], and in order to control for contagion effects, this variable counts for each country during each year the number of large devaluations that occur in the same year and the same geographic region (excluding the country of reference). Geographic regions are in accordance to the World Bank definition of region, as described above.
- REGIONAL CAPITAL FLOWS (REGFLOW): Corresponds to the yearly private capital flows to the region divided by the year's regional GDP. Net private capital flows consist of private debt and non-debt flows. Private debt flows include commercial bank lending, bonds, and other private credits; non-debt private flows are foreign direct investment and portfolio equity investment. Data are in current U.S. dollars and is obtained from the 2001 WDI CD-ROM, code BN.KLT.PRVT.CD.DT. GDP in current U.S. dollars is obtained from the same source. The regions correspond to the World Bank regions as described above.

Notes

[1]See Diaz Alejandro [1965], Cooper [1971a], Krugman and Taylor [1978], van Wijnbergen [1986] for early support to that view. For a survey of the arguments see Edwards [1986, 1989].

[2]This episodic approach has advantages and drawbacks discussed in detail in Edwards [1989].

[3]Edwards and Santaella [1993] apply a similar methodological approach to investigate the di®erences between devaluations undertaken within the context of an IMF program and devaluations implemented without formal IMF-sponsored program during the Bretton Woods period. The authors look at the e®ects on the real exchange rate, the current account and net foreign assets.

[4]In studying the output e®ect of devaluations Cooper [1971b], Edwards [1989], Morley [1992] and Barro [2001] concentrate on sharp devaluations indicators. All these authors have found devaluations to be contractionary.

[5]See Heckman et al. [1997] and Blundell and Costa Dias [2000]. An application to labor economics can be found in Blundell, R., A. Duncan and C. Meghir [1998]. Matching estimators have been previously applied in the context of international economics by Persson [2001] and Edwards and Magendzo [2001].

[6]I use only currencies of developed countries as reference currencies. In a case such as Sri Lanka that the reference currency is the Indian Rupee, and the reference currency of the Indian Rupee is the US dollar, I take the reference currency of Sri Lanka to be the US dollar as well.

[7]See the Appendix for the de¯nition of geographical regions and other variables.

[8]Data sources are described in the Appendix.

[9]See Barro [1996], Caselli et al. [1996] and Beck et al. [1999].

[10]Di®erent empirical studies have di®erent results for education variables. As explained by Caselli et al. [1996]: \di®erent models lead to di®erent predictions on the expected sign of the coe±cient on the human capital variables". Barro [2001] found average year of school attainment of male 25 and over at the secondary and higher levels to be only marginally signi¯cant.

[11]See Rosenbaum and Rubin [1983] and Heckman et al. [1997].

[12]Some of these variables were found not to explain currency crises by Krueger et al. [2000].

[13]See Kaminsky and Reinhart [1999], Reagle and Salvatore [2000] and Kamin and Babson [1999].

[14]See also Krueger et al. [2000] for evidence on regional contagion.

[15]See Eichengreen et al. [1996]

[16]See Edwards [2001].

[17]Using the other equations yields similar results.

[18]The median growth rate of exports for devaluers is 1.7%, for the rest of the observations is 9.0% and it goes down to 3.7% for the matching observations.

[19]This estimator improves on kernel regression in two ways: a) the bias of the LLR estimator does not depend on the design density of the data (i.e. on the density f(P(x)); and b) the order of convergence is the same at the boundary points as at the interior points. For details see Fan [1992, 1993].

[20]As we increase the threshold level of in°ation we get two opposing e®ects on the number of episodes. First, some large devaluations coming from high in°ation periods will not be left out of the de¯nition. Second, large but milder devaluations become now part of the normal period and are more likely to qualify as large devaluations. The net e®ect can go either way.

References

Barro, R.J. (1996); \Determinants of Economic Growth: A Cross-Country Empirical Study", National Bureau of Economic Research Working Paper 5698.

| | | | (2001); \Economic Growth in East Asia Before and After the Financial Crisis", National Bureau of Economic Research Working Paper 8330.

| | | | and J. Lee, \International Data on Educational Attainment: Updates and Implications" CID Working Paper 42.

Beck, T., R. Levine and N. Loayza (2000); \Finance and the Source of Growth", Journal of Financial Economics 58, 261-300.

Bernanke, B.M., M. Gertler and S. Gilchrist (1998); \The Financial Accelerator in a Quantitative Business Cycle Framework", National Bureau of Economic Research Working Paper 6455.

Blundell, R., A and Costa Dias, M. (2000); "Evaluation Methods for Non-Experimental data," Fiscal Studies, 21, 427-468

| | | |, A. Duncan and C. Meghir (1998); "Estimating Labor Supply Responses using Tax Reforms", Econometrica 66(4), 827-861.

Caselli, F., G. Esquivel and F. Lefort (1996); \Reopening the Convergence Debate: A New Look at Cross-Country Growth Empirics", Journal of Economic Growth 1, 363-389.

C¶espedes, L.F., R. Chang and A. Velasco (2000); \Balance Sheets and Exchange Rate Policy", National Bureau of Economic Research Working Paper 7840.

Connolly, M. (1983); \Exchange Rates, Real Economic Activity and the Balance of Payments: Evidence from the 1960s" in E. Classen and P. Salin (eds.) Recent Issues in the Theory of the Flexible Exchange Rates, Amsterdam, North Holland.

Cooper, R. (1971a); \Currency Devaluation in Developing Countries", in G. Ranis (ed.) Government Economic Development, New Heaven, Yale University Press.

Cooper, R. (1971b); \Currency Depreciation in Developing Countries", Princeton Essays in International Finance 86.

Diaz-Alejandro, C.F. (1965); Exchange Rate Devaluation in a Semi-Industrialized Economy: The Experience of Argentina 1955-61, Cambridge, MIT Press.

Dornbusch, R. (1980); Open Economy Macroeconomics, 2^{nd} edition, New York: Basic Books.

Doyle, M. and N. Sambanis (2000); International Peacebuilding: A Theoretical and Quantitative Analysis, American Political Science Review 94 (4), 779-801.

Edwards, S. (1986); \Are Devaluations Contractionary?", The Review of Economics and Statistics 68(3), 501-08.

| | | | (1989); Real Exchange Rates, Devaluation, and Adjustment: Exchange Rate Policy in Developing Countries, Cambridge, MIT Press.

| | | | (2001); \Are Currency Crisis Regional?", UCLA Working Paper.

| | | | and I.I. Magendzo (2001); \Dollarization, In°ation and Growth", Working Paper, UCLA.

| | | | and J.A. Santaella (1993); \Devaluation Controversies in the Developing Countries: Lessons from the Bretton Woods Era", in M.D. Bordo and B. Eichengreen A Retrospective on the Bretton Woods System: Lessons from International Reform, Chicago, The University of Chicago Press, 405-60.

Eichengreen, B., A.K. Rose and C. Wyplosz (1996); \Speculative Attacks on Pegged Exchange Rates: An Empirical Exploration with Special Reference to the European Monetary System" in M.B. Canzoneri, B. Matthew, W.J. Ethier and V. Grilli (eds.) The New Transatlantic Economy, Cambridge; New York and Melbourne: Cambridge University Press, 191-228.

Fan, J. (1992); "Design-adaptive Nonparametric Regression (in Theory and Methods)", Journal of the American Statistical Association 87(420), 998-1004.

| | | | (1993); "Local Linear Regression Smoothers and Their Minimax E±ciencies (in Curve Estimation)", Annals of Statistics 21(1), 196-216.

Goldfajn, I. and R.O. Vald³es (1997); \Are Currency Crises Predictable?", International Monetary Fund Working Paper 159.

Gylfason, T. and M. Schmid (1983); \Does Devaluation Cause Stag°ation?"Canadian Journal of Economics 16(4), 641-54.

| | | | and O. Risager (1984); \Does Devaluation Improve the Current Account?", European Economic Review 25(1), 37-64.

| | | | and M. Radetzki (1985); \Does Devaluation Make Sense in the Least Developed Countries?" Economic Development and Cultural Change 40(1), 1-25.

Heckman, J.J., I. Hidehiko and P.E. Todd (1997); "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme", Review of Economic Studies 64(4), 605-54.

Hutchison, M.M. (2001); \A Cure Worse than the Disease? Currency Crises and the Output Costs of IMF-Supported Stabilization Programs", National Bureau of Economic Research Working Paper 8305.

Kamin, S.B. and O.D. Babson (1999); \The contribution of domestic and External Factors to Latin American Devaluation Crises: An Early Warning Systems Approach", Board of Governors of the Federal Reserve System International Finance Discussion Papers 645.

Kamin, S.B. and M. Klau (1997); \Some Multi-Country Evidence on the E®ects of Real Exchange Rates on Output", Board of Governors of the Federal Reserve System International Finance Discussion Papers 611.

Kaminsky, G.L. and C.M. Reinhart (1999); \The Twin Crises: The Causes of Banking and

Balance-of-Payments Problems", American Economic Review 89(3),473-500.

Krueger, A.O. (1978); Liberalization Attempts and consequences, Cambridge, Bellinger.

Krueger, M., P. Osakwe and J. Page (2000); \Fundamentals, Contagion and Currency Crises: An Empirical Analysis", Development Policy Review 18, 257-74.

Krugman, P.R. (1999); \Balance Sheets, The Transfer Problem, and Financial Crisis", in P. Isard, A. Razin and A. Rose eds. International Finance and Financial Crises, Boston: Kluwer Academic.

| | | | and L. Taylor (1978); \Contractionary E®ects of Devaluation", Journal of International Economics 8(3), 445-56.

La Porta, R., F. Lopez de Silanes, A. Shleifer and R. Vishny (2000); \Investor Protection and Corporate Governance", Journal of Financial Economics; 58(1-2), 3-27.

Levy-Yeyeti, E. and F. Sturzenegger (2002); \Classifying Exchange Rate Regimes: Deeds vs. Words", Working Paper Universidad Torcuato Di Tella.

Lizondo, J.S. and P.J. Montiel (1989); \Contractionary Devaluation in Developing Countries: An Analytical Overview", International Monetary Fund Sta® Papers 36(1), 182-227.

Morley, S.A. (1992); \On the E®ect of Devaluation During Stabilization Programs in LDCs", The Review of Economics and Statistics 74(1), 21-27.

Persson, T. (2001); \Currency unions and Trade: How Large is the Treatment E®ect?" IIE, April.

Reagle, D. and D. Salvatore (2000); \Forecasting Financial Crises in Emerging Market Economies", Open Economies Review 11, 247-59.

Rosenbaum, P. and D.B. Rubin (1983); \The Central Role of the Propensity Score in Observational Studies for Causal E®ects", Biometrika, 70, 41-55.

Sachs, J.D., A. Tornell and A. Velasco (1996); \Financial Crises in Emerging Markets: The Lessons from 1995", Brookings Papers on Economic Activity 0(1), 147-98.

Upadhyaya, K.P. (1999); \Currency Devaluation, Aggregate Output, and the Long Run: An Empirical Study", Economics Letters 64, 197-202.

| | | | and M.P. Upadhyaya (1999); \Output E®ects of Devaluation: Evidence from Asia", The Journal of Development Studies 35(6), 89-103.

van Wijnbergen, S.J.G. (1986); \Exchange Rate Management and Stabilization Policies in Developing Countries" in S. Edwards and L. Ahamed ed. Economic adjustment and exchange rates in developing countries, Chicago and London: University of Chicago Press, 17-38.

Table I:
Large Devaluation Episodes

Country	Years	Country	Years	Country	Years
Albania	92 97	Guatemala	86 89	Oman	86
Algeria	90 94	Guinea	86	Pakistan	72
Angola	91	Guinea-Bissau	83 87 91 96	Papua New Guinea	74 83 90 94 97
Argentina	70 75 82 89	Guyana	87	Paraguay	84 89
Armenia	93 97	Haiti	91	Peru	75 88 99
Azerbaijan	99	Honduras	90	Philippines	70 83 97
Bangladesh	75	Hungary	82 86 94	Poland	92
Barbados	72 75	India	91	Poland	98
Belarus	92 97	Indonesia	78 83 86 97	Romania	90 95
Belize	72 75	Iran, Islamic Rep.	93	Russian Federation	92 98
Benin	94	Jamaica	78 83 91	Rwanda	90 94
Bhutan	91	Jordan	88	Samoa	79 83 88
Bolivia	72 79 85	Kazakhstan	94 99	Sao Tome and P.	87 91 94
Botswana	71 75 82 89 96	Kenya	75 81 93	Senegal	94
Brazil	79 89 99	Korea, Rep.	74 80 97	Seychelles	72 75
Bulgaria	91 94 97	Kyrgyz Republic	93	Sierra Leone	83 89 95
Burkina Faso	94	Lao PDR	85 95	Slovak Republic	93
Burundi	76 83 86 91 97	Latvia	92	Solomon Islands	74 81 85 97
Cambodia	90	Lesotho	75 84 89 96	South Africa	75 84 89 96
Cameroon	94	Liberia	98	Sri Lanka	77 98
Cape Verde	89	Libya	93	Sudan	91 96
Chad	94	Lithuania	92	Suriname	94 99
Chile	71 82 85	Macedonia, FYR	97	Swaziland	75 84 89 96
China	75 85 89 94	Madagascar	82 94	Syria	88
Comoros	94	Malawi	85 92 97	Tanzania	92
Congo, Dem. Rep.	76 79 87 93	Malaysia	73 93 97	Thailand	81 84 97
Congo, Rep.	94	Maldives	87	Togo	94
Costa Rica	74 81 88	Mali	94	Tonga	83
Cote d'Ivoire	94	Mauritania	85 91 98	Trinidad and Tobago	85 88 93
Croatia	97	Mauritius	72 79 98	Tunisia	75 81 86 91 97
Czech Republic	97	Mexico	76 82 85 94	Turkey	70 78 84 91 94
Dominican Rep.	85 90	Moldova	92	Uganda	87
Ecuador	70 82 92 99	Mongolia	92 96	Ukraine	93 98
Egypt, Arab Rep.	79 89	Morocco	74 78 90	Uruguay	72 82
El Salvador	86 90	Mozambique	87	Vanuatu	85 89
Equatorial Guinea	94	Myanmar	91	Venezuela, RB	84 89 94
Ethiopia	92	Namibia	84 89 96	Vietnam	85
Fiji	72 87 98	Nepal	75 81 85 91	Yemen, Rep.	95
Gabon	94	Nicaragua	79 88 93	Zambia	85 92
Gambia, The	73 84	Niger	94	Zimbabwe	75 82 91 97
Georgia	98	Nigeria	86 92 99		
Ghana	71 78 83	Central Africa	94		

Table II:

Devaluation and Growth

	(A) Devaluers	(B) Other	(C) Difference* (A) - (B)
	A. All countries		
Obs	264	2,312	
Countries	123	139	
Mean	-0.77	1.34	-2.11
			(-4.72)
Median	0.48	1.95	-1.47
			(-3.98)
	B. Latin American Countries		
Obs	58	666	
Countries	23	31	
Mean	-2.16	2.01	-4.18
			(-6.41)
Median	-1.11	2.20	-3.31
			(-4.48)
	C. 1990's		
Obs	123	872	
Countries	95	139	
Mean	-2.44	0.89	-3.34
			(-4.34)
Median	-0.66	1.71	-2.37
			(-4.01)

*: Numbers in parentheses are t-statistics

Table III:
Regression Analysis: Random E®ects Estimation

Dependant Variable: ¢Log($\frac{GDP}{POP}$)

	(1)	(2)	(3)	(4)	(5)	(6)
Log($\frac{GDP}{POP}$)initial	-0.009	-0.01	-0.01	-0.011	-0.015	-0.013
	(4.34)**	(4.11)**	(3.56)**	(5.90)**	(2.68)**	(2.62)**
Log(LIFE)	0.065	0.081	0.054	0.086	0.053	0.111
	(4.09)**	(4.78)**	(2.68)**	(6.24)**	(1.23)	(3.88)**
Log(AYOS)	0.003	-0.001	0	0.006	0.006	0.010
	(1.04)	(0.41)	(0.07)	(2.07)*	(0.52)	(1.22)
G/Y	-0.062	-0.060	-0.059	-0.049	-0.098	-0.053
	(4.05)**	(3.77)**	(3.85)**	(3.68)**	(2.90)**	(1.46)
¼	-0.001	-0.001	-0.001	-0.001	0	-0.002
	(3.30)**	(3.55)**	(3.66)**	(3.61)**	(1.70)	(3.06)**
BLACKMP	-0.003	-0.004	-0.004	-0.004	-0.003	-0.003
	(3.92)**	(4.77)**	(4.11)**	(4.68)**	(2.74)**	(1.45)
OPEN	0.003	0.002	0.005	0.002	-0.024	-0.006
	(0.68)	(0.29)	(0.83)	(0.46)	(2.00)*	(0.67)
CWAR	-0.016	-0.019	-0.019	-0.013	-0.016	-0.021
	(4.16)**	(5.14)**	(5.27)**	(3.87)**	(2.64)**	(3.27)**
TROPICAL	-0.010	-0.008	-0.011	-0.007	-0.016	0.001
	(2.96)**	(2.07)*	(2.38)*	(2.45)*	(1.99)*	(0.08)
PRIMARYX	-0.012	-0.010	-0.009	-0.009	-0.01	-0.005
	(3.74)**	(2.62)**	(2.41)*	(3.23)**	(1.65)	(0.70)
¢Log(TOT)	0.018	0.004	0.003	0.003	0.017	-0.014
	(2.00)*	(0.55)	(0.42)	(0.33)	(1.28)	(0.79)
GIND	0.011	-0.003	-0.001	| |	-0.067	-0.008
	(0.36)	(0.12)	(0.02)	(1.53)	(0.29)	
$GAP_{¡1}$	| |	-0.462	-0.463	-0.468	-0.419	-0.408
		(17.47)**	(17.54)**	(17.23)**	(10.16)**	(7.14)**
MSHOCK	| |	-0.006	-0.009	-0.003	0.018	0.016
		(0.43)	(0.65)	(0.23)	(0.94)	(0.79)
FINAN	| |	0.094	0.062	0.080	0.188	0.139
		(3.07)**	(1.80)	(3.45)**	(1.54)	(2.52)*
D	-0.023	-0.019	-0.020	-0.016	-0.031	-0.021
	(5.19)**	(4.90)**	(5.14)**	(3.94)**	(4.98)**	(3.55)**
Constant	-0.156	-0.241	-0.122	-0.293	-0.090	-0.380
	(2.66)**	(3.67)**	(1.56)	(5.36)**	(0.54)	(3.40)**
Yearly Dummies	No	No	No	Yes	No	No
Regional Dummies	No	No	Yes	No	No	No
Observations	1358	1317	1317	1317	461	415
Number of Countries	74	72	72	72	21	72

Absolute value of z statistics in parentheses

* signi¯cant at 5%; ** signi¯cant at 1%

Table IV: Treated Countries and Nearest Neighbors

Tread Country		Nearest Neighbor With Replacement		Nearest Neighbor Without Replacement	
Country	Year	Country	Year	Country	Year
Algeria	1990	Kenya	1980	Kenya	1980
Algeria	1994	Madagascar	1991	Benin	1981
Argentina	1982	Mozambique	1994	Egypt, Arab Rep.	1988
Argentina	1989	El Salvador	1985	El Salvador	1985
Bangladesh	1975	Lesotho	1992	Lesotho	1992
Benin	1994	Mozambique	1986	Chad	1979
Bolivia	1979	Kenya	1978	Kenya	1978
Bolivia	1985	Ghana	1994	Mali	1993
Botswana	1982	Central African Republic	1992	Central African Republic	1992
Botswana	1989	Ethiopia	1985	Ethiopia	1985
Botswana	1996	Costa Rica	1991	Costa Rica	1991
Brazil	1979	Mauritania	1984	Kenya	1991
Brazil	1989	Nigeria	1989	Nigeria	1989
Burkina Faso	1994	Ukraine	1997	Mozambique	1994
Burundi	1991	Cote d'Ivoire	1989	Cote d'Ivoire	1989
Burundi	1997	Cote d'Ivoire	1982	Cote d'Ivoire	1982
Cameroon	1994	Zambia	1998	Zambia	1998
Central African Republic	1994	Egypt, Arab Rep.	1988	Mozambique	1986
Chad	1994	Guinea	1998	Rwanda	1993
Chile	1982	Benin	1981	Benin	1981
Chile	1985	Bolivia	1984	Benin	1993
China	1985	Pakistan	1976	Pakistan	1976
China	1989	India	1997	India	1997
China	1994	Congo, Dem. Rep.	1996	Congo, Dem. Rep.	1996
Congo, Dem. Rep.	1976	Cameroon	1997	Cameroon	1997
Congo, Dem. Rep.	1979	Gabon	1985	Korea, Rep.	1985
Congo, Dem. Rep.	1987	Syrian Arab Republic	1994	Syrian Arab Republic	1994
Congo, Dem. Rep.	1993	Malaysia	1978	Malaysia	1978
Congo, Rep.	1994	Burkina Faso	1992	Ghana	1982
Costa Rica	1981	Argentina	1981	Ukraine	1997
Costa Rica	1988	Ghana	1992	Ghana	1992
Cote d'Ivoire	1994	Mozambique	1998	Ghana	1982
Czech Republic	1997	Mauritius	1989	Mauritius	1989
Dominican Republic	1985	Ghana	1982	Ukraine	1997
Dominican Republic	1990	Papua New Guinea	1982	Togo	1982
Ecuador	1982	Lesotho	1981	Lesotho	1981
Ecuador	1992	Senegal	1982	Senegal	1982
Egypt, Arab Rep.	1979	Yemen, Rep.	1994	Gambia, The	1981
Egypt, Arab Rep.	1989	Ethiopia	1991	Zambia	1998
El Salvador	1986	Gabon	1987	Gabon	1987
El Salvador	1990	Cameroon	1989	Cameroon	1989
Ethiopia	1992	Haiti	1987	Ghana	1982
Gabon	1994	Zambia	1983	Rwanda	1993
Gambia, The	1984	Benin	1993	Benin	1993
Ghana	1978	Senegal	1998	Senegal	1998
Ghana	1983	Lebanon	1991	Argentina	1981
Guatemala	1986	Ethiopia	1997	Ethiopia	1997
Guatemala	1989	Nepal	1980	Nepal	1980
Guinea-Bissau	1987	Rwanda	1989	Ghana	1982
Guinea-Bissau	1991	Panama	1990	Panama	1990
Guinea-Bissau	1996	Central African Republic	1989	Central African Republic	1989

Table IV: Treated Countries and Nearest Neighbors

Tread Country		Nearest Neighbor With Replacement		Nearest Neighbor Without Replacement	
Country	Year	Country	Year	Country	Year
Haiti	1991	Argentina	1987	Central African Republic	1991
Honduras	1990	Zambia	1991	Ukraine	1997
Hungary	1986	Turkey	1997	Turkey	1997
Hungary	1994	Nepal	1979	Nepal	1979
India	1991	Ethiopia	1988	Syrian Arab Republic	1983
Indonesia	1983	Cote d'Ivoire	1992	Cote d'Ivoire	1992
Indonesia	1986	Benin	1987	Benin	1987
Indonesia	1997	Cameroon	1988	Cameroon	1988
Jamaica	1978	Jordan	1978	Jordan	1978
Jamaica	1983	Sri Lanka	1996	Sri Lanka	1996
Jamaica	1991	Burkina Faso	1998	Burkina Faso	1998
Jordan	1988	Centralal African Republic	1985	Centralal African Republic	1985
Kenya	1981	Rwanda	1983	Rwanda	1983
Kenya	1993	Chad	1993	Ukraine	1997
Korea, Rep.	1980	Congo, Rep.	1993	Congo, Rep.	1993
Korea, Rep.	1997	Romania	1987	Romania	1987
Lesotho	1984	Central African Republic	1993	Central African Republic	1993
Lesotho	1989	Nepal	1974	Nepal	1974
Lesotho	1996	Tanzania	1997	Tanzania	1997
Madagascar	1982	Botswana	1992	Botswana	1992
Madagascar	1994	Guinea	1997	Guinea	1997
Malawi	1985	Congo, Dem. Rep.	1992	Congo, Dem. Rep.	1992
Malawi	1992	Sierra Leone	1994	Sierra Leone	1994
Malawi	1997	Bolivia	1988	Bolivia	1988
Malaysia	1993	Panama	1996	Panama	1996
Malaysia	1997	Benin	1991	Benin	1991
Mali	1994	Benin	1989	Ghana	1982
Mauritania	1985	Gambia, The	1981	Mozambique	1995
Mauritania	1991	Trinidad and Tobago	1984	Trinidad and Tobago	1984
Mauritania	1998	Bolivia	1982	Bolivia	1982
Mauritius	1979	Madagascar	1992	Madagascar	1992
Mauritius	1998	Korea, Rep.	1984	Korea, Rep.	1984
Mexico	1982	Rwanda	1981	Rwanda	1981
Mexico	1985	Malaysia	1983	Malaysia	1983
Mexico	1994	Uruguay	1981	Uruguay	1981
Morocco	1978	India	1987	India	1987
Morocco	1990	Dominican Republic	1984	Dominican Republic	1984
Mozambique	1987	Ethiopia	1986	Ghana	1982
Nepal	1975	Burundi	1989	Burundi	1989
Nepal	1981	Mauritania	1983	Mauritania	1983
Nepal	1985	Trinidad and Tobago	1982	Trinidad and Tobago	1982
Nepal	1991	Syrian Arab Republic	1996	Syrian Arab Republic	1996
Nicaragua	1979	Niger	1989	Niger	1989
Nicaragua	1988	Zambia	1984	Ghana	1982
Nicaragua	1993	Central African Republic	1991	Central African Republic	1991
Niger	1994	Syrian Arab Republic	1987	Egypt, Arab Rep.	1988
Nigeria	1986	Nicaragua	1991	Argentina	1981
Nigeria	1992	Niger	1985	Niger	1985
Pakistan	1972	Mali	1989	Mali	1989
Papua New Guinea	1983	Senegal	1993	Senegal	1993
Papua New Guinea	1990	Korea, Rep.	1985	Korea, Rep.	1985

Table IV: Treated Countries and Nearest Neighbors

Tread Country		Nearest Neighbor With Replacement		Nearest Neighbor Without Replacement	
Country	Year	Country	Year	Country	Year
Papua New Guinea	1994	Dominican Republic	1974	Dominican Republic	1974
Papua New Guinea	1997	Congo, Rep.	1987	Romania	1987
Paraguay	1984	Kenya	1991	Kenya	1991
Paraguay	1989	Togo	1984	Togo	1984
Peru	1988	Algeria	1987	Ghana	1982
Philippines	1983	Burkina Faso	1993	Burkina Faso	1993
Philippines	1997	Syrian Arab Republic	1983	Syrian Arab Republic	1983
Poland	1992	Pakistan	1997	Pakistan	1997
Poland	1998	Uruguay	1997	Uruguay	1997
Romania	1990	Chad	1979	Chad	1979
Romania	1995	Korea, Rep.	1994	Korea, Rep.	1994
Rwanda	1990	Lebanon	1998	Lebanon	1998
Rwanda	1994	Togo	1978	Ghana	1982
Senegal	1994	Haiti	1990	Ukraine	1997
Sierra Leone	1983	Senegal	1977	Senegal	1977
Sierra Leone	1989	Mozambique	1995	Mozambique	1995
Sierra Leone	1995	Guatemala	1982	Guatemala	1982
South Africa	1996	Brazil	1983	Brazil	1983
Sri Lanka	1977	Philippines	1979	Philippines	1979
Sri Lanka	1998	Dominican Republic	1976	Dominican Republic	1976
Syrian Arab Republic	1988	Burundi	1994	Ghana	1982
Tanzania	1992	Gabon	1992	Gabon	1992
Thailand	1981	Chad	1981	Chad	1981
Thailand	1984	Korea, Rep.	1979	Korea, Rep.	1979
Thailand	1997	Chad	1992	Chad	1992
Togo	1994	Rwanda	1993	Rwanda	1993
Trinidad and Tobago	1985	Peru	1982	Peru	1982
Trinidad and Tobago	1988	Seychelles	1998	Seychelles	1998
Trinidad and Tobago	1993	Mauritania	1989	Mauritania	1989
Tunisia	1981	Gambia, The	1991	Gambia, The	1991
Tunisia	1986	Cote d'Ivoire	1986	Cote d'Ivoire	1986
Tunisia	1991	Colombia	1990	Colombia	1990
Tunisia	1997	Barbados	1990	Barbados	1990
Turkey	1978	Peru	1983	Peru	1983
Turkey	1984	India	1974	India	1974
Turkey	1991	Peru	1985	Peru	1985
Turkey	1994	Togo	1982	Togo	1982
Uganda	1987	Benin	1997	Benin	1997
Ukraine	1998	Zambia	1990	Ghana	1982
Uruguay	1982	Syrian Arab Republic	1997	Syrian Arab Republic	1997
Venezuela	1984	Chad	1989	Ghana	1994
Venezuela	1989	Mali	1993	Mali	1989
Venezuela	1994	Cameroon	1978	Cameroon	1978
Yemen, Rep.	1995	Cote d'Ivoire	1981	Ghana	1982
Zambia	1985	Rwanda	1988	Rwanda	1988
Zambia	1992	Algeria	1989	Algeria	1989
Zimbabwe	1982	Gambia, The	1993	Gambia, The	1993
Zimbabwe	1991	Guinea	1995	Gabon	1992
Zimbabwe	1997	Algeria	1979	Algeria	1979

Table V:

Propensity Scores: Random Effects Probit Estimation

Dependant Variable: Large Devaluation Dummy

	(1)	(2)	(3)	(4)
$\Delta \text{Log}(X)_{-1}$	-0.017	-0.017	-0.018	-0.018
	(4.47)**	(4.44)**	(4.58)**	(3.90)**
$(\frac{DEBT}{X})_{-1}$	0.025	0.030	0.045	0.043
	(1.27)	(1.47)	(2.39)*	(1.96)*
OVERDEBT	1.435	1.611	1.389	1.458
	(3.38)**	(3.66)**	(3.42)**	(3.24)**
Log(RER)	0.792	0.796	0.687	0.813
	(4.68)**	(4.63)**	(4.25)**	(4.00)**
RERMIS	4.283	4.373	4.190	4.726
	(8.51)**	(8.63)**	(8.56)**	(8.46)**
BLACKMP	0.125	0.122	0.122	0.103
	(2.56)*	(2.44)*	(2.57)*	(1.91)
OPEN	0.030	0.030	0.032	0.036
	(3.26)**	(3.20)**	(3.47)**	(3.49)**
OPENHP	-0.031	-0.031	-0.032	-0.036
	(3.20)**	(3.12)**	(3.34)**	(3.27)**
DREG	0.042	0.046	0.060	0.002
	(2.57)*	(2.74)**	(3.42)**	(0.10)
REGFLOW	-14.075	-17.286	-18.055	-18.579
	(2.55)*	(3.01)**	(2.94)**	(2.51)*
GAP_{-1}		3.334	4.473	5.236
		(1.96)*	(3.30)**	(3.55)**
Constant	-5.245	-5.271	-3.835	-10.423
	(6.33)**	(6.27)**	(4.36)**	(8.52)
Yearly Dummies	No	No	No	Yes
Regional Dummies	No	No	Yes	No
Observations	1269	1263	1263	1263
Number of countries	91	91	91	91

Absolute value of z statistics in parentheses

* significant at 5%; ** significant at 1%

Table VI:

Matching Estimators: E®ect of Large Devaluation on Per Capita Growth

N^0 of neighbors	Replacement		(1)	(2)	(3)	(4)
		Controls	151	151	151	151
		Mean	-0.60	0.17	-0.92	0.02
1	No		(-0.74)	(0.24)	(-1.34)	(0.02)
		Median	-0.21	1.11	-0.20	0.55
			(-0.29)	(1.83)	(-0.28)	(0.99)
		Controls	414	410	361	377
		Mean	-0.61	-0.44	0.04	0.68
5	Yes		(-0.99)	(-0.73)	(0.06)	(1.12)
		Median	0.39	0.20	0.38	0.80
			(0.65)	(0.39)	(0.53)	(1.78)

Numbers in parentheses are t-statistics

* signi¯cant at 5%; ** signi¯cant at 1%

Table VII:

More Matching Estimators: E®ect of Large Devaluation on Per Capita Growth

N^0 of neighbors	Replace-ment		(1)	(2)	(3)	(4)
		Controls	118	118	109	110
		Mean	-0.85	1.04	0.22	1.55
1	Yes		(-1.12)	(1.39)	(0.29)	(2.05)
		Median	-1.06	1.23	0.18	1.29
			(-1.46)	(2.82)	(0.23)	(2.33)
		Controls	755	755	755	755
		Mean	-1.60	-1.50	-1.67	-1.54
5	No		(-2.53)	(-2.40)	(-2.72)	(-2.44)
		Median	-0.95	-0.09	-0.54	-0.77
			(-2.10)	(-0.29)	(-1.27)	(-1.30)
		Controls	1,128	1,125	1,125	1,125
			weighted	weighted	weighted	weighted
Local Linear		Mean	-0.39	0.31	0.44	0.15
Regression			(-0.83)	(0.66)	(0.91)	(0.32)
		Median	0.32	0.76	0.88	0.54
			(0.69)	(1.90)	(1.53)	(1.06)

Numbers in parentheses are t-statistics

* signi¯cant at 5%; ** signi¯cant at 1%

Table VIII:

Matching Estimators Using Exclusion Restrictions

N^0 of neighbors	Replace-ment		(1)	(2)	(3)	(4)
a. Residuals from equation 1 in Table III						
		Controls	122	122	122	122
		Mean	0.001	0.002	0.001	-0.001
1	Yes		(0.523)	(0.559)	(0.274)	(-0.272)
		Median	0.002	0.005	0.002	-0.002
			(0.483)	(1.624)	(0.636)	(-0.817)
		Controls	338	320	286	293
		Mean	0.001	0.001	0.002	0.002
5	No		(0.338)	(0.337)	(0.587)	(0.558)
		Median	0.003	0.005	0.005	0.007
			(0.836)	(2.708)	(2.179)	(2.612)
b. Residuals from equation 2 in Table III						
		Controls	120	120	120	120
		Mean	-0.002	0.003	0	-0.002
1	Yes		(-0.377)	(0.755)	(0.081)	(-0.475)
		Median	0.002	0.002	0.005	-0.003
			(0.452)	(0.600)	(1.162)	(-1)
		Controls	337	319	282	293
		Mean	-0.003	0.001	0.001	0.000
5	No		(-0.820)	(0.285)	(0.222)	(-0.024)
		Median	-0.001	0.002	0.003	0.003
			(-0.406)	(0.421)	(0.647)	(1.032)

Numbers in parentheses are t-statistics

* signi¯cant at 5%; ** signi¯cant at 1%

Table IX:
Changing the De¯nition of Devaluation

N^0 of neighbors	Replace-ment		(1)	(2)	(3)	(4)
a. 3.0 Standard deviations in equation 5						
		Controls	136	136	136	136
		Mean	-0.006	0	-0.005	-0.004
1	Yes		(-0.730)	(-0.041)	(-0.764)	(-0.525)
		Median	-0.005	-0.006	-0.009	0.000
			(-0.830)	(-0.787)	(-1.126)	(-0.004)
		Controls	359	347	314	329
		Mean	-0.009	0.007	0.001	0.004
5	No		(-1.367)	(1.063)	(0.221)	(0.668)
		Median	0.001	0.007	0.007	0.006
			(0.196)	(1.201)	(1.192)	(1.159)
b. 2.0 Standard deviations in equation 5						
		Controls	174	174	174	174
		Mean	-0.002	-0.001	0.002	-0.001
1	Yes		(-0.239)	(-0.134)	(0.224)	(-0.082)
		Median	0.003	0	0.003	-0.004
			(0.346)	(0.066)	(0.384)	(-0.629)
		Controls	427	446	444	392
		Mean	-0.005	0.003	0.000	0.004
5	No		(-0.830)	(0.500)	(0.059)	(0.759)
		Median	0.005	0.004	0.008	0.007
			(0.769)	(0.746)	(2.054)	(1.132)
c. Devaluations over 15%						
		Controls	147	147	147	147
		Mean	-0.013	-0.010	-0.010	-0.010
1	Yes		(-1.726)	(-1.377)	(-1.240)	(-1.257)
		Median	-0.008	-0.002	-0.012	-0.006
			(-0.859)	(-0.190)	(-1.434)	(-0.732)
		Controls	347	337	289	311
		Mean	-0.016	-0.006	-0.004	-0.003
5	No		(-2.504)	(-0.907)	(-0.670)	(-0.437)
		Median	-0.011	-0.002	0.002	0.001
			(-2.360)	(-0.240)	(0.422)	(0.200)

Numbers in parentheses are t-statistics
* signi¯cant at 5%; ** signi¯cant at 1%

Documentos de Trabajo Banco Central de Chile	Working Papers Central Bank of Chile
NÚMEROS ANTERIORES	PAST ISSUES

La serie de Documentos de Trabajo en versión PDF puede obtenerse gratis en la dirección electrónica: http://www.bcentral.cl/Estudios/DTBC/doctrab.htm. Existe la posibilidad de solicitar una copia impresa con un costo de $500 si es dentro de Chile y US$12 si es para fuera de Chile. Las solicitudes se pueden hacer por fax: (56-2) 6702231 o a través de correo electrónico: bcch@bcentral.cl.

Working Papers in PDF format can be downloaded free of charge from: http://www.bcentral.cl/Estudios/DTBC/doctrab.htm. Printed versions can be ordered individually for US$12 per copy (for orders inside Chile the charge is Ch$500.) Orders can be placed by fax: (56-2) 6702231 or e-mail: bcch@bcentral.cl.

DTBC-181 — Septiembre 2002
Política Monetaria y Mecanismos de Transmisión: Nuevos Elementos para una Vieja Discusión
Verónica Mies, Felipe Morandé y Matías Tapia

DTBC-180 — Septiembre 2002
Turnover and Regulation:The Chilean Pension Fund Industry
Solange Berstein y Alejandro Micco

DTBC-179 — Septiembre 2002
Empalme PIB: Series Anuales y Trimestrales de 1986 a 1995, Base 1996. Documento Metodológico
Víctor Correa, Antonio Escandón, René Luengo y José Venegas

DTBC-178 — Agosto 2002
An Evaluation of Monetary Regime Options for Latin America
Andrew Berg, Eduardo Borensztein y Paolo Mauro

DTBC-177 — Agosto 2002
Desestacionalización de Series Económicas: El Procedimiento Usado por el Banco Central de Chile
Héctor Felipe Bravo, Leonardo Luna, Víctor Correa y Francisco Ruiz

DTBC-176 — Agosto 2002
Is There Enough Evidence Against Absolute Convergence?
Rómulo A. Chumacero

DTBC-175 Agosto 2002
Dollarization and Economic Performance: What do We Really Know?
Sebastian Edwards e I. Igal Magendzo

DTBC-174 Agosto 2002
Productivity and Economic Growth: The Case of Chile
Harald Beyer B. y Rodrigo Vergara M.

DTBC-173 Agosto 2002
Greenfield FDI vs. Mergers and Acquisitions: Does the Distinction Matter?
César Calderón, Norman Loayza y Luis Servén

DTBC-172 Julio 2002
15 Years of New Growth Economics: What Have We Learnt?
Xavier Sala-i-Martin

DTBC-171 Julio 2002
Inflation Targeting in Brazil, Chile, and Mexico: Performance, Credibility, and the Exchange Rate
Klaus Schmidt-Hebbel y Alejandro Werner

DTBC-170 Julio 2002
Evaluation of the P900 Program: A Targeted Education Program for Underperforming Schools
Andrea Tokman

DTBC-169 Julio 2002
Industrial Policies and Growth: Lessons from International Experience
Marcus Noland y Howard Pack

DTBC-168 Julio 2002
Quantity and Quality of Economic Growth
Robert J. Barro

DTBC-167 Julio 2002
Monetary Union: European Lessons, Latin American Prospects
Eduard Hochreiter, Klaus Schmidt-Hebbel y Georg Winckler

DTBC-166 Junio 2002
Monetary Policy Implementation and Results in Twenty Inflation-Targeting Countries
Klaus Schmidt-Hebbel y Matías Tapia